The (Conditional) Resource Dilution Model:

State- and Community-Level Modifications

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Abstract

One of the most consistent patterns in the social sciences is the relationship between sibship size and educational outcomes: those with fewer siblings outperform those with many. The Resource Dilution (RD) model emphasizes how parental resources are increasingly divided within the nuclear family as the number of children grows, yet fails to account for instances where the relationship between sibship size and education is often weak or even positive. To reconcile, we introduce a Conditional Resource Dilution (CRD) model to acknowledge how nonparental investments might aid in children's development and condition the effect of siblings. We revisit the *General Social Surveys (1972-2010)* and find support for a CRD approach—the relationship between sibship size and educational attainment has declined during the first half of the 20th Century and this relationship varies across religious groups. Findings suggest that state and community resources can offset the impact of resource dilution—a more sociological interpretation of sibship size patterns than the traditional RD model.

The inverse association between number of siblings and years of educational attainment has been among the most consistently observed empirical patterns in the social sciences. To date, the Resource Dilution (RD) model remains the dominant explanation. Simply put, RD posits that parental resources are finite and additional children reduce the proportion of resources any one child accrues, resulting in fewer years of education. RD provides a simple, intuitive, and generalizable explanation for why children in larger families have noticeably lower levels of education than children in smaller families. Clearly parents' resources are limited and as most parents experience, raising additional children divides those resources.

Although considerable social science research has assessed the relationship between number of siblings, parental resources, and educational outcomes, relatively less theorizing has accompanied this growth in empirical studies. And while modifications to RD have been proposed by Blake (1981) and others (Powell et al. 2004; Downey 2001), these efforts have not produced a systematic alternative theory.

In addition, evidence is mounting that challenges the resource dilution model. Research findings from countries outside the U.S. reveal how the association between siblings and educational outcomes varies in magnitude (Anh et al. 1998; Chernichovsky 1985; Lu and Treiman 2008; Maralani 2008; Marteleto and de Souza 2012; Park 2008; Sudha 1997; Xu 2008). Interestingly, some scholars have even found a positive relationship between sibship size and education attained in Kenya (Buchmann 2000; Gomes 1984) and Botswana (Chernichovsky 1985). If siblings *do not always reduce* educational attainment, how are we to understand the RD model? From the RD perspective, large families should strain family resources and reduce educational outcomes in all contexts.

Below we discuss components of the RD perspective and then introduce an alternative, the *Conditional Resource Dilution* (CRD) model. The key conceptual improvement is that the CRD model recognizes state- and community-level contexts that influence the degree to which sibship size

strains resources in large families while, in contrast, the RD model is *acontextual*—focusing exclusively on parental resources. To demonstrate the utility of this new perspective, we revisit the *General Social Surveys*—one of the key data sources used to substantiate RD—and reveal important variations in the relationship between sibship size and educational attainment over time and among religious groups.

RESOURCE DILUTION

The resource dilution model was succinctly described by Blake in her 1981 *Demography* article, "Family Size and the Quality of Children."[1] Parental resources are: (1) settings—types of homes, necessities of life, and cultural objects (such as books, pictures, music, and so on); (2) treatments—personal attention, intervention, and teaching; and (3) opportunities—chances to engage the outside world or, from a child's perspective, "to get to do things" (Blake 1981). Thus, "the more children, the more these resources are divided (even taking into account economies of scale) and hence, the lower quality the output" (Blake 1981: 422).[2]

The RD model emphasizes the primacy of parental resources, downplaying the possibility that other actors—whether inside or outside the home—play a significant role in child

[1] The roots of resource dilution could be traced to Dumont's "law of capillary action" (Dumont, 1890), but Blake (1986) is widely considered the first scholar to use the term.

[2] Note the overlap between SRD and the quantity-quality model of fertility (Becker and Tomes 1976). The proponents write: "an increase in the quantity of children raises the cost or shadow price of the quality of children" (p. 143).

development.[3] For example, the inflexibility of the RD model can be illustrated by Blake's discussion of sibling interactions in the home (1981). Although Blake considered the possibility that siblings might, under some conditions, serve as resources rather than mere competitors, she ultimately rejects this possibility, noting "[the model] does not allow for the assumption that parents may create junior executives out of older children—pseudo parents on whom they can rely." (p.422). And later she writes: "the notion that older siblings typically, and on average, function in loco parentis assumes too much about sibling goodwill and maturity" (Blake 1989:12).

In addition to dismissing siblings as potential resources, Blake (1989) considers other confounding factors, such as extrafamilial support in Catholic communities, as only modest in effect. Thus, RD focuses on nuclear family processes, creating what we term a "strict resource dilution" (SRD) model where internal dynamics among siblings, the inputs of grandparents, aunts, uncles, and cousins and the impact of the larger community are downplayed or altogether ignored. We posit that SRD unnecessarily limits the scope and value of a RD explanation for the association between sibship size and education.

Evidence for the Strict Resource Dilution Model

Despite this theoretical weakness, scholars have uncovered considerable empirical evidence that can be viewed as consistent with a SRD explanation. In the United States, Blake (1989) performed the most expansive analysis to date. She assessed the relationship between sibship size and years of education attained in every available large-scale study at the time: *The General Social Surveys, 1972-1986*, *Occupational Changes in a Generation, 1962 and 1973*, the 1955 *Growth of American*

[3] A less common phasing of the RD model is "resource depletion theory" (e.g. Fingerman et al. 2009).

Families study, the 1970 *National Fertility Study,* the *Health Examination Survey, Youth in Transition,* and *High School and Beyond*. Her analyses revealed a consistent pattern: those with fewer siblings tended to attain more years of education than those with many. A wide range of additional studies in the U.S replicate the inverse relationship between number of siblings and educational outcomes (Alwin 1991; Blau and Duncan 1967; Featherman and Hauser 1978; Gailbraith 1982; Kidwell 1981; Kuo and Hauser 1997; Mercy and Steelman 1982; Parcel and Menaghan 1994; Steelman and Powell 1989). Further supporting this position, Downey (1995) analyzed data from the *National Education Longitudinal Study* to show just how RD likely operates in the home—when sibling size increased, parents saved less money for college, the frequency of talking in the home about school-related matters declined, as did the presence of computers in the home and other educational objects. Most importantly, he showed that these specific resources mediated the inverse relationship between sibship size and educational outcomes (Downey 1995).

Not only is the inverse relationship between sibship size and educational outcomes consistent, its magnitude is noteworthy too, especially when compared to other explanatory variables that have received proportionately more attention (McHale et al. 2012). For example, early status attainment researchers found the effects of sibship size were stronger than those for paternal occupational status (Blau and Duncan 1967; Featherman and Hauser 1978). Blake (1989) reported effect sizes of about .2 standard deviations, which typically exceeded those for all other familial variables (other than parental education). Similarly, Powell and Steelman (1993) estimated the effect of sibship size on high school graduation and college attendance, reporting that its magnitude was stronger than that of family income, gender, race, and parental structure.[4]

[4] Sibship size associations are generally stronger for educational outcomes such as years of education attained, high school and college graduation, but weaker for cognitive skills (Steelman et al. 2002).

Of course, scholars debate whether this association is causal (e.g., Guo and Van Wey 1999a; 1999b), a point we discuss more later, but they also question whether its simplicity can handle the nuance of empirical patterns (Downey 2001; Steelman et al. 2002). More work is needed in this regard. Marteleto (2010) notes that "rather than seeking uniformity [across empirical results]…more case studies and comparative studies are needed to disentangle the mechanisms behind the dynamic association between family size and educational attainment" (p.440). As we review next, the list of empirical studies that challenge the SRD model outside the U.S. is growing. And if we can also demonstrate that sibsize/educational attainment is modified by context in the U.S. case, we argue that a formal, more universal adjustment to SRD is warranted—one that both preserves the basic concept of resource dilution while identifying the key contextual characteristics that condition its effect.

