Human Capital Formation and the American Dust Bowl

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Abstract

I use variation in childhood exposure to the Dust Bowl, an environmental shock to health and income, as a natural experiment to explain variation in adult human capital. I also examine a variety of mechanisms by which the Dust Bowl influenced later-life wellbeing, and investigate the scope for recovery from this early-life shock.

I find that exposure to the Dust Bowl in childhood has statistically significant and economically meaningful adverse impacts on later-life outcomes, for instance, increasing disability and reducing fertility and college completion. These results hold even after accounting for the possibly confounding effects of the Great Depression, migration, and selective fertility or mortality.

The effects I find are more severe for those born in agricultural states, suggesting that the Dust Bowl was most damaging via the destruction of agricultural livelihoods. This collapse of farm incomes, however, had the positive effect of increasing high school completion amongst the exposed, likely by reducing the demand for child farm labor where such labor was not essential to production, and thus decreasing the opportunity costs of secondary schooling; in this outcome, unlike in college completion, family income and student ability were irrelevant.

Many of the worst adverse effects are found amongst those exposed prenatally and in early childhood, suggesting that congenital complications in capability development, together with low parental incomes *in utero* and thereafter, may be to blame for such later-life disadvantage. Together, these findings imply that the Dust Bowl acted largely "indirectly," as an economic shock that in turn affected *in utero* and early-life conditions, rather than "directly," through personal exposure (e.g. dust inhalation) in childhood.

Lastly, results—particularly those on New Deal expenditure—imply both that remediation from early-life disaster is possible under the right circumstances, and that post-shock investment may have compensated for rather than reinforced damage to child endowments. The findings in this study are consistent with a multi-stage model of human capability formation, in which investments in one period respond to endowments in a previous one, and may either reinforce or compensate for these endowments.

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“All day the dust sifted down from the sky, and the next day it sifted down. An even blanket covered the earth...Every moving thing lifted the dust into the air: a walking man lifted a thin layer as high as his waist, and a wagon lifted the dust as high as the fence tops, and an automobile boiled a cloud behind it. The dust was long in settling back again.”

– John Steinbeck, *The Grapes of Wrath* (1939, pp. 6, 4)

Chapter 1

Introduction

Figure 1.1: "Dust Bowl Farmer Raising Fence to Keep it from Being Buried under Drifting Sand," Cimarron County, Oklahoma, 1936 (Rothstein, 1936)

1.1 Overview

The American Dust Bowl, a prolonged ecological event characterized by drought, accelerated soil erosion, and severe dust storms, represents one of the most devastating environmental catastrophes in American history (Worster, 1979). During this crisis, which lasted throughout the 1930s, a series of dust storms ravaged the US Great Plains, destroying land, property, and agricultural livelihoods; disrupting public services; and causing injury to health and nutrition (Carlson, 1935; Worster, 1979; Hansen & Libecap, 2004; Burns, 2012; Hornbeck, 2012). Recent literature indicates that early-life shocks and interventions can radically change the course of human capability formation, and thus shape adult outcomes (Barker & Osmond, 1986; Almond, 2006; Heckman, 2007; Cunha & Heckman, 2008; Almond et al., 2009; Almond & Currie, 2011b; Bhalotra & Venkataramani, 2012). In light of this literature, the Dust Bowl's shocks to health and incomes would be expected to have non-negligible adverse effects on wellbeing in later-life; however, these long-run human costs have been little studied (Cutler et al., 2007), meaning we may currently underestimate the full toll of this seminal event in American history.

In this thesis, I analyze the long-term consequences of the Dust Bowl for the human capital of the children who lived through it. Specifically, I use exogenous variation in the severity of this environmental shock across space–proxied by Dust Bowl soil erosion–to explain variation in adult human capital and socioeconomic outcomes. I test for the degree to which the event scarred human capital in the long run, the mechanisms by which such scarring occurred, and the degree to which investment in human capital resulted in successful recovery.

I find that exposure to the Dust Bowl in childhood has statistically significant and economically meaningful adverse impacts on later-life outcomes, for instance, increasing disability and reducing fertility and college completion. Furthermore, these effects are more severe for those born in agricultural states, suggesting that the Dust

Bowl acted largely "indirectly," as an economic shock that in turn affected *in utero* and early-life conditions, rather than "directly," through personal exposure (e.g. dust inhalation) in childhood. Many of the worst adverse effects are found amongst those exposed prenatally and in early childhood, suggesting that congenital complications in capability development, together with low parental incomes *in utero* and thereafter, may be to blame for such later-life disadvantage. Lastly, results—especially those concerning New Deal spending—imply both that remediation from early-life disaster is possible under the right circumstances, and that post-shock investment may have compensated for rather than reinforced damage to child endowments. The findings in this study are consistent with a multi-stage model of human capability formation, in which investments in one period respond to endowments in a previous one, and may either reinforce or compensate for these endowments (Heckman, 2007).

1.2 Contribution to Scholarship

With this study, I make several cross-disciplinary contributions. Firstly, I fill a gap in the historical literature. The Dust Bowl's impacts on land management and conservation, agricultural recovery, and migration have been well studied, and qualitative sources on the Dust Bowl's short-term health effects abound (Worster, 1979; Helms et al., 1996; Hansen & Libecap, 2004; Cunfer, 2005; Hornbeck, 2012). However, only one study has attempted to quantify the long-term human costs of the natural disaster; in it, Cutler et al. (2007) find limited effects of the Dust Bowl using different data and methods from those employed here.[1] As such, it is possible the literature currently underestimates the damage caused by the Dust Bowl. By quantifying the event's later-life impacts on human capital, I more fully account for its human toll.

[1]A more detailed overview of the methods and findings in Cutler et al. (2007) is provided in Section 2.2. The same section also discusses Fishback & Thomasson (2014), a study which, while not strictly focused on the effects of the Dust Bowl on health, considers the long-term impact of 1930s-era income shocks which may to some extent incorporate Dust Bowl-induced income losses.

Secondly, I broaden the range of early-life shocks studied. Rather than the sharp shocks typically analyzed, the Dust Bowl represents prolonged stress more like famine Ravelli et al. (1999); Roseboom et al. (2000); Almond et al. (2007); Ó Gráda (2011) than the sharp shocks—for instance, a sudden disease outbreak, educational policy change, or health intervention—more often studied in works such as Almond (2006), Duflo (2001), and Bleakley (2007). The fact that individuals may have adjusted to this shock even while the event was ongoing—as in the case of endogenous fertility responses, which have received relatively little attention in the literature—adds to the realism and complexity of events studied. Furthermore, the Dust Bowl was an agrarian economic shock resulting from environmental catastrophe. For contrast, while studies of the health impacts of environmental disasters exist (e.g., Almond et al. (2009)), much research in early-life health focuses on shocks and interventions directly targeting health;[2] fewer still track the effect of short-term physiological changes and subsequent investments on adult outcomes (Bhalotra & Venkataramani, 2012). I, however, show that an environmental shock to income acted through childhood health and education to produce permanent damage to adult wellbeing.

Lastly, I contribute to our understanding of the technology of capability formation. Mine is one of the few studies in this literature to examine the question of remediation. By testing for the biological capacity for recovery from exogenous, Dust Bowl-induced changes in children's human capital endowments, I attempt to disentangle changes in human capital which result from the shock itself from those due to investments made to ameliorate or reinforce these changes to the endowment. Specifically, I identify farming regions where the nature of the shock may have led to a greater loss in human capital, where compensating investments may have responded to local labor market-driven variations in returns to human capital investments and thus resulted

[2] A great many studies, like Almond (2006), Bleakley (2007), and Bhalotra & Venkataramani (2012) focus on shocks and interventions directly targeting health; however, studies like Dehejia & Lleras-Muney (2004), Banerjee et al. (2010), and Adhvaryu et al. (2013) focus on the adverse health outcomes induced by shocks to childhood income.

in poorer recovery from childhood insults, and where such investments may have also been constrained by low incomes. In addition to assessing possible household investment responses to the shock, I also test whether state intervention in the form of New Deal spending was effective in aiding recovery from Dust Bowl damage. Thus, through these approaches to analyzing remediating investment, I provide empirical support for theories of self-productivity and dynamic complementarity in capability formation, such as those advanced by Becker & Tomes (1976) and Heckman (2007). Furthermore, in testing for the effects of the state's investment response via New Deal recovery spending, this study is one of the few in the capability formation literature that examines public policy responses at the time of a disaster. In this respect, this study is at a great advantage over contemporary studies of recovery, since its historical context allows for the long-range testing of early-life policy responses.

1.3 Organization of Thesis

The remainder of the thesis proceeds as follows.

In Chapter 2, I provide an overview of the two key literatures in which this study is grounded: that on the American Dust Bowl as a historical event, and that on early-life health and human capital formation as a complex biological process with both socioeconomic inputs and outputs. I also describe a multi-stage model of human capital formation which serves as a theoretical framework for understanding the results of this study holistically.

In Chapter 3, I describe my baseline methodological approach, including identification strategy and data. I also outline the results of this baseline analysis, and provide evidence that early-life exposure to the Dust Bowl has adverse effects on later-life wellbeing.

In Chapter 4, I begin a focus on the mechanisms by which the Dust Bowl impacted later-life wellbeing. Firstly, in Section 4.2, I disaggregate the timing of Dust Bowl exposure by age band at impact to establish that earlier developmental stages matter more to the Dust Bowl cohort's human capital formation process. I also show that identifying the age at the time of the shock helps clarify the pathways by which specific capabilities may be formed. Secondly, in Section 4.3, I examine the importance of agriculture-dependence in modifying the Dust Bowl shock. I show that the Dust Bowl was fundamentally an income shock, and that for a number of reasons, farm-state children were especially ill-positioned for both the shock and recovery from it. Finally, in Section 4.4, I examine in more detail the findings, originally arising in the preceding section on farming livelihoods, surrounding child labor and schooling. I show mixed evidence that the beneficial effect of the Dust Bowl on high school completion operates through a reduction in the opportunity cost of schooling as the demand for child labor collapsed with the Dust Bowl's adverse shock to agriculture. Instead, I find that those regions in which child labor was essential to agricultural production had little change in schooling outcomes relative to regions using child labor less intensively, suggesting that the positive baseline results on secondary schooling may be driven in part by relatively low levels of child labor demand in the sample.

In Chapter 5, I turn to an investigation of remediation, the possibility of recovery, and the mechanisms by which individuals, households, and policy may have responded to the Dust Bowl shock. I begin in Section 5.2 by investigating migration as a possible strategy for coping with the Dust Bowl shock. I find no evidence that the Dust Bowl drove children's contemporaneous or adults' eventual out-migration, nor do I find evidence of migrant selectivity. However, the results do suggest that for those who eventually left, this eventual migration, likely after the childhood period, may have contributed to recovery. Next, in Section 5.3, I examine whether fertility restrictions may have served as a margin of adjustment to the Dust Bowl shock, and if so, who availed themselves of this strategy. Namely, I test whether the Dust Bowl prompted an endogenous fertility response, whether across all Dust Bowl-exposed potential mothers or differentially by these women's socioeconomic status. Here, in contrast to acute famines, the Dust Bowl appears not to have spurred a change in fertility. I later return to issues of migration and fertility selection in Chapter 6, this time considering the implications of these phenomena for the larger results on later-life wellbeing. Lastly, in Section 5.4, I test for the capacity for recovery from early-life insults. I provide empirical evidence from New Deal spending indicating that remediation of early-life insults to human capital is possible, especially if intervention is timely and substantial.

In Chapter 6, I systematically address several potentially confounding phenomena—namely the Great Depression, Dust Bowl-era migration, fertility selection into the sample, and mortality selection out of the sample—and show that these issues do not, in fact, undermine the results presented in this thesis. Rather, the evidence suggests that if anything, the main results presented here are underestimates of the true Dust Bowl insult to later-life human capital and wellbeing.

In Chapter 7, I provide a summary of the main results outlined in the thesis and synthesize these findings in the context of a theoretical framework for understanding the dynamic process of human capital formation. In particular, I explain how childhood economic shocks resulted in increases in secondary schooling alongside permanent health damage, and discuss the implications of results on agriculture and public recovery spending in light of this existing model of capability formation. I also highlight the key contributions of this study to both scholarly and policy debates, particularly regarding disaster-remediation strategies and optimal human capital investment.

Chapter 2

Literature Review

Figure 2.1: “Approaching Dust Storm in Middle West” (Conard, 1935/1936)

2.1 America's Dust Bowl

2.1.1 Overview

During the period from 1930 to 1940, poor rainfall and strong winds precipitated a series of massive dust storms across the US Great Plains (Stephens, 1937; Lord, 1938; Wallace, 1938; Hansen & Libecap, 2004; Cunfer, 2005).[1] These storms blew away roughly 480 tons of fertile topsoil per acre, with many regions losing over 75% of their original topsoil by 1940 (Hansen & Libecap, 2004; Hornbeck, 2012). Winds decimated existing crops through exposure and abrasion, and prevented new ones from being planted (Soil Conservation Service, 1935). Meanwhile, dirt deposited downwind suffocated livestock and buried crops and property (Worster, 1979; Hansen & Libecap, 2004; Egan, 2006). Accordingly, agricultural yields were low (Bennett, 1939; Bonnifield, 1979) and recovery was slow (Hornbeck, 2012).

