THREE STUDIES ON THE DETERMINANTS AND CONSEQUENCES OF POVERTY

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ABSTRACT

Colin Campbell: Three Studies on the Determinants and Consequences of Poverty
(Under the direction of Arne L. Kalleberg)

In this dissertation I consider the effect of dropping out of high school on poverty and welfare receipt, the effect of sibship size on cognitive ability and early educational achievement, and the determinants of support for government involvement in lessening poverty. Using a variety of multivariate methods and data sources, (1) I find that dropping out of high school has an independent effect on poverty and welfare receipt net of unobserved background characteristics, but the consequences of dropping out and the role of unobserved background characteristics have changed over time; (2) I find a clear association between sibship size and cognitive development and early educational achievement in both low-income and affluent households, but I find little support for a causal interpretation of these findings; and (3) I find that the social and economic contexts when people come of age do not color beliefs about the government's role in lessening poverty, but beliefs do shift in response to economic hardship.

To my parents.

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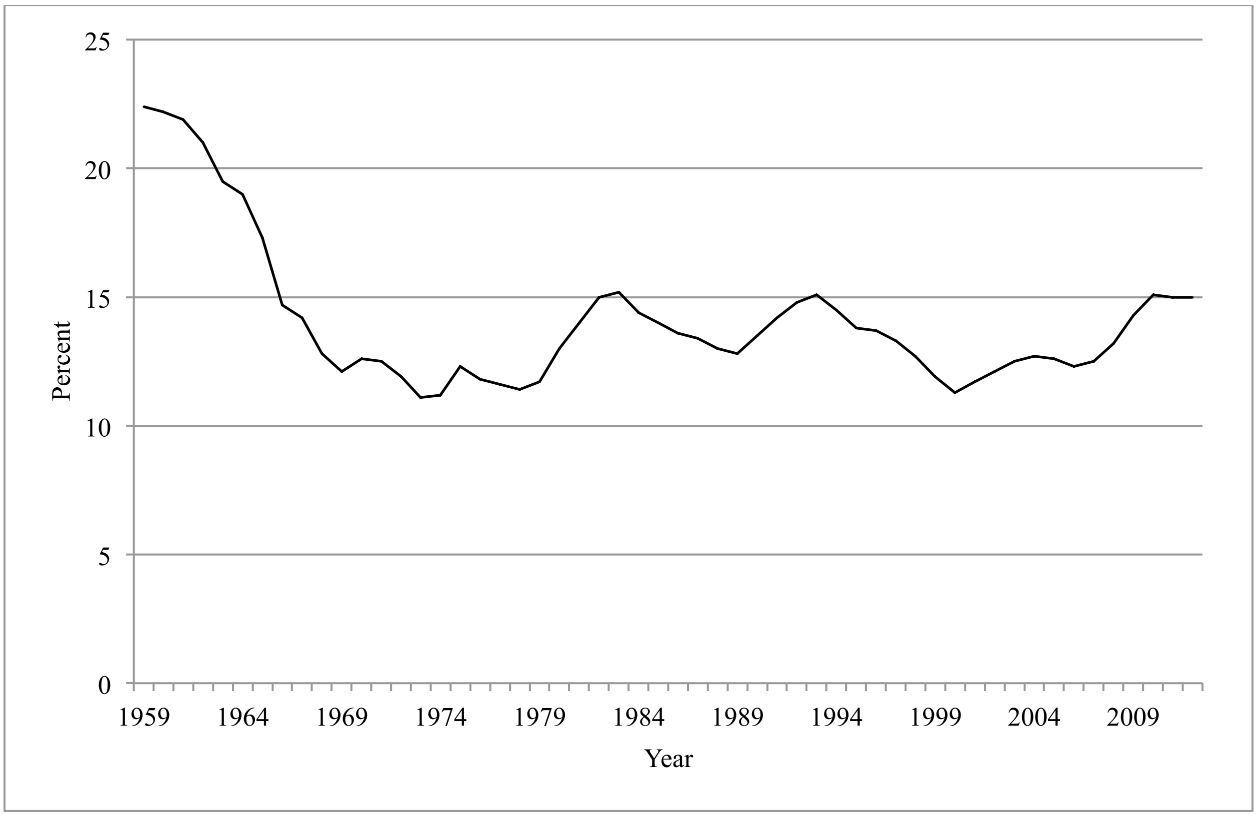
CHAPTER 1: INTRODUCTION AND OVERVIEW

In 1964, President Johnson famously declared an unconditional war on poverty, aiming not just to reduce the number of poor families or to make living in poverty more endurable, but to completely eliminate poverty. In the subsequent years, the federal government proposed a number of initiatives to help improve the education, health, and labor market outcomes of the poor. These initiatives gave birth to Medicare and Medicaid, and made a pilot food stamps program permanent. These initiatives also expanded Social Security benefits, increased benefits levels for Aid to Families with Dependent Children, and established the Jobs Corps and VISTA programs. These initiatives also created Head Start, Title I programs, and the now defunct Office of Economic Opportunity.

Social scientists at the time were optimistic that poverty could be greatly reduced or even eliminated. James Tobin, an adviser to President Johnson and an economics professor at Yale University, believed poverty could be completely eradicated in the US by 1976 (Tobin 1967). Robert Lampman, also an adviser to President Johnson and the founding director of the first federally funded poverty research center in the United States, was slightly less optimistic, arguing that poverty in the US could be completely eradicated, but not until 1980 (Lampman 1971).

In the decade following the announcement of the government's war on poverty, there was a steady decline in the poverty rate. In 1965, the poverty rate was 17.3 percent. By 1973, the poverty rate had declined to 11.1 percent (see Figure 1). However, instead of a continued march toward zero, declines in the poverty rate stalled, and 1973 marks the lowest point for the poverty rate in the past 50 years. For the 40 years since 1973, the poverty rate has hovered around 13 percent, moving with economic booms and busts but never abating to the single digits.

Figure 1.1 Poverty Rate, 1959-2012.



Source: US Census Bureau, Current Population Survey, 1960-2013 Annual Social and Economic Supplements.

At the same time, intergenerational mobility out of poverty has remained low. Children born in the poorest decile have a 31.5 percent chance of ending up in the poorest decile as an adult, and a 51.3 percent chance of ending up in the poorest quintile (Hertz 2005). For children from the poorest thirty percent of households, close to two-thirds will stay among the poorest thirty percent as adults. These numbers are astounding and disheartening: one in two children from the poorest tenth of households will end up among the poorest fifth as adults; only one in three children from the poorest thirty percent of households will move up beyond the poorest thirty percent of adults.

These trends are difficult to explain. In the mid-twentieth century, social scientists were optimistic that the poverty rate could be greatly diminished and the government took action to improve the chances of upward mobility for poor children. However, the poverty rate never dipped below one in ten and intergenerational mobility out of poverty remained limited. In turn, social scientists refocused their attention, seeking a better understanding of the social forces that maintain poverty. Unfortunately, progress has been slow and greatly hindered by methodological challenges.

A particular difficulty is parsing out the relationship between poverty and social and economic contexts. The poor face a large number of related problems. The complexity and interconnectedness of the issues affecting the poor makes it difficult to separate causes of poverty from symptoms of poverty. Consider the relationship between poverty and educational attainment. Growing up in poverty is associated with a decreased likelihood of completing high school. At the same time, not completing high school is associated with an increased risk of poverty. Separating the extent to which not completing high school leads to poverty and the extent to which growing up in poverty leads to not completing high school (and then subsequent poverty) is methodologically difficult. Given the complexity and the closely related nature of the issues affecting the poor, we are left with lingering questions.

This dissertation offers three pieces of empirical research that move forward our understanding of how social and economic contexts are related to poverty. In these papers, I investigate how social and economic contexts can lead to a biased understanding of what troubles the poor. I also examine how social and economic contexts can create divergent beliefs about the role of the government in lessening poverty. Taken as a whole, this dissertation offers a careful investigation of what ails the poor and what does not and also an improved understanding of the determinants of support for government action to lessen poverty.

Before providing a brief overview of each paper, it is worthwhile to consider why studying poverty is important. At the individual level, poverty has numerous consequences. Poverty leads to health problems, a shortened life expectancy, and an increased risk of criminal activity. These individual consequences have societal costs. Consider children raised in poor families. Children raised in poor families complete fewer years of education and have worse cognitive outcomes. As a result, the US economy wastes an immense amount of potential worker productivity and earnings. At the same time, children raised in poor families are more likely to be involved in crime and are also more likely to suffer from poor health. As a result, expenditures on the criminal justice system and health care are artificially high, and worker productivity is again squandered because of losses to health limitations or imprisonment. Holzer et al (2007) estimate that childhood poverty costs the US about $500 billion per year through the loss of worker productivity and expenditures related to health and crime.

Poverty also has a more general macroeconomic significance. In particular, increases in the poverty rate can slow economic growth. As the poverty rate increases, the market of available consumers shrinks, demand for goods and services decreases, and economic growth slows. Consumers not directly affected by the increased poverty rate, weary of increasing economic hardship, become less likely to spend disposable income and worsen the decreased demand for goods and services. Conversely, decreases in the poverty rate leads to an increase in the number of people able to purchase goods and services, and can help stimulate economic growth (Bluestone & Harrison 2000).

Moreover, myths and misperceptions about poverty are common. In many instances, misperceptions are entirely misplaced. For example, a common misperception is that the majority of poor are African American or that the majority of the poor are unemployed (Iceland 2012; O'Hare 1996). Both are inaccurate perceptions and are easily falsified. In other instances, misperceptions reign because of the absence of robust empirical research. The papers presented in this dissertation subject three common perceptions to rigorous analysis, assessing (1) to what extent the poor are poor because of low levels of educational attainment, (2) to what extent sibship size in low-income households shapes cognitive development and early educational achievement, and (3) to what extent economic hardship determines beliefs about lessening poverty.

Section 1.1: Overview of Dissertation

Paper 1: High School Dropouts, Poverty, and Welfare

Research on the determinants of poverty routinely highlights the role of education, arguing the poor are poor because of low levels of educational attainment. There is good reason to suspect that education is a determinant of poverty status. Educational attainment is an excellent predictor of a number of social and economic outcomes, including wages, health, employment status, and, importantly, poverty status. Notably, over one in four high school dropouts are in poverty compared to fewer than one in twenty college graduates.

Similarly, existing research proposes a number of mechanisms through which educational attainment has the potential to directly influence poverty status. Most obviously, human capital models show that increases in human capital are associated with increases in earnings. Those with less education acquire less human capital and are able to demand less from the labor market. As a result, they face an increased risk of poverty. Similarly, research on credentialism, social closures, and signaling shows how the presence of a degree can reap benefits for the credentialed even in the absence of skill differentials. In effect, individuals with less education may be formally limited to a smaller pool of available jobs or employers may doubt the abilities of the less educated, which in turn increases the risk of poverty.

However, the role of educational attainment in determining who is poor and who is not is muddied by nonrandom selection into different levels of educational attainment. Existing research finds that various sociodemographic groups face an increased risk of not completing high school, or conversely, that various sociodemographic groups face an increased "risk" of completing college. In particular, family and neighborhood characteristics are an important determinant of high school completion. Ultimately, those who do not complete high school are likely different from high school graduates in meaningful and important ways in the same way that college graduates are likely different from high school graduates in meaningful and important ways.

Acknowledging that educational attainment is far from random is not inherently problematic for accurately understanding the extent to which educational attainment leads to poverty. Instead, selection effects become a concern because of the innate difficulties in measuring factors that contribute to selection into different levels of educational attainment. For example, parental behaviors influence the likelihood of completing high school. If a researcher's analytic strategy does not account for parental behaviors, then findings will be inaccurate because of omitted variable bias. Researchers could attempt to account for parental behaviors and other parental characteristics but they would need extremely detailed data to feel confident that the effect of educational attainment is not biased by unmeasured parental traits. This is only one example of the difficulty of measuring relevant characteristics, but there are many others. Consider the importance of neighborhoods. Neighborhoods have an important affect on educational attainment. Consequently, researchers need to be careful to account for neighborhood effects when considering the consequences of low levels of educational attainment. However, neighborhood effects are notoriously difficult to observe and measure in survey data. Thus, researchers again run the risk of confounding neighborhood effects and educational attainment effects.

If we return to the guiding question—to what extent are low levels of educational attainment responsible for the high rates of poverty among the less educated—we realize that while education has become one of the most prominent explanations for understanding who is poor and who is not, the issue is far from resolved. In the first paper of this dissertation, I examine the extent to which dropping out of high school is responsible for the increased risk of poverty and welfare receipt among high school dropouts. To account for difficult to measure family and neighborhood characteristics, I base model estimation on within family differences. In effect, I compare siblings who completed high school to their siblings who dropped out of high school. This provides a straightforward control for unobserved background effects. We no longer need to worry about accounting for parental traits because siblings share the same parents.

Similarly, we no longer need to worry about observing neighborhood characteristics because siblings live in the same neighborhood.

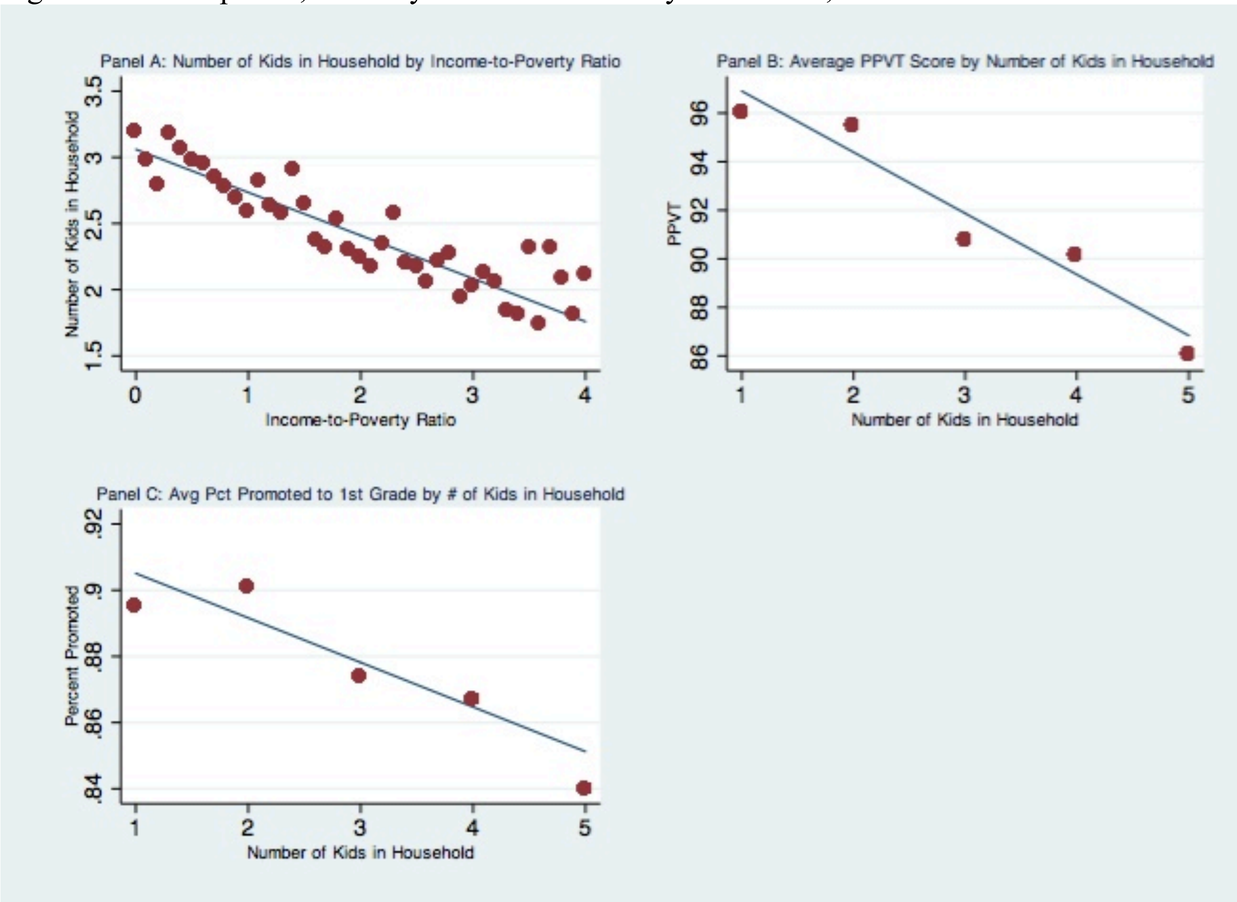
The research presented in this paper improves our understanding of the relationship between educational attainment and poverty. Increased educational attainment is routinely offered as a panacea for what ails the poor without a critical assessment of how social and economic contexts influence both poverty and educational attainment. This research offers a more nuanced and complete understanding of the extent to which dropping out of high school leads to poverty and welfare receipt.

Paper 2: Sibship Size, Cognitive Ability, and Early Educational Achievement

In the second paper in this dissertation, I consider whether variations in sibship size have meaningful consequences on cognitive ability or early educational achievement for children in low-income households. Figure 2 motivates this research. Panel A of Figure 2 shows variations in the average number of children in a household by mother's income-to-poverty ratio. Panel B of Figure 2 shows variations in cognitive test scores by sibship size. Panel C of Figure 2 shows the percent of kindergarteners not promoted to first grade by sibship size. We see that poorer mothers have more children on average and that the number of siblings in a household is negatively associated with both cognitive ability and first grade promotion.

Research consistently finds that sibship size leads to worse education outcomes for children. Couple this finding with the fact that poor families on average have more children, and sibship size seems like an obvious candidate for a pathway from living in poverty to poor education outcomes. Indeed, there is a popular narrative that argues poor families have too many children, and because of this, children in poor families have arrested cognitive development and worse education outcomes. The underlying reasoning to this narrative is straightforward. Parents

Figure 1.2 Sibship Size, Peabody Picture Vocabulary Test Score, and First Grade Promotion.



Note: Data come from the Fragile Families and Child Wellbeing Study. Peabody Picture Vocabulary Test (PPVT) is a standardized test that measures verbal ability and scholastic aptitude.

invest time and financial resources in their children. These investments are positively associated with cognitive development and early educational achievement. The more a parent is able to invest, the better off their child's cognitive development and early educational achievement. In poor households, resources that are already stretched thin are further stretched by large family size. The narrative argues if poor households had fewer children, each child could receive more resources and would have improved cognitive development and educational achievement. While this narrative is perhaps intuitively appealing, it is empirically thin.

Sibship size—like educational attainment—is not random. Thus, researchers must take selection effects seriously. The ability of Ordinary Least Squares regression models—the prevailing method used in past studies—to accurately capture the pathways from sibship size to outcomes is questionable. Therefore, I draw on a variety of multivariate approaches to examine the effects of sibship size on cognitive development and early educational achievement. Beyond OLS models, I conduct propensity score matching analysis and instrumental variable models. In the propensity score matching analyses, I pair children who are similar in meaningful ways and only differ in their number of siblings. In the instrumental variable models, I use an exogenous shock on sibship size—whether the mother of a poor household experiences a miscarriage—to obtain an unbiased sibship size effect.

Aside from selections effects and methodological concerns, there is an additional reason to suspect that sibship size may not matter or be less important in low-income households. Namely, the social and economic contexts that have the potential to lead to sibling penalties on cognitive development and early educational achievement may not be present in low-income households. For example, in middle class households an additional sibling may mean that parents elect to send their children to public school instead of private school. In low-income households, private school is unlikely an option regardless of sibship size. This research, therefore, adds to our understanding of how variations in social and economic contexts across income groups have the potential to yield different effects.

This research two important implications. First, this research focuses on a specific mechanism, which helps move poverty research beyond associations. The poor face a large number of related problems. To understand how living in poverty leads to adverse outcomes, we must separate mechanisms from correlations. It is not enough to find that growing up in poverty leads to adverse outcomes. It is critical to identify the actual mechanisms that lead to adverse outcomes. Yes, the poor on average have larger family sizes, and yes, poor children on average have worse education outcomes, but does sibship size negatively affect the education outcomes of poor children? This research offers an empirical test of a specific mechanism that may lead from living in poverty to worse outcomes.

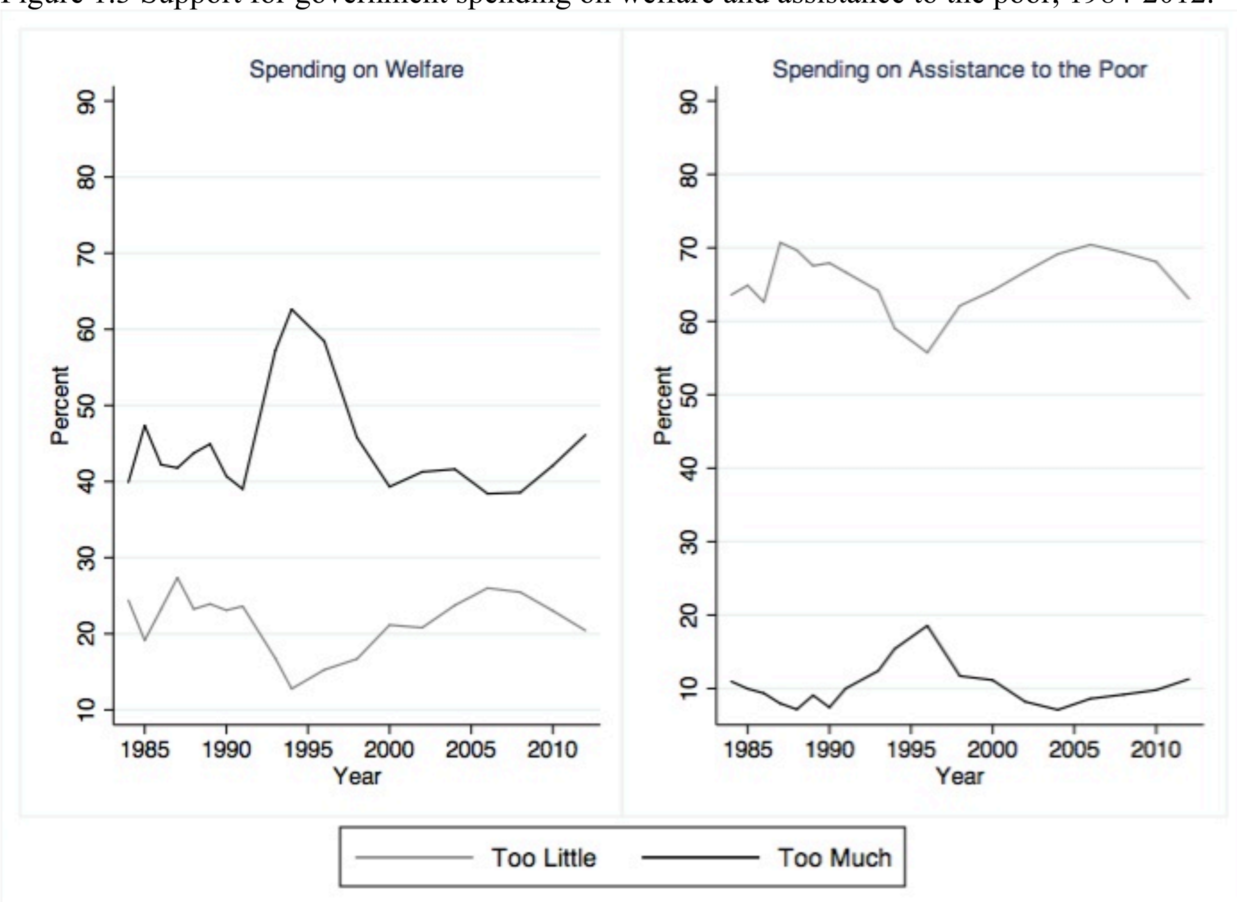
Second, variations in cognitive ability and early educational achievement are associated with an array of deleterious consequences, including decreased educational attainment, increased delinquency, and future economic disadvantage. Accordingly, the effect of sibship size on cognitive ability and early educational achievement has important implications for our understanding of achievement gaps between poor and non-poor children. If increases in sibship size negatively affect cognitive development and early educational achievement, then sibship size should help inform our understanding of achievement gaps in the same way that other family effects do.