CONDITIONAL RESOURCE DILUTION

Evidence that the relationship between sibship size and educational attainment varies by context has been mounting over the last two decades.[5] For example, the association is weak in Vietnam (Anh et al. 1998), and positive in Kenya (Buchmann 2000; Gomes 1984), Botswana (Chernichovsky 1985) and Brazil (Marteleto and de Souza 2012) (although the positive relationship in Brazil is evident only until the mid-1990s). In China, the relationship varies by rurality (Li et al. 2008) and, like Brazil, time period (Lu and Treiman 2008).

[5] And even without disconfirming evidence, concerns about spuriousness, at least for sibship and intelligence, have been debated (Guo and VanWey 1999b; Rodgers et al. 2000; see Steelman et al. 2002).

There is also evidence of group- and family-level differences within countries. For example, Shavit and Pierce (1991) replicated the typical inverse relationship between number of siblings and educational attainment among Ashkenazi and Oriental Jews, but not among Muslim Arabs. And studies in China (Ye and Wu 2011) and Taiwan (Parish and Willis 1993; Chu et al. 2007) demonstrate how state policies can disrupt within-family resources, like the way that gender and sibling spacing shape educational attainment. Taken together, this body of research demonstrates variation in the relationship between sibship size and educational attainment across several countries beyond the U.S. case.

To reconcile this literature with evidence of RD in the U.S., we suggest that understanding how siblings matter for educational attainment depends on a more contextual and flexible conceptual framework—what we term the *Conditional Resource Dilution* (CRD) model. While still maintaining the general tenet of resource dilution—parental resources are diluted as sibship size increases—CRD is sensitive to contextual features, *specifically the degree to which children's development is a product of factors other than parental investments.*

Blake's Resource Dilution model was developed within a culturally specific context—one dominated by the nuclear family. As a result, it provides little guidance for thinking about the relationship between siblings and outcomes in, for example, polygamous cultures or those practicing child fosterage. Even in cultures characterized by nuclear family structure, the model fails to acknowledge important variations in the extent to which children's development is shared by nonparents. We argue that resource dilution is correct in noting that parents play a critical role in children's life chances, but misplaced in its emphasis on parents as playing the *only* role.

Our proposed CRD model recognizes that dilution processes likely occur within families, but it embeds families within larger contexts. To borrow from Bronfenbrenner's ecological framework for human development (1979), Blake's RD model is a microsystem approach derived

from a unique historical period in the United States. This approach is not unreasonable—children's development is primarily a result of parental investments. Yet, allowing for other meaningful influences provides a more accurate understanding of both parental and non-parental influences.

For example, the meso- and macrosystems surrounding family life (the community and the larger political economy) can also be influential, potentially conditioning the influence of nuclear family resources (Bronfenbrenner 1979). Consider the difference between two societies—society A has publicly funded schooling while society B does not. In society A, the child's development is affected in important ways by public education, mitigating nuclear family processes because the costs of education are reduced. In society B, public schools do not exist and so nuclear family processes are more salient because educational development is tightly linked to resources available in the home. We posit that how sibship size matters depends on these broader societal characteristics.

With no known study demonstrating how the sibsize/educational attainment relationship varies in the U.S. case, we borrow two themes from the international literature: both state- and community-level investments in children can modify the impact of parental resources, sibship size and subsequent educational levels of children. Although there are other considerations (e.g. sibling spacing, gender of child) (see Chu et al. 2007), we focus on how countries (state) and faith-based and ethnic communities might exert significant influence on the degree to which family resources matter for a child's educational goals. While several authors have hinted at such a model (Park 2008; Xu 2008; Shavit and Pierce 1991; Lu and Treiman 2008; Maralani 2008), our contribution is to formally modify the RD model and then to test it in the U.S. context.

State Investments and Nuclear Family Dilution

In the context of the United States, Powell et al. (2004) provide a thoughtful discussion of how the centrality of the nuclear family for childhood development is affected, in part, by the extent

to which the state shares the parenting burden. The authors highlight a wide range of education and social welfare policies that have direct and indirect effects on how the number of siblings shape individual's educational outcomes. They conclude that "when access to services is socialized, or at least widely available and affordable, we should expect to see weaker effects of factors like large family size…" (Powell et. al 2004:130). Our CRD model is in accord with this claim, positing that parents' resources matter less when external resources (e.g., state investments) are increased.

This pattern is evident in China, where from 1966-1976 there was virtually no relationship between sibsize and educational attainment likely due to the Cultural Revolution that led to egalitarian school policies designed to increase the enrollment of disadvantaged populations (Lu and Treiman 2008). Conversely, in the periods before and after (1950-1965 and 1977-1996), Lu and Treiman (2008) find the association between sibsize and educational was strong. The authors suggest that the stronger relationship was due to an economic collapse and a nationwide famine in the period prior to the Cultural Revolution that, in turn, depleted family resources. In the period after the Cultural Revolution the relationship strengthened, most likely because of educational decentralization (that raised the cost of education) and land use policies that incentivized poor families to farm, thereby increasing the value of children's labor over schooling (Lu and Treiman 2008).

Other cross-cultural evidence is in accord. For example, two studies making cross-national comparisons using PISA data found that the association between sibship size and adolescent literacy scores was weaker in nations with more progressive social policies (Park 2008; Xu 2008). Park (2008) pinpoints the kinds of government policies that were most commonly associated with weak sibship effects. He concluded that countries with "stronger public support for childcare, universal child benefits, larger public expenditures on education and family, show a much less negative effect

of growing up in large families" (2008: 874). Others have also that found sibship size effects are more pronounced when public goods are scarce (Desai 1995; Lu and Treiman 2008; Sudha 1997).

Despite this large literature, there is surprisingly little research examining whether the public policy context shapes the sibship size patterns in the U.S., although there are reasons for suspecting that it does. For example, the link between social origins and educational attainment has changed over time in the United States, weakening from the early 1900s to the 1950s (Boudon 1976; Breen and Jonsson 2005) and 60s (Biblarz et al. 1996; Grusky and DiPrete 1990; Hauser and Featherman 1977; Hout 1988) and as late as the 1980s by some estimates (Mayer and Lopoo 2005). (There are some signs of reversal beginning in the 1980s and 90s, see Aaronson and Mazumder 2008 and Mayer and Lopoo 2005). During the period of "educational mobility" during the first half of the century, important shifts in the economy were well underway (Breen and Jonsson 2005), public education expanded and the costs of higher education decreased, most notably during the 1940s, 50s and 60s. This was also a time when the GI bill sent millions to college (Bound and Turner 2002) and Pell Grants and other forms of federal aid for college were created, broadening educational opportunities (Dynarski 2003).

The size of the welfare state also increased during much of the twentieth century. At the century start, the welfare state was relatively modest, just 7.5 % of Gross Domestic Product (GDP) in 1913, prior to WWI (Tanzi and Schuknecht 2000). In the 1930s, the modern welfare state emerged, spawning numerous social programs: Social Security (1935), Aid to Families With Dependent Care (1940), Food Stamps (1964), Head Start (1965), Medicare (1966), and Women, Infants, and Children (WIC) (1972). Government expenditures on such programs expanded to 27 % of GDP in 1960 (Tanzi and Schuknecht 2000), with continued growth in the years following (32.4 % of GDP in 1996).

The decades after midcentury growth were marked by a period of emerging skepticism regarding the government's ability to effectively address social welfare issues (Tanzi and Schuknecht 2000; e.g. Murray 1984). This skepticism mirrored the trend-line of educational expansion. Overall, the median years of education attained increased from 7.4 at the beginning of the century to 13.8 at the end (Fischer and Hout 2006), but public support for investments waned in the 80s and 90s. These trends, coupled with recent increases in economic immobility (Aaronson and Mazumder 2008) and income inequality (Atkinson et al. 2011), suggest that the state increasingly shared the burden of parenting and so the impact of large families on resources for schooling likely declined over most of the 20th century (although this pattern may have leveled off in the most recent decades).