The Dust Bowl was not just an environmental crisis, but a human tragedy as well (Worster, 1979; Hurt, 1981; Egan, 2006; Burns, 2012). As the country struggled with the Great Depression, the Dust Bowl compounded misfortunes, especially for the farming communities of the Great Plains (Wallace, 1938; Cutler et al., 2007). Economic hardship was widespread: meager harvests meant low incomes, nutritional deprivation–and, for some, farm foreclosure and migration (Bonnifield, 1979; Worster, 1979). Financial hardships prompted taxpayers to slash school budgets, while school attendance fell during major dust storms (Soil Conservation Service, 1935; World Meteorological Organization, 2009; Hansen & Libecap, 2004; Egan, 2006; Burns, 2012).[2]

More directly, dust storms were both an everyday nuisance and a serious health hazard to Great Plains inhabitants (Agricultural Adjustment Administration, cited in Cunfer, 2005). Poor air quality and dust inhalation led to respiratory illnesses

[1] In the evocative words of Gove Hambidge in his executive summary in Wallace (1938), "water and wind have flayed the skin off the unprotected earth, causing widespread destruction."

[2] Egan (2006) suggests that the demand for schools existed; closures were largely due to lack of funding.

such as "dust pneumonia" and asthma, which could often be life-threatening (Soil Conservation Service, 1935; Worster, 1979; Hurt, 1981; Hansen & Libecap, 2004; Egan, 2006; Burns, 2012). Eye infections and influenza, too, were reported at greater rates in Dust Bowl-affected counties (Hansen & Libecap, 2004). "Black blizzards," as the worst dust storms were called, could be particularly dangerous: Hurt (1981), recounting an incident in which a 7-year-old Kansas boy was stranded outside in a dust storm and was found the next day smothered to death, and another, in which the same storm left a 9-year-old boy alive but tangled in barbed wire, aptly describes the experience of becoming lost in these storms as "traumatic and sometimes fatal" (Hurt, 1981, p. 51). Such prolonged deprivation and stress thus also had a psychological dimension: demoralized residents felt uncertain and powerless, under constant siege by the environment and subject to a litany of indignities large and small (Lord, 1938; Steinbeck, 1939; Worster, 1979; Egan, 2006; Burns, 2012). What had once been home became surreal and terrifying, "as nearly a literal hell on earth as can be imagined" (Lord, 1938).

2.1.2 Key Studies

Whatever its effects on people, the bulk of work on the Dust Bowl to date—particularly that taking a quantitative approach—has nevertheless tended to focus on issues related to agriculture and the environment. Below I provide a more detailed overview of a few select studies which are some of the most influential in this vein.

Although other works of similar scope and vintage exist, (e.g., Bonnifield, 1979; Hurt, 1981), the seminal work on the Dust Bowl is Donald Worster's widely-cited 1979 book, "Dust Bowl: The Southern Plains in the 1930s." In this wide-ranging historical study, Worster investigates the origins of the disaster, its immediate consequences for farming communities of the Plains, and the measures taken in the aftermath of the disaster to ensure a sustainable and ecologically responsible future for Plains agriculture. Having explored these overarching questions more generally, Worster then examines the Dust Bowl through the lens of two particularly hard-hit counties which serve as case studies: Cimarron County, Oklahoma, and Haskell County, Kansas.

A strong theme in this work is the role of capitalist expansion in creating incentives for the reckless and short-sighted plunder of the natural resources of the Great Plains, with ultimately devastating results. The targets of his criticism are varied—from desperate early Homesteaders and speculative, absentee "suitcase farmers," to politically-driven sub-optimality in farm sizes and irresponsible use of the disk plow, to the preposterous notion that rain would follow the plow—but all find their roots in what he sees as a relentlessly growth-minded and exceptionalistic American culture, one that believes that man can achieve dominion over nature by sheer force of will. Given this view, Worster is pessimistic about America's ability to avoid the next such environmental crisis, and advocates for a far-sighted agricultural sector that prioritizes conservation and sustainable production practices, although he is vague on what these strategies might look like in practice.

Geoff Cunfer's "On the Great Plains: Agriculture and Environment" (Cunfer, 2005) builds on the work in Worster (1979) by marshaling a wealth of data on county-level environmental conditions and farming practices to tell a systematic and comprehensive story of the causes and consequences of the Dust Bowl, grounded in scientific theories of agroecology. Whereas Worster (1979) echoes the conventional wisdom of early policymakers and government scientists[3] who viewed the Dust Bowl as a largely man-made disaster wherein expedient and wrong-headed cultivation techniques pushed marginal lands to their ecological limits,[4] Cunfer (2005)'s analysis of

[3]Geographer Edwin James and Major Stephen H. Long provide just one such example; in the report of their 1819-1820 expedition to the Great Plains, they state, "We do not hesitate in giving the opinion, that it [the Great Plains region, here ominously dubbed "The Great American Desert"] is almost wholly unfit for cultivation, and of course uninhabitable by a people depending upon agriculture for their subsistence. Although tracts of fertile land, considerably extensive, are occasionally to be met with, yet the scarcity of wood and water, almost uniformly prevalent, will prove an insuperable obstacle in the way of settling the country." (Long & James, 1823, p. 361)

[4]This perspective has once again gained traction in popular discourses about the Dust Bowl

weather and land management patterns leads him to a different and less damning conclusion.

Citing the fact that dust storms were a regular feature of the Great Plains well before the arrival of the impetuous Homesteaders popularly blamed for the Dust Bowl, and analyzing climatic data from the 1930s, he finds that "human land-use choices were less prominent in creating dust storms than was the weather" (Cunfer, 2005, p. 163). He goes on to recommend that "it is time for environmental historians to consider the possibility that dust storms, rather than being evidence of human ecological failure, are instead normal forms of ecological disturbance on the southern plains that occur whenever the region experiences extended periods of low rainfall and high temperature" (Cunfer, 2005, p. 163).

He credits irrigation from the mid-1930s to 1950s, particularly from the region's Ogallala Aquifer, with allowing farmers to eventually surpass climate and soil constraints, at least in the short term. This solution is framed as just one in a line of many technological advances in Plains agriculture, beginning with the introduction of draft horses and leading up to the use of synthetic fertilizers, that allowed humans to briefly exceed the natural limits of their environment, often at considerable cost. The solution's impermanence is presented as emblematic of the need for humans to adjust and readjust to "shifting cultural and natural contexts" (Cunfer, 2005, p. 200)—e.g., through improvements in production technology—in halting pursuit of productive and sustainable agriculture.

A possible shortcoming of Cunfer (2005)'s study lies in the fact that in conducting analysis at the county level, he ignores crucial differences in incentives and agricultural practices at the farm level, particularly those that were likely to depend on farm size. Here, Hansen & Libecap (2004) provide perspective at a granular level. In particular,

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thanks to Ken Burns' recent documentary film, which while excellent—and particularly groundbreaking in its extensive use of newly-collected oral histories—at times lacks accuracy and scholarly rigor (Burns, 2012).

they investigate the ways in which farm size constrained farmers' ability to coordinate erosion control efforts on a private basis.

Hansen & Libecap (2004) begin by establishing the prevalence of small farm sizes in the Great Plains in the 1930s: in 1930, for instance, the share of farms smaller than 500 acres in Plains counties with a crop share greater than or equal to 40%—that is, the percentage of farms less than half the productively efficient size—was 68%. Given the propensity of small farms to cultivate more intensively and to invest less in soil conservation than larger farms, such farms are likely to exacerbate any natural blowing externalities for farms downwind. However, farmers were unable to meet the need for coordination that arose from the fact that productively optimal farm sizes are smaller than the sizes necessary to manage wind erosion.[5] For example, even if a farmer pursued wind erosion control strategies such as strip cropping and strip fallow on his own farm, his investments could be rendered useless by an upwind farmer's failure to control erosion.

Indeed, Hansen & Libecap (2004) find that the prevalence of small farms both exacerbated erosion and inhibited efforts to stop erosion. Specifically, they find that smaller farms had lower fallow shares of land (i.e., invested less in erosion control), that the prevalence of small farms in a region depressed the fallow share for all farms in the region, irrespective of size; and that regions with lower shares of fallow land experienced more severe erosion. They also show that without coercive government institutions, the collective action necessary to control erosion could not be achieved. Here, they provide evidence that the soil conservation districts created by the Soil Conservation Service in 1937 and enforced by contract helped solve this collective action problem. In their analysis, Hansen & Libecap (2004) thus suggest a role for timely policy intervention similar to the one that I explore in a public health context in Section 5.4. Their argument also implies, however, possible endogeneity in the

[5]Due to the legacy of Homesteading the realized farm sizes in the Great Plains were often even smaller than would have been productively efficient (Hansen & Libecap, 2004).

severity of (at least wind) erosion.[6]

Hornbeck (2012) joins Hansen & Libecap (2004) as one of the latest studies of the Dust Bowl in the fields of environmental and agricultural economics. In the paper, Hornbeck examines the speed and magnitude of agricultural adjustments to the Dust Bowl in the short- and long-run, and finds that the disaster had a devastating and persistent effect on farms in the Great Plains. In particular, he finds that farmers in more-eroded counties (that is, counties more severely affected by the Dust Bowl) were slow to adjust land use, and that there was limited scope to make these adjustments, largely due to credit constraints. He identifies population adjustment—even though incomplete—as the main mechanism behind economic adjustment. These observed relative population declines in more-eroded counties were the result not only of out-migration, but also of diverted in-migration,[7] and were large enough to reestablish labor market equilibrium in the absence of an increase in labor demand.[8]

[6] I show in Chapter 3 that for the purposes of my analysis of later-life human capital, which uses population-weighted erosion data at the state level, it is fair to consider erosion severity to be plausibly exogenous. Additionally, work like that of Cunfer (2005) seems to suggests that small farm sizes did not precipitate the Dust Bowl crisis, both since farms of this size had existed on the Plains for some time before the combination of high temperatures and low precipitation created the conditions for disaster, and also since major dust storms had been reported well before the large-scale settlement and cultivation of Plains land. Similarly, although he does not comment on whether small farm sizes created and/or exacerbated the Dust Bowl while the disaster was ongoing, Hornbeck (2012) discounts small farm sizes as a barrier to the land-use adjustments necessary for agricultural recovery, a matter relevant to my analysis insofar as it suggests a weak connection, if any, between farm size as a characteristic associated with erosion severity and farm size as a characteristic associated with economic success and the capacity for economic recovery.

[7] Taken in tandem with recent work on Dust Bowl-era migration patterns by Long & Siu (2014), it seems that the diverted in-migration effect may actually be the more important component of population declines in Dust Bowl counties.

[8] As Hornbeck (2012) is perhaps the most well-known of the recent studies on the Dust Bowl as a historical event and ecological disaster, it is worth taking a moment to delineate the differences between its approach and findings, and those presented in this thesis. As will be described in Chapter 3, my study makes use of Hornbeck's erosion data, by which Dust Bowl severity is identified in both studies, but transforms it to suit the purposes of a state- rather than county-level analysis focused on human populations rather than on land resources. Most crucially, the research questions are fundamentally different and pertain to entirely distinct thematic literatures. Hornbeck (2012) investigates the short- and long-term costs of the Dust Bowl to agriculture, the speed and scope for adjustment in both timeframes, and the mechanisms by which relative adjustment took place (e.g., adjustments in land use, reallocations of labor and capital, large-scale out-migration). Accordingly, the study contributes to debates on agricultural recovery and the speed and magnitude of short- and long-run adjustment to changing environmental conditions. This thesis, on the other hand, asks how and why the Dust Bowl affected individuals' health and later-life wellbeing, and investigates the

The key works outlined here establish the basic historical facts of the Dust Bowl, as well as its short- and long-term effects as an event affecting agriculture and the environment. The findings of these studies are essential to my analysis in that they clearly show the Dust Bowl's capacity to act as a large and adverse income shock through the farming sector. They also raise worthy questions regarding the plausible exogeneity of the Dust Bowl. Crucially, however, these studies do not address the costs of this disaster (or even just those of the agricultural collapse it precipitated) to individuals.[9] With this thesis, I take a systematic and rigorous approach to assessing the Dust Bowl's impact on people. In particular, I quantify the event's effects—both through agriculture and more broadly—on the long-term health and welfare of human beings. In so doing, I seek to fill this important gap in our current understanding of the Dust Bowl.

2.2 Early-Life Health & Human Capital Formation

2.2.1 Overview

Current research[10] indicates that stress, deprivation, and early-life shocks to income and health can have long-term adverse impacts on wellbeing (Barker & Osmond, 1986;

technology of human capital formation and recovery from childhood disadvantage. Its contributions are thus primarily to the early-life health literature at the intersection of health and labor.

[9] Any discussion of human populations in these studies tends to be anecdotal, e.g., in Worster (1979), or incidental, as in the discussion of population declines in Hornbeck (2012) or in the investigation of migration flows in Long & Siu (2014). In all cases, with the exception of Cutler et al. (2007), which is discussed in detail in Section 2.2, any discussion of the Dust Bowl's effects on humans focuses on the short term.