Paper 3: Economic Hardship and Beliefs about Government Spending on Poverty

Opposition to antipoverty policies in the US is commonplace. However, opposition is inconsistent across policies. As an illustrative example, consider Figure 3, which shows support for government spending on welfare (left panel) and government spending on assistance for the poor (right panel) from 1984 to 2012. Here we see the percentage of people who believe the government spends *too much* on welfare, the percentage of people who believe the government spends *too much* on assistance for the poor, the percentage of people who believe the government spends *too little* on welfare, and the percentage of people who believe the government spends *too little* on assistance for the poor. The differences are striking. Few people believe the government spends too much on assistance for the poor. Yet, the plurality of people consistently believes the government spends too much on welfare.

There is an impressive body of research that attempts to explain the discrepancies observed in Figure 3 and a similarly impressive body of research that investigates public support for government spending on poverty in general. Research on attitudes toward government spending on welfare and poverty largely rely on theories that emphasize the role of economic

Figure 1.3 Support for government spending on welfare and assistance to the poor, 1984-2012.



Note: Data come from the General Social Survey. Question reads: "We are faced with many problems in this country, none of which can be solved easily or inexpensively. I'm going to name some of these problems, and for each one I'd like you to tell me whether you think we're spending too much money on it, too little, or about the right amount. Are we spending too much, too little, or about the right amount on welfare/assistance to the poor?" Trend for "about right" not shown.

precarity. Unfortunately, past research has a large blind spot: it exclusively focuses on current conditions while overlooking past experiences. In effect, existing theories privilege current social and economic contexts—income, job security, educational attainment, macroeconomic climate—at the expense of past experiences. While current social and economic contexts are undoubtedly important—and existing research demonstrates the significant role of current social and economic contexts—entirely overlooking the role of past contexts is imprudent.

In the third paper in this dissertation, I consider how views of the current are colored by the past. I draw on a rich body of literature from social psychology that shows experiences that occur during late adolescence and early adulthood can have a profound impact on life long attitudes and explore whether generational differences in beliefs exist net of current social and economic contexts. Are the cohorts who came of age during the war on poverty more or less likely to support government spending on poverty than the cohorts who came of age during welfare reform in the mid-1990s?

Existing research is further limited because it largely relies on cross-sectional data. As a result, sociological understandings of the fluidity of beliefs about government spending on poverty are incomplete. Are attitudes toward government spending on poverty fixed or do they respond to changes in economic contexts? When an individual experiences a change in her economic standing, do her beliefs follow? To answer these questions, I draw on longitudinal data to examine the stability and fluidity of beliefs about government spending on poverty. In particular, I examine whether suffering an economic hardship produces an increase in support for government spending on poverty.

The three papers presented in this dissertation pay careful attention to how social and economic contexts are related to poverty. In the first two papers, I examine how improved accounting for social and economic contexts can influence our understanding of the determinants and consequences of poverty. In the third paper, I show how social and economic contexts can have a meaningful effect on beliefs about government spending on poverty. Taken together, these three papers serve as a call for poverty researchers to judiciously examine how social and economic contexts relate to poverty and a reminder of how symptoms of poverty can mistaken as a causes of poverty.

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CHAPTER 2: THE SOCIOECONOMIC CONSEQUENCES OF DROPPING OUT: EVIDENCE FROM TWO COHORTS OF SIBLINGS

Introduction

The belief that dropping out of high school causes pervasive and persistent socioeconomic disadvantage is widespread. Viewed through the lens of cross-sectional associations, the relationship between dropping out and future disadvantage is obvious—poverty rates, unemployment rates, and rates of welfare receipt for high school dropouts are significantly higher than the respective rates for high school graduates (Boisjoly et al 1998; Caspi et al 1998; Iceland 2012; National Center for Education Statistics 2012; US Census Bureau 2010). However, before concluding that dropping out is responsible for the increased risk of hardship, researchers must be careful to account for ways the population of dropouts differs from the population of high school graduates.

Put simply, high school dropouts likely face an elevated risk of socioeconomic disadvantage irrespective of their dropout status. The question, therefore, is not whether high school dropouts face an increased risk of disadvantage. They do. Instead, the question is whether dropping out of high school is responsible for the increased risk of disadvantage. If high school dropouts had completed high school, would they be any better off?

Research on the consequences of dropping out is surprisingly underdeveloped. Nearly three decades ago, Natriello, Pallas, and McDill (1986) called for researchers to investigate the social and economic effects of dropping out, writing:

> There is a clear need for research on the consequences of dropping out. We know rather little about either the economic or social consequences of

> dropping out . . . In order to do this, we need detailed information on the experiences and characteristics of dropouts before they left high school, as well as data on their labor market experiences … (p. 175).

This call for research on the consequences of dropping out has largely gone unanswered. Instead, discussions of the costs of dropping out have had to rely on cross-sectional associations (for example, see Rumberger 1987; Cheeseman Day & Newburger 2002). While cross-sectional associations reveal the level of disparity between high school graduates and high school dropouts, they do not speak to the extent that dropping out contributes to the observed disparity.

The absence of research is likely due to the difficulty of assessing the true costs of dropping out. Natriello, Pallas, and McDill point out the need for detailed data on dropouts before they leave school. Without such data, researchers run the risk of confounding dropout effects with the effects of early life social and economic contexts. This is particularly important because dropouts come disproportionately from already disadvantaged backgrounds (Alexander, Entwisle, & Horsey 1997; Brooks-Gunn, Guo, & Furstenberg 1993; Duncan et al 1998; Rumberger 1987; Stearns and Glennie 2006), a strong determinant of both high school completion and economic hardship. The early and adolescent disadvantage that lead an individual to drop out likely also creates an increased risk of adult economic hardship. By the time an individual drops out of school, the risk of economic hardship may already be firmly in place.

In this study, I examine the effect of dropping out on future poverty and welfare receipt. I base model estimation on within-family differences, comparing individuals who dropped out of high school to their siblings who completed high school. This method provides a straightforward control for unobserved background effects, leveraging the shared backgrounds of siblings to isolate a dropout effect that is independent of background characteristics. By basing model estimation on within-family differences, the research presented here is better able to account for heterogeneity in the population of dropouts.

I make two additional contributions. First, I present conventional ordinary least squares (OLS) regression estimates of the effect of dropping out on future poverty and welfare receipt using the same data. By comparing conventional and within-family estimates, I am able to discern the extent to which differences in the backgrounds of dropouts are responsible for cross-sectional associations between dropping out and future socioeconomic disadvantage. In particular, I am able to assess how unobserved characteristics may bias OLS estimates. Second, I analyze data from two cohorts of siblings, one cohort was high school age in the 1970s and the other was high school age in the 1990s. By comparing across cohorts, I am able to assess how the costs of dropping out have changed over time and also able to determine how the role of social background effects have increased or decreased in importance.

I first present arguments for why dropping out of high school likely leads to future poverty and welfare receipt, highlighting the role of skill differentials, credentialism, social closures, and signaling theory. I next discuss reasons for why the increased risk of disadvantaged faced by dropouts is likely overstated, emphasizing the difficulty of isolating an independent dropout effect using conventional OLS methods. I then outline the data, measures, and analytic strategy used in this paper. Lastly, I present findings from conventional OLS regression estimates and within-family estimates. I conclude with a discussion of how recent policy shifts have the potential to worsen the socioeconomic standing of low-ability students.

Background

There are strong theoretical reasons to suspect that dropping out increases the risk of economic hardship net of background characteristics. First, it is possible that dropping out of high school produces an actual skill differential between dropouts and high school graduates. In effect, because dropouts do not complete all years of schooling, they do not develop the same level of skills and competencies. In this scenario, observed differences in poverty are indeed the result of dropping out. Had dropouts stayed in school, they would have acquired more skills and be able to demand more from the labor market.

The extent to which additional years of schooling leads to an increase in skills and an increase in future earnings is uncertain. The ambiguity is due to differences in who completes more years of education. Without experimental data, it is hard to assess whether differences in outcomes by educational attainment are the result of more years of education or the result of who completes more years of education. This is often referred to as "ability bias"—those with greater ability likely choose to complete more years of education. A large body of research has attempted to identify the true effect of education on a variety of outcomes and has produced mixed evidence (for a review, see Card 1999). While most of this research examines the effects of educational attainment in general, the few studies that do consider high school dropouts in particular have produces similarly equivocal results.

For example, Angrist and Krueger (1991) famously approximate the conditions of a natural experiment by leveraging compulsory schooling laws and season of birth, noting that individuals born in the first quarter of the calendar year become eligible to drop out of high school a school year earlier than individuals born later in the calendar year. Angrist and Krueger find that students who are forced to attend school longer because of where their birthday falls in the calendar year earn higher wages as a result of their increased schooling. However, these findings are strongly disputed and often used as an example of the shortcomings of instrumental variables. In particular, Bound, Jaeger, & Baker (1995) and Staiger and Stock (1997) show that the instrumental variable used by Angrist and Krueger—season of birth—explains little of the variation in educational attainment, making it too weak of an instrument to be informative.

Similarly, there is debate over whether more years of schooling produces greater skills and ability between dropouts and high school graduates. Alexander, Natriello, and Pallas (1985) compare cognitive development for dropouts and high school graduates. They find that individuals who stay in school see more of an increase in cognitive skills than those who drop out. However, the effect sizes are modest—the average difference in cognitive test performance between dropouts and high school graduates was about one-tenth of a standard deviation. Additionally, even if staying in school does generate differences in skills and abilities, it is unclear whether that matters. Griffin, Kalleberg, and Alexander (1981) find that aptitude, class rank, and other school information has minimal and often insignificant effects on employment and other job related outcomes for high school graduates who enter the workforce directly upon high school completion. This conclusion is supported by a host of studies that find no effect or even a negative effect of high school grades on earnings (Kang & Bishop 1986; Miller 1998; Rosenbaum & Kariya 1991), and others who find that noncognitive traits—traits that are unlikely to be acquired through increased schooling—are much stronger predictors of labor market outcomes (Bowles & Gintis 2002; Cawley, Heckman, & Vytlacil 2001; Sewell & Hauser 1975). However, research that is not limited to high school dropouts does find an association between cognitive ability and earnings (Farkas 1996; Farkas et al 1997).

Leaving aside these debates, we can also point to credentialism, social closures, and signaling theory as offering a theoretical motivation for an independent effect of dropping out on future disadvantage. We can think of credentialism as the extent to which employment or wages are allocated based on the possession of an education credential at the time of hiring, and social closures as the requirement of a degree to obtain a given position (Bills & Brown 2011; Bol & van der Werfhost 2011; Weeden 2002). Thus, a high school diploma could be important for a number of reasons: employers may see the credential as a status marker or regulations may require an employer to hire an individual with a high school diploma. If we imagine two potential job candidates that are equal in all ways except that Candidate A has a high school diploma and Candidate B does not, we could reasonably expect a perspective employer, because of credentialism or social closures, to prefer Candidate A to Candidate B. Ultimately, because of credentialism or social closure mechanisms, dropouts may be restricted to a smaller pool of available jobs or have limited potential wage growth. Thus, while a dropout may be qualified or have the needed abilities for a given employment opportunity, she will be passed over for the position because of the availability of someone with greater credentials or formal restrictions preventing her from obtaining the position.

A potential employer may also prefer Candidate A because the employer believes Candidate A's high school diploma—or Candidate B's lack of diploma—is representative of other characteristics and traits. According to signaling theory, employers value information about job candidates; however, because obtaining information about job candidates is costly, employers rely on readily available "signals" (Rosenbaum et al 1990; Spence 1973). Therefore, rather than fully assessing the skills and competencies of all job candidates, employers interpret available information—such as educational credentials—to infer the abilities and qualities of job applicants (Blaug 1976).

Signaling theory has important implications for the socioeconomic standing of dropouts. Specifically, while a given high school dropout may possess the same skills and knowledge as a high school graduate, employers may see the high school diploma—or the absence of a high school diploma—as signaling unobservable characteristics, leading employers to prefer high school graduates.

Heckman and Rubinstein (2001) offer a classic example of the possible role of signaling in influencing labor market outcomes. They show that persons with a GED earn less than persons who completed high school. They argue that while a GED shows that an individual has comparable cognitive abilities to a high school graduate, it also signals to employers that GED holders "lack the ability to think ahead, to persists in tasks, or to adapt to their environments" (145). A similar conclusion holds for high school dropouts, only they also lack a certificate that asserts they possess cognitive abilities comparable to a high school graduate.

However, the evidence for signaling theory is far from decisive. Bills (1988) contends personality traits are as important to employers as formal education. Rosenbaum (2001) shows employers value soft skills over education related abilities. Cappelli (1995) contends employers rank character and attitude higher than education credentials. Moss and Tilly (2001) show employers privilege dependability and interpersonal communication above all else.

Overall, theories of labor market allocation offer strong motivation but mixed evidence for an independent effect of dropping out. There are clear reasons for why high school dropouts may be less qualified for a job, and there are similarly clear reasons for why an employer may prefer high school graduates to high school dropouts. However, the extent to which dropping out independently matters is less clear. Of particular concern is the ways in which dropouts and high school graduates differ beyond a high school diploma.

If individuals who drop out of high school are systematically different from those who complete high school, then researchers must be careful to account for these differences. If researchers are unable to account for differences between the two populations, there is reason to be concerned that observed differences between dropouts and high school graduates are due to unobserved variations. In effect, the observed differences in poverty and welfare receipt are not the result of dropping out, but instead are the result of the factors that lead individuals to drop out—the result of early life disadvantage and social contexts.

Past research has identified two populations that are at particular risk of dropping out: residents of economically disadvantaged neighborhoods and members of poor families. According to research on neighborhood effects, all things being equal, children who grow up in neighborhoods with a high concentration of disadvantage are more likely to drop out of high school than children who grow up in more affluent neighborhoods. Results from experiments and quasi-experiments such as Gatreaux and Moving to Opportunity have generally confirmed this finding: individuals who move from high poverty neighborhoods to middle class neighborhoods are more likely to graduate from high school (DeLuca & Dayton 2009). Methodological debates notwithstanding, observational studies also confirm the basic tenets of neighborhood effects (Brooks-Gunn et al 1993; Crowder & South 2011; Harding 2003; Wodtke, Harding, & Elwert 2011).

If neighborhood effects could be easily measured in observational data, then the confounding of neighborhood effects and dropout effects would be of little concern. Researchers could simply control for neighborhood disadvantage and obtain a reliable estimate of the dropout effect net of neighborhood effects. Unfortunately, neighborhoods effects are diverse and not easily measured. For example, Brooks-Gunn and colleagues (1993) find that living in a neighborhood with very few professional or managerial workers is associated with dropping out; Harding (2011) shows that neighborhood cultural context influences educational attainment; Crowder and South (2011) demonstrate that neighborhoods surrounding the immediate neighborhood of residence influence dropout rates. Overall, the diversity and complexity of neighborhood effects makes it difficult for researchers to confidently isolate a dropout effect using conventional OLS methods—should researchers include variables for the number of managers living in a neighborhood? how should a researcher operationalize neighborhood culture? what information about surrounding neighborhoods is relevant? Ruling out neighborhood effects with OLS methods and nonexperimental data is a near impossibility.

Similarly, the likelihood of dropping out varies by family socioeconomic status. Children and adolescents who grow up in poverty are less likely to complete high school (Alexander, Entwisle, & Horsey 1997; Duncan et al 1998). Numerous family characteristics influence educational attainment, and, like the confounding effects created by neighborhood disadvantage, researchers must account for relevant family characteristics to confidently estimate an isolated dropout effect.

Fully accounting for relevant family characteristics is a difficult task. For example, how often parents and children discuss school, whether parents are involved in parent-teacher organizations, and whether parents limit time watching television all influence the likelihood of high school completion (McNeal 1999). Similarly, parent-school connectivity matters in some contexts but not in others (Teachman et al 1996), and parenting practices are an important determinant of high school completion, but explain little of the difference in dropout rates between "intact" and "nonintact" families (Astone & McLanahan 1991). To control for relevant family characteristics using conventional regression methods, researchers must account for an array of variables that are difficult to observe and measure, let alone operationalize.

In sum, for researchers to confidently isolate a dropout effect, they must account for the determinants of dropping out. Using conventional regression methods, this would take a heroic effort and implausibly rich data. If relevant determinants of dropping out are omitted or measured inaccurately, then what appears to be a dropout effect on poverty may really be the result of omitted or poorly measured background characteristics. The implications are obvious and important: the same factors that lead adolescents to drop out may also lead to future poverty and welfare receipt—growing up in disadvantaged neighborhoods and families is strongly associated with adult poverty (Corcoran 1995; Duncan et al 1998; McLanahan 1985; Wagmiller et al 2006). For high school dropouts, the alternative to dropping out and poverty may be a high school diploma and poverty.

Data and Measures

Examining the consequences of dropping out requires detailed information on high school graduates and high school dropouts before they completed their education and also information on subsequent economic hardship. Because of the difficulty of measuring and observing all relevant background characteristics, a more robust method than conventional OLS methods is to compare siblings who have disparate education outcomes. I further elaborate on this point in the Analytic Strategy section of this paper, but for now it is sufficient to say that we need data that includes a sample of siblings and contains information on background characteristics, educational attainment, and subsequent economic hardship. The National

Longitudinal Study of Youth 1979 and 1997 (NLSY79 and NLSY97) cohorts meet these requirements.

The NLSY79 is a nationally representative sample of young adults who were between the ages of 14 and 22 when initially surveyed in 1979. From 1979 to 1994, follow-up waves were conducted annually. From 1994 to 2010, follow-up waves were conducted biennially. I transform the data to be structured by respondent age, not survey year. In effect, the baseline for all respondent is when they are 18 years of age, not 1979; t_2 is when respondents are 19 years old, not 1980; *t3* is when respondents are 20 years old, and so on.

The NLSY79 is comprised of three subsamples: a cross-sectional sample of 6,111 youths, a supplemental sample of 5,295 Hispanic, black, and economically disadvantaged youths, and a military sample of 1,280 youths. The full sample in the baseline survey was 12,686. All eligible respondents within a household were included in the initial sample. The 11,406 civilian respondents come from 7,490 unique households. 2,862 households include more than one NLSY79 respondent. The multi-respondent households include a total of 5,914 siblings.

To assess changes in the consequences of dropping out over time, I also use NLSY97. NLSY97 is a nationally representative sample of young adults who were between the ages of 12 and 16 when initially surveyed in 1997. Follow-up waves were conducted annually, and data are available through 2010. I impose the same time structure on NLSY97, where the baseline for all respondents is when they are 18 years of age, t_2 is when respondents are 19 years old and so on. The original NLSY97 sample included 8,984 respondents. 3,885 respondents have at least one sibling in the sample. When combined, the NLSY79 and NLSY97, provide data on 9,270 siblings from age 18 to 44, giving an analytic sample of 438,118 person years.

The two outcomes, poverty status and welfare receipt, are both measured with indicator variables. Poverty status is measured using the official US Census Bureau thresholds with thresholds adjusted by family size. A respondent is defined as in poverty if his or her household income fell below the poverty threshold. A respondent is defined as having received welfare if he or she reported any cash income from Aid to Families with Dependent Children (up to 1996) or Temporary Assistance to Needy Families (after 1996), the main cash welfare programs in the US. Note that while poverty and welfare receipt are related, they are only weakly correlated to each other (0.27). This is because many people living in poverty elect to not participate (often out of ideological commitments or an inability to meet participation requirements), are unaware they are eligible, are ineligible despite having an income below the poverty line, or are unable to successfully enroll.

A respondent is defined as having dropped out if she has not completed high school and is not currently enrolled in school. To account for within-family heterogeneity, I include measures of scholastic aptitude (standardized AFQT score), delinquency (regular use of alcohol or tobacco before the age of 18), and teen pregnancy (becoming a father or mother before the age of 18). The measures of within family heterogeneity help account for sibling differences that are associated with dropping out. For example, by including a measure of scholastic aptitude, we can be more confident that differences by dropout status are not really differences in scholastic aptitude. This issue is discussed in more detail in the analytic strategy. I also include demographic and background variables that are related to dropping out and economic hardship, including sex (male/female), race (white/black/Hispanic), whether respondent is the oldest sibling, marital status (single/married/divorced, separated, or widowed), parents' educational attainment (total years), urbanicty (central city/SMSA/not in SMSA), and region of residence

Table 2.1 Summary Statistics and Coding for All Variables.