The Community as a Buffer

How much parents matter also depends on the local community. In contexts where the community plays a significant role in children's development, we expect a more modest sibship size relationship to years of education. There are several mechanisms by which extra-familial resources may operate. For example, in faith communities that normatively support high fertility (i.e. Catholic, Mormon, Muslim), other families may deliberately share clothes, toys, educational resources, and childcare as a way of easing the burden of raising many children.[6] This aligns with Shavit and Pierce (1991), who found that the size of the nuclear family did not deter educational attainment for Muslims, especially when size of the hamula (extended kinship) in their broader community was large.

[6] Of course, pro-fertility communities likely support parents of both large and small families, but this help may matter more for children in large families, where parental resources are stretched thin.

Even Blake (1989), a leading advocate of the dilution model, recognized the possibility that community-level factors might challenge a strict resource dilution approach. She acknowledged that the Catholic Church and surrounding communities provide special support to large families, potentially mitigating dilution processes. For example, she noted that Catholic schools sometimes charge less for additional siblings or even charge on a per family rather than per child basis and that during the Depression, parochial schools often accepted the children of the unemployed without charge. But, to assess these potential mitigating factors, Blake analyzed the *General Social Surveys 1972-1986* and found that the negative effect of sibship size on educational attainment was only marginally smaller among Catholic versus Protestant families. We argue that evidence of CRD may be more apparent, however, by broadening the analysis to other religious groups, specifically Mormons (otherwise known as Latter-day Saints).

The Mormon Case. Mormons have the largest families among major religious groups in America, with 21% of self-identified Mormons with three or more children compared to 15% of Muslims, 11% of Catholics, 8% of Jews, and 6% of Protestants (the national average is 9%; Pew Research Center 2008).[7] Thus, Mormons offer a unique case to examine how large families might offset the strains of resource dilution.

[7] Interestingly, as the relationship between education level and expected number of children is negative for the average American, the expected number of children does not vary as education levels increase among Mormons (Heaton et al. 2004). Specifically, using the *General Social Survey*, Heaton et al. find (2004) a slight increase from 3.5 expected children among Mormon high school graduates (2.5 U.S. average) to 4.0 among individuals with a graduate degree (2.0 U.S. average).

There are many reasons to believe that Mormon children receive substantial investments from local community members, thereby buffering the typically observed negative consequences of large sibships. For example, there are extensive resource transfers among families in the Mormon community (Cooper 2014; Curtis et al. 2013, Dunn 1996, Mangum and Blumell 1993, McBride 2007). The most common form of support is provided through tithes (the donation of 10% of family income to the church). According to the Pew Research Center, nearly 80% of Mormons indicate that they tithe (2012).[8] Compared to Catholics and Protestants, Mormons contribute a considerably higher percentage of their incomes to their church organizations (Hoffmann et al. 2010; see also Dahl and Ransom 1999). In addition to tithing contributions, Mormon families are asked to provide a monthly fast offering that is dispersed by the church to members in need. These funds provide extensive social services available within the church including employment centers, access to food and clothing, thrift stores, counseling services, and direct cash assistance (Dunn 1996; Mangum and Blumell 1993).[9]

[8] Curtis et al. (2013) find that tithing contributions are more likely among lifelong Mormons than converts.

[9] Unlike other denominations, the LDS congregation size is capped around 600 members with membership in a given ward determined by preset "ward boundaries" (Chaves 2006). This results in two potentially advantageous outcomes for families in need. There are no large Mormon "megachurches" (which would likely limit interaction with leadership), and because ward boundaries are typically drawn to include a socioeconomically diverse membership, wards are more socioeconomically diverse than they might otherwise be if members were to choose their own congregations.

Another form of community support is through its youth program. Mormon youth enjoy extensive support from other Mormon adults (non-parents) (Dean 2010; Smith and Denton 2005). In the *National Survey of Youth and Religion* (2002-2003), 22% of Mormon teens reported having 10 or more "adults in a religious congregation or religious youth group attending teens can turn to for support, advice, and help (not including parents)" (Smith and Denton 2005). Youth in other religious groups are less likely to report such support from other adults (7% of mainstream Protestant, 4% of Catholic, and 2% of Jewish youth).[10]

Given that the Mormon Church has a rotating lay clergy (Davies 2003), nearly every member of a Mormon congregation has a "calling" or assignment within the church, often to work with youth (Davies 2003; Ludlow 1992; McBride 2007; Taber 1993). As assignments are rotated, youth have exposure to many different adults in the congregation. This is important to consider, because youth with mentors obtain higher levels of education than youth without mentors (Erickson et al. 2009). Specifically, Erickson and Phillips (2012) found and that the link between adolescent religiosity and college enrollment is partly mediated by *religious* mentors and that Mormon youth have the highest likelihood of religious mentorship relative to other religious groups. And unlike most other religious denominations, increased religiosity among Mormons is associated with higher (rather than lower) levels of education (Albrecht 1998; Albrecht and Heaton 1998). Thus, Mormon religious mentors are likely to have higher pro-educational values compared to less committed members, which likely benefits the educational outlook of the youth they serve.

EXTENDING PAST LITERATURE

[10] We should note, as with most studies of Mormons, these statistics are generated from a small number of cases (n≈50).

We extend our understanding of how sibship size matters by introducing a key conceptual distinction between our model (Conditional Resource Dilution) and the ideas that have dominated sibship size research. Our contribution is simple: we posit that within-family dilution processes are contingent on the degree to which children's development is promoted by state- and community-level investments. As the literature suggests, when the burden of parenting is broadly shared, we expect within-family dilution to be less consequential. To provide an empirical testof this claim, we first examine the association between sibship size and educational attainment across most of the 20th century in the U.S., when state investments in children's development increased. Second, we explore the extent to which religious affiliation in childhood can offset the relationship between sibsize and educational attainment.

METHODS

Sample. We employ data from the General Social Surveys (GSS), a series of surveys conducted nearly annually from 1972-2010. The GSS surveys are nationally representative of adults 18 or older living in non-institutionalized settings. The GSS sample was selected through a multi-stage sampling design. Households were sampled within Metropolitan Statistical Areas and each adult member of a selected household was given an equal probability of being interviewed. Interviews were conducted in person during March of each survey year (Davis et al. 2009).

Of the original 55,087 cases, we placed several restrictions on the sample. First, we exclude sample individuals who were younger than 25 years of age at the time of interview (n=4,428) as they may still be completing their education. To focus our analyses and limit the impact of outliers, we also excluded individuals born before 1900 and after the 1970s (n=2,439). A final restriction

excluded outlier cases where individuals indicated 24 or more siblings (including foster, adopted, and step-siblings). This resulted in only 40 dropped cases.[11]

To account for missing data, we used multiple imputation (Allison 2002). Many of the variables had missing values ranging between 1 percent (i.e. parent education level, family structure, population size of residence) and 30 percent of the sample (i.e. family income, occupational prestige of the father). We avoided traditional missing data treatments (i.e. listwise deletion) because biased parameter estimates may result (Allison 2001; Enders 2010). We generated 20 imputed data sets, each using all variables from the models reported in the study, and analyzed the subsequent data by implementing Stata's *mi* commands (Stata Corp. 2013). After imputations, the total sample size is 46,397.

Measures. We gauge sibship size with the following question, "How many brothers and sisters do you have? Please count those born alive, but no longer living, as well as those alive now. Also include stepbrothers and stepsisters, and children adopted by your parents."[12] We include this

[11] Analyses have been performed with and without these restrictions with little change in the estimates. For the sibling measure, the 24 sibling cut-point is admittedly arbitrary but did represent a slight drop in the number of cases moving from 23 (n=14) to 24 siblings (n=4) reported. We also analyzed the sibling variable as categorical (0, 1, 2, 3, 4 compared to 5 or more) with similar declining associations of educational attainment across decades of birth.

[12] Note that this measure does not allow us to distinguish between siblings living in the household versus not, or to determine precisely how long a particular sibling lived in the home. For example, an individual may have a stepsibling who did not become part of the family until after the respondent completed their education.

variable in the model as a continuous measure. We considered other modifications of the sibling variable (e.g. dichotomous variables for each category of 0 to 5+ siblings) with no discernable change to our findings.