[10] Although much of the research I present here exploits natural experiments to investigate capability formation, there are a number of studies which use other empirical approaches. For instance, Heckman et al. (2010) and Campbell et al. (2014) use purpose-built experimental data to evaluate the impact of educational interventions such as the Perry Preschool and Abecedarian programs. Here, the randomized control design enables causal inference, and facilitates the estimation of human capital production model parameters. Meanwhile, clinical studies such as Roseboom et al. (2000) enable testing of personal health outcomes not usually available to social scientists.

Cutler et al., 2007; Heckman, 2007; Almond & Currie, 2011b; Bhalotra & Venkataramani, 2012). Human capital development may be impaired directly through childhood illness, and indirectly, through poor prenatal conditions (e.g. maternal stress, illness, or malnutrition) or deprivation in early life. These poor early-life circumstances hamper the successful development of capabilities such as cognitive and non-cognitive skills, metabolism, and immunity,[11] and have been implicated in conditions from coronary heart disease to schizophrenia (Ravelli et al., 1999; Roseboom et al., 2000; Cutler et al., 2007; Heckman, 2007). [12] In turn, low human capital stocks negatively impact other outcomes such as later-life income and employment (Heckman, 2007; Bhalotra & Venkataramani, 2012). Given the Dust Bowl's documented destruction (Worster, 1979; Nealand, 2008; Burns, 2012), Dust Bowl survivors are likely to experience just such damage to human capital.

Indeed, work on famines, events to which the Dust Bowl bears considerable resemblance, indicate that acute nutritional deprivation in early life can have devastating short- and long-term consequences. For instance, prenatal exposure to the Great Chinese Famine resulted in increases in male mortality, lower rates of literacy, and poorer marriage and labor market outcomes (Almond et al., 2007). The Great Irish Famine of the 19th Century similarly rendered individuals vulnerable to disease, and thus to excess mortality Ó Gráda (2000), while the Dutch Hunger Winter, due in part to mismatches between the fetal and post-natal environment, led to obesity and heart disease in adult survivors exposed *in utero* (Ravelli et al., 1999; Roseboom et al., 2000; Stein et al., 2007).

Of course, malnutrition need not be so extreme to have later adverse consequences. Almond et al. (2015), for instance, show that individuals subject to fetal nutritional deprivation due to their mother's Ramadan fasting attain lower test scores at age 7.

[11]For instance, infections such as pneumonia in early life have been shown to subvert cognitive development by producing an inflammatory response that diverts resources from normal mental development processes as survival is prioritized (Bhalotra & Venkataramani, 2012).

[12]See overview of famine and fetal programming literature in Ó Gráda (2011).

This finding implies that cognitive development is sensitive to fetal nutrition, that it takes place early in childhood capability formation, and that targeted prenatal investments could be more effective in boosting academic performance than more typical schooling interventions. Other recent work in this arena has focused on the content of the nutrition received: for instance, addressing the early survival advantages conferred by breastfeeding in the era before modern public health services (e.g. Arthi & Schneider, 2016), and establishing both the problem of micronutrient deficiency and the later-life wellbeing benefits of government-mandated iron and iodine fortification (Feyrer et al., 2013; Niemesh, 2015). These nutritional channels for development are especially relevant in the case of the Dust Bowl, which through its impacts on incomes and farming,[13] had the capacity to raise rates of maternal and child malnutrition.

The Dust Bowl also posed environmental hazards, which the early-life health literature suggests could produce its own set of adverse consequences for later wellbeing. Almond et al. (2009), for example, find that children exposed to nuclear fallout following the Chernobyl disaster suffer impaired cognitive function. Meanwhile, and more readily analogous to dust exposure, reductions in air pollution are associated with improvements in infant mortality, prematurity, and birthweight (Chay & Greenstone, 2003; Currie & Walker, 2011), and increases in such pollution, with worse chances of infant survival (Jayachandran, 2009).

However, it is not easy to extrapolate the effects of the Dust Bowl solely based on its similarity to other shocks. Indeed, most relevant to policy and to our theoretical understanding of capability formation are the mechanisms at play, which are in turn shaped by factors such as the context and capabilities under investigation. As an example, what are the effects on later-life health of early-life improvements in public health infrastructure? Troesken (2008) investigates the impact of the use of lead pipes

[13] Notably, although in many cases agricultural destruction had adverse effects, for example, on nutrition Ó Gráda (2000, 2011), it could also have beneficial effects. To wit, and in a pooint I will return to in Section 4.4, Greenbaum (2009) and Baker (2015) show that agricultural blight resulted in educational improvements for children previously active in child farm labor.

in municipal water supply on infant mortality in late 19th Century Massachussetts, and finds that their use—especially where pipes were new or water was acidic, both of which made for greater lead leaching—led to substantial increases in infant deaths. Alsan & Goldin (2015), too, examine the effects of early water sanitation efforts; however, they find that the introduction of sewerage systems, along with clean water initiatives, led to decreases in infant mortality in turn-of-the-century Boston. Clearly, the specifics of the infrastructural upgrades, and the channel by which they act on health, matter. In Troesken (2008), lead toxicity is the culprit, whereas in Alsan & Goldin (2015), one of the virtues of the new system is the mechanical (rather than chemical, as in Cutler & Miller (2005)) destruction of bacteria through reservoiring. Of course, new infrastructure alone cannot improve children's chances: although the presence of piped water is generally considered an indicator of human development, in 1970s Malaysia and in many other developing-country contexts, the use of such water in supplementing breastmilk raises rates of mortality in early infancy due to lags in its purification (Butz et al., 1984). Thus, human capital inputs (in this case, breastfeeding, water infrastructure, and sanitation programs) can intersect with and complement each other in complex and sometimes counterintuitive ways.

These studies underscore the complexity of the capability development process. For instance, as discussed above with respect to water supply infrastructure, seemingly similar interventions can have different (and sometimes opposing) effects through different channels, on different health outcomes, and in different geographic and historical contexts. Such seeming contradictions in outcomes, particularly when shocks are mediated by other factors, are not uncommon. For instance, as will be touched upon in Sections 6.2.2 and 6.5, respectively, the effect of recessions on public health depends on the degree of economic development, the sophistication of medical infrastructure, and the presence of social safety nets (Ruhm, 2000; Chay & Greenstone, 2003; Dehejia & Lleras-Muney, 2004; van den Berg et al., 2006; Banerjee et al., 2010;

Arthi et al., 2016a); and a region's poverty can determine the sign of the relationship between the early-life disease environment (often proxied by the infant mortality rate) and later-life stature (a measure of net nutritional status) (Deaton, 2007; Hatton, 2011). Furthermore, the same set of interventions in the very same context may have distinct effects depending on the capability (e.g. cognitive vs noncognitive skills) and the production technology associated with each (Cunha et al., 2010). Accordingly, researchers must be sensitive to the specifics of their case and cautious in extrapolating from other studies.

In the following section I highlight in greater detail a few studies which are particularly influential in the early-life health literature, and pertinent to the analysis presented in this thesis. For all the complexity of the capability formation process discussed above (and again from a theoretical perspective in Section 2.3, below)—and for all the tremendous diversity in this literature in the contexts and outcomes studied and theoretical and empirical approaches used—these works nevertheless emphasize some of the field's most robust and overarching themes. Namely, they foreground the sensitivity of early stages of development, and the importance of socioeconomic gradients in health, themes which will become especially relevant in the coming chapters.

2.2.2 Key Studies

The pioneering work in this field by epidemiologist David Barker establishes a framework for understanding capability formation as a biological process influenced by genetic, environmental, and epigenetic factors. Early research such as Barker & Osmond (1986), which linked the early mortality environment to later-life ischaemic heart disease, pointed to a connection between fetal and adult health. However, it was in Barker (1990) that a "fetal origins hypothesis," as it would come to be termed, was first proposed. Put simply, the fetal origins hypothesis proposes that many human capabilities—for instance, metabolism and mental health—may be programmed prenatally in response to conditions *in utero*.[14] In such a framework, accurately assessing fetal health, which is largely unobservable, becomes paramount. Consequently, Barker (1990) points to birthweight and the infant mortality rate at the time of birth as possible proxies for the intrauterine and neonatal environments, respectively. These measures—although they have their limitations—have since become widely used in studies of fetal and early-life programming (Wilcox, 2001).[15]

Barker and his coauthors have further developed this line of inquiry in the years since, gathering clinical evidence of the fetal origins of health outcomes including cardiovascular disease, obesity, and schizophrenia (e.g., Ravelli et al., 1999; Roseboom et al., 2000). Their work has sparked a boom in interdisciplinary research in this vein (e.g., Almond, 2006; Almond & Currie, 2011b), and has been influential in shaping the way social scientists view the acquisition of human capital and the interaction between nature and nurture as a part of this process. In turn, economists and economic historians have contributed much to this literature. For instance, they have formalized the theory behind fetal origins, enshrining key features of the clinical/epidemiological view of fetal programming within a more comprehensive model that describes the multi-dimensional, multi-stage, and intergenerational process of human capital formation (Heckman, 2007; Cunha & Heckman, 2008). They have also mined historical data for empirical support of the fetal origins hypothesis (e.g., Almond et al., 2009; Banerjee et al., 2010; Bhalotra & Venkataramani, 2012), a contribution which is especially valuable given the time, financial, and ethical constraints

[14] See Almond & Currie (2011b), Almond & Currie (2011a), and Ó Gráda (2011) for excellent and detailed overviews of fetal programming.

[15] Recent work such as Hanson et al. (2015) and Wilcox (2001) has challenged the usefulness of such measures in understanding true fetal health and growth processes, and has searched for fuller and more informative alternatives. For instance, birthweight represents an input (fetal health) into the production of adult health; however, it is itself the output of a complex fetal growth process. Thus, the birthweight measure can obscure important aspects of the fetal environment, and can lead to paradoxical results if and when the fetal conditions diverge sharply from those experienced in early life. Here, models of adaptive growth attempt to reconcile the paradox (Gluckman & Hanson, 2006; Gluckman et al., 2007; Schneider, 2014). Similarly, infant mortality rates at the time of birth could provide a flawed snapshot of the postnatal environment if the diseases that kill infants are different from those that scar them as infants or young children.

associated with purpose-built, long-range human experiments.

Accordingly, one of the most influential of the empirical economic studies in this literature is Almond (2006)'s research into the effects of the 1918 influenza pandemic in the United States. Almond uses the event as a natural experiment to test the long-range effects of *in utero* deprivation and maternal ill health during pregnancy. The flu pandemic struck the U.S. suddenly in October 1918, and had largely abated by the early months of 1919. The sharp timing of the shock, the narrowness of the plausible treatment band, and the availability of data on individuals' quarter of birth allows for particularly clean identification, while the focus on those experiencing prenatal exposure[16] allows for more accurate matching of individuals' birthplaces with their likely place (and therefore, severity of) exposure.

Almond (2006) pursues several strategies to triangulate the effect of the pandemic on the health of those exposed to it prenatally: first, an analysis of individual-level outcomes that exploits only the timing of the shock; second, an individual-level analysis that exploits both variation in pandemic timing and severity (i.e. virus strength, or the likelihood that influenza infection would develop into pneumonia) by census division; and third, an individual-level analysis that exploits temporal and spatial variation in pandemic exposure, where the spatial component is identified by state-level maternal influenza infection rates (Almond, 2006). The last strategy in particular attempts to control for culling and migration by controlling for attrition rates and infant mortality rates in the relevant birth cohort. Almond finds that a variety of adult outcomes—including rates of disability, welfare income, schooling, socioeconomic status, and criminality—are adversely affected by prenatal influenza exposure, especially for men.[17]

[16]The *in utero* focus is both a choice the study makes, as well as the result of the pandemic's idiosyncratic W-shaped mortality curve, which meant prenatal exposure was a likelier vehicle for scarring than childhood exposure.

[17]Recent work by Brown & Thomas (2013) suggests the adverse later-life effects attributed by Almond (2006) to prenatal influenza exposure are instead an artifact of a bundle of World War I effects including compositional changes in the birth cohort and negative selection on paternal

Thus, he provides evidence that fetal health may drive observed gradients between adult health and socioeconomic status, with the implication that investments during (or even before) pregnancy could have dramatic multiplier effects and, in the case of the U.S., could even help redress racial inequalities in health and socioeconomic outcomes. Importantly, he calls attention to the latency of congenital effects of early-life deprivation, which may go undetected until they manifest much later in life, particularly in poor disability and labor market outcomes. I provide further evidence to this effect throughout this thesis, but especially in Section 4.2, where I show outcomes such as poverty and fertility to have their genesis in developmental stages earlier than those that might intuitively be expected.