	Description and Coding	Time Variant	Mean
Outcome Variables			
Poverty Status	1 = income fell below US Census Bureau poverty threshold; 0 = income exceeded poverty threshold	Yes	0.22
Welfare Receipt	1 = reported any cash income from AFDC or TANF; 0 = did not report income from AFDC or TANF	Yes	0.04
Independent Variables			
Dropout Status	1 = not currently enrolled in school and does not have a high school diploma; 0 = completed high school only	Yes	0.17
Sex	1 = male; 0 = female	No	0.52
Race			
White	1 = non-Hispanic White; 0 = Black or Hispanic	No	0.54
Black	1 = non-Hispanic Black; 0 = Other	No	0.28
Hispanic	1 = Hispanic; 0 = Other	No	0.18
Marital Status			
Never Married	1 = never married; 0 = other	Yes	0.49
Married	1 = married; 0 = other	Yes	0.39
Separated, Divorced, or Widowed	1 = separated, divorced, or widowed; 0 = other	Yes	0.11
Urbanicity			
Central City	1 = lives in a central city; 0 = other	Yes	0.45
Not in Central City, in SMSA	1 = lives in SMSA but not in a central city; 0 = other	Yes	0.38
Not in SMSA	1 = does not live in a SMSA; 0 = other	Yes	0.17
Region of Residence			0.18
Northeast	1 = lives in Northeast; 0 = other	Yes	0.24
North Central	1 = lives in North Central; 0 = other	Yes	0.39
South	1 = lives in South; 0 = other	Yes	0.19
West	1 = lives in West; o = other	Yes	
Mother's Education	Number of years of education completed by respondent's mother	No	11.02
Father's Education	Number of years of education completed by respondent's father	No	10.95
Within Sibling Differences			
Aptitude	Respondent's AFQT score, renormed in 2006 and divided by 10	No	40.8
Underage Alcohol Use	1 = started consuming alcohol at least once per week before the age of 18; 0 = did not start consuming alcohol once per week before the age of 18	No	0.27
Underage Tobacco Use	1 = started smoking before the age of 18, conditional on smoking at least 100 cigarettes; 0 = did not start smoking before 18	No	0.31
Teen Pregnancy	1 = became a mother or father before the age of 18; 0=did not become a parent until after the age of 18	No	0.12
Oldest Sibling	1 = oldest sibling ; 0 = other	No	0.41

Note: N=9,270

(South, Northeast, Midwest, West). These additional covariates serve two purposes. First, they help ensure that observed differences between dropouts and high school graduates are not due to demographic differences in the two populations. Second, they help establish a baseline estimate for comparing between family estimates and within family estimates. Summary statistics and descriptions of the coding for each of these variables are presented in Table 2.1.

Analytic Strategy

To understand the relationship between dropping out and future poverty and welfare receipt, we must account for background effects that influence both the odds of dropping out and future risk of economic hardship. At issue is the nonrandom selection into dropping out. The decision to dropout is not independent of background characteristics. Models that omit background characteristics may produce biased estimates of future disadvantage.

Traditionally, researchers attempt to account for selection into dropping out through the use of controls in regression models. For example, poverty status for individual i in family f (Y_{if}) is a function of dropout status (X_{if}), a vector of observed controlled variables (Z_{if}), a family-specific error term (ε_f), and a random error term (ε_{if}), giving the equation:

$$\text{[Equation 1]} \quad (Y_{if}) = \alpha \, X_{if} + \beta \, Z_{if} + \varepsilon_f + \varepsilon_{if}$$

When we estimate Equation 1, α will estimate the impact of dropping out on poverty status net of social background characteristics. However, the dropout effect will be biased if there is a correlation between dropping out (X_{if}) and unobserved family characteristics (ε_f). By basing model estimation on variation within families only, sibling fixed-effects offer the ability to account for unobserved background characteristics and a means to separate background effects from dropout effects.

For example, assume a family has two children, Sibling A and Sibling B. Let Y_{1f} be the poverty status for Sibling A, Y_{2f} be the poverty status for Sibling B, X_{1f} and X_{2f} is dropout status for each sibling respectively, Z_{1f} and Z_{2f} are a vector of observed control variables, ε_{1f} and ε_{2f} are individual specific error terms, and ε_f is a family specific error term. The equation for Sibling A is

(Equation 2) $$Y_{1f} = \alpha X_{1f} + \beta Z_{1f} + \varepsilon_f + \varepsilon_{1f}$$

The equation for Sibling B is

(Equation 3) $$Y_{2f} = \alpha X_{2f} + \beta Z_{2f} + \varepsilon_f + \varepsilon_{2f}$$

If we subtract Equation 3 from Equation 2, we are left with

(Equation 4) $$Y_{1f} - Y_{2f} = \alpha (X_{1f} - X_{2f}) + \beta (Z_{1f} - Z_{2f}) + \varepsilon_{1f} + \varepsilon_{2f}$$

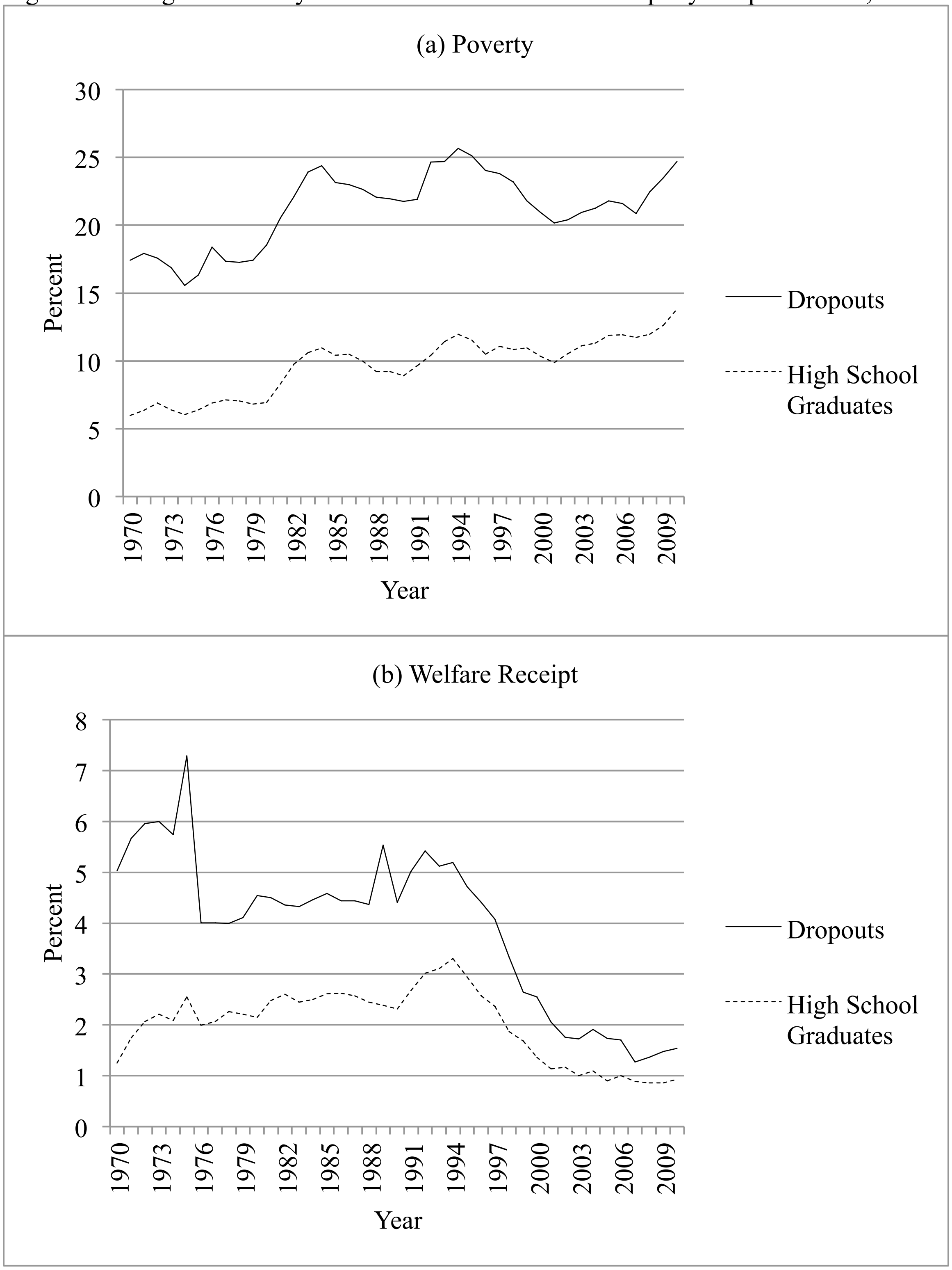
Therefore, Equation 4 removes the family and neighborhood background characteristics shared by siblings, leaving sibling differences in poverty status as a function of dropping out of high school. In effect, because siblings experience the same neighborhoods, families, and other social backgrounds, we are able to account for these effects without observing, measuring, or operationalizing them, which means the dropout effect is not biased by unobserved heterogeneity.

Researchers have used sibling fixed-effect models in a number of studies to address potential biases created by nonrandom allocation to different groups, such as the consequences of teenage pregnancy (experienced a teenage pregnancy versus did not experience a teenage pregnancy) (Bronars & Grogger 1994; Geronimus & Korenman 1992; Rosenzweig & Wolpin 1995), returns to Head Start participation (attended Head Start versus did not attend Head Start) (Currie & Thomas 1995), and the impact of neighborhoods on educational outcomes (living in different neighborhoods) (Aaronson 1998). However, it is important to note two limitations of the family fixed-effects estimator. First, siblings are seldom the same age, and thus may experience different social contexts. For example, family income or parenting styles may change over time. If these differences are correlated with dropping out, this could bias the dropout effect. Second, there may be differences among siblings that are correlated with dropping out. This has the potential to bias the dropout effect; however, to the extent that these differences are observed (such as including a proxy for scholastic aptitude as measured by standardized test scores), we can reduce this potential bias. Accordingly, I include measures of within family differences.

Additionally, because the outcomes of interest—poverty status and welfare receipt—are nonlinear and discrete variables, some fixed-effects strategies are not viable. Specifically, in a non-linear fixed-effects model, the number of nuisance parameters grows as sample size increases, which produces biased covariate estimates. This is known as the incidental parameter problem. Consequently, I estimate both Chamberlain's (1980) conditional logistic and linear probability models. The Chamberlain model estimates logistic fixed-effects using conditional likelihood functions, which excludes all siblings that do not vary on poverty or welfare status. The linear probability model, while adding ease of interpretation, produces predicted probabilities that are not constrained between 0 and 1. As is common, I present the results from the linear probability models.

Note that in the in the conventional OLS estimates I correct standard errors for household clustering. For the sibling fixed-effects, I correct standard errors for clustering within individuals and households.

Figure 2.1 Weighted Poverty Rate and Rate of Welfare Receipt by Dropout Status, 1970-2010.



Note: Data from March Supplements of Current Population Survey, 1970-2010. For Panel A, n=3,785,905. For Panel B; n=3,488,487. Percentages are weighted to reflect cross-sectional national averages.

Findings

Figure 1 displays the percent poor and percent receiving welfare by dropout status from 1970-2010. In the top panel of Figure 1, Panel A, we see a sizable gap in poverty rates between dropouts and high school graduates. The trends in poverty rate largely mirror each other and the overall difference in poverty has changed little since the 1970s. In the bottom panel of Figure 1, Panel B, we see a large gap in the rate of welfare receipt between dropouts and high school graduates that narrows substantially over time. However, most of the narrowing occurs immediately following welfare reform in the mid-1990s and continues through 2010. Clearly, dropouts face an increased risk of poverty and welfare receipt, but would the risk of poverty or welfare receipt be eliminated if dropouts had completed high school?

Table 2.2 reports estimates of the effect of dropping out of high school on future poverty. Columns 1 through 4 present coefficients from conventional estimates (Equation 1). The first model estimates a baseline dropout effect and controls only for cohort. We see that dropping out increases the probability of living in poverty by 29 percent. Next, I add a set of standard demographic covariates (Column 2). These demographic covariates reduce the dropout effect slightly. The dropout effect decreases by 15 percent, but the effect remains large and significant. I next add controls for AFQT score (Column 3) and other background covariates (Column 4). The addition of these covariates reduces the dropout effect substantially. Once sibling order, parent education, scholastic aptitude, delinquency measures, and teen pregnancy are added to the model, the dropout effect is reduced by an additional 37 percent. However, most of this reduction is attributable to differences in scholastic aptitude. By not completing high school, dropouts increase the probability of living in poverty by 16 percent. The other covariates in the model report consistent and expected findings: women face a higher risk of poverty than men; blacks

Table 2.2 The Effect of Dropping Out on Poverty.

	Conventional Estimates				Sibling Fixed-Effects		
	Column 1	Column 2	Column 3	Column 4	Column 5	Column 6	Column 7
	Coefficient Std. Err.	Coefficient Std. Err.	Coefficient Std. Err.	Coefficient Std. Err.	Coefficient Std. Err.	Coefficient Std. Err.	Coefficient Std. Err.
Dropout	0.289***	0.247***	0.175***	0.155***	0.140***	0.117***	0.107***
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
Young Cohort	0.178***	0.150***	0.125***	0.123***	0.194***	0.177***	0.184***
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.02)
Black		0.138***	0.073***	0.072***			
		(0.01)	(0.01)	(0.01)			
Hispanic		0.111***	0.024*	0.030**			
		(0.01)	(0.01)	(0.01)			
Female		0.078***	0.070***	0.056***	0.063***	0.062***	0.050***
		(0.01)	(0.01)	(0.01)	(0.00)	(0.00)	(0.01)
North Central		0.023*	0.033***	0.030**	-0.020	-0.022	-0.025
		(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
South		-0.000	-0.002	0.000	-0.022*	-0.019	-0.014
		(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
West		-0.029**	-0.002	-0.005	-0.049***	-0.053***	-0.057***
		(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
SMSA Only		-0.097***	-0.075***	-0.063***	-0.062***	-0.061***	-0.055***
		(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
Central City		-0.058***	-0.037***	-0.033***	-0.051***	-0.051***	-0.048***
		(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
Married		-0.162***	-0.141***	-0.130***	-0.121***	-0.117***	-0.105***
		(0.01)	(0.01)	(0.01)	(0.00)	(0.00)	(0.00)
Separated		0.020*	0.024*	0.019	0.020**	0.017*	0.023**
		(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
Mother's Education			-0.004*	-0.002			
			(0.00)	(0.00)			
Father's Education			-0.002	-0.002			
			(0.00)	(0.00)			

Oldest Sibling			-0.005	-0.002		-0.005	-0.008
			(0.00)	(0.01)		(0.00)	(0.00)
AFQT			-0.022***	-0.019***		-0.019***	-0.016***
			(0.00)	(0.00)		(0.00)	(0.00)
Teen Tobacco Use				0.016*			0.018**
				(0.01)			(0.01)
Teen Alcohol Consumption				0.004			-0.003
				(0.01)			(0.01)
Teen Parenthood				0.103***			0.067***
				(0.01)			(0.01)
Constant	0.142***	0.183***	0.342***	0.294***	0.253***	0.332***	0.298***
	(0.00)	(0.01)	(0.02)	(0.02)	(0.01)	(0.01)	(0.01)

Note: * p < .05, ** p < .01, ***p < .001; 90,704 person years; 3,121 sibling group and Hispanics face a higher risk of poverty than whites; increases in AFQT score are associated with a lower risk of poverty; delinquency is associated with an increased risk of poverty; teen parenthood increases the risk of poverty.

Column 5, Column 6, and Column 7 present coefficients from sibling fixed-effects estimates (Equation 4). In Column 5, I account for only demographic covariates. In Column 6, I account for demographic covariates and sibling differences in AFQT. In Column 7, I account for additional sibling differences that may influence dropping out—alcohol use during high school, tobacco use during high school, and becoming a parent during high school. When only demographic variations are accounted for, dropping out increases the likelihood of living in poverty by 14 percent. Once sibling differences in aptitude, delinquency, and parenthood are modeled, the dropout effect is reduced to 11 percent (Column 7). Importantly, differences in scholastic aptitudes are associated with only a minimal change in the likelihood of poverty. The differences between the full random effects estimates presented in Column 4 and the full fixed-effects estimates presented in Column 7 demonstrate the difficulty of capturing background effects with proxy measures for social background and also reveal that traditional estimates overstate the effects of dropping out on poverty.

Table 2.3 reports estimates of the effect of dropping out on welfare receipt. I follow the same modeling strategy outlined above, first estimating four conventional models and then three sibling fixed-effects models. In the baseline model, Column 1, we see that dropping out of high school increases the likelihood of receiving welfare by 8 percent. Once basic demographic covariates are accounted for (Column 2), the dropout effect is largely unchanged. Controlling for scholastic aptitude (Column 3) results in a decrease in the likelihood of receipt of public assistance by 21 percent. The addition of other background covariates (Column 4) reduces

Table 2.3 The Effect of Dropping Out on Welfare Receipt.

	Conventional Estimates				Sibling Fixed-Effects		
	Column 1	Column 2	Column 3	Column 4	Column 5	Column 6	Column 7
	Coefficient Std. Err.	Coefficient Std. Err.	Coefficient Std. Err.	Coefficient Std. Err.	Coefficient Std. Err.	Coefficient Std. Err.	Coefficient Std. Err.
Dropout	0.080***	0.077***	0.061***	0.042***	0.044***	0.042***	0.031***
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
Young Cohort	-0.020***	-0.022***	-0.028***	-0.031***	-0.004	-0.003	-0.012
	(0.00)	(0.00)	(0.00)	(0.00)	(0.01)	(0.01)	(0.01)
Black		0.056***	0.037***	0.030***			
		(0.00)	(0.01)	(0.01)			
Hispanic		0.030***	0.006	0.007			
		(0.01)	(0.01)	(0.01)			
Female		0.082***	0.075***	0.067***	0.078***	0.078***	0.066***
		(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
North Central		0.035***	0.032***	0.030***	0.012	0.012	0.019*
		(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
South		-0.016***	-0.021***	-0.018***	-0.018**	-0.017*	-0.010
		(0.00)	(0.00)	(0.01)	(0.01)	(0.01)	(0.01)
West		0.018**	0.021**	0.022**	0.009	0.010	0.013
		(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
SMSA Only		-0.028***	-0.021***	-0.014**	-0.022***	-0.023***	-0.018***
		(0.00)	(0.00)	(0.01)	(0.00)	(0.00)	(0.00)
Central City		-0.005	0.003	0.005	-0.004	-0.006	-0.006
		(0.01)	(0.01)	(0.01)	(0.00)	(0.00)	(0.00)
Married		-0.035***	-0.028***	-0.022***	-0.020***	-0.019***	-0.014***
		(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
Separated		0.031***	0.036***	0.031***	0.021***	0.021***	0.018**
		(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
Mother's Education			0.000	-0.000			
			(0.00)	(0.00)			
Father's Education			-0.001	-0.001			
			(0.00)	(0.00)			

Oldest Sibling			0.003	0.005		0.000	0.004
			(0.00)	(0.00)		(0.00)	(0.00)
AFQT			-0.007***	-0.006***		-0.005***	-0.003***
			(0.00)	(0.00)		(0.00)	(0.00)
Teen Tobacco Use				0.006			0.010*
				(0.00)			(0.00)
Teen Alcohol Consumption				0.001			-0.003
				(0.00)			(0.00)
Teen Parenthood				0.071***			0.070***
				(0.01)			(0.01)
Constant	0.042***	-0.001	0.040***	0.031**	0.028***	0.049***	0.027**
	(0.00)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)

Note: * p < .05, ** p < .01, ***p < .001; 90,704 person years; 3,121 sibling groups

the dropout effect substantially. Compared to the model that omits background covariates (Column 3), the full model reduces the likelihood of receiving public assistance by 45 percent. By not completing high school, dropouts increase their probability of receiving welfare by 4 percent.

Columns 5, 6, and 7 of Table 2.3 report estimates of the probability of receiving welfare from sibling fixed-effects models. Here we see consistency between the conventional and sibling fixed-effects estimates. When we first account for only demographics in Column 4, we see that dropping out increases the likelihood of welfare receipt by just over 4 percent. When we account for differences in scholastic aptitude (Column 6), the likelihood of welfare receipt changes little. Once we add measures of within-family heterogeneity (Column 7), the dropout effect is reduced to less than 3 percent. The random and fixed-effects estimates from Columns 4 and 7 differ modestly. Unobserved background effects play only a minor role in the probability of welfare receipt for high school dropouts.

The above results indicate that dropping out of high school has a significant and important effect on future poverty and welfare receipt. However, it is possible that the dropout effect and the role of background characteristics have changed over time. Accordingly, I run the analyses separately by cohort to examine possible differential effects.

Table 2.4 shows estimates of the effect of dropping out on future poverty for the older cohort only, and Table 2.5 shows estimates of the effect of dropping out on future welfare receipt for the older cohort only. For both tables, I follow the same modeling strategy as reported in Table 2.2 and Table 2.3. In the baseline model (Column 1), we see that dropping out increases the probability of poverty by close to 30 percent. Once demographic covariates are added to the model (Column 2), the risk of poverty for dropouts decreases modestly. Differences in scholastic

Table 2.4 The Effect of Dropping Out on Poverty, 1979 Cohort.