We measure respondent's education with their self-reported number of schooling years completed and determine the respondent's religious affiliation with the question, "In what religion were you raised?" We distinguish among individuals that self-identify as being raised Protestant, Jewish, Catholic, Mormon, Other, and Non-Religious. In line with our expectations regarding Mormon mentoring during adolescence, we are careful to distinguish between raised and convert Mormons as a way to isolate the role of Mormon *upbringing* on educational attainment—only raised Mormons will have a weaker sibship association with years of education. The Mormon Convert measure required moving 205 individuals from other religious categories. A total of 133 raised Protestants, 33 Catholics, 1 Jew, 38 "Other Religions," and 9 non-Religious self-identified as Mormon in adulthood and were therefore coded into the mutually exclusive category of "Mormon Convert."[13]

In an attempt to isolate the association between sibling size and educational attainment we include several socio-demographic features of the family in which the respondent was raised as controls, including total family income and highest level of parental education (less than high school, high school, junior college, bachelors, or graduate). In addition, we control for father's occupational prestige score and whether the respondent's mother was employed when the respondent was 16.[14]

[13] We also considered separating Utah Mormons from other Mormons but there were too few cases in Utah and surrounding states to conduct reliable analyses.

[14] The wording for this question changed after 1993 and so we include a binary variable ("Mother Employment Flag") indicating whether the survey was pre- or post-1993.

These categorical variables were treated as continuous in the analyses for ease of interpreting interactions.[15] Family structure captures whether the respondent was raised by two biological parents at age 16. We include dichotomous variables for white, black, or other race/ethnicity. These simple category distinctions were necessary given the limited racial/ethnic diversity of respondents born in the first decades of the 20th Century. Finally, to account for any variations in regional support (see Mayer and Lopoo 2008), using two dichotomous measures we control for the population density (rural vs. non-rural) and region of the country in which the respondent was raised (South vs. non-South).[16]

Analytic Strategy

To assess how the relationship between sibship size and educational attainment has changed across the twentieth century, we regress years of education on number of siblings for each year during the twentieth century in multivariate models to understand the relationship over time. These results are cumbersome to present, however, and so we split our groups into cohorts by decade: 1900-1909, 1910-1919, 1920-1929, 1930-1939, 1940-1949, 1950-1959, 1960-1969, and 1970-1979. We then predicted years of education attained in models that include these cohorts with 1900-1909 as the referent. To determine if the relationship between sibship size and educational attainment has changed over time, we interact sibship size with each cohort. To assess how the relationship

[15] When comparing analyses with categorical vs. continuous treatment of the family background measures we found that these transformation did not impact our results in any meaningful way.

[16] When these measures were analyzed as categorical variables (i.e. suburb, city, etc.; East South Central, Middle Atlantic, etc.), our substantive results were relatively unchanged.

between sibship size and educational attainment compares across religious groups, we predict educational attainment with interaction terms for religious upbringing and sibling size.

RESULTS

The average years of education attained in the sample is 12.77 (Table 1). In analyses not reported, we find that for those born in the first decade of the twentieth century (1900s) the average was 9.83. For subsequent decades average educational attainment increased to 10.86 (1910s), 11.67 (1920s), 12.48 (1930s), and appeared to reach its peak in the 1940s (13.36) and 50s (13.59). Subsequent decades reveal smaller changes (13.70 in the 1960s and 13.79 in the 1970s). The average respondent reported 3.99 siblings, which declined from 5.27 for those born in the 1900s to 3.12 for the individuals born in the 1970s. As others have noted, average sibship size tends to be considerably higher than fertility rates (Blake 1989).

The Sibship Size Association During a Period of State Growth

Did the association between sibship size and educational attainment weaken as state investments in children increased during the twentieth century? Our results suggest that it did. Figure 1 presents the results of regressing years of education on number of siblings for each year from 1905 to 1977 in multivariate models.[17] The patterns are smoothed averages over ten years. Across roughly 75 years, the sibship size association with education is rather stable, but declines

[17] Birth years of 1900-1904 and 1978-1979 had too few cases for independent regression analyses. Also, as the data begin in 1905, Figure 1 starts in 1915, an artifact of the ten-year smoothed averages.

during the last few decades, reaching its weakest point (closest to zero) among cohorts born in the 1960s and 1970s.[18]

Like Figure 1, we find a similar pattern across cohorts when examining the interaction of birth cohorts with sibsize (Table 3, model 2). With 1900-1910 as the referent, the coefficients for each cohort interaction with sibsize are positive and statistically significant, indicative of a weakening relationship between sibsize and educational attainment over time—statistically significant interactions emerge from the 1950s on. This pattern is even clearer when background factors are accounted for in model 3. Adjustments to the size of the sibsize association begins to emerge in the 1930s ($b=0.07$, $p<.001$) to the highest values of $b=0.17$ ($p<.001$) in the 1960s.

One way to understand how coming from a large family changed during the century is to consider the difference in years of education attained for those with zero versus six or more siblings over time. In 1900, only children attained 1.5 more years of education than their counterparts from

[18] Of course, one potential challenge for this analysis is the fact that the dependent variable is a moving target—years of education attained increased in significant ways over the century. The relationship between sibship size and years of education attained might be sensitive to this overall change. In supplemental analyses we addressed this possibility by predicting the deviation from the average years of education attained for those born in the same year, thereby normalizing the dependent variable by each year. We also did this for sibling size. Normalizing the dependent variable in this way or the independent variable of siblings did not change the overall patterns (see Appendices A, B and C). Also, in supplemental models we found little evidence that these patterns vary by urban/rural status.

families with six or more children.[19] In contrast, for respondents born in the 1960s, only children earned just a half year more of education than children from large families (six or more children)

While the association between sibship size and education weakened during the century, how much of that pattern was the result of a more general decline in the influence of family background on educational attainment? As the difference between model 2 and model 3 estimates reveal, family background has an important relationship to educational attainment that impacts our estimates. We gauged variation over time in the relationship between family background and educational attainment with a series of interactions between decade of birth and family background measures (race, total family income, parents' education, and father's occupational prestige) (Table 3, model 4).

As suggested in the literature, we find that the relationship between family background and educational attainment became weaker over time (see Figure 2).[20] Thus, a meaningful portion of the sibship X cohort interactions are explained when we account for the declining importance of family background on educational attainment in general. For example, the size of the association between sibship size and educational attainment among those born in the 1930s, 1940s, 1950s, 1960s and 1970s almost halved once we control for the interaction of family background and birth cohort. Considering the early 1900s as the reference group, this evidence suggests that the decline in the relationship between sibship size and educational attainment over time may be the result of the

[19] The coefficient for sibship size from Table 3 model 3 is -0.25 (-0.25*6=-1.5).

[20] Results indicate the relationship between family background and educational attainment became weaker during the 20th Century. Family income (0.62), father's occupational prestige (0.34), and parental education (1.11) all have positive coefficients for the main effect. All three measures have a negative coefficient for the interaction with cohort, indicating that the relationship with educational attainment became weaker (i.e. less positive).

expanding size of state-sponsored investments in education and related programs that unlink family background from educational opportunities.

Although we have no direct way with the available data to pinpoint this effect, we employed an indirect method to assess this possibility. Using data from the US Census of Governments, we gauge per capita spending on higher education in the state in which the respondent was raised (at age 16) and separated state spending into quintiles (5 groups).[21] In Figure 3, we find that the relationship between sibship size and educational attainment was strongest in states with the lowest spending on higher education ($b=-0.16$, $p<.001$) with a linear decline in the coefficient for sibship size as state spending on higher education increases. Among the highest spending states the coefficient for sibship size is the weakest ($b=-0.10$, $p<.001$). Thus, within the U.S., the sibship size association with educational attainment was about 60 percent greater for individuals growing up in high-spending versus low-spending states.