Bleakley (2007), in another important paper in the early-life health literature, moves the focus forward in an individual's developmental process to examine the effects of a positive health intervention in childhood. Using a differences-in-differences strategy similar to the one presented in this thesis,[18] Bleakley tests for the effect of a Rockefeller Sanitation Commission-sponsored hookworm eradication campaign in the U.S. South on children's contemporaneous and long-term schooling and socioeconomic achievement. Hookworm disease, as a parasitic infection, is hypothesized to depress the returns to investment in human capital by draining children of the energy necessary to fully benefit from schooling. As such, the eradication of the disease should then raise the returns to schooling and lead to better educational and knock-on labor market outcomes for children, but not for those who were already beyond

socioeconomic characteristics as fitter, higher-status men left the pool of potential influenza-cohort parents and for military service in Europe. Nevertheless, the authors' identification of childhood economic circumstances as the driver of later-life disadvantage remains supportive of the fetal origins hypothesis and leaves the fundamental policy implication of these findings—that fetal health matters, and that investments in early-life health are likely to yield higher returns than those in later-life health—largely unchanged.

[18] One notable difference in identification is that while I identify spatial differences in the intensity of treatment by using observed post-shock measures (i.e., Dust Bowl-induced soil erosion), Bleakley (2007) uses pre-shock measures (i.e., pre-eradication rates of hookworm infection) to proxy the effects of this intervention, based on the logic that places with high pre-intervention levels of disease would stand to gain the most from the intervention and thus should be considered more intensively treated.

school-age at the time of the intervention.

Here, in contrast to the latent congenital effects Almond (2006) finds, the specific circumstances of this intervention and its role in human capital production lead Bleakley (2007) to find immediate effects: hookworm eradication leads to large relative gains in contemporaneous school enrollment and literacy, and appears to have affected human capital formation primarily through the intensive margin. In analyzing the later-life effects of childhood hookworm eradication, Bleakley (2007) finds relative increases in earnings for treated cohorts which are not attributable to changes in labor supply, but which are driven largely by relative increases in the returns to schooling.

Thus, these results, too, support the proposition that intervention-induced increases in the quality (rather than necessarily the quantity) of schooling are an important mechanism by which the intervention boosted short- and long-term wellbeing. The findings in Bleakley (2007) provide evidence of the concept of self-productivity (the idea that higher levels of capability in one period beget higher levels of capability in the next, in ways that are both "self-reinforcing and cross-fertilizing" (Heckman, 2008)), which will be discussed in greater detail in Section 2.3. They also parallel my findings in this thesis, which show that although education would have been an intermediate input into the production of adult socioeconomic status, higher rates of high school attainment did not result in lower levels of adult poverty, likely because these rises in schooling do not appear to be associated with rises in student quality.

Of all the studies on human capital formation, there is one that is especially relevant to the analysis presented in this thesis: Cutler et al. (2007). While studies such as Fishback et al. (2007) and Fishback et al. (2011) have quantified the short-term effects of Dust Bowl-era income and climate shocks on infant health, mortality, and demographic outcomes, only one prior study, Cutler et al. (2007), has investigated the *long-term* effects of the Dust Bowl on health and wellbeing.[19] In Cutler et al. (2007), variation in childhood economic conditions (i.e. childhood crop yields, income, employment and the region-year level and father's farm status at the individual level) is posited to drive variation in adult human capital (e.g. the likelihood of diabetes, stroke, and high blood pressure, and other individual-level adult health outcomes taken from the Health and Retirement Survey).

In their analysis, Cutler et al. find generally intuitive relationships between early-life economic conditions in the 1930s and later-life outcomes, but little of statistical significance. Comparisons between Cutler et al. (2007) and the study presented in this thesis are difficult, both because many of the outcome variables are not analogous, but also because both the sources and the empirical strategy differ, particularly in terms of the conceptual, spatial and temporal definitions of "exposure." Where the studies may be reasonably compared (for instance, on disability outcomes, one of the only set of outcome variables roughly comparable between the two studies), the sign of the relationship and the magnitudes of coefficients are similar. Statistical significance, then, appears to be the reason why the two studies reach different conclusions as to the Dust Bowl's effects: in Cutler et al. (2007)'s case, the disability coefficients are largely statistically insignificant, which may simply be a function of sample size (the IPUMS data used here consists of over 4 million observations, while the HRS data consists of a little under 60,000).[20]

Thus, the empirical work connecting early-life conditions to later-life wellbeing suggests that the Dust Bowl is likely to have had meaningful effects on individuals'

[19]One other recent study by Fishback & Thomasson (2014) examines later-life human capital outcomes in this period, but focuses on the Great Depression as a macroeconomic shock rather than on the Dust Bowl as an environmental one. Accordingly, in this study, the authors use childhood income shocks as the source of variation in childhood circumstances. I discuss this study in further detail in Section 6.2.2.

[20]The insignificance of Cutler et al. (2007)'s effects could also stem from their use of an unusually stringent approach to multiple comparisons testing, one that is more conservative even than the cautious approach used in this thesis. I discuss my approach to the multiple comparisons issue in Section 3.5.

human capital formation through childhood and especially prenatal exposure. It is also likely that the effects of such early-life deprivation would be latent for much of an individual's life-course, justifying the choice in this thesis to focus on adult outcomes.

This thesis contributes to the early-life health literature by clarifying aspects of the technology by which capabilities are formed, by providing empirical support for theories of human capital formation, and by doing so using a type of shock atypical in the literature. In particular, I help clarify the pathways by which income shocks and health hazards such as those posed by the Dust Bowl affect the development of specific capabilities. Importantly, in an empirical literature that has rarely examined questions of recovery and remediation, I also provide evidence that the compensatory investments in human capital offered by the New Deal were effective in ameliorating early-life insults.

2.3 Theoretical Framework

The multi-stage model of human capability formation outlined in Heckman (2007) provides a useful and flexible framework to organize our thinking about the potential effects of the Dust Bowl on human capital. Indeed, the results presented in this thesis are consistent with this model, wherein investments at different stages of development have nonlinear effects, and where such inter-temporal investments are complementary rather than perfectly substitutable. Since the technical aspects of this model have been discussed in great depth in Almond & Currie (2011a), it is not my aim to re-tread this ground here; rather, the purpose of this section is to briefly describe how this model provides a conceptual framework for understanding the findings presented in this thesis, and in particular, to outline the predictions that should be expected under Dust Bowl conditions.

In the Heckman (2007) model, an individual's human capital in a given period depends on their human capital in the previous period, as well as on any investments made since then. Accordingly, the constant elasticity of substitution function he proposes is recursive, with human capital in the current period expressed as a function of a series of lifetime investments, starting with the individual's initial human capital endowment (i.e. human capital at birth, itself a function of parental health and prenatal investments), and including any and all subsequent investments in human capital.

In this model, parents derive utility from their children's human capital as well as from their own consumption, and they can trade one for the other subject to a budget constraint. Thus, if the human capital endowment at the end of one period is reduced by an adverse shock which is independent of this endowment, the parents may choose in the next period to remediate this loss by converting their consumption into child capabilities via human capital investment. Whether, and how much, they will invest in recovery will depend on a number of factors, including the size of the initial endowment, the magnitude of the adverse shock to that endowment, and the income available to parents to shift between consumption and children's human capital.

Importantly, and in contrast to earlier approaches to describing capability production, in the model outlined in Heckman (2007), investments in one period need not substitute perfectly for investments in another. An implication of this is that the allocation of investments in human capital across periods matters. Specifically, the effect on human capital of the allocation of the total investment between periods will depend not just on the weight accorded to a given period in the capability production function, but also on the inter-temporal elasticity of substitution. This model allows for both dynamic complementarity in investment, or the notion that investments in a later period are more productive at higher levels of capability in the previous period; and self-productivity in investment, or the idea that higher capability levels in earlier periods create higher levels of capability in later ones (Almond & Currie, 2011a).

In light of this feature of capability production, and returning to the example above in which parents choose how to respond to adverse shocks to their children's human capital, the investment response depends not just on the pre-shock endowment, the size of the shock, and parental income. Rather, and perhaps most importantly, the decision of whether to compensate for the shock or to reinforce it will also depend on the elasticity of substitution of human capital investments across periods in the child's development. Where the elasticity of substitution is high—for instance in this case, where the effects of an investment in the pre-shock period on human capital are very similar to the effects on human capital of the same investment in the post-shock period—parents will make compensatory investments aimed at recovery. However, where there is less inter-temporal substitutability of investments, parents may decide that the level of investment necessary to achieve recovery—if recovery even remains possible—is inefficiently high, and will so instead decide to reinforce the effect of the shock.[21]

The process of capability formation described above is dynamic and complex, with the outputs of one stage of development becoming inputs to the next, and with different dimensions of the capability vector interacting within and across periods to produce feedback loops (e.g. better nutritional and health status raises the productivity of educational investments; schooling in turn promotes health) (Heckman, 2012). However, as Conti & Heckman (2013) note, few of these inter-temporal and cross-capability linkages have been estimated empirically.

It is not the aim of this thesis to test this model outright; indeed, this model is notoriously difficult to test empirically and in a well-identified manner, for instance, because of its multidimensional complexity and the difficulty in obtaining suitable

[21]See Almond & Currie (2011a) for further discussion of the effect a shock on human capital outcomes, which in the case of perfect substitutability of investment across periods depends only on the weighting given to each period, and in the case of imperfect inter-temporal substitutability, implies diminishing marginal productivity of investments.

data.[22] As in most studies in the early-life health literature, the data available here does not support the estimation of cross-capability linkages, nor of the model parameters, including the ones of greatest policy relevance, such as the inter-temporal elasticity of substitution of capability-specific investments. However, this model is nevertheless useful as a theoretical framework for understanding the findings of this thesis as a whole. In particular, it provides several predictions which are relevant in the context of the Dust Bowl.

Firstly, the Heckman (2007) model predicts that if investments in different periods of development are imperfect substitutes, then the age at exposure to the Dust Bowl should matter. That is, if an input (e.g. investment or shock) in childhood has the same effect on later-life human capital irrespective of the period in which the input is made, then inputs are perfectly substitutable across periods, and we should expect no differences in the later-life human capital by the age at Dust Bowl exposure. If, however, such differences exist, then the ages at exposure corresponding to the greatest impact on later-life human capital are sensitive periods for the development of the capability in question.

Relatedly, since the model predicts that exposure to the Dust Bowl during those periods that result in the greatest adverse effects on later-life human capital are those periods which are most important to capability formation, it suggests that if the biological and economic processes governing the production of each capability is the same, then all later-life outcomes should be determined by exposure in the same developmental stage. If the sensitive period for two capabilities is different, however, it is evidence that the two capabilities are governed by different production processes.

The model also suggests several individual characteristics that should lead to worse later-life consequences of early-life Dust Bowl exposure. Firstly, given the model's implication that higher levels of initial investment make subsequent investments more

[22] This is especially the case in historical studies, where rich longitudinal data, and/or information on intermediate investments, can be scarce.

productive, individuals with lower initial stocks of human capital should experience worse later-life outcomes as a result of the Dust Bowl shock than those with higher pre-shock stocks of human capital. Similarly, those with low levels of household income with which to remediate shocks, and those with low returns to human capital investment, will suffer worse outcomes, respectively, than those who face the same shock but are unconstrained in their investment in recovery and those who have higher incentives to compensate for the shock. Those who experience the largest effective shock (e.g. income shocks in addition to environmental hazards to health) will also be expected to suffer greater losses in later-life human capital than those who only experienced one dimension of the Dust Bowl shock.

Lastly, it predicts that if the period of exposure is truly a critical one developmentally (i.e. the inter-temporal elasticity of substitution is very low and the weight accorded to the period of exposure is very high), then there should be no/very little recovery when income constraints are lifted. However, if the binding constraint was not biology or private incentives, but rather, income, then state intervention should result in recovery.

Throughout this study, I show that my results are consistent with many features of this model. By showing the conditions under which they align well with the predictions outlined above, I provide empirical evidence that both substantiates and clarifies this multi-stage, multi-capability approach to understanding the process of human capital formation. I also use this conceptual framework to outline plausible interpretations of the empirical findings in cases where a range of alternate scenarios are possible given the complexity of the model and the lack of data on key variables such as period- and individual-specific investment.

2.4 Conclusion

The Dust Bowl was one of the most catastrophic environmental events in American history. It decimated the agricultural heartland and, together with the Great Depression, left many Great Plains residents in desperate poverty. Meanwhile, the iconic dust storms that characterized the Dust Bowl also posed grave hazards to health. The disaster resulted in prolonged and at times acute deprivation—deprivation of the sort that has been shown to have long-term scarring effects on human populations, particularly when experienced in early life. Given these facts, it is reasonable to expect that the Dust Bowl may have undermined human capital formation in cohorts exposed to this environmental insult in childhood. In the coming chapters, I provide empirical evidence to this effect, and build on the theoretical framework presented here to explain how and why both scarring and recovery took place.

Chapter 3

Later-Life Effects of Early-Life Dust Bowl Exposure

Figure 3.1: Children Preparing to Leave for School, Lakin, Kansas, 1935 (Green Family Collection, 1935)

3.1 Background

The Dust Bowl posed an exogenous shock to health and income in the Great Plains of the 1930s. As such, this environmental event may have persistent effects on the wellbeing of surviving cohorts. In this chapter, I provide an overview of the baseline empirical design, and test for evidence of Dust Bowl-induced human capital scarring in individuals exposed to the disaster in early life. I find robust evidence that those exposed to the Dust Bowl *in utero* and as children experienced statistically significant and economically meaningful reductions in later-life human capital and wellbeing. Outcomes such as the probability of college completion, the probability of poverty, the magnitude of transfer payments, and the probability of disability are especially adversely affected.