	Conventional Estimates				Sibling Fixed-Effects		
	Column 1	Column 2	Column 3	Column 4	Column 5	Column 6	Column 7
	Coefficient Std. Err.	Coefficient Std. Err.	Coefficient Std. Err.	Coefficient Std. Err.	Coefficient Std. Err.	Coefficient Std. Err.	Coefficient Std. Err.
Dropout	0.296***	0.260***	0.180***	0.160***	0.135***	0.120***	0.110***
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
Black		0.165***	0.088***	0.092***			
		(0.01)	(0.01)	(0.01)			
Hispanic		0.071***	0.001	0.002			
		(0.01)	(0.01)	(0.01)			
Female		0.080***	0.072***	0.060***	0.068***	0.068***	0.056***
		(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
North Central		0.034***	0.038***	0.036***	0.002	0.004	0.007
		(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
South		-0.003	-0.004	-0.000	-0.010	-0.003	0.004
		(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
West		0.008	0.016	0.017	-0.037**	-0.029*	-0.027*
		(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
SMSA Only		-0.095***	-0.073***	-0.058***	-0.061***	-0.059***	-0.053***
		(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
Central City		-0.058***	-0.034***	-0.028**	-0.051***	-0.050***	-0.046***
		(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
Married		-0.156***	-0.138***	-0.124***	-0.123***	-0.119***	-0.104***
		(0.01)	(0.01)	(0.01)	(0.00)	(0.00)	(0.01)
Separated		0.021*	0.025*	0.020	0.013	0.012	0.019*
		(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
Mother's Education			-0.006***	-0.005**			
			(0.00)	(0.00)			
Father's Education			-0.002	-0.003*			
			(0.00)	(0.00)			
Oldest Sibling			-0.003	0.001		-0.004	-0.008
			(0.01)	(0.01)		(0.00)	(0.00)

AFQT			-0.020***	-0.017***		-0.017***	-0.013***
			(0.00)	(0.00)		(0.00)	(0.00)
Teen Tobacco Use				0.020**			0.021**
				(0.01)			(0.01)
Teen Alcohol Consumption				0.013			0.001
				(0.01)			(0.01)
Teen Parenthood				0.106***			0.066***
				(0.01)			(0.01)
Constant	0.141***	0.167***	0.354***	0.301***	0.245***	0.310***	0.259***
	(0.00)	(0.01)	(0.02)	(0.02)	(0.01)	(0.01)	(0.01)

Note: * p < .05, ** p < .01, ***p < .001; 75,595 person years; 2,209 sibling groups

Table 2.5 The Effect of Dropping Out on Welfare Receipt, 1979 Cohort.

	Conventional Estimates				Sibling Fixed-Effects		
	Column 1	Column 2	Column 3	Column 4	Column 5	Column 6	Column 7
	Coefficient Std. Err.	Coefficient Std. Err.	Coefficient Std. Err.	Coefficient Std. Err.	Coefficient Std. Err.	Coefficient Std. Err.	Coefficient Std. Err.
Dropout	0.086***	0.085***	0.065***	0.045***	0.045***	0.044***	0.031***
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
Black		0.067***	0.043***	0.037***			
		(0.01)	(0.01)	(0.01)			
Hispanic		0.022***	0.000	0.001			
		(0.01)	(0.01)	(0.01)			
Female		0.087***	0.078***	0.072***	0.083***	0.083***	0.071***
		(0.00)	(0.00)	(0.01)	(0.00)	(0.00)	(0.00)
North Central		0.039***	0.034***	0.033***	0.012	0.010	0.019*
		(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
South		-0.021***	-0.024***	-0.021***	-0.030***	-0.029***	-0.021**
		(0.00)	(0.00)	(0.01)	(0.01)	(0.01)	(0.01)
West		0.026***	0.026***	0.027***	0.002	0.004	0.005
		(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
SMSA Only		-0.030***	-0.022***	-0.015**	-0.024***	-0.024***	-0.019***
		(0.01)	(0.01)	(0.01)	(0.00)	(0.00)	(0.00)
Central City		-0.006	0.002	0.005	-0.006	-0.007	-0.006
		(0.01)	(0.01)	(0.01)	(0.00)	(0.00)	(0.00)
Married		-0.033***	-0.027***	-0.020***	-0.021***	-0.019***	-0.014***
		(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
Separated		0.033***	0.037***	0.032***	0.021***	0.022***	0.019**
		(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
Mother's Education			-0.001	-0.001			
			(0.00)	(0.00)			
Father's Education			-0.001	-0.001			
			(0.00)	(0.00)			
Oldest Sibling			0.002	0.005		-0.001	0.004
			(0.00)	(0.00)		(0.00)	(0.00)

AFQT			-0.007***	-0.005***		-0.005***	-0.003**
			(0.00)	(0.00)		(0.00)	(0.00)
Teen Tobacco Use				0.007			0.011*
				(0.00)			(0.01)
Teen Alcohol Consumption				0.003			-0.003
				(0.01)			(0.01)
Teen Parenthood				0.073***			0.081***
				(0.01)			(0.01)
Constant	0.041***	-0.007	0.043***	0.035**	0.036***	0.055***	0.027**
	(0.00)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)

Note: * p < .05, ** p < .01, ***p < .001; 75,595 person years; 2,209 sibling groups

ability reduce the risk of poverty substantially (31 percent), and the full set of observed background covariates further reduces the risk of poverty. In the full model, the risk of poverty for dropouts is 16 percent. In Column 5, Column 6, and Column 7, I report estimates from the sibling fixed-effects models. When we only account for demographic covariates, the risk of poverty is 14 percent for high school dropouts. When we add sibling differences in scholastic ability to the model, the risk of poverty is reduced to 12 percent. When we add sibling differences in alcohol use, tobacco use, and teen parenthood to the model, the risk of poverty is reduced to 11 percent. Importantly, we observe notable differences between the random effects estimate and the sibling fixed-effects estimate, indicating that unobserved background characteristics are biasing the dropout effect.

We observe a similar pattern for the effect of dropping out on welfare receipt for the older cohort (Table 2.5). In the baseline model, dropping out increases the risk of welfare by 9 percent for the older cohort. Accounting for demographic covariates does little, but the full set of background covariates reduces the risk to fewer than 5 percent. The sibling fixed-effects models report modestly lower estimates of the effect of dropping out. In the full sibling fixed-effects model, dropping out increases the risk of welfare receipt by 3 percent. The minimal differences between estimates presented in Columns 4 and 7 of Table 2.5 again indicate that the dropout effect on welfare is only biased minimally by unobserved background characteristics.

However, for the younger cohort, we observe notable differences in the effect of dropping out on future poverty and welfare receipt, reported in Table 2.6 and Table 2.7 respectively. In particular, in Column 4 of Table 2.6, we see that the risk of poverty for high school dropouts is a little over 12 percent. In Column 7, we see a significant difference in the sibling fixed-effect estimates. Whereas estimates from the conventional and fixed-effect estimates were more similar for the older cohort, the sibling fixed-effects estimate is substantially different for the younger cohort. Once unobserved background characteristics and sibling differences are modeled, the risk of poverty is a little over 6 percent, nearly half of the conventional estimate.

The findings reported in Table 2.6 and Table 2.7 have three important implications. First, for the younger cohort, estimates of the dropout effect are biased by unobserved heterogeneity. While dropping out still increases the risk of poverty for the younger cohort, much of the risk comes from unobserved differences between dropouts and high school graduates. Second, background characteristics that lead to dropping out are becoming more important in determining poverty across cohorts. Third, the effect of dropping out on poverty is decreasing over time.

We also see important differences between cohorts in the risk of welfare receipt for dropouts. In the conventional models, the risk of welfare receipt for dropouts in the younger cohort goes from over 4 percent in the baseline model to under 2 percent in the full model. The dropout effect in the full model, however, approaches statistical significance but is not statistically significant. Estimates from conventional models are significantly lower for the younger cohort compared to the older cohort, and estimates from the sibling fixed-effects are not statistically significant. For the younger cohort, differences in rate of welfare receipt among high school dropouts are minimal and largely due to unobserved characteristics.

Table 2.6 The Effect of Dropping Out on Poverty, 1997 Cohort.

	Conventional Estimates				Sibling Fixed-Effects		
	Column 1	Column 2	Column 3	Column 4	Column 5	Column 6	Column 7
	Coefficient Std. Err.	Coefficient Std. Err.	Coefficient Std. Err.	Coefficient Std. Err.	Coefficient Std. Err.	Coefficient Std. Err.	Coefficient Std. Err.
Dropout	0.259***	0.211***	0.140***	0.126***	0.090***	0.067***	0.062**
	(0.02)	(0.02)	(0.03)	(0.03)	(0.02)	(0.02)	(0.02)
Black		0.049**	-0.006	-0.006			
		(0.02)	(0.03)	(0.03)			
Hispanic		0.209***	0.134***	0.131***			
		(0.02)	(0.03)	(0.03)			
Female		0.066***	0.046**	0.042*	0.039**	0.033*	0.032*
		(0.01)	(0.02)	(0.02)	(0.01)	(0.01)	(0.01)
North Central		-0.062**	-0.048	-0.046	0.013	-0.012	-0.015
		(0.02)	(0.03)	(0.03)	(0.05)	(0.05)	(0.05)
South		-0.051**	-0.046	-0.047	-0.067	-0.062	-0.066
		(0.02)	(0.03)	(0.03)	(0.04)	(0.04)	(0.04)
West		-0.086***	-0.060	-0.056	-0.030	-0.058	-0.061
		(0.02)	(0.03)	(0.03)	(0.04)	(0.05)	(0.05)
SMSA Only		-0.073***	-0.091**	-0.092**	-0.032	-0.034	-0.034
		(0.02)	(0.03)	(0.03)	(0.02)	(0.02)	(0.02)
Central City		-0.040	-0.061	-0.064*	-0.043	-0.052*	-0.052*
		(0.02)	(0.03)	(0.03)	(0.02)	(0.02)	(0.02)
Married		-0.128***	-0.136***	-0.137***	-0.103***	-0.101***	-0.102***
		(0.01)	(0.02)	(0.02)	(0.01)	(0.01)	(0.01)
Separated		0.068**	0.061	0.058	0.059*	0.042	0.041
		(0.02)	(0.03)	(0.03)	(0.02)	(0.03)	(0.03)
Mother's Education			-0.001	-0.000			
			(0.00)	(0.00)			
Father's Education			-0.001	-0.001			
			(0.00)	(0.00)			
Oldest Sibling			-0.010	-0.008		-0.014	-0.011
			(0.02)	(0.02)		(0.01)	(0.01)

AFQT			-0.019***	-0.019***		-0.013***	-0.014***
			(0.00)	(0.00)		(0.00)	(0.00)
Teen Tobacco Use				0.014			0.043**
				(0.02)			(0.02)
Teen Alcohol Consumption				-0.028			-0.035*
				(0.02)			(0.02)
Teen Parenthood				0.043			0.044*
				(0.02)			(0.02)
Constant	0.326***	0.364***	0.499***	0.498***	0.430***	0.496***	0.489***
	(0.01)	(0.03)	(0.06)	(0.06)	(0.04)	(0.04)	(0.04)

Note: * p < .05, ** p < .01, ***p < .001; 15,109 person years; 1,150 sibling groups

Table 2.7 The Effect of Dropping Out on Welfare Receipt, 1997 Cohort.

	Conventional Estimates				Sibling Fixed-Effects		
	Column 1	Column 2	Column 3	Column 4	Column 5	Column 6	Column 7
	Coefficient Std. Err.	Coefficient Std. Err.		Coefficient Std. Err.	Coefficient Std. Err.		Coefficient Std. Err.
Dropout	0.051***	0.042***	0.020	0.012	0.020*	0.021*	0.019
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
Black		-0.001	-0.006	-0.007			
		(0.01)	(0.01)	(0.01)			
Hispanic		0.049***	0.035**	0.032**			
		(0.01)	(0.01)	(0.01)			
Female		0.052***	0.033***	0.029***	0.045***	0.039***	0.038***
		(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
North Central		0.003	-0.006	-0.005	0.030	0.037	0.037
		(0.01)	(0.01)	(0.01)	(0.03)	(0.03)	(0.03)
South		-0.007	-0.004	-0.006	0.053	0.063	0.063
		(0.01)	(0.01)	(0.01)	(0.03)	(0.03)	(0.03)
West		0.017	0.011	0.012	0.019	0.038	0.038
		(0.01)	(0.01)	(0.01)	(0.02)	(0.03)	(0.03)
SMSA Only		0.005	0.002	0.002	-0.001	-0.009	-0.009
		(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
Central City		0.023**	0.008	0.008	0.002	-0.007	-0.007
		(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
Married		-0.016***	-0.011*	-0.013*	-0.013**	-0.017**	-0.018***
		(0.00)	(0.01)	(0.01)	(0.00)	(0.01)	(0.01)
Separated		0.012	0.003	-0.000	0.008	-0.018	-0.019
		(0.01)	(0.01)	(0.01)	(0.02)	(0.02)	(0.02)
Mother's Education			0.000	0.001			
			(0.00)	(0.00)			
Father's Education			-0.000	-0.000			
			(0.00)	(0.00)			
Oldest Sibling			0.010	0.009		0.006	0.006
			(0.01)	(0.01)		(0.00)	(0.01)

AFQT			-0.002	-0.002			0.000
			(0.00)	(0.00)			(0.00)
Teen Tobacco Use				-0.004			0.011
				(0.01)			(0.01)
Teen Alcohol Consumption				-0.003			-0.006
				(0.01)			(0.01)
Teen Parenthood				0.035**			0.026*
				(0.01)			(0.01)
Constant	0.028***	-0.020*	-0.003	-0.005	-0.017	-0.022	-0.027
	(0.00)	(0.01)	(0.01)	(0.01)	(0.02)	(0.03)	(0.03)

Note: * $p < .05$, ** $p < .01$, ***$p < .001$; 15,109 person years; 1,150 sibling group

Table 2.8 The Effect of Dropping Out on Poverty for Ages 18-30, 1979 Cohort.

	Conventional Estimates				Sibling Fixed-Effects		
	Column 1	Column 2	Column 3	Column 4	Column 5	Column 6	Column 7
	Coefficient Std. Err.	Coefficient Std. Err.	Coefficient Std. Err.	Coefficient Std. Err.	Coefficient Std. Err.	Coefficient Std. Err.	Coefficient Std. Err.
Dropout	0.290***	0.266***	0.180***	0.163***	0.123***	0.109***	0.102***
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
Black		0.192***	0.106***	0.104***			
		(0.01)	(0.01)	(0.01)			
Hispanic		0.071***	-0.010	-0.010			
		(0.01)	(0.01)	(0.01)			
Female		0.093***	0.084***	0.068***	0.076***	0.076***	0.062***
		(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
North Central		0.046***	0.049***	0.046***	-0.007	-0.003	0.001
		(0.01)	(0.01)	(0.01)	(0.02)	(0.02)	(0.02)
South		0.003	-0.002	0.000	-0.008	-0.000	0.006
		(0.01)	(0.01)	(0.01)	(0.02)	(0.02)	(0.02)
West		0.017	0.025	0.022	-0.037*	-0.028	-0.027
		(0.01)	(0.01)	(0.01)	(0.02)	(0.02)	(0.02)
SMSA Only		-0.105***	-0.083***	-0.069***	-0.065***	-0.062***	-0.061***
		(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
Central City		-0.065***	-0.037***	-0.032**	-0.053***	-0.050***	-0.050***
		(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
Married		-0.143***	-0.130***	-0.117***	-0.121***	-0.117***	-0.098***
		(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
Separated		0.064***	0.062***	0.042**	0.051***	0.048***	0.047***
		(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
Mother's Education			-0.009***	-0.008***			
			(0.00)	(0.00)			
Father's Education			-0.003*	-0.003*			
			(0.00)	(0.00)			
Oldest Sibling			-0.007	-0.003		-0.007	-0.010*
			(0.01)	(0.01)		(0.00)	(0.00)

AFQT			-0.022***	-0.018***		-0.018***	-0.013***
			(0.00)	(0.00)		(0.00)	(0.00)
Teen Tobacco Use				0.007			0.004
				(0.01)			(0.01)
Teen Alcohol Consumption				0.019*			0.007
				(0.01)			(0.01)
Teen Parenthood				0.118***			0.064***
				(0.01)			(0.01)
Constant	0.155***	0.143***	0.379***	0.327***	0.251***	0.315***	0.273***
	(0.00)	(0.01)	(0.02)	(0.02)	(0.01)	(0.02)	(0.02)

Note: * p < .05, ** p < .01, ***p < .001; 32,586 person years; 2,200 sibling groups

Lastly, to further compare the risk of poverty for high school dropouts across cohorts, I include a final set of models (Table 2.8). In these models, I examine the risk of poverty for the older cohort, limiting the sample to the years where respondents are below the age of 30—the oldest age where data are available for the younger cohort. This set of models allows me to compare the risk of poverty for the older and younger cohorts during the same age range. Overall, we observe a pattern similar to the previously reported findings. The dropout effect is larger for the older cohort even when limited to the same age range. A t-test confirms that the observed differences in the risk of poverty are statistically different across cohorts in a one tailed test at .05 or at .10 in a two tailed test (t =1.71) Moreover, even when the sample is restricted for younger ages for the older cohort, the unobserved background covariates explain less of the difference between the conventional estimates and sibling fixed-effects estimates. This further suggests that unobserved covariates are more influential for the younger cohort.

Discussion

There is widespread public concern about the economic fate of dropouts. These concerns are well intentioned, but lack empirical support because of the difficulties of estimating an unbiased dropout effect. Dropouts differ from high school graduates in more ways than just a high school diploma, and researchers must be careful to account for these differences or they run the risk of misattributing background effects to dropout effects.

In this study, I present both conventional regression estimates and sibling fixed-effects estimates on the effect of dropping out on poverty and welfare receipt, a surprisingly understudied topic. The conventional estimates are inline with cross-sectional results, noting a strong effect of dropping out on poverty and welfare receipt that is nearly halved by controlling for a relatively limited set of demographic and social background covariates. The sibling fixed-effects estimates attenuate this finding: when model estimation is based on within-family differences, I find smaller estimates of the consequences of dropping out on future poverty and welfare receipt. However, the dropout effect remains substantial and significant. The implications of this are clear and important: by not completing high school, dropouts are increasing the probability of experiencing poverty and receiving welfare.

The research presented here also unearths an important temporal change in the consequences of dropping out. For the older cohort, those who were high school age in the 1970s, the conventional estimates and sibling fixed-effects estimates are more closely aligned. For the younger cohort, those who were high school age in the 1990s, we observe large differences between the conventional and within-family estimates.

For the younger cohort, conventional estimates substantially overstate the cost of dropping out. Unobserved background characteristics are important and are able to explain nearly half of the increased probability of poverty among dropouts and completely explain away the increased probability of welfare. While cohort difference in welfare receipt among dropouts is likely due to welfare reform, a takeaway remains: for the younger cohort, once unobserved background characteristics are accounted for, dropouts are no more likely to receive welfare than high school graduates.

However, it remains to be seen if this disparity across cohorts persists. The NLSY97 cohort is only in early adulthood in the most recent survey years. It is possible that over time the conventional and sibling fixed-effects estimates will converge and more closely resemble the estimates of the older cohort.

If the estimates do not converge over time, then we will need to reconsider our understanding of how educational attainment influences outcomes and also how social scientists attempt to measure the effects of educational attainment. For the older cohort, the bias in the conventional estimates is minimal. This is consistent with other work that attempts to measure the amount of bias in OLS estimates of educational effects on earnings (Card 1999). For the younger cohort, if the gap between the within family estimates and between family estimates remains large, social scientists will have more difficulty estimating the effects of educational attainment.

These findings have important policy implications. If unobserved background characteristics explained the differences in the increased likelihood of poverty or welfare receipt among dropouts, then policies aimed solely at increasing graduation rates would be misguided. Instead, policies would need to address the background determinants that make dropouts differ from high school graduates. This would create a difficult task for policymakers—policies aimed at family changes are more challenging than policies aimed at school changes. However, this is not the case. A dropout effect remains even after accounting for unobserved differences. Thus, policies that are able to increase graduation rates should reduce the rate of poverty among students who would have dropped out otherwise.

Recent education reforms largely push for an increase in standards and added requirements for high school graduation. The findings presented here support past calls for a careful reconsideration of how to balance improving the quality of education without increasing the dropout rate (Alexander et al 1985; McDill et al 1986). Consider high school exit examinations. Approximately two-thirds of all American high school students must pass an exit examination to earn their diploma (Warren 2007). High school exit examinations are no doubt well intentioned and there are many good reasons to ensure that high school graduates meet certain skills and knowledge requirements; however, high school exit examinations also increase dropout rates among low-ability students (Jacob 2001). Students who would have completed high school if not for the exit examination now face an increased risk of poverty.

Finally, the results indicate that a large portion of the disparity between high school graduates and high school dropouts is explained by social and economic background. This speaks to the limit of schools and also supports recent shifts in anti-poverty policies that focus on intervention at the community level (e.g. Promise Neighborhoods and Promise Zones). Efforts to improve the socioeconomic position of high school dropouts do not need to be limited to classroom walls.

This study also suggests potentially fruitful avenues for future research. First, this research shows cohort differences in the consequences of dropping out, but is unable to speak to the source of these differences. Over the past forty years, the labor market has changed dramatically, seeing a large growth in job precarity (Kalleberg 2011). Thus, it seems likely that changes in the labor market drive cohort differences; however, additional research is needed. Additionally, this research demonstrates that dropping out has an effect on poverty independent of background characteristics, but does not address the root cause of this difference. What is it about dropping out? Is it the loss of a credential? Diminished human capital and skills? Research that explicitly tests the mechanisms that lead from dropping out to poverty is needed. Lastly, while the effect of dropping out on poverty and welfare receipt is understudied, the effect of dropping out on other social outcomes is common. In particular, a large body of research examines the effects of dropping out on crime. The analytic strategy and findings presented in this study offer a potentially productive line of investigation for criminologists interested in the relationship between dropping out and crime.

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CHAPTER 3: SIBSHIP SIZE, COGNITIVE DEVELOPMENT, AND EARLY EDUCATIONAL ACHIEVEMENT IN LOW-INCOME HOUSEHOLDS

Introduction

Household income is positively related to cognitive development and educational achievement. Compared to children who grow up in more affluent households, children in low-income households have a smaller vocabulary on the first day of kindergarten, (Farkas & Beron 2004; Hart & Risley 1995), score worse on standardized tests (Reardon 2011), are less likely to graduate from high school (Alexander, Entwisle, & Kabbani 2001), and are less likely to attend college (Deil-Amen & Lopez Turley 2007). Past research has identified an array of community level and family factors that contribute to these cognitive development and educational achievement gaps. At the neighborhood level, poverty, crime, and culture are all strong determinants of educational achievement (Brooks-Gunn et al 1993; Duncan et al 1994; Crowder & South 2001; Harding 2003, 2011). At the household level, parental education, financial resources, and parenting practices have a strong influence on outcomes (Astone & McLanahan 1991; Bradley & Caldwell 1984; Harris & Marmer 1996; McLeod & Shanahan 1993; McNeal 1999; Parcel & Menaghan 1994). While this line of research has proved fruitful, existing research overlooks the possible role of sibship size, a common explanation for variations in cognitive development and educational achievement.