The Sibship Size Association and Community Context

Is the association between sibsize and educational attainment weaker among Mormons than other religious groups? Our models indicate that it is (Table 4, model 3). The interaction term (Raised Mormons*sibs) is statistically significant and positive ($b=0.10$, $p<.05$), suggesting that each

[21] The analyses required access to the GSS sensitive data, which contains data on the state in which the respondent was raised at age 16. We linked the GSS sensitive data with data from the US Census of Governments, which captures all spending on higher education from state and local sources every five years. State spending is measured the decade after the respondent's birth. Because of sample size limitations, we also had to restrict analyses to those born in the 1950s, 60s and 70s.

additional sibling is associated with just 0.06 years less of education among Mormons (compared to 0.16 years for Protestants). Thus, the effect of siblings on educational attainment is about one-third as strong for Mormons as for Protestants.

In addition, these patterns are different for Raised Mormon (n=479) compared to Mormon Converts (individuals who converted to Mormonism at some point in adulthood) (n=215). If our argument is right, only Mormons raised in the faith would enjoy the kinds of resources that would offset resource dilution in their homes and therefore improve their chances of obtaining higher level of education. Convert Mormons, however, were raised in non-Mormon homes and therefore their parents would not have benefitted by the extra-familial resources available in the Mormon community. Indeed, we find evidence consistent with this position—sibling interactions are stronger among those raised Mormons ($b=0.10$, $p<.05$) versus converts ($b=-0.03$, ns).[22] [23] It is also noteworthy that individuals that were religiously unaffiliated in childhood (potentially without a faith community to help compensate for resource dilution) exhibit a stronger sibsize/educational attainment association than do Protestants.

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[22] In supplemental analyses we also explored whether the Mormon pattern also changed over time—here we found little evidence that the religion-based patterns also interacted with the historical changes, but sample sizes became small, limiting our confidence in this analysis.

[23] We also attempted to explain the Mormon interaction with indicators in the GSS of "community support." Here we relied on past researchers' who have gauged "social capital" (Paxton 1999) using the GSS data with indicators such as trust in individuals and voluntary associations. In supplemental analyses these indicators did not reduce the interaction to non-significance. We are uncertain about the value of these analyses, however, because our measures of social capital are taken among adults, yet we are most interested in community-level investments respondents received while growing up.

CONCLUSION

The Resource Dilution Model's usefulness has waned. Although providing a provocative and useful starting point, it no longer accounts for the growing body of empirical work demonstrating significant variation in the sibship size/education relationship. We addressed this problem in two ways. First, we introduced the Conditional Resource Dilution model, which offers a straightforward but significant modification that more persuasively accounts for the wide range of observations, mostly outside the United States. Simply put, when child development is shared more broadly with non-parents, sibship size matters less. In some places, times, and among some groups, the division of family resources among children matters little for educational attainment. Second, we produced findings consistent with our model—they are suggestive of how broad policy changes and community support may influence the distribution of educational investments in families.

Specifically, the negative association between sibling size and educational attainment nearly halved comparing individuals born in the 1900s with those in the 1960s. Of course there is the possibility of a reversal—that with growing income inequality and the impact of family background on educational outcomes (Reardon 2011), sibling size may once again matter as much as it once did at the start of the 20th century, especially in states with limited educational investments. Also, it is clear that Mormons, whether by direct resources or ideological support, effectively offset (by 1/3rd) the impact of large families on educational attainment, a pattern mirrored in other faith communities beyond the U.S. context (Shavit and Pierce 1991).

Here we have produced patterns in the General Social Surveys that are consistent with CRD expectations. But while we think our modification of the resource dilution model is plausible, our study is merely a first step toward a more sophisticated understanding of how siblings matter. We have provided a conceptual framework from which to approach sibship size research, but it is now important to identify and measure non-parental resources directly and test whether they condition

the sibship size effect in the way we suspect. We believe our assumptions about how the parenting burden changed across the century (due to the growth in the state) and how Mormon communities offer greater support to children are reasonable, but future research can advance CRD by testing these claims more directly. For example, we suggested that one reason the effect of siblings on educational attainment is weaker among Mormons than Protestants is that Mormon youth enjoy greater inputs from non-relative adults. Measuring these inputs and testing them directly with a large sample of Mormons is an important next step.[24] Also, exploring variations in the sibsize relationships across countries with various level of state support for child rearing and school tuition could be another future avenue for scholars to pursue.

Of course, the way we have interpreted our results depends on causal claims, a non-trivial issue. Most sibship size research has moved away from OLS regression models because they are too vulnerable to omitted-variable bias. Scholars have employed instrumental variables based on twins or sex composition (Angrist et al. 2010; Black et al. 2010; Black et al. 2007; Cáceres-Delpiano 2006; Conley and Glauber 2006; Guo and VanWey 1999a; Li et al. 2008; Marteleto and de Souza 2012; Rosenzweig and Wolpin 1980) to more persuasively estimate unbiased sibship size effects. Our data do not allow for these techniques and so it is likely that our estimates of sibship size are inflated. In other words, if we could better control for the differences between the kinds of families that have

[24] In supplemental analyses we made attempts in this direction but confronted data obstacles. For example, to understand mechanisms for the Mormon interaction coefficient, we performed supplementary analyses of the sibship size/educational outcome relationship with the Adolescent Health data. We found that the estimate for the Mormon/sibling size interaction was in the right direction but not statistically significant. This may be in part due to the Adolescent Health data where the number of Mormons in the sample was small (n=210).

few versus many children, there is reason to believe that the sibship size "effects" reported here would be smaller.

Relatedly, it is possible that, rather than the sibship size "effect" declining over the 20th century, what changed were the selectivity criteria. From this perspective, the kinds of people who had small versus large families may have been more distinct in the early decades of the century compared with subsequent cohorts. Similarly, the selectivity processes distinguishing Mormon parents who have few versus many children may be less prominent than the distinctions among Protestant parents. There are some patterns in our data that are consistent with this interpretation.

While acknowledging this limitation, the CRD model has theoretical value on multiple counts. First, it provides a simple explanation for *why* the sibship size/education association is stronger in some contexts versus others. While sibship size researchers had noted variation across studies, they had yet to articulate the critical issue shaping those many variations. We direct attention to the extent to which children's development is shared by nonparents. Second, the CRD model provides a more flexible framework for understanding how siblings matter. For example, sibship size is positively associated with educational attainment in Kenya (Gomes 1984) and with greater social skills among kindergartners (Downey and Condron 2004) and a lower likelihood of divorce among adults (Bobbitt-Zeher, Downey, and Merry 2013). These studies undermine the Resource Dilution assumption that siblings are mere competitors for parental resources. In contrast, the CRD model provides the theoretical flexibility to accommodate this possibility. Under some conditions, brothers and sisters (perhaps mostly older siblings) may be providers rather than competitors for resources. Siblings may reduce the parental resources a given child receives, but they could counter those losses by serving as resources themselves.

Explanations like Resource Dilution are attractive because of their simplicity, but in this case that simplicity comes at too great a cost. A wide range of empirical findings outside the U.S. context

no longer fit the model's predictions and our study extends this problem to patterns within the U.S. In keeping with the parsimony of the dilution explanation, we have attempted to identify a parsimonious solution. If our modification holds up to future scrutiny, then our contribution is straightforward—siblings matter more when parents shoulder most of the responsibility for children, they matter less when that responsibility is more widely shared.

Table 1 Descriptive Statistics: General Social Surveys, 1972-2010.