3.2 Empirical Strategy

3.2.1 Methods

I exploit exogenous environmental shocks to income and health in the form of Dust Bowl-induced soil erosion to identify impacts on later-life human capital outcomes.[1] I use a differences-in-differences strategy to identify the treatment impact of Dust Bowl exposure, with a baseline regression as follows, estimated by OLS:

$$h_{ibst} = \alpha + \beta_1 \times treated_b \times erosion_s + {x_{ibst}}' \psi + \theta_s + \lambda_b + \eta_t + \gamma_s + u_{ibst} \qquad (3.1)$$

Here, h_{ibst} represents the later-life human capital outcome of interest for individual i born in year b in state s and observed in census year t.

The term $treated_b \times erosion_s$ is the chief variable of interest. It refers to whether

[1] I discuss the plausibility of the assumption that the Dust Bowl was exogenous below, in Section 3.2.2.

an individual was a child during the Dust Bowl and whether they were exposed to high erosion. It may thus be interpreted as the reduced form effect of childhood Dust Bowl exposure on adult outcomes.

$Treated_b$ is equal to 1 for individuals aged -1 to 12 at any time during the Dust Bowl (that is, children born 1918-1941, inclusive) so as to capture the effects of the shock during childhood, which has been defined so as to span the period *in utero* up to the onset of puberty.

$Erosion_s$ represents the erosion intensity in a given state over the Dust Bowl period–that is, the degree of exposure to this random environmental shock. Since county-level erosion is known but birth county is not, in the baseline, $erosion_s$ is constructed as the proportion of the state population in 1930 living in high-erosion counties within that state, and so may be interpreted as the probability an individual born in a given state was born into a high-erosion county.

Since census data do not allow for the identification of those who may have migrated away from the birth state during childhood, the $treated_b \times erosion_s$ term may be interpreted as an intent-to-treat estimate. Thus, its coefficient, β_1, describes the causal effect of Dust Bowl treatment relative to those not exposed. Whether a rise ($\beta_1 > 0$) or fall ($\beta_1 < 0$) in a given outcome is interpreted in practical terms as a beneficial versus as an adverse effect will of course depend on the desirability of the outcome in question. For instance, a reduction in the probability of college completion would be considered an adverse effect since a lower value for this outcome reflects damage to education, while reductions in welfare income or the probability of physical disability would represent beneficial impacts since lower values here reflect higher socioeconomic status and better health.

x_{ibst} represents a vector of controls (in the baseline, the individual's race; in later robustness checks, characteristics such as race, veteran status, state-level drought, and birth-state-birth-year per capita income).

Lastly, θ_s, λ_b , and η_t represents birth-state fixed effects, birth-year fixed effects, and census-year fixed effects, respectively, while γ_s represents linear birth-state trends.

Together, the set of individual controls and fixed effects allow me to control for individual characteristics, such as race, that may affect the degree to which Dust Bowl exposure manifests in later-life human capital, as well as to remove any time-invariant state heterogeneity (such as differences by state in quality of land), unobserved heterogeneity across time (such as differences by birth year in public service expenditure per capita), and unobserved heterogeneity across census waves (such as systematic differences in enumeration methods). The inclusion of state trends further allows me to strip away the effects of larger-scale trends in outcomes during this period, such as the baby boom or declining ages at marriage (see Figures 3.7 & 3.6, respectively), such that β_1 may be interpreted as the causal effect of Dust Bowl exposure above and beyond these phenomena. Note that the un-interacted $treated_b$ and erosions terms have been excluded because they are perfectly collinear with the birth-year (λ_b) and birth-state (θ_s) fixed effects, respectively.

Standard errors are clustered at the birth-state and birth-year levels so as to adjust for serial correlation in outcomes (Bertrand et al., 2004), and the equation is estimated separately for men and women in order to identify differences by gender in Dust Bowl impacts.

The preceding description has outlined the baseline specification; details of robustness checks and further tests are discussed in Section 3.5.

3.2.2 Dust Bowl Exogeneity

The exogeneity of the Dust Bowl as an environmental event, and of erosion as a proxy for this shock's severity, is crucial to identification. Luckily, as an environmental phenomenon, the Dust Bowl is on its face exogenous. In fact, a long line of studies, e.g. Almond et al. (2009), Durante (2010), and Burgess et al. (2014), have mined climatic patterns such as those in temperature, rainfall, and wind, for exogenous variation in treatment.

Nevertheless, the Dust Bowl shock could be endogenous if human activity exacerbated adverse weather conditions which were perhaps necessary but insufficient conditions for an environmental disaster of this scale. As discussed in Chapter 2, the contribution of Great Plains settlers to land degradation and dust storms has been long debated by scholars of the Dust Bowl, and the popular view has tended to blame people and policy for the catastrophe (Worster, 1979; Burns, 2012). However, the most recent, rigorous, and in-depth analysis by Cunfer (2005) suggests that the Dust Bowl was largely driven by exogenous and cyclical climatic conditions and was not, as previous scholars have proposed and as had been popularly assumed, a man-made disaster. Indeed, Cunfer shows that storms like those seen in the Dust Bowl had been a regular feature of the Great Plains ecology long before the 1930s, or even the large-scale settlement of the Plains. Thus, it is less likely that people created the conditions for the Dust Bowl, and more likely both that the Dust Bowl was an especially severe ecological event, and that by 1930, enough people lived in the region to for such an event to be noteworthy as a substantial human crisis.

The Dust Bowl might also be endogenous for the purposes of this analysis if individuals were selected into residence in the Great Plains. For instance, if homesteading led the poorest and most desperate farmers to settle marginal lands and incentivized them to cultivate sub-optimally, then we might expect that individuals born in more severely-stricken states were substantially more sensitive to Dust Bowl shocks, and/or that they were more prone to pursue farming strategies that depleted the land. However, there is no clear indication that erosion severity and pre-Dust Bowl incomes were strongly associated on the eve of the Great Depression: the correlation between Dust Bowl erosion and per capita personal income in 1929, both of which are given in Table 3.1, is a weak 0.2897. Furthermore, the differences-in-differences design accounts for the pre-Dust Bowl period. That is, it naturally compares individuals born in Dust Bowl states before and after the shock to those born in Dust Bowl states during the shock. Thus, if homesteading in the late 19th Century led to a selected group of Great Plains residents, then the inclusion in the (untreated) control group of individuals selected in the same manner as those who were treated, and using the same sorts of farming techniques as those treated, would help mitigate the concern over the endogeneity of Dust Bowl "victims."[2] Lastly, after controlling, as I do, for state fixed effects and trends, it is unlikely that selective settlement or cultivation patterns in the lead-up to the 1930s would have effects on later-life outcomes except through the Dust Bowl.

For further discussion of Dust Bowl exogeneity, see Section 4.4.3.3.

3.3 Data

3.3.1 Childhood Dust Bowl Severity

To identify the scope and intensity of Dust Bowl exposure, I use measures of soil erosion for 15 Great Plains and adjacent states[3] (see Figure 3.2), taken from the US Soil Conservation Service. Beginning in 1934 in response to the Dust Bowl, and continuing over its course, the Soil Conservation Service conducted comprehensive

[2]The only way these two related concerns regarding settler sensitivity and influence on erosion might remain would be if the effects of land degradation were cumulative and only reached the relevant threshold to, alongside adverse weather conditions, produce dust storms and erosion in the 1930s.

[3]The states in the sample account for 23% of US population (United States Census Bureau, 2002), and 47% of US landmass (United States Census Bureau, 2013) in 1930. The choice of these 15 states accords with qualitative and quantitative accounts such as the Soil Conservation Service (1935), the Great Plains Committee (cited in p. 669 of Hansen & Libecap (2004)), Worster (1979), Hansen & Libecap (2004), Nealand (2008), and Hornbeck (2012), that place the epicenter of the Dust Bowl in the Great Plains region and nearby states. Indeed, previous studies using erosion-based methodologies to study Dust Bowl phenomena, such as Hansen & Libecap (2004) and Hornbeck (2012), have used a similar sample of states. By including states on either side of the core Great Plains region, I ensure sufficient variation in erosion severity to allow for differences-in-differences analysis.

surveys of soil conditions across the United States, identifying thousands of regions of erosion classified by the severity (e.g. slight, moderate, severe) and type (e.g. sheet, wind, gullying) of damage observed (Soil Conservation Service, 1935).[4]

By 1948, the office had created the first of a series of maps cataloging the cumulative erosion experienced throughout the full Dust Bowl period (Hornbeck, 2012). I take my erosion figures from this cumulative erosion map, which characterizes erosion severity by percentage of original topsoil lost: high (75% or more), medium (25-50%), and low (0-25%) erosion (Hornbeck, 2012). To control for possible changes to county boundaries during the Dust Bowl period, per Hornbeck (2012), 1910 county boundary definitions are enforced for measures of erosion, population, and farm value (Hornbeck, 2012).[5]

Since the severity of the Dust Bowl in human terms depends on population distribution over space within a given state, I weight erosion measures by population,

[4]This map, published in 1935 (Soil Conservation Service, 1935) and based on a 1934 reconnaissance survey commissioned specifically to gather information on the Dust Bowl erosion problem, was the very first systematic and comprehensive attempt in American history to catalog erosion patterns. In the preliminary stages of this thesis, I digitized this map at its most granular (sub-county) level and used the resulting information to create new measures of erosion severity. Like the findings presented in this thesis, whose analysis is based on data from an alternate map which captures erosion occurring over the entirety of the 1930s, the results from this preliminary analysis (not reported) suggested long-term adverse consequences of early-life Dust Bowl exposure, although the effects were smaller in magnitude than those reported here.

Although, to my knowledge, this particular map has not previously been used by economists or economic historians, its novelty as a source was ultimately outweighed by two main drawbacks, which led me to use erosion severity measures constructed from a later map. First, the map on which erosion severity is based in this thesis is the same as used by one of the major recent studies on the Dust Bowl, Hornbeck (2012). This more recent map is not only different in the temporal coverage offered by the Soil Conservation Service (1935) map, but it also uses a different scale and set of erosion categories, making standardization challenging. As such, using the same map as in Hornbeck (2012) allows some measure of comparability with the existing literature. Second, and perhaps more important, the newer map used in this thesis covers the entire Dust Bowl period, whereas the Soil Conservation Service (1935) map covers only a short portion of the Dust Bowl—notably, a period before many of the most severe dust storms occurred. Accordingly, it is likely not only to systematically underestimate "final" erosion severity in all regions, but also to incorporate some degree of classical measurement error since the spatial distribution of dust storms and thus of erosion would also have varied over time. It is likely that such mis-measurement of erosion severity in the data drawn from the 1935 map would lead to attenuation bias of the sort that could explain the smaller but nevertheless intuitively-signed coefficients found in my preliminary analysis.

[5]For further discussion of the Soil Conservation Service and its erosion maps, see Hornbeck (2012).

taken from the US Census,[6] such that states with a greater proportion of population residing in high-erosion counties are deemed to have been more severely treated by the Dust Bowl. Erosion measures are constructed on the state level so as to correspond to individual outcomes, which can only be measured at the birth state level.[7]

Although the 1948 map captures the erosion wrought by many of the Dust Bowl's worst storms, such as April 1935's Black Sunday (Carlson, 1935), a notable weakness of the erosion data is the lack of baseline erosion measures, since no erosion surveys were made prior to 1934 (Soil Conservation Service, 1935; Hornbeck, 2012). In order to account for possible measurement error due to the absence of information on pre-1930 erosion levels, I substitute data from the US Census of Agriculture[8] on the percentage change in farm values between 1930 and 1940 for the erosions term in 3.1 as a robustness check, since 1930 farm values proxy the quality of land prior to the Dust Bowl. As a further robustness check, I use drought as the measure of Dust Bowl severity. For this analysis, I use state-level climate station data on rainfall during the period 1930-40, taken from the National Climatic Data Center (2013) and constructed, using the Standardized Precipitation Index method, as the total magnitude of all drought events in the state during the period (McKee et al., 1993; see below for details of variable construction).

The key variables measuring Dust Bowl severity and exposure are constructed as follows:

Erosion – The proportion of the state population in 1930 living in high-erosion counties in the individual's birth state (constructed using 1930 county population figures and county-level erosion classifications from the U.S. Census) (Hornbeck, 2012). This

[6]For underlying sources, see Hornbeck (2012).

[7]IPUMS data and censuses do not include more granular nativity, such as birth county or birth city.

[8]For underlying sources, see Hornbeck (2012).

variable may be interpreted as the probability that an individual in a given state experienced high erosion. Census data, through which only birth state, rather than birth county or birth city is known, do not allow me to pinpoint the actual Dust Bowl severity experienced by an individual in childhood. I attempt to overcome these limitations by constructing this variable such that it accounts for the distribution of population over space. By incorporating county-level information on human geography, I arrive at a measure of Dust Bowl severity more appropriate to a study of the human costs of the disaster than those not "weighted" by population, such as those weighted by county area.