On average, low-income households have more children than affluent households. The consequences of this are unclear. There are compelling theoretical reasons to believe that increases in sibship size impede cognitive development and educational achievement for children in low-income households. However, empirical evidence is wanting. Past research shows a clear relationship between sibship size and cognitive development and educational achievement in general, but the findings are incomplete because (1) they may not apply to low-income households and (2) they do not adequately account for selection effects. This study addresses both of these shortcomings and has important implications for our understanding of income-based achievement gaps. Specifically, if increases in sibship size negatively affect cognitive development and educational achievement, then sibship size should help inform our understanding of achievement gaps in the same way that neighborhood effects, school effects, and other family effects do.

Additionally, in this study I compare sibship size effects in low-income households and more affluent households, asking if the consequences of increases in sibship size vary across household contexts. Ultimately, this study examines two questions. First, would low-income children have better cognitive and education outcomes if they had fewer siblings? Second, do sibship size effects vary between low-income and affluent households? In turn, this study contributes to our understanding of how sibship size affects the achievement gap and improves our understanding of the contexts in which sibship size influences cognitive development and educational achievement.

Background

A negative association between sibship size and cognitive development and educational achievement is well documented (Anastasi 1956; Blake 1981, 1985, 1989; Cicirelli 1978; Downey 1995, 2001; Featherman & Hauser 1978; Jaeger 2009; Mare & Chen 1986; Park 2008; Powell & Steelman 1993; Steelman 1985; Steelman, Powell, Werum & Carter 2002; Xu 2008). The size of the effect varies across studies and across outcomes—Blau & Duncan (1967) found that each sibling was associated with a 0.2-year decrease in schooling; Blake (1989) found that each sibling was associated with a decrease on standardized tests by one-tenth of a standard deviation—but is consistently negative in western settings. This relationship is often discussed in terms of a "quality-quantity tradeoff," where increases in sibship size (quantity) result in decreases in child capacities (quality) (Becker & Lewis 1974). The prevailing explanation points to resource dilution. Specifically, the resource dilution hypothesis argues that as sibship size increases, parental resources become strained (Blake 1981, 1989; Downey 1995, 2001). This line of thinking sees parents as investing money, time, attention, and support in their children. To the extent that each child requires individual investments, the more children there are in a household, the more constrained resources become. Ultimately, the resource dilution hypothesis argues that children with many siblings suffer a cognitive and education penalty because of the thinning of available resources across all siblings.

However, the applicability of sibship size effects to different social and economic contexts is uncertain. Comparative research shows that the negative relationship between sibship size and educational achievement is found in other OECD countries (Park 2008; Xu 2008); but the association between sibship size and education is weak in Vietnam (Anh et al 1998) and varies by time period in China (Lu & Treiman 2008). Moreover, sibship size has a *positive* effect on educational achievement in Kenya (Buchmann 2000; Gomes 1984), Botswana (Chernicovsky 1985), and Brazil through the mid-1990s (Marteleto & Souza 2012). The comparative findings are illustrative, showing how sibling relationships, parental investments and expectations, family dynamics, and the cost of raising children vary by setting and time period. For example, Gomes (1984) suggests that in Kenya it is possible that older children in a household provide resources for their younger siblings. In short, context matters and it is easy to imagine how differences in social and economic contexts across low-income and affluent households could yield differences in sibship size effects in the US.

While research shows a strong association between sibship size and cognitive development and educational achievement in the US and other Western settings, it is unclear whether this finding extends to low-income households. In low-income households, the potential for resource strain is acute. The financial constraints are obvious: families with less money have less to spend per child than more affluent households. However, in low-income households increases in sibship size and investment in children may have an economies of scale not found in more affluent households. Because financial resources are limited, the per child loss is likely smaller than in more affluent households. For example, consider childcare. High quality center-based childcare has short and long term benefits (Bainbridge et al 2005; Belsky et al 2007; Loeb et al 2004; Phillips & Lowenstein 2011), and childcare centers in general provide a better education than informal and home-based childcare that is available in poor neighborhoods (Fuller et al 2004). In more affluent households, an additional sibling may cost a child high quality center-based childcare. In poor households, high quality center-based childcare is likely not an option regardless of sibship size.

Similarly, in the areas where low-income households are able to financially invest in child development, it is possible that they see greater per child returns to that investment. Kaushal, Magnuson, and Waldfogel (2011) find that an increase in income for families in the bottom income quintile is followed by an increase in spending on child enrichment items. More than one child can share enrichment items like books and toys; more than one child cannot share high quality center-based childcare or experiential enrichment activities like music lessons or ballet lessons. This suggests that increases in sibship size may impose less of a penalty in low-income households when compared to middle-income households.

Moreover, Kornrich and Furstenberg (2013) find that there is a common floor for monetary investment in children. Households in the poorest three deciles of the income distribution spend similar dollar amounts on their children as middle-income households. Thus, increases in sibship size should at worst impose a financial constraint that is similar across low-income and middle-income households and at best is less detrimental in low-income households because of economies of scale not found in middle-income and more affluent households.

However, while increases in sibship size may only produce a modest financial burden in low-income households, increases in sibship size may levy an additional imposition on time that is not found in affluent households. Many low-income and single parent families rely heavily on social support networks (Dominguez & Watkins 2003; Heflin et al 2011; Jackson 1998; Stack 1974), and rely on social support networks—kin in particular—to provide significant amounts of childcare (Kisker & Ross 1997). Recent research shows notable variations in the amount of social support received by families. Harknett and Hartnett (2011) show that families with a large number of children are less likely to receive childcare support through social networks, arguing that the burden of providing support is a determinant of the type and amount of support offered. In effect, offering childcare assistance to a family with two children is less of an imposition than offering childcare to a family with three children. Larger sibship size could cost low-income households social support.

Research on the psychological costs of economic hardship is similarly relevant. First, economic hardship influences family relationships and emotional investments. In short, economic hardship is stressful and this stress spills over to family dynamics (Brody et al 1994;

Elder 1974; Elder et al 1992; Harris & Marmer 1996; McLeod & Kessler 1990). Second, attention is a finite resource (Luck & Vogel 1997). Mani et al (2013) propose that economic hardship overburdens the cognitive system. Specifically, because the poor must constantly consider expenses, budgets, and economic tradeoffs, they are left with fewer cognitive resources. Thus, attention is limited by economic hardship, further reducing available resources for parents in low-income households to offer their children. In effect, when a new sibling is added to a household, parents may have a smaller pool of available attention to draw on. Taken together, these lines of research suggest—when compared to more affluent households—siblings in low-income households are competing for attention and support that are heavily taxed by economic conditions. Economic hardship does not only limit available financial resources, it also limits available psychological resources.

Lastly, in evaluating sibship size effects in different eras of Chinese economic development, Lu and Treiman (2008) note that when educational opportunities are limited, sibship size is unlikely to matter—resource dilution is not a concern if opportunities do not exist. We can extend this reasoning to low-income households in the US. Children in low-income households face a number of challenges. These challenges may outweigh any effects sibship size could have. For example, the physical environment of the home influences learning outcomes (Brooks-Gunn et al 1995), and poor children are much more likely to live in unsafe and overcrowded homes (Mayer 1997), to live in neighborhoods with high crime rates (Massey 1996), and to live in homes that lack cognitive stimulation (Guo & Harris 2000). Sibship size may not matter in low-income households because educational opportunities are already limited.

Determining whether sibship size matters in low-income households is not an easy task. Research on sibship size effects in general is dogged by a lingering question over causality.

Much of the debate over causality stems from inadequate data and methods. Existing research on sibship size largely relies on cross-sectional data and OLS regression, which may not be able to successfully identify the effect of sibship size. In particular, sibship size, because it reflects parental choices, is endogenous. Put simply, parents that have more children are likely different from parents that have fewer children. Consequently, any dilution of resources that may occur with increases in sibship size may have no bearing on the cognitive and education outcomes of children. These nuances are difficult to pickup with snapshot data and OLS regression.

However, rather than quell debate, the availability of longitudinal data and improved methods has further stoked disagreement about the causality of sibship size effects. For example, Guo and VanWey (1999) use an innovative sibling fixed-effects model that calls into question the causal interpretation of sibship size. However, Downey et al (1999) dispute this finding as relying on overly conservative methods and mistaking the absence of sibling spacing effects as the absence of sibship size effects. Specifically, because of data limitations, Guo and VanWey used an analytic sample that only included widely spaced siblings.

More recent studies have attempted to account for selection into sibship size through the use of exogenous variables and have similarly produced conflicting findings. Several studies use twin births to isolate the effect of sibship size on education and cognitive outcomes, with some reporting that the effect of sibship size on educational attainment is not significantly different from zero (Angrist, Lavy, & Schlosser 2010), others noting small but significant effects of sibship size on private school enrollment (Caceres-Delpiano 2006), and others finding modest effects on cognitive test scores (Black et al 2010). In an analogous line of investigation, researchers have used sibling sex composition to isolate the effect of sibship size, noting parents who have two children that are both boys or girls are more likely to have a third child than parents who have a boy and a girl. These studies conclude that sibship size has a small effect on the likelihood of attending private school and repeating a grade in school (Conley & Glauber 2005) but no effect on cognitive test scores (Black et al 2010). I build on these previous analyses, focusing on comparisons across different levels of household disadvantage and using a different source of exogenous variation in sibship size—whether the mother of a household experiences a miscarriage.

Data and Measures

I draw data from Fragile Families and Child Wellbeing Study, a longitudinal study following a birth cohort of nearly 5,000 children born in 75 hospitals across 20 different US cities with populations greater than 200,000 (For detailed information on the study design, see Reichman, Teitler, Garfinkel, & McLanahan 2001). The Fragile Families data are advantageous for a number of reasons. In particular, the study was designed specifically for the purpose of examining how economic disadvantage shapes child wellbeing. Consequently, the data include a large sample of children born into low-income households and also a comparison sample of children born into affluent households. Longitudinal studies of low-income households are often limited by a lack of statistical power. This is not the case with Fragile Families. Moreover, Fragile Families provides rich data on social and economic contexts in both low-income and affluent households, and also detailed measures of cognitive development and educational achievement. Importantly, Fragile Families includes measure of cognitive development (PPVT score) and early educational achievement (first grade promotion) that are used in past research on sibship size effects (e.g. Conley & Glauber 2005; Guo & VanWey 1999).

I create two separate analytic samples, (1) a sample of low-income households and (2) a sample of affluent households. To obtain a sample of low-income households, I limit the sample to children born into households with an income less than two times the poverty threshold. To obtain a sample of more affluent households, I limit the sample to children born into households with an income two times or more the poverty threshold. Baseline data were collected between 1998 and 2000. Follow up waves occurred after one year, three years, and five years (t1, t3, and t5). Data were collected about the focal children, mothers, and fathers. Mothers and fathers were interviewed separately. Additionally, the kindergarten teacher of the focal child was interviewed at t5.

Outcome Variables. Measures of educational achievement are only available at t5. Measures of cognitive development are available at both t3 and t5; however, to isolate the effect sibship size through the use of instrumental variable estimation, I only use cognitive development measures at t5. This is explained in greater detail in the Analytic Strategy section of this paper.

To assess *cognitive ability*, I use the Peabody Picture Vocabulary Test (PPVT), a regularly used measure of intellectual development. To assess *educational achievement*, I use two measures of school performance. The first measure is a reference variable for whether the child was promoted from kindergarten to first grade (1 = promoted; 0 = was not promoted) as reported by the child's kindergarten teacher. The second measure is an additive scale composed of the child's kindergarten teacher's assessment of the child's ability in three areas—language and literacy, science and social studies, and mathematics. For each item, teachers could select (1) Far Below Average, (2) Below Average, (3) Average, (4) Above Average, (5) Far Above Average. Higher scores on the scale indicate greater educational achievement. The scale has a Cronbach's α of .8941. Note that the subset of kindergarten teachers who completed the survey is smaller than the full sample.

Sibship Size. Sibship size is measured as the number of children residing in the household at t5. This includes all children, not just biological brothers and sisters. I select this measure of sibship size—as opposed to a measure that limited sibship size to siblings who share the same mother and father—because it is most consistent with social theory: all children in a household draw on available resources, not just those related through a common mother and father.

Miscarriage. At t1, t3, and t5 mothers are asked if they have experienced a miscarriage since the previous interview. I use this question to create a reference variable for whether a mother experienced a miscarriage between baseline and t1 and between t1 and t3. I include only miscarriages that occurred between the baseline survey year and t3 because cognitive and education outcomes are measured at t5. Accordingly, miscarriages that occurred between t3 and t5 would not necessarily have affected sibship size prior to the measurement of the outcome variables. This variable and its uses are discussed in more detail in the Analytic Strategy section.

Additional Covariates. I also include a range of demographic and background variables that are related to child educational achievement (whether the mother completed high school, the number of years the child was in poverty between birth and t5, the household income-to-poverty ratio at the time of the child's birth) or a strong predictor of sibship size (whether the child is part of a twin birth, whether the mother identifies as Catholic, whether the child is the mother's first child, whether the parents of the child are married or cohabiting from birth through t5). Importantly, these demographic and background variables are also used as observed covariates of sibship size in the matching analysis. This is discussed in more detail in the Analytic Strategy section.

Summary statistics and a description of all variables are presented in Table 3.1.

Table 3.1. Description and Summary Statistics of All Variables.

Variable	Description	Mean	Min	Max	SD
PPVT	Standardized score on Peabody Picture Vocabulary Test at t5	93.06	40	139	15.78
Promoted to First Grade	Kindergarten teacher report of whether child was promoted from kindergarten to first grade	0.88	0	1	
Ed. Achievement Scale	Indexed scale of kindergarten teacher's assessment of child's language and literacy, science and social studies, and mathematical skills	9.03	3	15	2.32
Sibship Size	Number of siblings living in the household at t5	2.54	1	6	1.23
Miscarriage	Whether mother of household experience a miscarriage between birth of child and t3	0.09	0	1	
First Child	Whether the focal child is the first born	0.38	0	1	
Twins	Whether the focal child is part of a twin birth	0.02	0	1	
Catholic	Whether the mother is Catholic	0.25	0	1	
Mother High School Grad	Whether mother of child completed high school	0.63	0	1	
Parents Live Together	Whether parents of child are married or cohabiting from birth through t5	0.31	0	1	
Poverty Ratio at Birth	Income-to-poverty ratio at birth	1.94	0	9.9	1.85
Time in Poverty	Time in poverty between birth and t5	1.65	0	4	1.49

Analytic Strategy

OLS and Logistic Regression

Would poor children fare better if they had fewer siblings? Or, to phrase this as a counterfactual, if a poor child had one fewer sibling, would her education and cognitive outcomes improve? I first estimate a basic linear model:

$$\text{(Equation 1)} \quad Y = \alpha + X\beta + \varepsilon$$

where Y is a cognitive or education outcome (e.g. PPVT score) and X is sibship size. We can assume that the effect of X (sibship size) on Y (PPVT score) is causal if X and ε are uncorrelated. If X and ε are correlated, then X will be biased. We are particularly concerned about unobserved factors that influence sibship size. Correlation between X and ε is often referred to as omitted variable bias. A potential solution is to elaborate Equation 1 by adding additional covariates. Accordingly, I include several control variables in these models, but without perfect data it is nearly impossible to remove the correlation between X and ε. Thus, it is important to consider how the estimates of sibship size obtained using this model may be biased by omitted variables.

Propensity Score Matching

Following the conventional OLS estimates, I conduct a matching analysis. Propensity score matching models have a long history in the social sciences but are only recently becoming adopted by sociologists (Morgan and Harding 2006). To further examine the relationship between sibship size and cognitive and education outcomes, I perform a propensity score matching analysis where I compare different sibship sizes (i.e. 1 vs. 2 siblings, 2 vs. 3, 3 vs. 4, 4 vs. 5, and 5 vs. 6). Propensity score matching compares like to like, maximizing efficiency and potentially improving causal inference (Rosenbaum & Rubin 1985; Smith 1997). In effect, I group households that are similar in all ways except sibship size. In the language of experimental research, we can think of the control group as the smaller sibship size and the treatment group as the larger sibship size. For example, in households with two or three siblings, the households with two siblings are the control group and the households with three siblings are the treatment group. The control and treatment groups are matched on a variety of characteristics, and the propensity score reports the effect of having an additional sibling in the household.

There are two steps to the matching model. I first estimate a logistic regression model that generates a propensity score for sibship size for each respondent. The propensity score is the conditional probability that a respondent will be in the control or treatment group given a series of characteristics (mother's education, mother's age, mother's religious affiliation, mother's race/ethnicity, mother's nativity status, mother's relationship status, whether the child is part of a twin birth, whether mother experienced a miscarriage, the child's poverty status at birth, the child's poverty ratio at birth, and amount of time the child spent in poverty between birth and t5). I then use nearest neighbor matching to pair each treatment case with its closest control case, estimating the average treatment effect of having one additional sibling.

While likely an improvement over OLS estimates, the propensity score matching estimates may still be biased. Note that the accuracy of propensity score matching estimates hinge on the assumption that assignment to the control or treatment group is not biased by unobserved covariates (Rosenbaum & Rubin 1983). This is often referred to as "ignorable treatment assignment." If assignment to the treatment or control group is not random and not accounted for by observed covariates, then estimates of the treatment effect will be biased. Consequently, because propensity score matching estimates rely on observed covariates, propensity score matching may not remove selection bias (Heckman et al 1996). Thus, it is again important to be cautious of the potential for omitted variable bias.

Miscarriage as a Quasi-Experiment and Instrumental Variable Estimation

Lastly, I conduct a series of analyses where I treat miscarriages as a quasi-experiment. Consider an experimental design in which we are able to randomly assign households to control and treatment groups. In the control group, households expect an additional child and receive an additional child. In the treatment group, households expect an additional child but do not receive an additional child. With this experimental design, we could then compare outcomes for children living in the control and treatment households and isolate the effect of having an additional sibling.

A true experimental design as described above is not possible; however, miscarriages may provide analytic purchase. In the event of a miscarriage, a household expects an additional child but does not receive an additional child. Miscarriage accounts for selection into pregnancy—both women who carry a pregnancy to term and women who experience a miscarriage first become pregnant. Thus, if miscarriages are random, they could serve as a quasi-experiment—we have a control group (children whose mothers became pregnant and an additional sibling was added to the household) and a treatment group (children whose mothers became pregnant but an additional sibling was not added to the household).

A body of research examines the potential correlates of miscarriage to evaluate whether miscarriages can be considered random, and largely contends that miscarriages can be treated as sufficient for a natural experiment (Hotz et al 1997). This is important. If assignment to the control or treatment group is not random, then observed treatment effects might be spurious. For example, if housing characteristics and miscarriage were strongly related, then observed differences may be due to differences in housing characteristics, not sibship size. However, there are other potential issues with treating miscarriages as random. First, miscarriages are often underreported. Some women who experience a miscarriage may not report having a miscarriage. Second, some women who experience a miscarriage may have gone on to have an abortion had they not experienced a miscarriage. Third, even if the miscarriage event is random, miscarriages may have a lasting effect on mothers. For example, experiencing a miscarriage may lead to depression (Neugebauer et al 1997).

With these limitations in mind, I estimate two models. I first treat miscarriage as a true random event, estimating a basic experimental model:

$$\text{(Equation 2)} \quad Y = \alpha + Miscarriage\beta_1 + X\beta_2 + \varepsilon$$

where "miscarriage" is an indicator variable for whether the mother of a household experiences a miscarriage between the birth of the focal child and t3 and *X* is a vector of control variables. In effect, β_1 estimates the effect of having one fewer sibling than expected.

Additionally, I use miscarriage as an instrumental variable (IV). Like propensity score matching, IV estimation has a long history in the social sciences, but is only recently becoming widely used in sociology (Morgan & Winship 2007) (see Kirk 2009; Lizardo 2006; Offer and Schneider 2007). IV estimation provides an effective solution to the biases created by omitted variables (Morgan & Winship 2007), a previously noted concern. Here we have reservations about selection into pregnancy—that households that have more children are different from households that have fewer children in ways that are difficult to measure using conventional methods. IV estimation relies on an instrumental variable, *Z*, which is correlated with the treatment variable (sibship size) and uncorrelated with the outcome variable (cognitive development or educational achievement). In the research presented here, whether the mother of a child experienced a miscarriage serves as an instrument. Thus, miscarriage serves as a "treatment effect," and the IV estimate assesses the cognitive and education gains achieved by having one fewer sibling—in effect, siblings in these households have one fewer sibling than expected because the mother of the household experienced a miscarriage. The IV estimate can be expressed as:

$$\text{(Equation 3)} \quad \beta_{iv} = \frac{Cov(Y,Z)}{Cov(X,Z)} = \frac{(Cov(X,Z)\beta + Cov\ (\varepsilon,Z)}{Cov(X,Z)}$$

Thus, because *Cov*(*Y,Z*) is equal to *Cov*(*X,Z*)β+*Cov*(ε*,Z*); and, if *Z* and ε are uncorrelated, then *Cov*(ε*,Z*) is equal to zero, the last expression of Equation 3 reduces to β. In effect, we can think of *Z* (miscarriage) as an exogenous shock to the effect of *X* (sibship size) on *Y* (cognitive and education outcomes).

I specify a two-stage estimation process. In the first stage, I estimate the key explanatory variable (sibship size) as a function of the instrumental variable (miscarriage) and a vector of control variables (mother's education, mother's relationship status, mother's age, mother's religion), providing a predicted value of sibship size. In the second stage, I estimate the outcomes variables (cognitive ability and educational achievement) as a function of the predicted value of sibship size (obtained from the first stage of estimation) and a vector of control variables. In effect, Equation 1 is updated to:

$$\text{(Equation 4)} \quad Y = \alpha + \gamma\widehat{X_\iota} + \varepsilon$$

where *X* (sibship size) from Equation 1 is replaced with the predicted value of sibship size ($\hat{X}$).