Variable	Range		Mean	Std. Dev.
Years of Education	0	20	12.77	3.25
Years of Education (Standardized)	-5	3	0.00	1.00
Number of Siblings	0	23	3.99	3.14
Number of Siblings (Standardized)	-1.56	6.42	0.00	1.00
Interview Year	1972	2010	1991.19	10.99
Cohort				
1900-1909	0	1	0.04	0.21
1910-1919	0	1	0.09	0.28
1920-1929	0	1	0.12	0.33
1930-1939	0	1	0.13	0.34
1940-1949	0	1	0.20	0.40
1950-1959	0	1	0.21	0.41
1960-1969	0	1	0.14	0.34
1970-1979	0	1	0.06	0.24
Religion				
Protestant	0	1	0.61	0.49
Raised Mormon	0	1	0.01	0.10
Mormon Convert	0	1	0.00 *	0.07
Catholic	0	1	0.28	0.45
Jewish	0	1	0.02	0.15
Other Religion			0.02	0.15
Non-Religious	0	1	0.04	0.21
Race/Ethnicity				
White	0	1	0.82	0.38
Black	0	1	0.13	0.34
Other Race	0	1	0.04	0.20
Female	0	1	0.56	0.50
Parental Education Level	0	4	0.94	1.13
Income Level	1	5	2.75	0.87
Father's Occupational Prestige	1	5	2.78	1.43
Mother's Employment	0	1	0.59	0.49
Mother's Employment Flag	0	1	0.46	0.50
Two Biological Parents in Household	0	1	0.73	0.44
South	0	1	0.16	0.36
Rural	0	1	0.28	0.45

N = 46,397

* 205 cases or 0.5% of sample population

Table 2 OLS Regression Predicting Years of Education with Number of Siblings and Covariates: General Social Surveys, 1972-2010.

	Model 1		Model 2		Model 3	
	β	SE	β	SE	β	SE
Number of Siblings	-0.33***	(0.00)	-0.28***	(0.00)	-0.16***	(0.00)
Decade of Birth (ref=1900s cohort)						
1910s			0.84***	(0.08)	0.80***	(0.07)
1920s			1.56***	(0.08)	1.37***	(0.07)
1930s			2.29***	(0.07)	1.93***	(0.07)
1940s			3.08***	(0.07)	2.40***	(0.07)
1950s			3.36***	(0.07)	2.34***	(0.07)
1960s			3.40***	(0.07)	2.14***	(0.07)
1970s			3.32***	(0.08)	1.96***	(0.09)
Religious Affiliation (ref=Protestant)						
Raised Mormon			0.85***	(0.14)	0.33**	(0.13)
Mormon Convert			0.38	(0.20)	0.25	(0.18)
Catholic			0.14***	(0.03)	-0.08**	(0.03)
Jewish			2.20***	(0.09)	1.24***	(0.09)
Other Religion			0.87***	(0.09)	0.52***	(0.09)
Non-Religious			-0.22**	(0.07)	-0.27***	(0.06)
Race/Ethnicity (ref=White)						
Black					-0.26***	(0.04)
Other Race/Ethnicity					-0.28***	(0.07)
Female					-0.16***	(0.02)
Parent's Education Level					0.76***	(0.01)
Income Level					0.24***	(0.02)
Occupational Prestige (Father)					0.22***	(0.01)
Mother Employed					0.14***	(0.03)
Mother Employment Flag					0.36***	(0.03)
Two Biological Parents in Household					0.51***	(0.03)
South					-0.36***	(0.04)
Rural					-0.72***	(0.03)
Intercept	14.09***	(0.02)	11.23***	(0.07)	9.31***	(0.09)

N = 46,397

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

Table 3 OLS Regression Predicting Years of Education with Number of Siblings and Cohort Interactions: General Social Surveys, 1972-2010.

	Model 1		Model 2		Model 3		Model 4	
	β	SE	β	SE	β	SE	β	SE
Number of Siblings	-0.29***	(0.00)	-0.32***	(0.02)	-0.25***	(0.02)	-0.22***	(0.02)
Decade of Birth (ref=1900s cohort)								
1910s	0.86***	(0.08)	0.97***	(0.14)	0.72***	(0.13)	1.64***	(0.32)
1920s	1.56***	(0.08)	1.55***	(0.14)	1.14***	(0.12)	2.46***	(0.32)
1930s	2.29***	(0.07)	2.18***	(0.13)	1.54***	(0.12)	3.02***	(0.31)
1940s	3.09***	(0.07)	2.98***	(0.13)	1.91***	(0.12)	3.61***	(0.30)
1950s	3.37***	(0.07)	2.99***	(0.13)	1.61***	(0.12)	3.74***	(0.30)
1960s	3.41***	(0.07)	2.98***	(0.13)	1.36***	(0.13)	3.52***	(0.31)
1970s	3.34***	(0.08)	3.17***	(0.15)	1.34***	(0.14)	3.21***	(0.36)
Interactions								
1910s * Sibs			-0.03	(0.02)	0.00	(0.02)	0.01	(0.02)
1920s * Sibs			0.00	(0.02)	0.03	(0.02)	0.02	(0.02)
1930s * Sibs			0.02	(0.02)	0.07***	(0.02)	0.04	(0.02)
1940s * Sibs			0.02	(0.02)	0.09***	(0.02)	0.04*	(0.02)
1950s * Sibs			0.08***	(0.02)	0.15***	(0.02)	0.09***	(0.02)
1960s * Sibs			0.10***	(0.02)	0.17***	(0.02)	0.10***	(0.02)
1970s * Sibs			0.03	(0.03)	0.13***	(0.03)	0.08**	(0.03)
Intercept	11.59***	(0.07)	9.57***	(0.12)	7.55***	(0.12)	8.10***	(0.27)
Controls?	No		No		Yes		Yes	
Background * Cohort Interations?	No		No		No		Yes	

N = 46,397

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

Note: Controls reported in Table 2 included in models.

Table 4 OLS Regression Predicting Years of Education with Number of Siblings and Religion Interactions: General Social Surveys, 1972-2010.

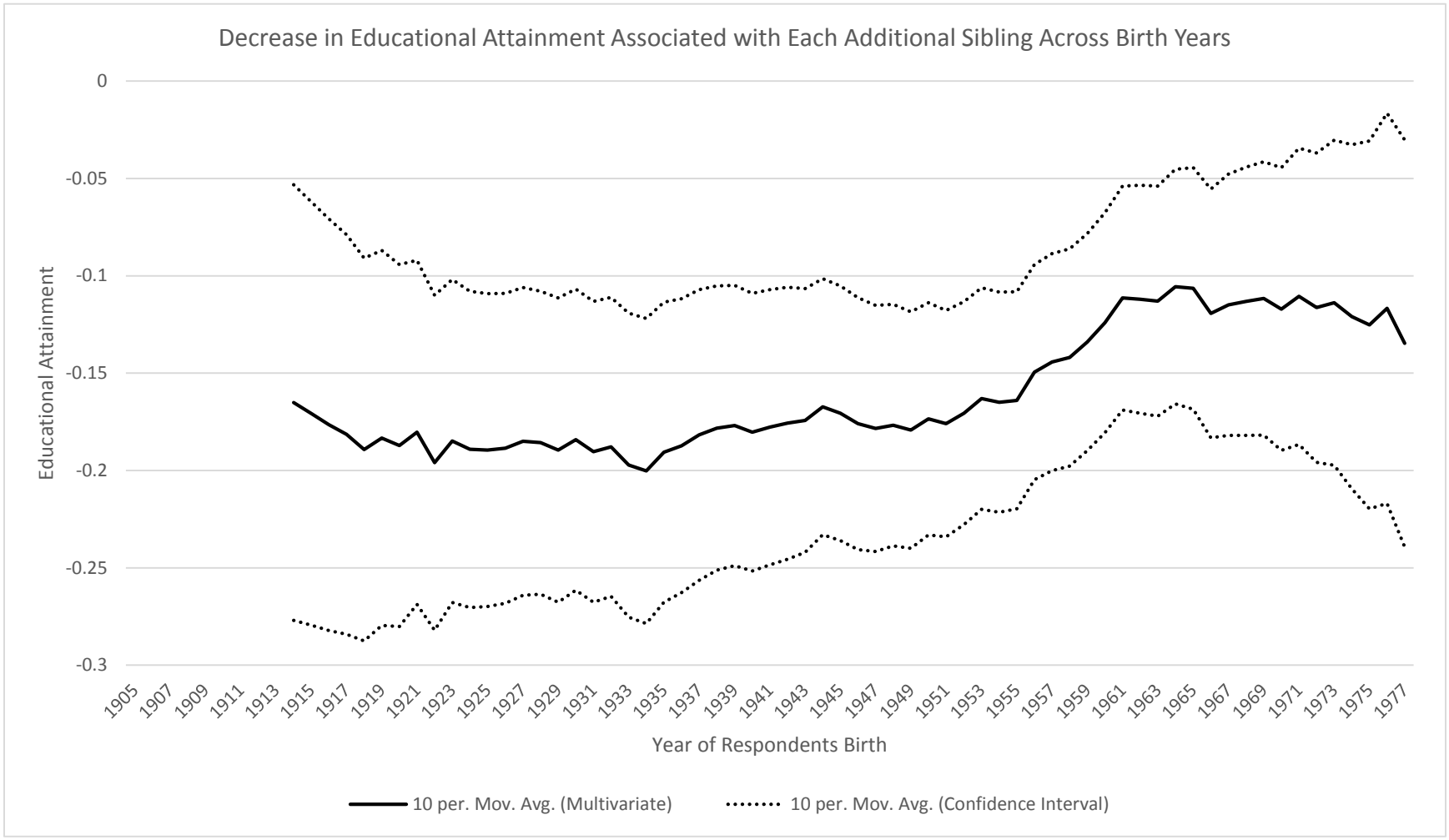
	Model 1		Model 2		Model 3	
	β	SE	β	SE	β	SE
Number of Siblings	-0.32***	(0.00)	-0.33***	(0.01)	-0.16***	(0.01)
Religious Affiliation (ref=Protestant)						
Raised Mormon	1.21***	(0.14)	0.54	(0.28)	-0.03	(0.25)
Mormon Convert	0.56**	(0.22)	0.14	(0.35)	0.20	(0.30)
Catholic	0.37***	(0.03)	0.26***	(0.05)	0.10*	(0.05)
Jewish	2.08***	(0.10)	2.28***	(0.15)	1.37***	(0.13)
Other Religion	1.35***	(0.10)	1.09***	(0.16)	0.31*	(0.14)
Non-Religious	0.17*	(0.07)	0.47***	(0.11)	-0.01	(0.10)
Interactions						
Raised Mormon * Sibs			0.14**	(0.05)	0.10*	(0.05)
Mormon Convert * Sibs			0.10	(0.06)	0.03	(0.06)
Catholic * Sibs			0.03**	(0.01)	-0.02*	(0.01)
Jewish * Sibs			-0.09	(0.05)	-0.09*	(0.04)
Other Religion * Sibs			0.07*	(0.03)	0.07*	(0.03)
Non-Religious * Sibs			-0.09***	(0.02)	-0.05*	(0.02)
Intercept	13.86***	(0.03)	13.89***	(0.03)	10.76***	(0.07)
Controls?	No		No		Yes	