Change in Farm Values – The proportion of the state population in 1930 living in high-farm value-loss counties in the individual's birth state (constructed using 1930 county population figures and 1930 and 1940 county-level farm values from the U.S. Census of Agriculture; Hornbeck, 2012). To construct this figure, I calculate for each county the percentage change in farm values over the Dust Bowl period, between 1930 and 1940. I then classify the tercile of counties in my sample that experienced the greatest drop in farm values over the period as experiencing "high" farm value loss. I then calculate the proportion of the 1930 population in each state living in high farm value loss counties. By accounting for farm values prior to the Dust Bowl, this variable overcomes the weaknesses in erosion data, for which pre-Dust Bowl baselines do not exist (Soil Conservation Service, 1935; Hornbeck, 2012). Furthermore, farm values may capture Dust Bowl effects—for instance, economic ones—that erosion alone does not.

Drought – The sum of drought magnitudes for all official drought events occurring between 1930 and 1940 in the individual's birth state. Drought figures are calculated using monthly climate station-level National Oceanic and Atmospheric Administration data on historical rainfall collected from 1910 to 2010 (National Climatic Data Center, 2013); the 100-year span of data used exceeds the minimum roughly 50-year sample recommended to establish state baselines (McKee et al., 1993; Wu et al., 2005). Since not all states had climate stations in every county in the early- to mid-20th Century, it is not possible to gather county-level drought data nor is it possible to weight drought by county-level population as in the erosion and farm value measures of Dust Bowl severity. Instead, I summed the precipitation levels of each climate station within a state in a given month and divided it by the number of climate stations in that state in that month to create monthly state-level precipitation averages. To normalize the rainfall data to allow comparability both across states and across time within a state, these raw precipitation figures were converted to a monthly Standardized Precipitation Index (SPI) figure using a 12-month timescale to capture both short- and longer-term drought effects. The SPI method is preferred by climatologists to other common drought indices, such as the Palmer Drought Index, because of its simplicity and its incorporation of climatological timescales (McKee et al., 1993; Guttman, 1999; Wu et al., 2005; World Meteorological Organization, 2009). Per McKee et al. (1993), the monthly state-level SPI figures were used to identify official drought events (McKee et al., 1993), and the magnitudes of these events (which capture both severity and duration of drought) were summed over the 1930 to 1940 period to create a single state-level variable representing the total severity of drought in a state during the Dust Bowl timeframe, relative to the state's own historic rainfall baseline.

Treated (Baseline) – 1 if the individual was aged -1 to 12 at any point during the Dust Bowl period of 1930 to 1940, and 0 if not. This age span represents childhood, here defined as the time *in utero* to the onset of puberty. This variable denotes whether an individual was a child during the Dust Bowl, and thus may plausibly have been exposed to Dust Bowl shocks such as dust storms.

Maps detailing the severity of Dust Bowl exposure by measures of erosion, change in farm values, and drought are given in Figures 3.3-3.5. Summary statistics listing erosion severity and relevant economic performance indicators by state are provided in Table 3.1.

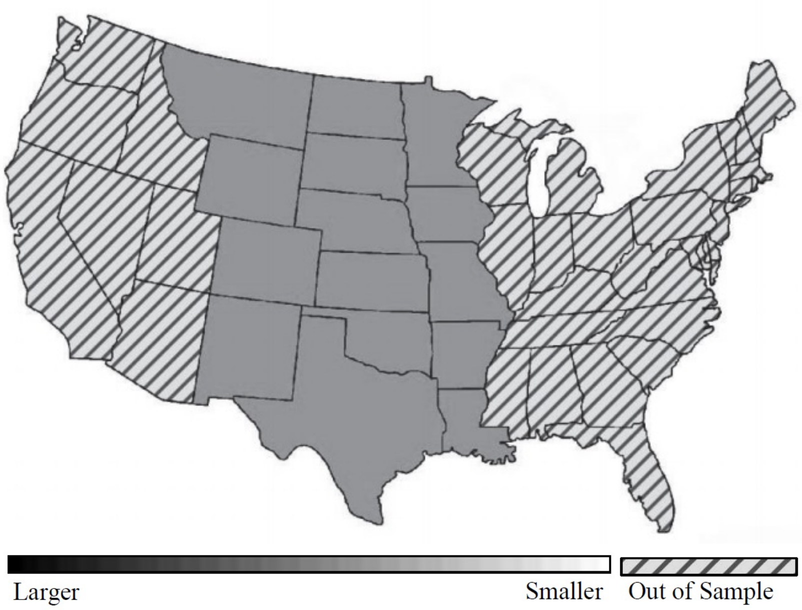

Figure 3.2: Region Under Study

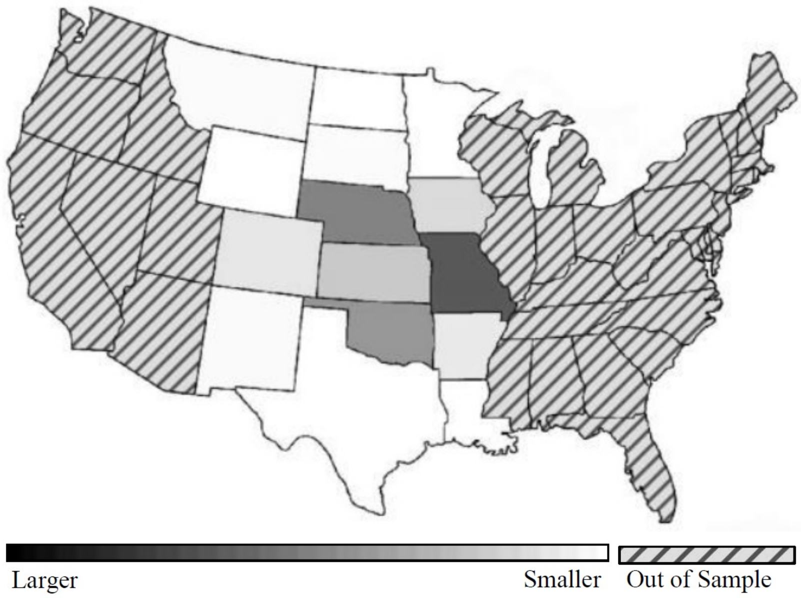

Figure 3.3: Dust Bowl Severity: Proportion of State Population Living in High Erosion Areas

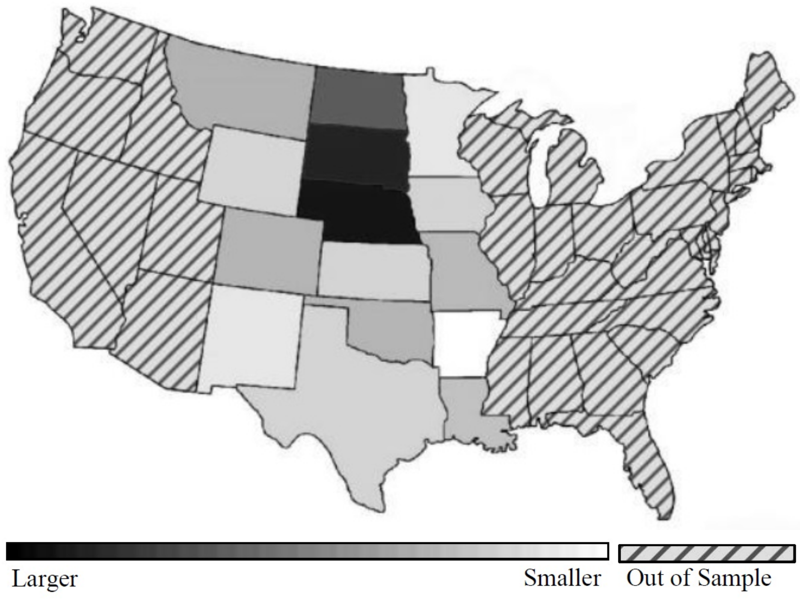

Figure 3.4: Dust Bowl Severity: Proportion of State Population Living in High Farm Value Loss Areas

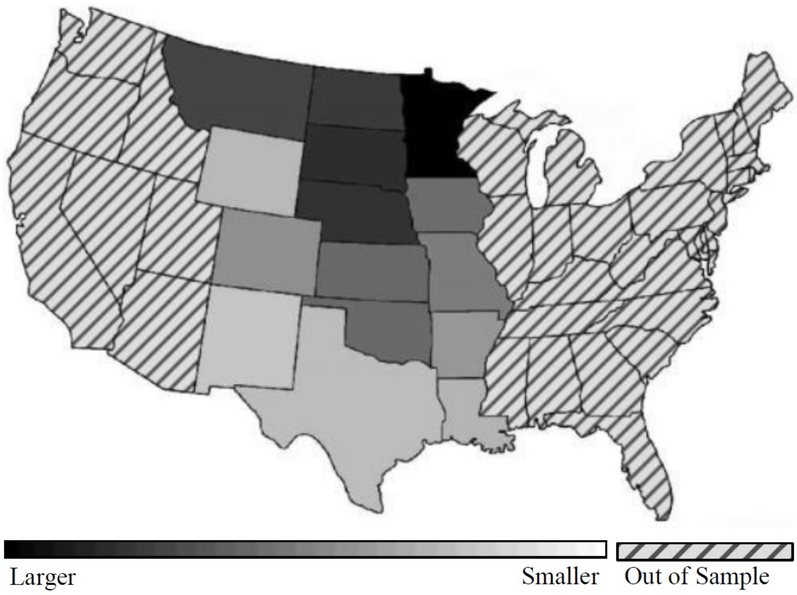

Figure 3.5: Dust Bowl Severity: Sum of Magnitudes of Drought Events Between 1930 and 1940

3.3.2 Adult Human Capital Outcomes

Individual-level data on later-life health, educational, and socioeconomic outcomes are taken from the Integrated Public Use Microdata Series (IPUMS), a dataset drawn from decennial US Census responses (Ruggles et al., 2010). I use 5%-sample data collected in the 1980, 1990, and 2000 US Censuses to capture stable adult outcomes of those who were children between 1930 and 1940–that is, individuals born between 1918 and 1941, inclusive.[9] The sample is restricted to individuals born between the years 1900 and 1959, inclusive (of whom individuals born between 1918 and 1941, inclusive, are deemed treated by the Dust Bowl, as outlined in Section 3.3.1) in the 15 Great Plains and adjacent states shown in Figure 3.2: Arkansas, Colorado, Iowa, Kansas, Louisiana, Minnesota, Missouri, Montana, Nebraska, New Mexico, North Dakota, Oklahoma, South Dakota, Texas, and Wyoming.

The IPUMS data contain individual-level information on demographics, income,

[9]Those who were children (aged -1-12) during the Dust Bowl would be roughly 39-62 in 1980.

Table 3.1: Summary Statistics: Dust Bowl Severity by State

State	Erosion (1)	Δ Farm Values (2)	Drought (3)	PCY, 1929 (4)	PCY, 1933 (5)	PCY, 1940 (6)
Arkansas	0.086	0.004	68.41	305	153	255
Colorado	0.110	0.297	73.14	630	351	543
Iowa	0.149	0.249	99.68	572	251	493
Kansas	0.215	0.187	101.59	528	249	422
Louisiana	0.000	0.249	50.79	410	226	363
Minnesota	0.000	0.104	166.65	594	308	519
Missouri	0.673	0.286	87.63	618	333	517
Montana	0.026	0.317	124.33	590	296	563
Nebraska	0.495	0.918	134.49	586	272	437
New Mexico	0.037	0.116	40.35	404	208	374
North Dakota	0.017	0.656	129.58	377	148	349
Oklahoma	0.407	0.297	102.53	452	220	371
South Dakota	0.005	0.887	136.47	416	133	355
Texas	0.003	0.189	44.42	476	255	436
Wyoming	0.000	0.169	47.80	669	368	595

Note: Erosion in Column 1 refers to the proportion of individuals in the state living in high-erosion counties in 1930. Change in Farm Values in Column 2 refers to the proportion of individuals in the state living in counties in 1930 which experienced high loss in farm values between 1930 and 1940. Drought in Column 3 refers to the total magnitude of all state-level drought events from 1930-1940. Columns 4-6 refer to the per capita personal income in 1929, 1933, and 1940, respectively, each in that year's dollars.

educational attainment, and disability (Ruggles et al., 2010); my analysis tests for the effects of Dust Bowl exposure on outcomes such as age at first marriage, the number of children ever born, and the probability of cognitive disability. Furthermore, via disaggregation by developmental stage cohort (as will follow in Section 4.2), these data allow me to test for both direct (e.g. respiratory illness, absence from school) as well as indirect (e.g. poor nutrition due to low incomes, developmental complications due to poor maternal health) effects of Dust Bowl exposure in childhood.

Individual-level outcome variables are constructed as follows:

Age at First Marriage – The individual's age at first marriage ("AGEMARR" in

IPUMS). Regressions on age at marriage are restricted to only those individuals who have been married at least once, and for whom age at marriage is known.

Children Ever Born – The total number of children born to the individual, excluding stillbirths, adopted children, and stepchildren (reported only for female respondents in the census; "CHBORN" in IPUMS). IPUMS "Not applicable" values have been excluded, and regressions on children ever born are restricted to women where the number of children born is known.