Again, it is important to consider how well miscarriage serves as an instrument for sibship size. Numerous studies use miscarriage as an instrument for an array of social outcomes dealing with fertility and family size (e.g. Buckles & Munnich 2012; Fletcher & Wolfe 2011; Hotz, McElroy, & Sanders 1997, 2001; Levine, Clifton, & Pollack 2007; Robson & Pevalin 2007), and miscarriage is even offered as an example instrument in calls for researchers to think more seriously about endogeneity (Moffitt 2005). The impressive number of studies that use miscarriage as an instrument has led to some concerns about whether miscarriages satisfy the conditions of instrumental variable estimation. Of particular, concern is the possibility that miscarriage is related to unobserved covariates. If this is the case, the instrumental variable estimate will be biased.

Table 3.2 shows variations in educational attainment, income-to-poverty ratio, time in poverty, family structure, and mother's age by miscarriage status for low-income and affluent households. While the key assumption is that miscarriage is unrelated to unobserved covariates, by comparing across observed covariates, we can see the extent to which the two populations appear similar. Overall, we see some differences between women who experience a miscarriage and women who did not experience a miscarriage. In the low-income sample, women who experience a miscarriage are slightly less likely to be high school graduates, are modestly younger, have spent more time in poverty but have higher household incomes at t5, and are also less likely to be married or cohabiting. In the affluent sample, there are few differences in education or relationship status by miscarriage, but women who experience a miscarriage are younger and have a lower income-to-poverty ratio than women who did not experience a miscarriage. The overall similarities between the two populations provide some confidence that miscarriage is not a biased treatment, but there are observed differences. Again, while the key assumption is on unobserved covariates, it is reassuring that the two populations appear similar on observed covariates.

Ultimately, IV estimates—like OLS estimates—are based on a number of assumptions. These assumptions force us to interpret the results with caution. However, to the extent that miscarriages satisfy the assumptions of IV estimation, we can be confident that the coefficients for sibship size accurately capture the effect of sibship size on education outcomes. If miscarriage is uncorrelated with the cognitive development and early educational achievement of children in a household, and because miscarriage by definition accounts for unobserved factors that select women into pregnancy (women who miscarry must first become pregnant), then we can be confident that the mechanism is correctly identified. However, this does not speak to potential changes produced by a miscarriage. Specifically, a miscarriage could produce behavioral changes in a mother or family, which could lead to confounding sibship size effects with changes in mother's behavior or family contexts. For example, if experiencing a miscarriage increased family stress, then it is possible that the IV estimate could mistake a family stress effect as a sibship size effect.

Table 3.2 Demographics by Miscarriage Status.

	Low-Income Household			Affluent Household		
	Experienced Miscarriage	Did Not Experience Miscarriage	Difference	Experienced Miscarriage	Did Not Experience Miscarriage	Difference
High School Graduate	0.44	0.51	-0.07	0.8	0.81	-0.01
Poverty Ratio at t5	1.22	1.1	0.11	2.21	2.71	-0.5+
Time in Poverty	2.38	2.29	0.09	0.48	0.5	-0.02
Age	22.94	23.9	-0.96+	24.69	26.39	-1.7*
Married or Cohabit with Father	0.14	0.21	-0.07*	0.44	0.45	-0.01

Significant at + p<0.1, *p<.05

Note: In low-income sample, 9.44 percent of mothers experienced a miscarriage. In affluent household sample, 8.83 percent of mothers experienced a miscarriage.

[1] 1303

[2] 736

Findings

OLS Estimates

Table 3.3 reports the OLS estimates of the effect of sibship size on standardized PPVT score, first grade promotion, and early educational achievement for children in both low-income households and more affluent households. In the first and fourth column, we see that each additional sibling in a household is associated with a decrease in PPVT scores for children in low-income and affluent households. For children in low-income households, each additional sibling is associated with a 1.29 decrease in PPVT score. For children in more affluent households, each additional sibling is associated with a 1.6 decrease in PPVT score. In the second and fifth column, we see that sibship size does not have an effect on the odds of being promoted from kindergarten to first grade for children in low-income households or for children in more affluent households. In the third and sixth column, we see that sibship size is negatively associated with educational achievement in low-income and affluent households. Each additional sibling in a household is associated with a 0.2 decrease in a child's score on the educational achievement scale for children in low-income households and a 0.24 decrease for children in more affluent households.

Overall, these findings indicate that—net of demographic characteristics and economic hardship—that sibship size is negatively associated with cognitive development and early educational achievement in low-income households; however, this is not unique to low-income households. Affluent households see a similar relationship between sibship size and the tested outcomes. As sibship size increases, cognitive test scores and educational achievement decrease, but sibship size has no effect on the odds of promotion from kindergarten to first grade.

Moreover, the effect of each additional sibling is smaller in low-income households. The associated penalty for each additional sibling is greater in more affluent households.

The other covariates reported in Table 3.3 report expected results. Time spent in poverty is a significant predictor of several outcomes. For children in low-income households, each year in poverty between birth and t5 is associated with a 1.81 decrease in PPVT scores and a 0.12 decrease in the odds of being promoted to first grade. For children in more affluent households, a poverty spell is costly. Each year in poverty between birth and t5 is associated with a 2.01 decrease in PPVT scores and a 0.29 decrease on the educational achievement scale. Mother's education is positively associated with PPVT score for low-income children—low-income children with mothers who completed high school score 2.27 points higher on the PPVT than children whose mothers did not complete high school—and for children in more affluent households—children in affluent households with a mother who completed high school score an average of 5.15 points higher on the PPVT and 0.97 higher on the educational achievement scale.

The OLS estimates presented in Table 3.3 illustrate a clear association between sibship size and cognitive development and educational achievement. The relationship is found in both low-income households and affluent households. However, we should be cautious of interpreting these estimates as the effect of sibship size on cognitive development and educational achievement. At issue are the unobserved ways that households with more siblings vary from households with fewer siblings, leading to a possible spurious association between sibship size and cognitive development and educational achievement.

Table 3.3 OLS Regression Estimates of the Effect of Sibship Size on Cognitive Development and Early Educational Achievement.

	Low-Income Household			Affluent Household		
	PPVT[1]	Promoted[2]	Kindergarten Achievement[2]	PPVT[3]	Promoted[4]	Kindergarten Achievement[4]
	b/se	b/se	b/se	b/se	b/se	b/se
Sibship Size	-1.292***	-0.006	-0.195*	-1.596**	-0.103	-0.237+
	(0.37)	(0.11)	(0.09)	(0.58)	(0.20)	(0.13)
First Child	-1.492	-0.098	0.384	-0.176	0.175	-0.388
	(1.01)	(0.31)	(0.24)	(1.16)	(0.41)	(0.26)
Part of Twin Birth	-2.757	-0.852	-0.044	1.005	-1.040	1.456
	(2.88)	(0.84)	(0.80)	(4.84)	(1.22)	(1.26)
Catholic	-2.847**	0.499	0.005	-0.445	0.731+	0.001
	(1.01)	(0.34)	(0.23)	(1.12)	(0.42)	(0.24)
Mother Completed High School	2.265**	0.109	0.021	5.153***	0.482	0.968**
	(0.87)	(0.28)	(0.21)	(1.38)	(0.46)	(0.31)
Parents in Stable Relationship	-0.954	-0.059	0.609*	3.272**	-0.453	0.053
	(1.07)	(0.34)	(0.25)	(1.12)	(0.40)	(0.25)
Poverty Ratio at Birth	-0.410	-0.621+	-0.163	1.001***	-0.042	0.056
	(1.01)	(0.34)	(0.26)	(0.28)	(0.09)	(0.06)
Time in Poverty	-2.175***	-0.272*	-0.153	-2.069**	-0.176	-0.288+
	(0.40)	(0.13)	(0.10)	(0.69)	(0.23)	(0.16)
Constant	98.785***	2.914***	9.276***	94.493***	2.288**	9.451***
	(2.15)	(0.71)	(0.52)	(2.39)	(0.83)	(0.53)

Significant at + p<0.1, *p<.05, **p<.01, ***p<.001
[1] N=1303
[2] N=493
[3] N=736
[4] N=375

Propensity Score Matching

Estimates from the propensity score matching models are presented in Table 3.4. The first column and third column of Table 3.4 report the average treatment effect for an additional sibling across different sibship size groups on PPVT scores for children in low-income and affluent households respectively. For children in affluent households, there is no observed effect of an additional sibling across any of the tested pairs. For children in low-income households, we see that having three siblings in a household instead of two does lead to a decrease in PPVT scores by close to three percentage points. Similarly, adding an additional sibling from three to four in low-income households leads to a 2.57 percentage point decrease in PPVT score. However, the increases from one to two siblings, from four to five siblings, and from five to six siblings do not result in decreases in PPVT score.

In Column 2 and Column 5 of Table 3.4, we see the effect of variations in sibship size on the promotion to first grade for children in low-income and affluent household. For children in affluent households, we again find no difference across sibling groups. However, for children in low-income households, we observe an effect for some sibling pairs. An additional sibling from three to four or from four to five, results in a modest decrease in the odds of promotion to first grade, but there is no significant effect of sibship size on first grade promotion for increases from one to two siblings, from two to three siblings, or five to six siblings.

Table 3.4 Average Treatment Effect of an Additional Sibling on Cognitive Development and Early Educational Achievement.

Sibship Size	Low-Income Household PPVT ATT	SE	Promoted ATT	SE	Kindergarten Achievement ATT	SE	Affluent Household PPVT ATT	SE	Promoted ATT	SE	Kindergarten Achievement ATT	SE
1 vs. 2	-0.89	1.548	.04	.04	1.17+	.63	2.29	3.55	-.02	.06	-0.64	.46
2 vs. 3	-2.94**	1.211	-.05	.07	-.65**	.26	-3.45	2.57	-.05	.03	-1.04**	.29
3 vs. 4	-2.57+	1.522	-.05+	.03	.19	.51	0.485	3.062	-.01	.07	0.12	.46
4 vs. 5	.27	1.173	.17**	.05	.33	.51	-2.26	2.097	-.01	.05	.07	.43
5 vs. 6	-1.52	1.61	.054	.05	.030	.17	0.12	2.31	-.03	.04	-0.51	.46

Significant at + p<0.1, *p<.05, **p<.01, ***p<.001

Table 3.5 Average Treatment Effect of Additional Siblings on Cognitive Development and Early Educational Achievement with a Common Referent Group.

	Low-Income Household PPVT ATT SE	Promoted ATT SE	Kindergarten Achievement ATT SE	Affluent Household PPVT ATT SE	Promoted ATT SE	Kindergarten Achievement ATT SE
Plus One Sibling	-3.489**	.003	-.812	-3.762	-.158**	-1.277
	1.617	.05	.425	2.461	.044	.789
Plus Two Siblings	-4.775	.138	-.299	-5.787**	-.167	-2.213**
	3.574	.113	.373	2.961	.092	.912

Significant at + p<0.1, *p<.05, **p<.01, ***p<.001
Note: common referent group is two siblings in household.

In the third and sixth columns of Table 3.4, we see the effect of sibship size on educational achievement for children in low-income and affluent households. Here, we see that for children in more affluent households, an increase from two to three siblings leads to decrease in educational achievement, but there is no significant effect of increases in sibship size for other sibling sizes. For children in low-income households, an increase from one to two siblings has a positive effect on educational achievement, while an increase from two to three siblings has a modest negative effect on educational achievement. The gains made from one to two suggest that children's kindergarten performance may be improved by the presence of a sibling. This is not entirely unexpected. Downey and Condron (2004) show that children who grow up with siblings have better interpersonal and social skills and are better able to negotiate peer relationships in kindergarten. Keep in mind that the educational achievement scale is based on kindergartner teacher assessment. Consequently, children with better interpersonal and social skills may appear more competent to their kindergartner teachers.

Table 3.5 expands the matching analysis, reporting propensity score matching estimates with a common referent group. Specifically, the control group is two siblings in the household. In the first row, we see the treatment effect of having one additional sibling beyond two. In the second row, we see the treatment effect of having two additional siblings. For children in low-income households, an additional sibling is related to a lower PPVT score but has no effect on promotion to first grade or kindergarten achievement. The presence of two additional siblings has no effect on any of the tested outcomes. For children in more affluent households, the effect of an additional sibling on PPVT score approaches statistical significance but is not statistically significant; however, it should be noted that the magnitude of the effect is similar to the effect observed for children in low-income households. The presence of two additional siblings is associated with a statistically significant decrease in PPVT score, and is also associated with a decrease in kindergarten achievement. Moreover, an additional sibling has a small effect on promotion to first grade for children in affluent households.

Overall, the propensity score matching estimates diminish the relationship reported by the OLS estimates. While there are some differences in cognitive development and educational achievement by sibship size, these differences are rare. Similarly, there are some consistent findings across the OLS and propensity score matching estimates, but these too are rare. However, like the OLS models, the propensity score matching estimates are limited by relying on observed covariates. Consequently, we should be cautious of how omitted variables may bias placement in the treatment and control groups and how this may lead to spuriousness.

Miscarriage as a Quasi-Experiment and Instrumental Variable

Table 3.6 reports estimates of the effect of sibship size when we consider miscarriage a random event. The miscarriage coefficients report the effect of having one fewer sibling than expected for children in low-income and affluent households on PPVT score (in Column 1 and Column 4), on first grade promotion (in Column 2 and Column 5), and educational achievement (in Column 3 and Column 6). For children in affluent households, an additional sibling has no effect on any of the tested outcomes. For children in low-income households, an additional sibling has no effect on PPVT score or the odds of being promoted from kindergarten to first grade, but the effect on educational achievement approaches statistical significance and is significant in a one-tailed test. However, the effect is in the opposite direction from predicted: children who have one sibling less than expected because of miscarriage have a lower educational achievement score.

Table 3.6 Miscarriage as a Quasi-Experiment and the Effect of Sibship Size on Cognitive Development and Early Educational Achievement.

	Low-Income Household			Affluent Household		
	PPVT[1]	Promoted[2]	Kindergarten Achievement[2]	PPVT[3]	Promoted[4]	Kindergarten Achievement[4]
	b/se	b/se	b/se	b/se	b/se	b/se
Mother Experienced Miscarriage	-0.201	0.501	-0.704+	-1.205	-0.457	-0.223
	(1.43)	(0.55)	(0.37)	(1.75)	(0.53)	(0.38)
Catholic	-2.787**	0.499	0.015	-0.296	0.706+	0.008
	(1.01)	(0.34)	(0.24)	(1.12)	(0.42)	(0.24)
Mother Completed High School	2.416**	0.115	0.039	5.143***	0.446	0.984**
	(0.88)	(0.28)	(0.21)	(1.38)	(0.46)	(0.32)
Parents in Stable Relationship	-1.080	-0.024	0.452+	2.446*	-0.558	0.036
	(1.07)	(0.34)	(0.25)	(1.08)	(0.39)	(0.24)
Poverty Ratio at Birth	-0.307	-0.625+	-0.108	1.066***	-0.034	0.065
	(1.02)	(0.33)	(0.26)	(0.28)	(0.09)	(0.06)
Time in Poverty	-2.368***	-0.277*	-0.173+	-2.353***	-0.210	-0.312*
	(0.40)	(0.13)	(0.10)	(0.68)	(0.23)	(0.16)
Constant	94.955***	2.816***	8.933***	91.323***	2.239***	8.770***
	(1.83)	(0.63)	(0.46)	(1.77)	(0.59)	(0.39)

Significant at + $p<0.1$, * $p<.05$, ** $p<.01$, *** $p<.001$

[1] N=1303

[2] N=493

[3] N=736

[4] N=375

There are three possible explanations for this unexpected result. First, miscarriages may produce a change in household dynamics that influences the child. If this is the case, then the observed effect should be thought of as a miscarriage effect, not a sibship size effect. Second, the relationship may be spurious. Consider Table 3.2, which summarizes household characteristics by miscarriage status. Households that report experiencing a miscarriage are different in ways other than miscarriage. If miscarriages are not sufficiently random, spuriousness is a real concern. Third, it is possible that increases in sibship size may produce better educational achievement in kindergarten. Children who grow up with siblings have better interpersonal and social skills in kindergarten (Downey & Condron 2004). The educational achievement scale is based on Kindergarten teacher perceptions. It is possible that kindergarten teachers conflate social skills and ability.

Overall, the results presented in Table 3.6 muddy our understanding of the relationship between sibship size and cognitive development and educational achievement. When we treat miscarriage as a random event, we find that the effect of sibship size is not statistically different from zero for most outcomes and in the opposite direction for educational achievement in kindergarten for children in low-income households. While the OLS estimates demonstrate an association between sibship size and cognitive development and educational achievement, these results undermine a causal argument.

Table 3.7 presents IV estimates. IV estimation is sensitive to small sample sizes. Consequently, I combine both the low-income and affluent analytic samples. The IV estimates report no significant effects of sibship size on cognitive or education outcomes. Mother's education and income-to-poverty ratio are both positively associated with PPVT score and kindergarten achievement but not first grade promotion. Similarly, time in poverty is negatively associated with PPVT score and kindergarten achievement.

However, the lack of significant sibship size effects should be interpreted with caution. The partial R squared shows the correlation between the instrument and endogenous regressors. As the partial R squared approaches zero, the bias of the instrument increases (Bound et al 1995). The partial R squared from the first stage regression results is small. Similarly, the first stage F statistic does not exceed the critical value, which further suggests that miscarriage is a weak instrument (Staiger & Stock 1997). Moreover, the first stage results (not shown) of the instrumental variable model show that miscarriage is actually positively associated with the number of children in the household. Households who report experiencing a miscarriage on average have 0.211 more siblings in the household. If we consider the differences observed in Table 3.2, it appears that miscarriages may not be sufficiently random to satisfy the conditions of IV estimation.

Table 3.7 Instrumental Variable Estimate of the Effect of Sibship Size on Cognitive Development and Early Educational Achievement.

	PPVT[1] b/se	Promoted[2] b/se	Kindergarten Achievement[2] b/se
Sibship Size	5.184+	-0.036	0.402
	(3.00)	(0.06)	(0.45)
Mother Completed High School	4.220***	0.013	0.358+
	(1.25)	(0.03)	(0.19)
Parents in Stable Relationship	-1.246	0.002	0.131
	(1.82)	(0.04)	(0.27)
Poverty Ratio at Birth	1.511***	0.001	0.143**
	(0.33)	(0.01)	(0.05)
Time in Poverty	-3.476***	-0.010	-0.299**
	(0.70)	(0.02)	(0.10)
Constant	82.002***	0.972***	7.865***
	(7.18)	(0.15)	(1.08)
N	908	908	892
First Stage Partial R Squared	.0219	.0219	.0233
F Statistic	6.706	6.706	7.043
10 % Critical Value	9.08	9.08	9.08

Significant at + p<0.1, *p<.05, **p<.01, ***p<.001

[1] N=2136

[2] N=938

Discussion

The negative relationship between sibship size and education outcomes is one of the more replicated findings in sociology. Recently, researchers have begun to question (1) whether this finding is causal (e.g. Conley & Glauber 2006; Guo & VanWey 1999) and (2) in what contexts sibship size effects may apply (e.g. Lu & Treiman 2008; Marteleto & Souza 2012). In the research presented here, I conduct several empirical tests of the effect of sibship size in low-income and affluent households with a careful eye to causality. Overall, a cautious interpretation of the results should add to growing skepticism of the applicability of sibship size effects to different contexts.

There is a clear association between sibship size and PPVT score and educational achievement in kindergarten for children in both low-income and affluent households. It is less clear whether this relationship is causal. Propensity score matching estimates offer modest support for a causal interpretation of the effect of sibship size in low-income households on PPVT score, showing that in some cases an additional sibling in a household reduces PPVT score but not for all sibship size increases. Similarly, the propensity score estimates find no effect of sibship size on first grade promotion in low-income households, but modest effects on kindergarten educational achievement for some sibship size increases. In affluent households, the matching models find an effect on educational achievement when sibship size increases from two to three siblings, but no effects for any other sibship sizes or outcomes. However, the propensity score matching models may not adequately adjust for unobserved biases in sibship size. The miscarriage and IV models offer little support for a causal effect of sibship size, but these results should be interpreted with caution because of concerns the instrument does not sufficiently explain variations in sibship size. At the very least, these findings add to the growing suspicion that sibship size effects can be easily measured with cross-sectional data and OLS regression methods and echo the call for more careful consideration of the social contexts where sibship size effects may or may not be relevant.

It is important to note the overall modest effects of sibship size in low-income households. Children living in low-income households do worse in school. They have lower test scores. They complete fewer years of education. They are more likely to repeat a grade. They are more likely to dropout before completing high school. There are many factors that contribute to children living in low-income households doing worse in school, but too many siblings appears to be a small concern. If the observed associations are causal, the overall effect is modest. Moreover, the association between sibship size and cognitive development and educational achievement is smaller in low-income households.

The research presented here also has important implications for our understanding of the income achievement gap. If we had observed that sibship size influences cognitive development and educational achievement, then variations in family size by household income would matter. That is, variations in sibship size would need to inform our understanding of cognitive development and educational achievement gaps in the same way that other family effects do. However, the findings here offer little support for this argument. While the OLS estimates demonstrate a clear association between sibship size and cognitive development and early educational achievement in low-income households, evidence for a causal argument is mixed and empirically thin. Moreover, the observed associations between sibship size and cognitive development and early educational achievement in the OLS models are smaller in low-income households.

A limitation of the research presented in this paper is its focus on early childhood outcomes. It is possible that variations in sibship size may have a stronger impact on education and cognitive outcomes in later life. Though early achievement is a strong predictor of later achievement, which provides some confidence that the effect of sibship size will not change with age, research on outcomes during late childhood and adolescence is needed.