N = 46,397

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

Note: Controls reported in Table 2 included in models.

F**igure 1** Multivariate Sibship Size/Education Pattern across the Birth Years: General Social Surveys, 1972-2010.



F**igure 2** Association of Family Background and Education by Decade: General Social Surveys, 1972-2010.

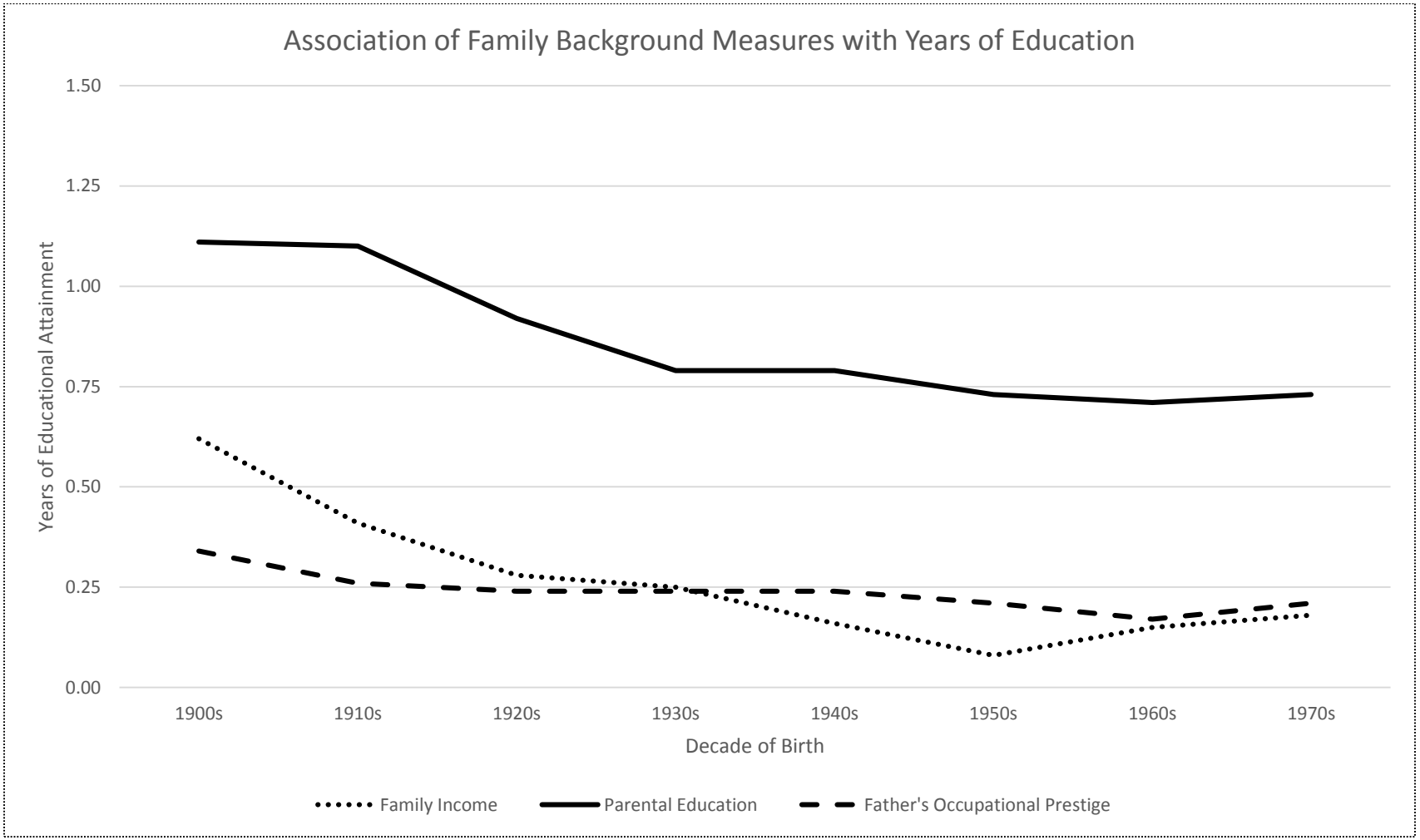
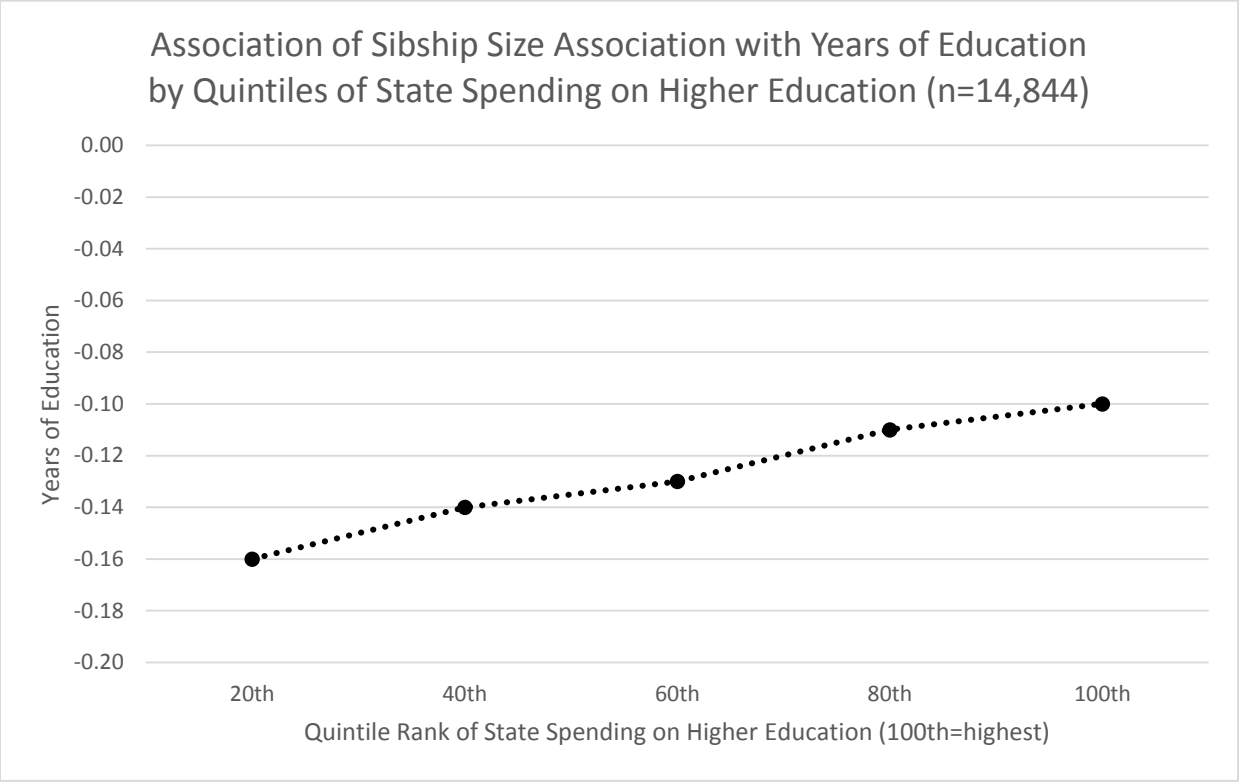