Probability of Completing High School – 1 if the individual completed high school, and 0 if not (Constructed based on "HIGRADE" in IPUMS, such that individuals with 13 or more years of schooling [HIGRADE $\geq$ 15] are deemed to have completed high school).

Probability of Completing College – 1 if the individual completed college, and 0 if not (Constructed based on "HIGRADE" in IPUMS, such that individuals with 17 or more years of schooling [HIGRADE $\geq$ 19] are deemed to have completed college).

Welfare Income – The contemporary dollar amount of pre-tax income, if any, the individual received through public assistance programs including federal/state Supplemental Security Income, Aid to Families with Dependent Children, and General Assistance, but excluding private charitable sources of income ("INCWELFR" in IPUMS).

Probability of Poverty – 1 if the individual is living at or below the official federal poverty threshold designated for their household size and structure, and 0 if not (Constructed based on "POVERTY" in IPUMS, such that individuals for whom

POVERTY $\leq$ 100 are deemed to be living in poverty).

Probability of Cognitive Disability – 1 if the individual has cognitive difficulties such as those involved in learning, decision-making, concentrating, or remembering; and 0 if not (Constructed based on "DIFFREM" in IPUMS such that individuals experiencing any sort of cognitive impairment are deemed to suffer from cognitive disability).

Probability of Physical Disability – 1 if the individual suffers from conditions that significantly limits one or more essential day-to-day physical activities, such as walking, lifting, climbing, etc.; and 0 if not (Constructed based on "DIFFPHYS" in IPUMS such that individuals experiencing any sort of ambulatory difficulty are deemed to suffer from physical disability).

Probability of Vision & Hearing Difficulty – 1 if the individual has long-lasting blindness, deafness, or other severe vision or hearing difficulty; and 0 if not (Constructed based on "DIFFSENS" in IPUMS such that individuals experiencing any sort of vision or hearing impairment are deemed to suffer from cognitive disability).

Probability of Self-Care & Independent Mobility Difficulty – 1 if the individual has lasting (i.e. non-temporary) conditions that cause difficulty in performing basic personal activities either within or outside the home (e.g. bathing, dressing, shopping, visiting the doctor); and 0 if not (Constructed based on "DIFFCARE" and "DIFFMOB" in IPUMS such that individuals experiencing any sort of impairment in either variable are deemed to suffer from self-care and independent mobility difficulty).

Summary statistics on adult outcomes by gender are given in Table 3.2. Graphs of outcome summary statistics by gender and year, with 95% confidence intervals, are provided in Figures 3.6-3.15. In these graphs, the thick line represents the average value of the outcome by birth-year cohort, while the thin lines represent the upper and lower bounds of the associated 95% confidence interval. Values for men are given in blue, while those for women are given in pink.

3.3.3 Additional Baseline Variables

Although data on Dust Bowl exposure in childhood and human capital outcomes in adulthood are the focus of this analysis, the baseline empirical strategy outlined in Section 3.2 uses several additional variables, the constructions of which are outlined below:

Sex – 1 if the individual is female, 0 if male.

Race – 1 if the individual is non-white, 0 if white. (Constructed based on "RACE" in IPUMS such that all individuals not identifying exclusively as "white" are deemed non-white).

Birth State – The individual's state of birth (identified using "BPL" in IPUMS). Note that in censuses from 1940 onwards, no detail finer than birth state (such as birth county) is available. In this study, only those born in Arkansas, Colorado, Iowa, Kansas, Louisiana, Minnesota, Missouri, Montana, Nebraska, New Mexico, North Dakota, Oklahoma, South Dakota, Texas, and Wyoming are included in the sample. These states are selected for being states in the Great Plains region or states neighboring the Great Plains. Studies using Dust Bowl erosion to explain agricultural and environmental phenomena have used a similar if slightly smaller set of states (Hansen & Libecap, 2004; Hornbeck, 2012); my sample adds states such as Missouri and Arkansas that both my own archival research on Dust Bowl-era residents of mi-

Table 3.2: Summary Statistics: Adult Human Capital Outcomes

Outcomes		**All** Mean	**All** N	**Men** Mean	**Men** N	**Women** Mean	**Women** N
Age at First Marriage	Unweighted	22.09	1,379,007	23.47	638,996	20.90	740,011
	Weighted	22.09	27,580,180	23.47	12,779,960	20.90	14,800,220
Children Ever Born	Unweighted	2.40	1,601,986			2.40	1,601,986
	Weighted	2.37	31,377,111			2.37	31,377,111
Probability of Completing High School	Unweighted	0.70	1,590,689	0.70	760,484	0.70	830,205
	Weighted	0.70	31,813,820	0.70	15,209,720	0.70	16,604,100
Probability of Completing College	Unweighted	0.15	1,590,689	0.18	760,484	0.12	830,205
	Weighted	0.15	31,813,820	0.18	15,209,720	0.12	16,604,100
Welfare Income	Unweighted	101.08	4.256,983	73.56	1,997,206	125.41	2,259,777
	Weighted	101.00	82,669,561	72.70	38,846,949	126.09	43,822,612
Probability of Poverty	Unweighted	0.12	4,256,983	0.10	1,997,206	0.14	2,259,777
	Weighted	0.12	82,669,561	0.10	38,846,949	0.14	43,822,612
Probability of Cognitive Disability	Unweighted	0.08	4,256,983	0.07	568,197	0.09	657,791
	Weighted	0.08	23,175,971	0.07	10,730,470	0.08	12,445,501
Probability of Physical Disability	Unweighted	0.19	1,225,988	0.17	568,197	0.21	657,791
	Weighted	0.19	23,175,971	0.17	10,730,470	0.20	12,445,501
Probability of Vision & Hearing Difficulty	Unweighted	0.08	1,225,988	0.09	568,197	0.08	657,789
	Weighted	0.08	23,175,971	0.09	10,730,470	0.07	12,445,501
Probability of Self-Care & Independent Mobility Difficulty	Unweighted	0.12	2,666,294	0.10	1,236,722	0.14	1,429,572
	Weighted	0.12	50,855,741	0.10	23,637,229	0.14	27,218,512

Note: Table reports average values of human capital outcome variables by IPUMS observation (unweighted) and by IPUMS observation weighted by the IPUMS variable PERWT (weighted); the latter yields the average value of the human capital outcome variables for the entire population represented by the observations included.

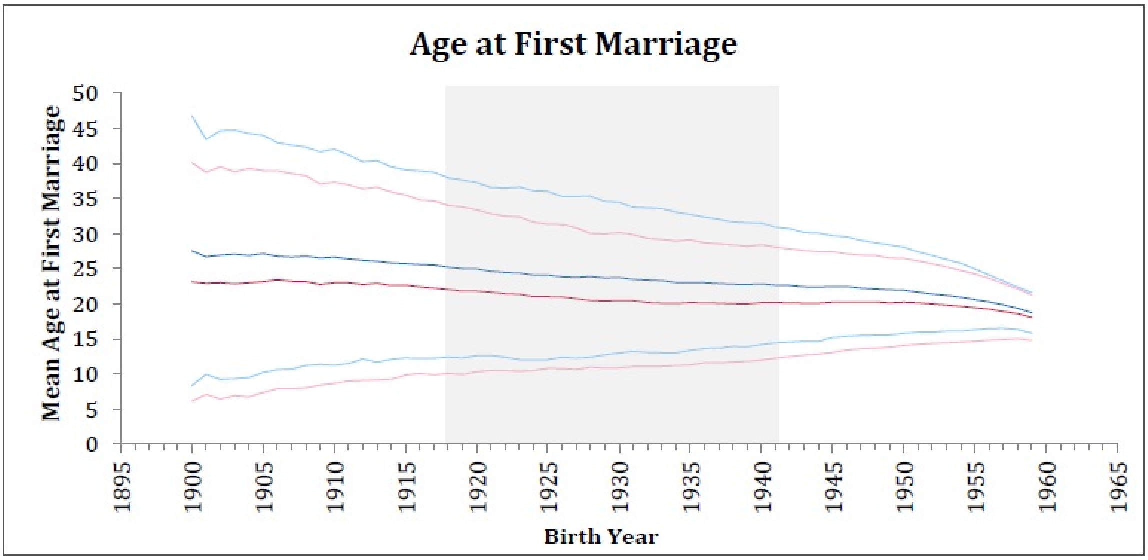

Figure 3.6: Average Later-Life Outcomes by Birth Year and Sex: Age at First Marriage

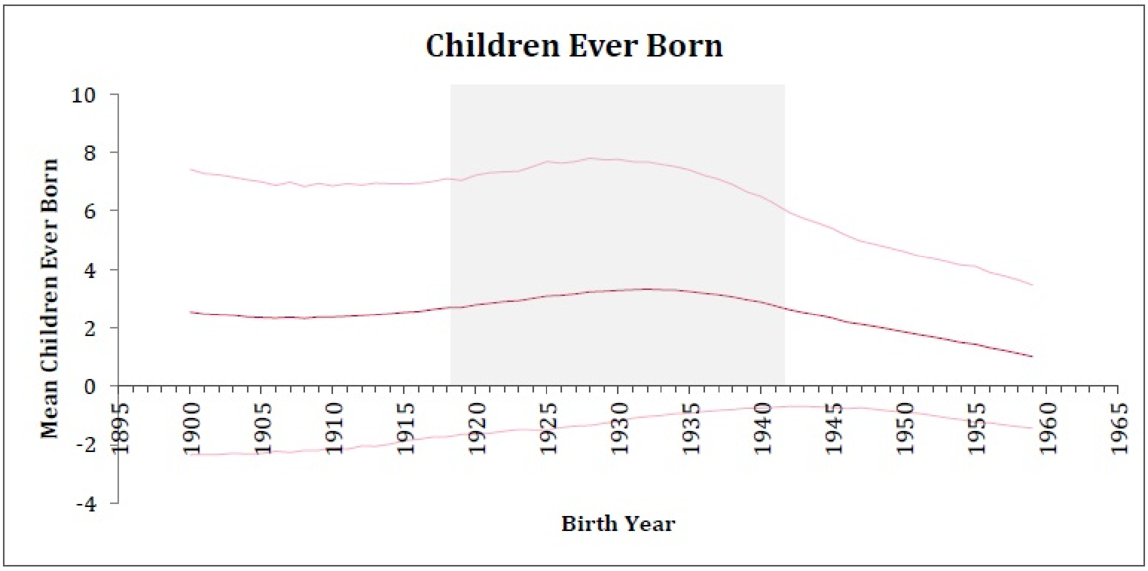

Figure 3.7: Average Later-Life Outcomes by Birth Year and Sex: Children Ever Born

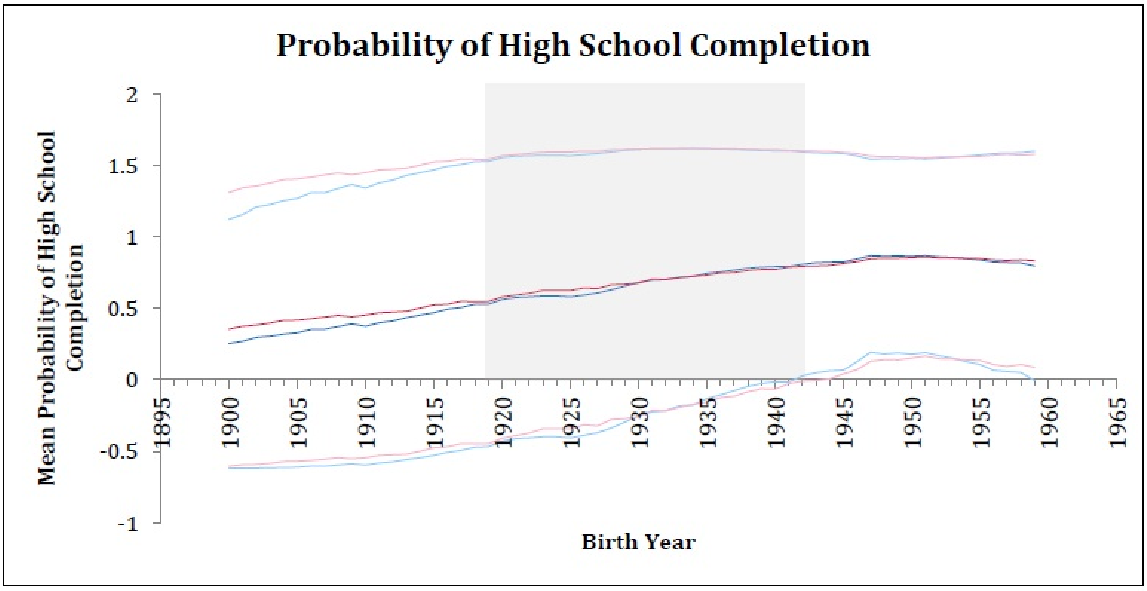

Figure 3.8: Average Later-Life Outcomes by Birth Year and Sex: Probability of High School Completion