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CHAPTER FOUR: THE FORMATIVE YEARS, ECONOMIC HARDSHIP, AND BELIEFS ABOUT THE GOVERNMENT'S ROLE IN LESSENING POVERTY

Introduction

A large body of research considers the social determinants of support for government spending on antipoverty programs. This research is limited in two key ways. First, explanations of support for government spending on poverty largely focus on current economic and social contexts. At the individual level, researchers have considered how demographics, financial security, and political beliefs influence support for antipoverty programs (Gilens 1999; Hasenfeld & Rafferty 1989; Kluegel & Smith 1981). At the macro-level, researchers have considered how changes in the unemployment rate or the onset of an economic recession influences support for government involvement in lessening poverty (Kam & Nam 2008; Kluegel 1987). By emphasizing contemporary contexts, existing research overlooks the potentially profound effect of past experiences. Consider economic recessions. There are compelling reasons to suspect that an economic recession would have an immediate effect on beliefs about lessening poverty. However, it is similarly likely that economic recessions have a lasting effect. After an economic recession passes, changed beliefs may remain. Consequently, research on beliefs about the government's role in lessening poverty should consider not just contemporary contexts but also past experiences.

A second gap in existing research stems from data limitations. Most existing research on support for government spending on poverty relies on cross-sectional data. As a result, understandings of the fluidity in beliefs about anti-poverty spending are incomplete. Because existing research chiefly draws on cross-sectional data, it implicitly treats beliefs about spending on poverty as fixed and immutable. Accordingly, we know little about the stability or fluidity of beliefs about government spending on poverty or the determinants of attitude stability. For example, consider the relationship between household income and support for government spending on poverty. Household income and support for government spending on anti-poverty programs are negatively related. However, what happens if household income decreases? Do beliefs about government's role in lessening poverty follow or do beliefs remain fixed?

These two blind spots in existing research are notable. If past events are an important determinant of current beliefs, then the effect of an event will continue long after the event has ended. Consider the recent economic recession. The recent economic recession may color the lens through which people view the government's role in lessening poverty for years to come. Similarly, if beliefs are sensitive to individual level economic shocks, then a receding tide not only lowers all boats but it also influences beliefs.

In this study, I use both cross-sectional and panel data from the General Social Survey to (1) examine generational differences in beliefs about what the federal government should do about poverty and (2) examine whether beliefs about the government's role in helping the poor are sensitive to micro and macro economic shocks. I first outline relevant social theories on the determinants of support for government involvement in lessening poverty and provide an overview of existing research. I next describe the data and analytic strategy used in this paper. Lastly, I discuss the findings and their implications.

Background

Existing research on support for government spending on antipoverty programs draws heavily on self-interest and economic vulnerability models (Abner 2012, Gilens 1999, Hasenfeld & Rafferty 1989, Schlesinger & Heldman 2001). The self-interest hypothesis argues that individuals evaluate whether antipoverty policies would further or impede their own goals. In this context, individuals are making rational and utility maximizing decisions. Consider the strong and negative relationship between household income and support for spending on poverty (AuClaire 1984; Cook & Barrett 1992; Hasenfeld & Rafferty 1989). According to the self-interest model, members of poor and near-poor households are making a rational decision by supporting spending on assistance to the poor because they are most likely to benefit from such spending. Members of higher income households are similarly making a rational decision by opposing spending because they are unlikely to directly benefit from that government spending or believe that increased spending will come through an increased tax burden.

The economic vulnerability hypothesis is similar to the self-interest hypothesis and argues an individual's level of support for government spending on anti-poverty programs is determined by the precarity of her social and economic position (Abner 2012, Gilens 1999, Hasenfeld & Rafferty 1989, Schlesinger & Heldman 2001). The economic vulnerability hypothesis like the self-interest hypothesis contends that attitudes about government spending on poverty are informed by potential returns to that spending: those who would benefit from increased spending are likely to support spending, and those who are unlikely to benefit from increased spending are likely to oppose spending (Hasenfeld & Rafferty 1989; Kluegel 1987).

As a result of the focus on self-interest and economic vulnerability, past studies principally emphasize how demographics and current contexts influence beliefs, revealing modest relationships that are at times contradictory. For example, the poor and less educated are more likely to support government spending on poverty (Gilens 1999; Hasenfeld & Rafferty 1989), a finding consistent with self-interest and economic vulnerability models. Yet, women are no more likely to support government spending on poverty than men, and African Americans are more likely to support spending regardless of current economic position (Gilens 1999). Moreover, during economic recessions, a time when we should expect increases in social and economic vulnerability, attitudes about government spending on poverty change little (Kluegel 1987).

More recently, scholars have begun to consider how community-level factors shape beliefs about spending on poverty (Abner 2012; Hopkins 2009; Merolla, Hunt, & Serpe 2011). Community context is usually measured as the percent poor or percent belonging to a demographic group at the neighborhood or county level. This line of research generally underscores the role of exposure to a social group or the prevalence of a belief in a community on individual attitudes, arguing variations in community contexts produce variations in individual beliefs.

The role of current community context has also produced inconsistent findings that are difficult to explain. For example, Merolla, Hunt, and Serpe (2011) find that living in a zip code with a high level of poverty leads to increases in individualist explanations of the causes of poverty, a strong predictor of opposition to government spending on poverty programs (Feagin 1975; Kluegel & Smith 1981). However, they also find that living in a zip code with a high level of poverty simultaneously leads to increases in structural explanations of poverty, a strong predictor of support for government spending on poverty programs. They label this inconsistent mindset "dual consciousness." Similarly, Hopkins (2009) finds that individuals who live in areas where the poor are mostly white are less likely to attribute poverty to individual failings, a belief that generally leads to support for government spending on poverty; however, this relationship is relatively weak when compared to the effect of percent republican, leading to the conclusion that exposure to poverty matters but not as much as exposure to beliefs about poverty.

Missing from existing research is an understanding of how present beliefs are colored by the past. A large body of research in social psychology notes that beliefs and attitudes are largely formed during late adolescence (Cutler 1974; Dennis 1973; Krosnick & Alwin 1989). This timeframe is often referred to as the formative years or impressionable years. According to the formative years hypothesis, during late adolescence individuals are flexible and open to competing ideas, attitudes, and beliefs. Importantly, experiences that occur during the formative years have a profound impact. Beliefs and attitudes that are developed during the formative years are thought to become fixed and largely insusceptible to change.

The social context in which individuals experience their formative years influences the attitudes and beliefs that are developed. Consequently, the contexts in which individuals experience their formative years are generation specific, making attitudes and beliefs generational. Following the formative years, attitudes are crystallized and then remain stable as an individual ages.

Abundant scholarship notes the potential role of early life socializing influences on life long attitudes (Carlsson & Karlsson 1970; Cutler 1974; Dennis 1973; Krosnick & Alwin 1989; Ryder 1965; Schuman & Corning 2012). For example, timing of labor market entry influences life satisfaction and job satisfaction, with individuals who enter the labor market during recessions experiencing higher levels of job satisfaction and life satisfaction throughout their life (Bianchi 2013). Similarly, growing up in a recession has a strong effect on attitudes toward government redistribution (Giuliano & Spilimbergo 2010), and growing up during the Great Depression had a lasting impact on attitudes and beliefs (Elder 1999). Additionally, research on collective memory, while generally not explicitly connected to the formative years hypothesis, has led to a nuanced understanding of the creation of generational effects on attitudes. Schuman and Scott (1989) find that cohorts remember world events and changes differently, and that events and changes that occurred during adolescence or early adulthood are especially important.

The formative years hypothesis is logically consistent with other research. Foremost, as individuals age they become increasingly disconnected from issues not immediately connected to their own life, which ultimately leads to fewer chances to be confronted with attitude-challenging information (Bryne 1971). Moreover, social psychological research notes that when individuals make a decision, they draw on past knowledge (Olson & Zanna 1993; Ostrom & Brock 1969; Wood 1982). As individuals age, they accumulate an ever growing body of attitude-relevant knowledge to draw on. In a sense, attitudes beget attitude consistency.

Ample research supports the conclusion that basic attitudes are fixed by early adulthood and then remain largely unchanged (Glenn 1974, 1989; Inglehart 1977; Jennings & Niemi 1981; Markus 1979; Sears 1983). For example, Fendrich and Lovoy (1988) show that attitudes during college are an excellent predictor of later life attitudes. Similarly, Marwell et al (1987) show that between 1965 and the mid 1980s, the attitudes of white Civil Rights activists remained largely stable. Moreover, Jennings and Niemi (1978) find that a wide range of political and social orientations were highly stable over an eight-year period. Additionally, they note that younger individuals were most likely to change attitudes, and that attitudes appeared to harden after early adulthood. Accordingly, past experiences are likely a strong predictor of current attitudes. There are clear implications for this study. Specifically, because macro social and economic contexts vary by generation, we should expect cohort differences in support for government spending on poverty.

Also missing from existing research is a clear understanding of when support changes. Event driven models of attitude change offer an informative but underutilized mechanism for understanding support for antipoverty programs. Rather than focusing on contemporary contexts, event driven models of attitude change focus on changes in contemporary contexts. Put succinctly, event driven models of attitude change contend that large-scale events have a causal effect on attitudes (Smith 1984). The model is simple: a discrete, widespread episode occurs and, in turn, individuals change their attitudes. For example, if we consider the outbreak of war a significant event, then we should expect the start of war to have some effect on attitudes. Event driven models of attitude change are supported by empirical research. In particular, research notes that presidential popularity (MacKuen & Turner 1984; Edwards 1990) and evaluations of the most important issues facing the nation (Smith 1985) can be explained by an event driven understanding of attitude change.

If we accept that significant events produce changes in attitudes, then an important question is: do large-scale economic shocks influence beliefs about government spending on poverty? In describing the event driven model, Smith (1984) explicitly lists economic crises as an example of an event that should produce attitude change. However, the documented effects of increases in economic hardship on attitudes are often underwhelming. Kenworthy and Owens (2012) find that recessions have modest lasting effects on attitudes, while Kluegel (1987) concludes that the recession of the 1970s had no effect on attitudes about welfare spending.

We can extend event driven models to the individual level. At the individual level, an event driven model of attitude change contends that a change in individual level social or economic context is followed by a change in belief. For example, consider a particularly bad flu outbreak. A large spike in reported cases of the flu may lead an individual to change his or her perception of the flu vaccine. At the same time, if an individual were to contract the flu him or herself, this too would likely influence beliefs about the flu vaccine. Here what matters is not whether there is economic hardship at the macro level but that an individual experiences economic hardship. If an individual experiences economic hardship, does she change her beliefs about government spending on poverty?

Data, measures, and methods

I draw data from two sources: the cumulative cross-sectional file of the General Social Survey (GSS) and the recently introduced GSS longitudinal data (Smith et al 2012). From the cross-sectional data, I use 19 separate surveys that cover a span of 28 years (1984 to 2012). From the longitudinal data, I combine three panels. In the first panel, respondents were interviewed in 2006, 2008, and 2010. In the second panel, respondents were interviewed in 2008, 2010, and 2012. In the third panel, respondents were interviewed in 2010 and 2012. Respondents in the third panel will be interviewed again in 2014.

The cross-sectional and longitudinal data have complementary strengths. The pooled cross-sectional data allow me to explore temporal trends in beliefs about government involvement in lessening poverty, allow me to test for differences across cohorts, and allow me to examine how macro-economic hardship influences beliefs. The longitudinal data allow me to observe how individual level changes in economic hardship influence beliefs. Instead of focusing on the effect of an individual's contemporary social and economic contexts, the longitudinal data allow me to examine how changes in contexts create changes in belief.

Outcomes Measures

In both the cross-sectional and panel surveys, respondents are asked three questions related to the government's role in lessening poverty. The first question reads:

> "Some people think that the government in Washington should do everything possible to improve the standard of living of all poor Americans. Other people think that it is not the government's responsibility, and that each person should take care of himself. Where would you place yourself on this scale?"

Respondents are offered a 5-item response set, ranging from (1) government action to (5) people should help selves with (3) serving as a midpoint for agreeing with both sides. I collapse (4) and (5) into one category and (1) through (3) into a separate category, creating an indicator variable where 1 = believe people should help selves and 0 = government should help.

The second questions reads,

> "We are faced with many problems in this country, none of which can be solved easily or inexpensively. I'm going to name some of these problems, and for each one I'd like you to tell me whether you think we're spending too much money on it, too little, or about the right amount. Are we spending too much, too little, or about the right amount on assistance to the poor?"

I collapse "about right" and "too little" into one category, creating a reference variable where 1=government is spending too much on assistance to the poor and 0=other.

The final question reads the same as above, except replaces "assistance to the poor" with "welfare." Specifically,

> "We are faced with many problems in this country, none of which can be solved easily or inexpensively. I'm going to name some of these problems, and for each one I'd like you to

tell me whether you think we're spending too much money on it, too little, or about the right amount. Are we spending too much, too little, or about the right amount on welfare?"

I again collapse "about right" and "too little" into one category, creating a reference variable where 1=government is spending too much on welfare and 0=other.

Measures of Economic Hardship

I include two measures of economic hardship in the cross-sectional models: (1) a reference variable for whether the respondent's annual household income falls in the bottom income quartile, and (2) a reference variable for whether there was a large increase in the poverty rate from the previous year. A large increase is defined as an increase in the poverty rate greater than half of a percent from the previous year. This occurs in 4 out of 19 possible survey years and coincides with the economic recession in the early 1990s and the most recent economic recession. Other thresholds (.3 and .6) were tested and produced substantively similar results (results not shown).

In the longitudinal models, I measure economic hardship as a loss of annual household income between t and t+1. Following others (Owens & Pedulla forthcoming), I measure income loss with an indicator variable where 1=lost 20 percent or more of household income and 0=other.

Demographic and Background Variables

I also include several demographic and background variables, including gender, race, educational attainment, marital status, number of children in the household, region of residence, cohort, and year. Summary statistics and description of the coding for all variables are presented in Table 4.1.

Table 4.1. Descriptive Statistics and Description of All Variables.

Variable	Description and Coding	Mean	Min	Max
Self Help	People should help themselves out of poverty without government assistance; 1=agree; 0=disagree	0.26	0	1
Spending Too Much on Assistance	The government is spending too much on assistance to the poor; 1=agree; 0=disagree	0.11	0	1
Spending Too Much on Welfare	The government is spending too much on welfare; 1=agree; 0=disagree	0.45	0	1
Bottom Income Quartile	Annual household income falls in bottom income quartile; Cross-sectional models only; 1=bottom quartile; 0=other	0.25	0	1
Loss of Income	Annual household income fell by 20 percent or more between waves; panel models only; 1=yes; 0=no	0.17	0	1
Large Increase in Poverty Rate	Increase in the poverty rate from previous year is greater than 0.49; 1=yes; 0=no	0.17	0	1
Education	Highest degree completed			
Less Than High School	1=less than high school; 0=high school	0.17	0	1
Some College	1= some college; 0=high school	0.07	0	1
College+	1=college+; 0=high school	0.23	0	1
Race	Respondent's race; 1=non-Hispanic black; 0=non-Hispanic white	0.15	0	1
Gender	Respondent's gender; 1=female; 0=male	0.56	0	1
Marital Status	Respondent's marital status1=married; 0=not married	0.5	0	1
Number of Children	Number of children	1.9	0	1
Age	Respondent's age; grand-mean centered and divided by 10	4.59	18	89
Region	Current region of residence			
Northeast	1=Northeast; 0=South	0.19	0	1
Midwest	1=Midwest; 0=South	0.26	0	1
West	1=West; 0=South	0.19	0	1
Cohort	5-year birth cohort	1948	1900	1985
Period	Survey year	1997	1984	2012

Note: N for cross-sectional data=22,915; N for panel-data=3,052.
Means for all variables come from cross-sectional data except for "Loss of Income."

Analytic Strategy

To assess variations in attitudes toward the government's role in lessening poverty, I first analyze the GSS cross-sectional data using a cross-classified random effects model (Raudenbush & Bryk 2002; Yang 2008). In this approach, individual level attributes such as income, sex, and education are treated as level-one covariates and are modeled as fixed-effects. These individual level covariates are nested within a time period by birth-cohort cross-classified matrix, where period and cohort are level-two covariates that are modeled as random effects. This allows me to separately and simultaneously consider the influence of period effects and cohort effects net of individual level characteristics.

The level one within cell model can be expressed as:

$$Y_{ijk} = \alpha_{jk} + \beta_{1jk} X + e_{ijk} \quad \text{(Equation 1)}$$

where Y_{ijk} is the response outcome for the *i*th respondent in *j*th period and *k*th cohort. β denotes level-one coefficients. X denotes a vector of level-one covariates. α_{jk} is the cell mean for the reference group at the mean for period j and cohort k. e_{ijk} is the within cell residual.

The level-two between-cell model can be expressed as:

$$\alpha_{jk} = \pi_0 + t_{0j} + c_{0k} \quad \text{(Equation 2)}$$

where π_0 is the expected mean at the zero values for all level-one covariates. t_{0j} is the overall period effect of period j averaged over all birth cohorts, and c_{0k} is the overall cohort effect of cohort k averaged over all time periods. Thus, α_{jk} is the random intercept for period j and cohort k, and specifies that the overall mean varies across periods and across cohorts.

The period and cohort effects are of particular interest. The period effects show whether attitudes toward the government's role in lessening poverty are sensitive to period events. The cohort effects show whether the timing of when individuals come of age and the associated contexts have a lasting impression on attitudes toward government's role in lessening poverty.

I next estimate a pooled cross-sectional logit analysis of the form:

$$\log(P_{it} / 1 - P_{it}) = X_{it}\beta_1 + PovRateInc_t\beta_2 + v_t + \varepsilon_{it} \quad \text{(Equation 3)}$$

where X is a vector of individual level covariates measured at time t for person i, PovRateInc is a indicator variable for whether there a large increase in the poverty rate from the previous year, v is a time-specific error term, and ε is the random error term. I cluster standard errors by year. This analysis allows me to determine how large increases in the poverty rate shift attitudes toward the government's involvement in lessening poverty.

Lastly, to assess whether beliefs about government's involvement in lessening poverty are sensitive to individual level changes in economic hardship, I use the panel data from the GSS and a fixed-effects model. The basic model can be expressed as:

$$y_{it} = \mu + \beta_1 Income\ Loss_{it} + \gamma Year + \varepsilon_{it} \quad \text{(Equation 4)}$$

where y is the response for person i at time t, and is a function of time-variant income loss, year-fixed effects, and a time-varying error term (ε). This model allows me to assess the extent to which a loss of income leads to a change in beliefs about the government's role in lessening poverty.

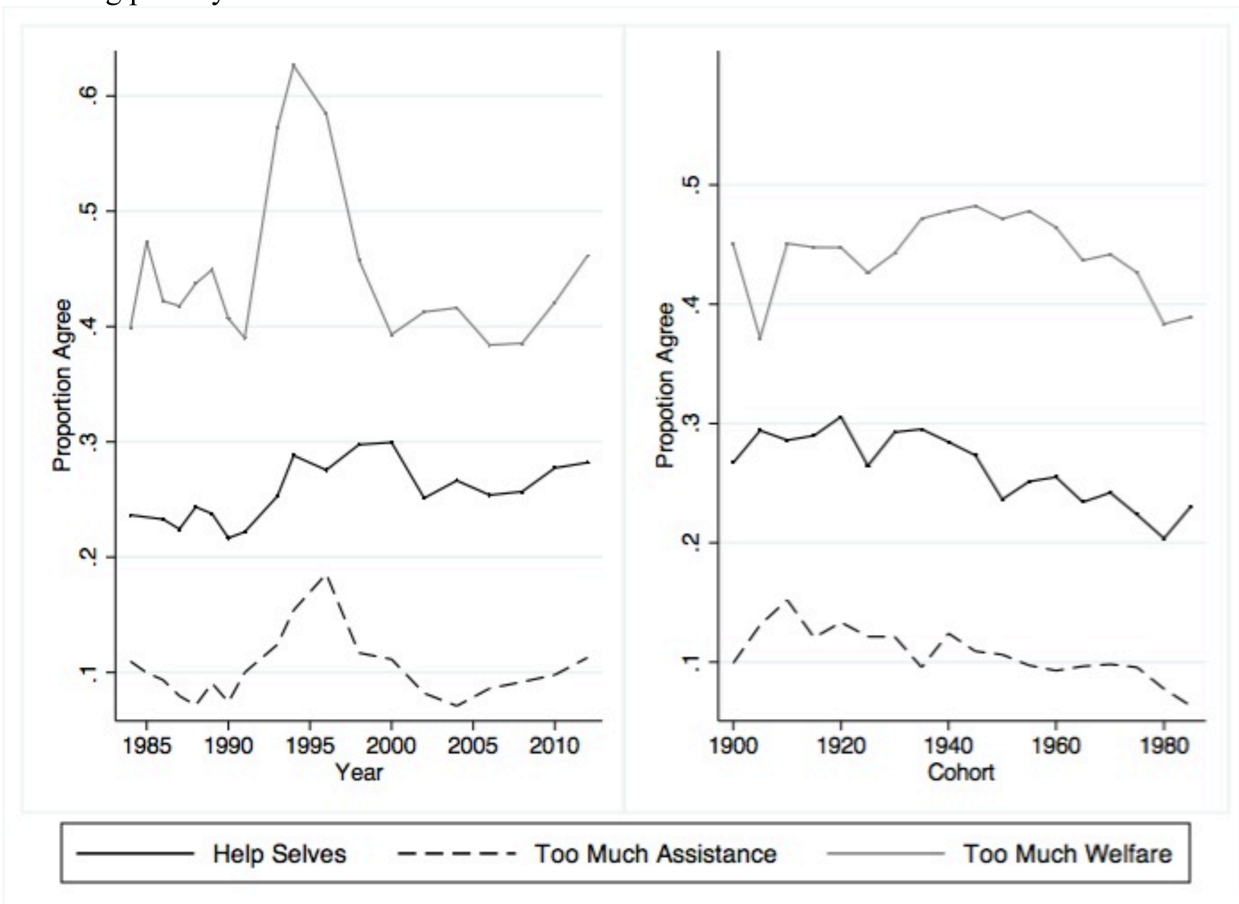
As a robustness check, I perform a "placebo" regression where I use the same model to estimate how loss of income influences attitudes toward end of life and gun laws. Placebo regression tests for effects where a causal pathway seems unlikely or impossible. The end of life question reads, "When a person has a disease that cannot be cured, do you think doctors should be allowed by law to end the patient's life by some painless means if the patient and his family request it?" (1=yes; 0=no). The gun law question reads, "Would you favor or oppose a law which would require a person to obtain a police permit before he or she could buy a gun?" (1=favor; 0=oppose). A statistically significant relationship between income loss and attitudes toward end of life laws or between income loss and gun laws is unlikely causal, and should make us skeptical of any observed relationships between income loss and changes in beliefs about the government's role in lessening poverty.