Figure 3 Association of Sibship Size and Education by Quintile of State Spending on Higher Education for Cohorts born in the 1950s, 60s, and 70s in the United States: General Social Surveys, 1972-2010 and US Census of Governments.



Appendix A OLS Regression Predicting Years of Education (standardized) with Number of Siblings (standardized): General Social Surveys, 1972-2010.

	Model 1		Model 2		Model 3	
	β	SE	β	SE	β	SE
Number of Siblings	-0.31***	(0.00)	-0.28***	(0.00)	-0.16***	(0.00)
Decade of Birth (ref=1900s cohort)						
1910s			0.17***	(0.02)	0.20***	(0.02)
1920s			0.35***	(0.02)	0.36***	(0.02)
1930s			0.54***	(0.02)	0.53***	(0.02)
1940s			0.77***	(0.02)	0.67***	(0.02)
1950s			0.79***	(0.02)	0.61***	(0.02)
1960s			0.70***	(0.02)	0.54***	(0.02)
1970s			0.63***	(0.03)	0.49***	(0.03)
Religious Affiliation (ref=Protestant)						
Raised Mormon			0.27***	(0.04)	0.08*	(0.04)
Mormon Convert			0.11	(0.06)	0.06	(0.06)
Catholic			0.05***	(0.01)	-0.02	(0.01)
Jewish			0.69***	(0.03)	0.39***	(0.03)
Other Religion			0.25***	(0.03)	0.16***	(0.03)
Non-Religious			-0.10***	(0.02)	-0.09***	(0.02)
Race/Ethnicity (ref=White)						
Black					-0.07***	(0.01)
Other Race/Ethnicity					-0.10***	(0.02)
Female					-0.05***	(0.01)
Parent's Education Level					0.24***	(0.00)
Income Level					0.08***	(0.01)
Occupational Prestige (Father)					0.07***	(0.00)
Mother Employed					0.04***	(0.01)
Mother Employment Flag					-0.21***	(0.01)
Two Biological Parents in Household					0.16***	(0.01)
Atlantic					-0.11***	(0.01)
Rural					-0.23***	(0.01)
Intercept	0.00	(0.00)	-0.62***	(0.02)	-1.05***	(0.03)

N = 46,397

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

Appendix B OLS Regression Predicting Years of Education (standardized) with Number of Siblings (standardized) and Cohort Interactions: General Social Surveys, 1972-2010.

	Model 1		Model 2		Model 3		Model 4	
	β	SE	β	SE	β	SE	β	SE
Number of Siblings	-0.29***	(0.00)	-0.31***	(0.02)	-0.24***	(0.02)	-0.21***	(0.02)
Decade of Birth (ref=1900s cohort)								
1910s	0.18***	(0.03)	0.18***	(0.03)	0.19***	(0.02)	0.45***	(0.09)
1920s	0.35***	(0.02)	0.34***	(0.02)	0.34***	(0.02)	0.70***	(0.09)
1930s	0.54***	(0.02)	0.54***	(0.02)	0.50***	(0.02)	0.89***	(0.09)
1940s	0.78***	(0.02)	0.77***	(0.02)	0.64***	(0.02)	1.08***	(0.09)
1950s	0.79***	(0.02)	0.78***	(0.02)	0.58***	(0.02)	1.13***	(0.09)
1960s	0.71***	(0.02)	0.70***	(0.02)	0.51***	(0.02)	1.07***	(0.09)
1970s	0.63***	(0.03)	0.63***	(0.03)	0.47***	(0.03)	0.95***	(0.10)
Interactions								
1910s * Sibs			-0.03	(0.02)	0.00	(0.02)	0.01	(0.02)
1920s * Sibs			-0.01	(0.02)	0.02	(0.02)	0.01	(0.02)
1930s * Sibs			0.00	(0.02)	0.06**	(0.02)	0.03	(0.02)
1940s * Sibs			0.00	(0.02)	0.08***	(0.02)	0.03	(0.02)
1950s * Sibs			0.07**	(0.02)	0.14***	(0.02)	0.08***	(0.02)
1960s * Sibs			0.09***	(0.02)	0.15***	(0.02)	0.10***	(0.02)
1970s * Sibs			0.03	(0.03)	0.12***	(0.02)	0.08**	(0.03)
Intercept	-0.59***	(0.02)	-0.58***	(0.02)	-1.03***	(0.03)	-1.47***	(0.08)
Controls?	No		No		Yes		Yes	
Background * Cohort Interations?	No		No		No		Yes	

N = 46,397

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

Note: Controls reported in Table 2 included in models.

Appendix C OLS Regression Predicting Years of Education (standardized) with Number of Siblings (standardized) and Religion Interactions: General Social Surveys, 1972-2010.

	Model 1		Model 2		Model 3	
	β	SE	β	SE	β	SE
Number of Siblings	-0.31***	(0.00)	-0.31***	(0.01)	-0.16***	(0.01)
Religious Affiliation (ref=Protestant)						
Raised Mormon	0.34***	(0.04)	0.31***	(0.05)	0.09*	(0.04)
Mormon Convert	0.15*	(0.07)	0.15*	(0.07)	0.08	(0.06)
Catholic	0.09***	(0.01)	0.09***	(0.01)	0.01	(0.01)
Jewish	0.66***	(0.03)	0.62***	(0.04)	0.33***	(0.03)
Other Religion	0.33***	(0.03)	0.33***	(0.03)	0.18***	(0.03)
Non-Religious	-0.03	(0.02)	-0.04	(0.02)	-0.08***	(0.02)
Interactions						
Raised Mormon * Sibs			0.14**	(0.05)	0.09*	(0.04)
Mormon Convert * Sibs			0.08	(0.06)	0.03	(0.06)
Catholic * Sibs			0.02*	(0.01)	-0.02*	(0.01)
Jewish * Sibs			-0.07	(0.04)	-0.08*	(0.04)
Other Religion * Sibs			0.07*	(0.03)	0.06*	(0.03)
Non-Religious * Sibs			-0.08***	(0.02)	-0.06**	(0.02)
Intercept	-0.05***	(0.01)	-0.05***	(0.01)	-0.66***	(0.02)
Controls?	No		No		Yes	

N = 46,397

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

Note: Controls reported in Table 2 included in models.

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