Findings

Formative Years, Period Events, and Beliefs about Anti-Poverty Spending

Figure 4.1 shows period and cohort variations in beliefs about government involvement in lessening poverty. In the first panel, we see period variations in the belief that individuals should help themselves out of poverty without government help, belief that the government is spending too much on assistance to the poor, and belief that the government is spending too much on welfare. This panel reveals two key findings. First, people are much more resistant to welfare spending than other government spending or action. Second, the period trends largely mirror each other across outcomes. There is an increase in opposition to government involvement throughout the early and mid-1990s for all outcomes; however, following welfare reform, there is a prominent decline in opposition.

Figure 4.1 Period and cohort variation in support for government involvement and spending on lessening poverty.



In the second panel of Figure 4.1, we see variations in these outcomes by cohort. For belief that individuals should help themselves out of poverty without government assistance and belief that the government is spending too much on assistance to the poor, we see a modest downward trend across cohorts with younger cohorts becoming more supportive of government involvement and government spending on assistance. For belief that the government is spending too much on welfare, the cohort trend is U shaped with baby boom cohorts most strongly believing the government is spending too much on welfare. However, the overall cohort differences for all outcomes are slight.

Moving from the descriptive findings to the multivariate findings, Table 4.2 presents estimates of the individual level fixed-effects coefficients and the random-effect variance components from the cross-classified random effects model for all outcomes. We see the effect of all level-one covariates on the odds of opposing government involvement in helping people out of poverty in the first column, the odds of believing the government is spending too much on assistance for the poor in the second column, and the odds of believing the government is spending too much on welfare in the third column.

Most notably, for all outcomes there is a significant period effect, but there is not a significant cohort effect. In fact, cohort effects have to be excluded from the second and third models because the intercepts for the cohort effects are estimated to be zero (models not shown). This indicates that attitudes toward the government's role in lessening poverty, toward government spending on assistance to the poor, and toward government spending on welfare all respond to period events, but there is no significant cohort effect. Generational grouping does not appear to influence belief about the government's role in addressing poverty. When individuals come of age does not leave a lasting impression. What matters is the contemporary.

Table 4.2 Odds Ratio Estimates from Logit Cross-Classified Random Effects Models of Attitudes toward Government Involvement and Government Spending on Poverty.

Fixed-Effects	Self Help[1]	Too Much Assistance[2]	Too Much Welfare[3]
Bottom Income Quartile	-0.3839***	-0.3968***	-0.4744***
Education			
Less Than High School	-0.3981***	-0.1501+	-0.1724***
Some College	0.1058+	0.05301	0.03852
College+	0.2234***	0.09208	-0.3917***
African American	-1.0737***	-1.358***	-0.8061***
Female	-0.4202***	-0.177***	-0.03897
Married	0.1438***	0.09673+	0.1141**
Number of Children	0.02021+	-0.00764	-0.00776
Age	-0.0088	0.004605	0.0211***
Age2	0.000188**	0.000044	-0.00021***
Region			
Northeast	-0.38***	-0.4279	-0.09607*
Midwest	-0.198***	-0.0958	-0.1715***
West	-0.141**	-0.1052	-0.1377**
Intercept	-0.5923***	-1.8055***	0.2155*
Random Effects - Variance Components			
Period Effect	0.01137*	0.07589**	0.08293**
Cohort Effect	0.002243	-	-

+ p < .1; * p < .05; ** p < .01; *** p < .001
[1] N=22,915
[2] N=18,113
[3] N=17,145

Figure 4.2. Overall Period Effects on Support for Government Involvement and Government Spending on Poverty.

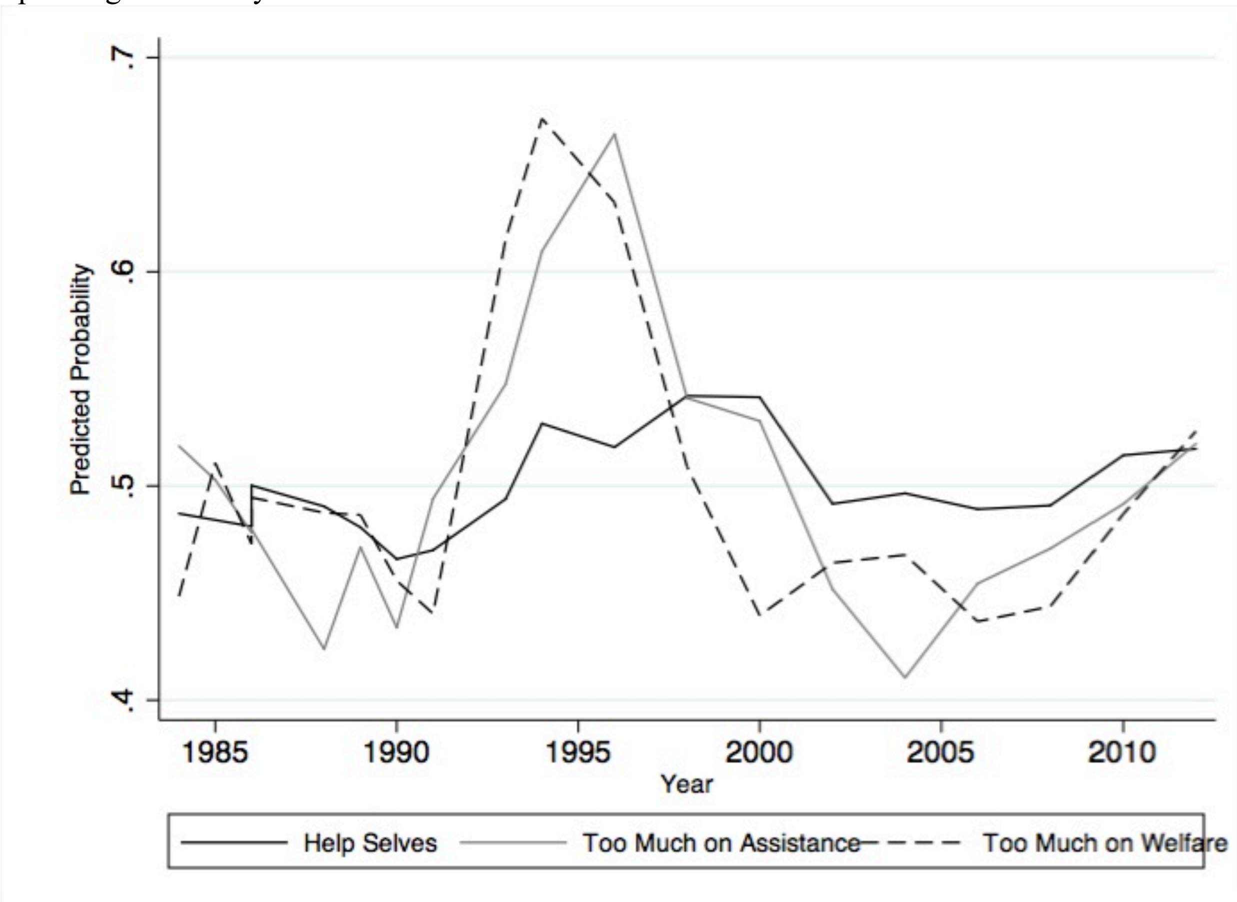


Figure 4.2 displays the overall period trends for each outcome in terms of predicted probabilities. The period trends largely mirror each other and are very similar to the trends displayed in the descriptive findings. Overall, we see smaller fluctuations in the belief that the individuals should help themselves out of poverty, and a prominent spike in the belief that the government is spending too much on assistance to the poor and on welfare that crescendos in the early-1990s, peaks in the mid-1990s, and then falls throughout the late-1990s.

While it is difficult to explicitly test for the effect of welfare reform, two associations are clear. First, in the years leading up to welfare reform, there was growing belief that the government was spending too much on assistance to the poor and too much on welfare. Second, attitudes shifted after welfare reform. Following welfare reform, there was a precipitous decline in the belief that the government was spending too much on assistance to the poor and too much on welfare.

The individual level covariates are largely in-line with past research. Members of the bottom income quartile are much more likely to support government involvement and government spending than those with higher incomes. Compared to whites, African Americans are more likely to support an active role of the government in addressing poverty. Men are more likely than women to believe individuals should help themselves out of poverty without government help and are also more likely to believe the government spends too much on assistance to the poor, but there are no significant differences between men and women on beliefs about spending on welfare.

There are also notable regional differences with the South less likely to support government involvement than all other regions. Residents of the Northeast, Midwest, and West are also less likely to believe the government is spending too much on welfare. However, there are only minimal regional differences in beliefs about government spending on assistance to the poor—residents of the South are less likely to support government spending on assistance to the poor compared to residents of the Northeast, but there are no regional differences between the South and Midwest or the South and West.

There is also no clear pattern for educational attainment across outcomes. Increasing education is generally associated with increased belief that individuals should help themselves out of poverty without government help. However, this trend does not hold for beliefs about government spending on welfare or government spending on assistance to the poor. High school dropouts and college graduates are less likely than high school graduates to believe the government is spending too much on welfare, and there are no significant differences by educational attainment in the belief that the government is spending too much on assistance to the poor.

Are Beliefs Sensitive to Changes in Macro Level Economic Hardship?

Table 4.3 presents results from the pooled data for all outcomes. For each outcome, I estimate two models. In the first model, I separately estimate the effect of a large increase in the poverty rate and having an income in the bottom quartile net of other demographic covariates. In the second model, I include an interaction term for having an income in the bottom quartile during a time period that sees a large increase in the poverty rate.

Overall, we observe a similar pattern for belief that individuals should move out of poverty without government aid and belief that the government is spending too much on assistance to the poor. In the first and third column, we see that members of the bottom income quartile are much less likely to believe that individuals should move out of poverty without

Table 4.3. Logistic Regression of Pooled Cross-Sectional Data Predicting Attitudes toward Government Involvement and Government Spending on Poverty.

	Self Help[1]		Too Much Assistance[2]		Too Much Welfare[3]	
	Model 1	Model 2	Model 3	Model 4	Model 5	Model 6
	Odds Ratio SE	Odds Ratio SE	Odds Ratio SE	Odds Ratio SE	Odds Ratio SE	Odds Ratio SE
Large Increase in Poverty Rate	0.892+		0.815+		0.775*	
	(0.06)		(0.09)		(0.09)	
Bottom Income Quartile	0.696***		0.665***		0.621***	
	(0.03)		(0.07)		(0.02)	
Interaction						
Top Inc Quartiles During Large Increase in Pov Rate		0.929		0.889		0.775*
		(0.08)		(0.12)		(0.1)
Bottom Inc Quartile During Not Large Increase in Pov Rate		0.719***		0.712**		0.621***
		(0.03)		(0.07)		(0.02)
Bottom Quartile During Large Increase in Pov Rate		0.555***		0.383***		0.482***
		(0.03)		(0.04)		(0.06)
Age	1.011***	1.011***	1.009***	1.009***	0.999	0.999
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
Female	0.658***	0.657***	0.843***	0.842***	0.957	0.957
	(0.03)	(0.03)	(0.04)	(0.04)	(0.03)	(0.03)
African American	0.340***	0.340***	0.256***	0.256***	0.454***	0.454***
	(0.02)	(0.02)	(0.05)	(0.05)	(0.03)	(0.03)
Married	1.121***	1.121***	1.08	1.08	1.151***	1.151***
	(0.04)	(0.04)	(0.06)	(0.06)	(0.04)	(0.04)
Number of Children	1.012	1.013	0.989	0.989	0.999	0.999
	(0.01)	(0.01)	(0.02)	(0.02)	(0.01)	(0.01)
Northeast	0.681***	0.680***	0.657***	0.655***	0.914	0.914
	(0.04)	(0.04)	(0.05)	(0.05)	(0.06)	(0.06)
Midwest	0.820***	0.819***	0.901	0.901	0.838***	0.838***
	(0.03)	(0.03)	(0.06)	(0.06)	(0.04)	(0.03)

West	0.866*	0.867*	0.904	0.904	0.873*	0.873*
	(0.05)	(0.05)	(0.06)	(0.06)	(0.05)	(0.05)
Less Than High School	0.674***	0.674***	0.861*	0.860*	0.829***	0.829***
	(0.03)	(0.03)	(0.06)	(0.06)	(0.04)	(0.04)
Some College	1.106	1.107	1.053	1.056	1.032	1.032
	(0.08)	(0.08)	(0.09)	(0.09)	(0.07)	(0.07)
College+	1.240***	1.240***	1.089	1.088	0.692***	0.692***
	(0.04)	(0.04)	(0.07)	(0.07)	(0.03)	(0.03)
Constant	0.589***	0.585***	0.182***	0.180***	1.300*	1.300*
	(0.04)	(0.04)	(0.03)	(0.03)	(0.14)	(0.14)

+ $p < .1$; * $p < .05$; ** $p < .01$; *** $p < .001$
Note: Standard Errors are clustered by year.
[1] N=22,915
[2] N=18,113
[3] N=17,145

government aid (odds ratio: 0.70 and 0.67). Similarly, in years where there is a high increase in poverty, individuals are less likely to believe that individuals should help themselves out of poverty or that the government is spending too much on assistance to the poor (odds ratio: 0.89 and 0.82). However, both of these odds ratios are only significant in a one-tailed test (p=.096 and p=.077).

In the second and forth columns of Table 4.3, the interaction effect between membership in the bottom income quartile and high poverty year produces two noteworthy findings. First, for individuals with higher incomes, there is no difference in attitudes between high poverty years and other years. Second, the disparity between the bottom income quartile and all others widens during high poverty years. This suggests that broader conditions influence beliefs for the poor, but not the non-poor, and the near significant result observed in Columns 1 and 3 are likely largely explained by shifts in the bottom income quartile.

Columns 5 and 6 present odds ratios for believing the government is spending too much on welfare. Here we see a modest variation from the pattern shown in Columns 1 through 4. First, a large increase in the poverty rate has a clear and significant effect (Column 5). The odds of believing that the government is spending too much on welfare during high poverty years are .22 times the odds in other years. Second, a spike in the poverty rate does not produce a shift only in the bottom income quartile (Column 6). In years where there is a high poverty rate, the odds a member of middle or high-income quartiles thinks the government is spending too much on poverty are .22 times the odds in other years. Otherwise, the patterning is the same as the results presented for the other outcomes: members of the bottom income quartile are far less likely to believe that the government is spending too much on welfare and the income gap in belief widens during high poverty years.

Are Beliefs Sensitive to Micro-Level Economic Hardship?

Table 4.4 presents the results from the fixed-effects models using the longitudinal data. The first three columns show the effect of economic hardship on the main outcomes. In the first column we see that a loss of income leads to individuals becoming less likely to believe that the poor should help themselves out of poverty. In the second column we see that a loss of income makes individual less likely to believe the government is spending too much on assistance to the poor. In the third column we see that a loss of income does not lead to a change in beliefs about government spending on welfare. While beliefs about government's role in moving people out of poverty and belief about government spending on assistance to the poor respond to individual level economic hardship, beliefs about government spending on welfare do not.

The last two columns of Table 4.4 show the effect of economic hardship on the placebo outcomes. In the fourth column we see that a loss of income does not produce a change in belief about end of life laws. In the fifth column we see that a loss of income does not produce a change in support for gun laws. The lack of significant results for the effect of income loss on end of life and gun laws should gives us confidence in the robustness of the results presented in the first three columns.

Table 4.4 Fixed-Effects Models of the Effect of Income Loss on Attitudes Toward Government Involvement in Lessening Poverty, Government Spending on Poverty, and Placebo Outcomes.

	Self Help[1]	Too Much Assistance[2]	Too Much Welfare[3]	End of Life[4]	Gun Law[5]
	Model 1	Model 2	Model 3	Model 4	Model 5
	b/se	b/se	b/se	b/se	b/se
Lost ≥ 20% Income	-0.034*	-0.016*	-0.010	-0.006	-0.012
	(0.01)	(0.01)	(0.02)	(0.01)	(0.01)
Year 2008	0.012	-0.005	-0.009	-0.013	-0.012
	(0.02)	(0.01)	(0.02)	(0.01)	(0.01)
Year 2010	0.075***	0.023*	0.040+	-0.004	-0.034*
	(0.02)	(0.01)	(0.02)	(0.02)	(0.02)
Year 2012	0.080***	0.034**	0.078**	0.001	-0.033+
	(0.02)	(0.01)	(0.02)	(0.02)	(0.02)
Intercept	0.226***	0.072***	0.379***	0.706***	0.795***
	(0.01)	(0.01)	(0.02)	(0.01)	(0.01)

+ $p < .1$; * $p < .05$; ** $p < .01$; *** $p < .001$
[1] N=2,781
[2] N=3,052
[3] N=2,045
[4] N=2,722
[5] N=2,771

Discussion

Attitudes about government involvement and spending on poverty are not inconsequential. Does public opinion determine public policy? No; however, research clearly shows that public opinion plays an important role in shaping policy (Burstein 1979; Brooks & Manza 2006; Page & Shapiro 1982). Anti-poverty legislation, in particular, is susceptible to public opinion. Elected officials fear repercussions from the electorate, and anti-poverty legislation and rhetoric routinely serves as a rallying point for small government proponents.

In seeking to understand how an individual's economic and social position influences attitudes toward government spending on poverty, existing research has largely focused on current contexts. The research presented here replicates many of these findings, showing that current economic position and demographics are important determinants of beliefs, and also extends this research by considering how generational experiences may shape current belief and how beliefs change over time. Specifically, I examine if cohort membership and economic hardship at the macro or micro level influences beliefs.

There are strong theoretical reasons to expect distinct cohort differences. Research on other political beliefs and attitudes shows that the shared contexts and experiences of cohorts have a lasting influence on beliefs (Cutler 1974; Dennis 1973; Krosnick & Alwin 1989). However, this is not the case for beliefs about the government's role in lessening poverty. There is no significant or systematic variation by cohort. Cohorts who came of age during President Johnson's War on Poverty are no more or less likely to support anti-poverty spending or government involvement than other cohorts. The social and economic contexts of when people come of age do not color beliefs about the government's role in lessening poverty. The absence of cohort differences suggests that beliefs about the government's role in lessening poverty do not follow the formative years hypothesis.

However, beliefs do shift in response to economic hardship. Most notably, in years where there is a large increase in the poverty rate, there is a modest decrease in the belief that individuals should help themselves out of poverty without government aid and a modest decrease in the belief that the government is spending too much on assistance to the poor. However, members of the bottom income quartile, who are particularly responsive to increases in the poverty rate, drive much of this shift. In years where there is a large increase in the poverty rate, the gap between the bottom income quartile and other households widens substantially for these two outcomes. Meanwhile, for more affluent households, there is no significant difference in belief during high poverty years and other years. There is, however, a modest deviation from this pattern for beliefs about government spending on welfare. In particular, there is a significant difference for higher income quartiles in years where there is a large increase in the poverty rate compared to other years. In high poverty years, high-income households are less likely to believe the government is spending too much on welfare. These results offer nuanced support for event driven models of attitude change. Attitudes do respond to events, but not for all outcomes and not for all groups. Those who are most affected by the event have more malleable attitudes, while those who are unaffected have more static beliefs.

The longitudinal results offer further mixed support for event driven models of attitude change at the individual level and reveal that beliefs about government involvement in lessening poverty are fluid. Rather than fixed and stable, individual economic hardship does lead to a shift in beliefs. When individuals lose a portion of their annual household income, they become less likely to believe that individuals should help themselves out of poverty without government help and they become less likely to believe the government is spending too much on assistance to the poor. However, beliefs about government spending on welfare remain unchanged.

The research presented here offers mixed support for self-interest and economic vulnerability models. Individuals who are most likely to benefit from government spending and government involvement are significantly more likely to support government involvement and government spending on assistance to the poor. However, an increase in economic hardship is not followed by a change in support for government spending on welfare. This is likely because of the symbolic importance of welfare beliefs. Research shows that much of the opposition to welfare is rooted in ideology (Feagin 1975; Feldman & Zaller 1992; Kluegel & Smith 1981; Williamson 1974). The stability of welfare beliefs following a loss of income offers some evidence that ideological commitments shape poverty beliefs. Moreover, of the three outcomes examined in this paper, the belief that individuals should help themselves out of poverty without government help is most closely related to what Huber and Form (1973) called the dominant ideology. This belief in self-help is the most stable across period events with much smaller fluctuations in the overall trend. This too offers some evidence that ideological commitments are more stable and less driven by self-interest.

Additional research is needed. In particular, research is needed on long-term shifts in outcomes. The research presented in this paper shows that loss of income leads to a decrease in opposition to government spending on assistance to the poor and decreased belief that individuals should help themselves out of poverty without government assistance. It is unclear whether these are temporary or long lasting shifts. Because the GSS panel data only includes three waves, it is not possible to explore whether economic hardship has a long lasting influence on belief. However, it seems unlikely that the observed shifts are permanent. The absence of cohort effects and the clear importance of current economic position in the cross-sectional models suggest that the changes in belief that occur after experiencing economic hardship will be short lived. What matters in determining beliefs about the government's role in lessening poverty appears to be current position and contexts. This has a notable implication for public policy. In particular, the opportunity window for passing policy with broad support may be narrow. If attitudes respond to economic hardship, then policymakers will enjoy the most support for efforts to help the poor during economic hard times.

An additional limitation comes at the expense of one of the strengths of the longitudinal data. The longitudinal data were collected between 2006 and 2012, a time where an abnormally large number of households faced economic hardship. For research purposes, this is advantageous: we have a large analytic sample of households that experienced economic hardship. However, this also requires us to carefully consider how the larger social and economic contexts may influence the presented findings.

Of particular concern is that what we are observing is not the effect of losing income on attitudes, but rather the effect of losing income during an economic recession. This is an important distinction. It is possible that a loss of household income during booming economic times does not have the same effect. Perhaps during economic good times individuals who lose a portion of their household income are more likely to blame themselves and think nothing of the role of the government in aiding the poor. Research on the effects of income loss during economic booms is needed. Unfortunately, such data are not available at present.

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