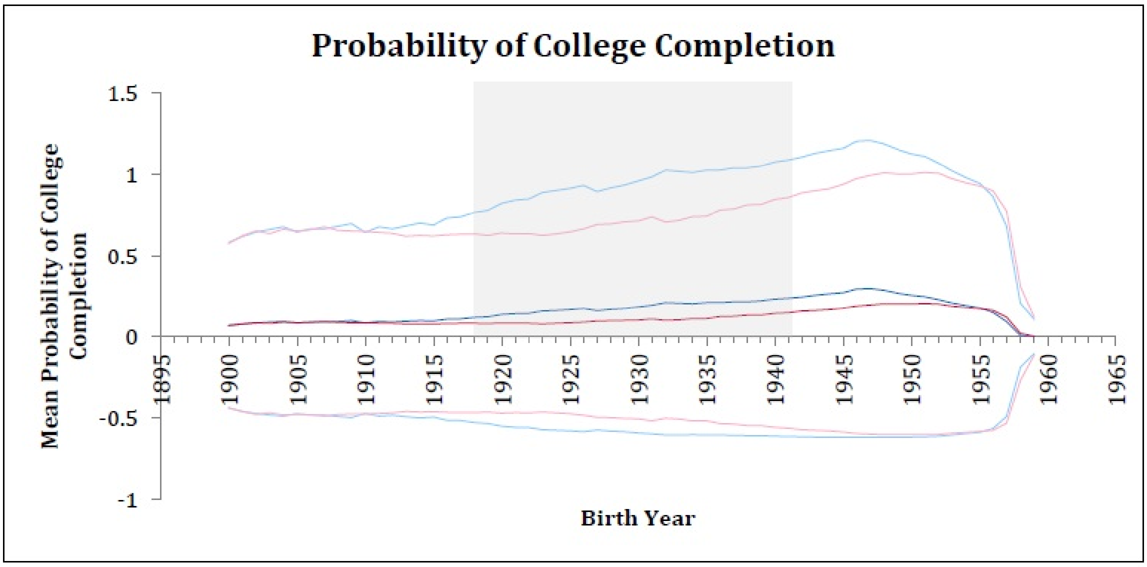

Figure 3.9: Average Later-Life Outcomes by Birth Year and Sex: Probability of College Completion

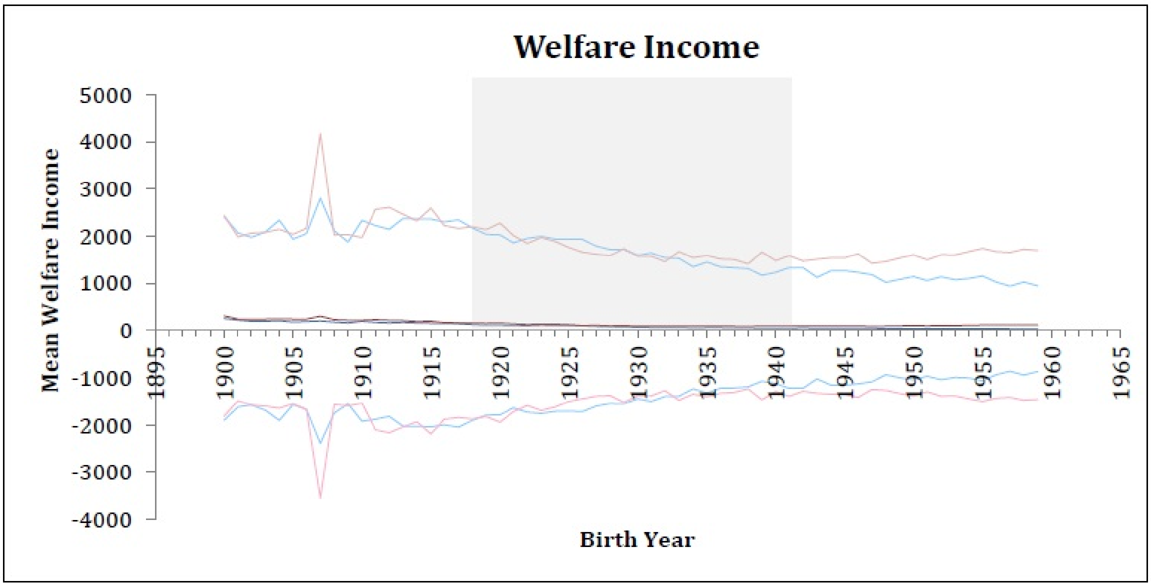

Figure 3.10: Average Later-Life Outcomes by Birth Year and Sex: Welfare Income

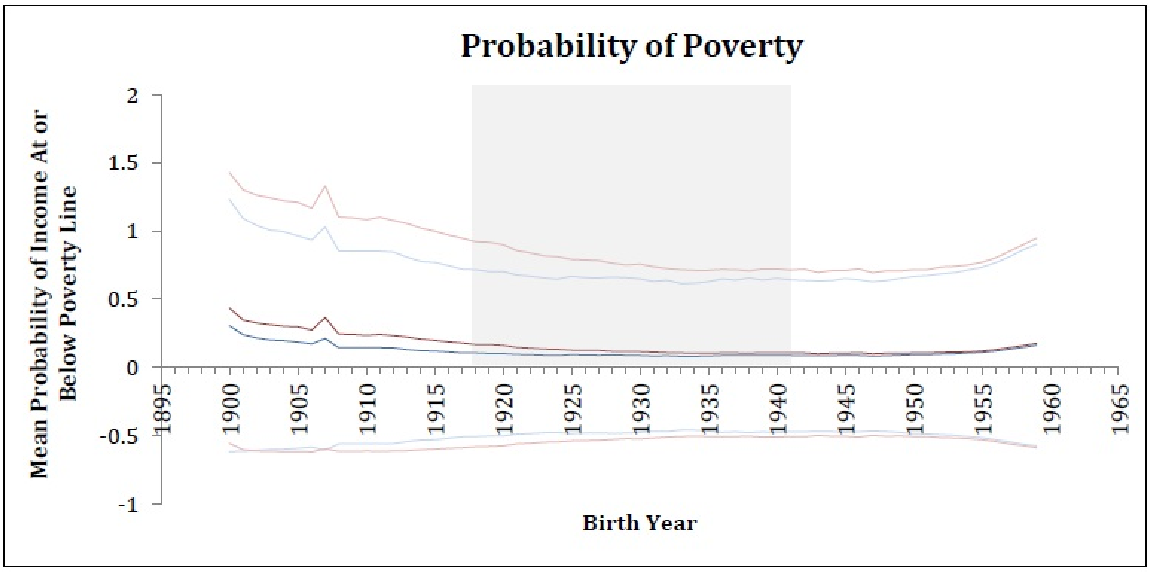

Figure 3.11: Average Later-Life Outcomes by Birth Year and Sex: Probability of Poverty

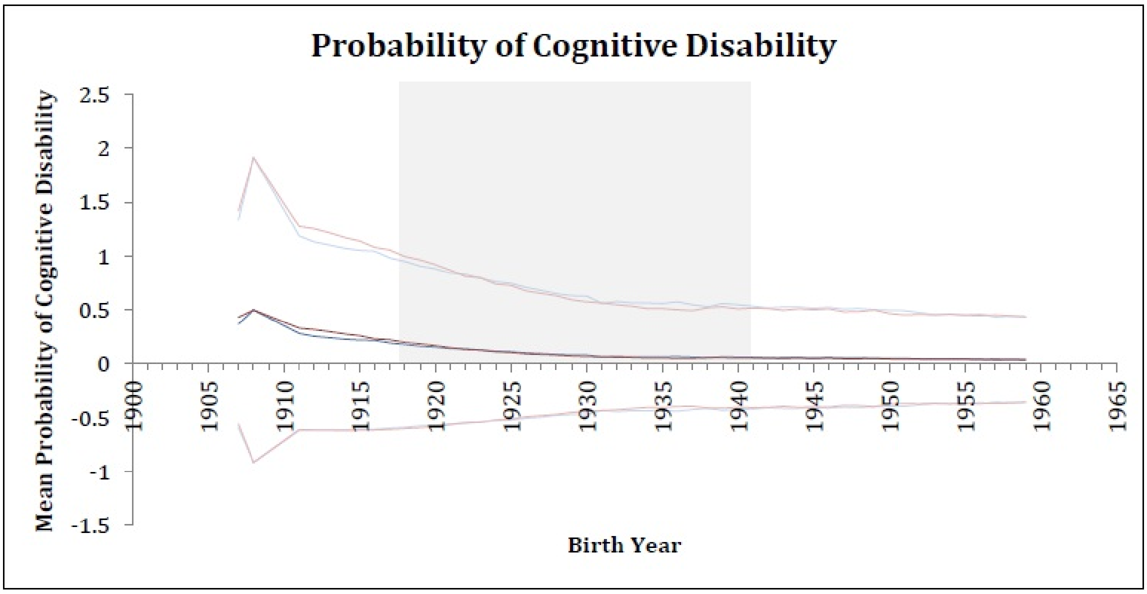

Figure 3.12: Average Later-Life Outcomes by Birth Year and Sex: Probability of Cognitive Disability

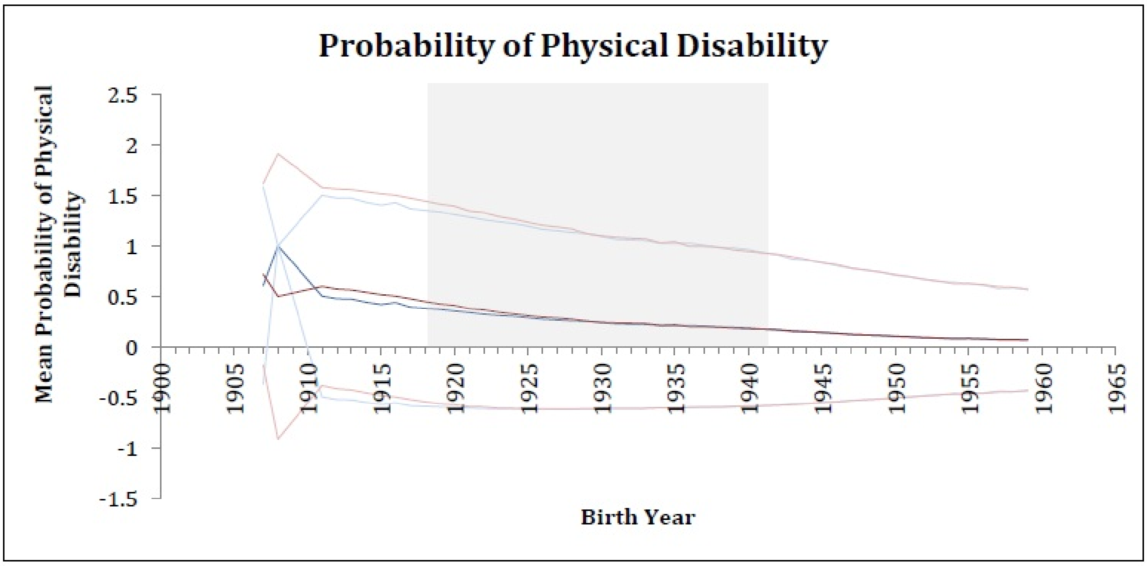

Figure 3.13: Average Later-Life Outcomes by Birth Year and Sex: Probability of Physical Disability

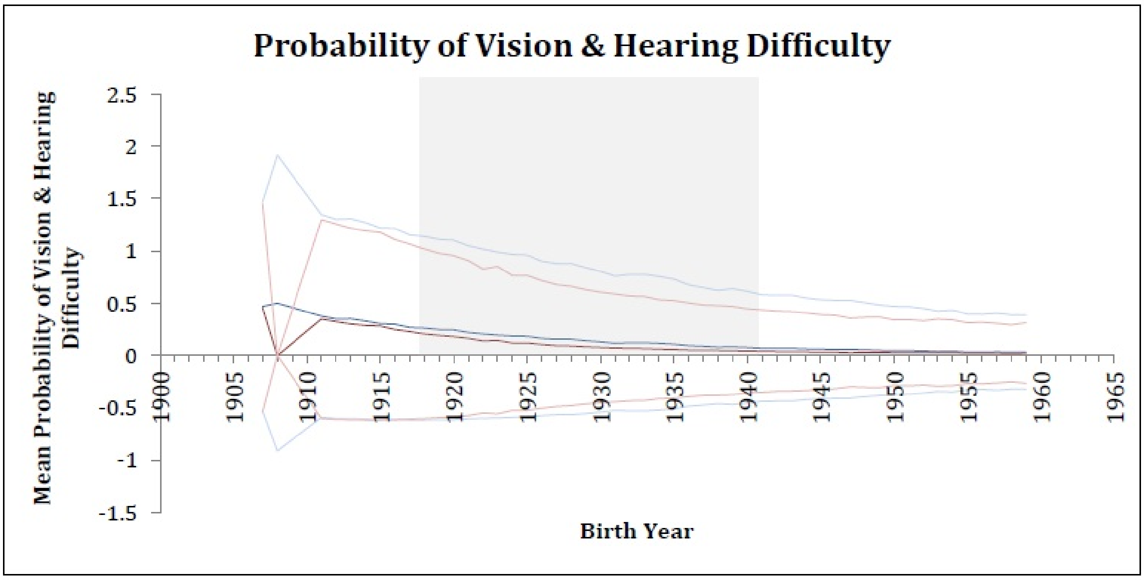

Figure 3.14: Average Later-Life Outcomes by Birth Year and Sex: Probability of Vision & Hearing Difficulty

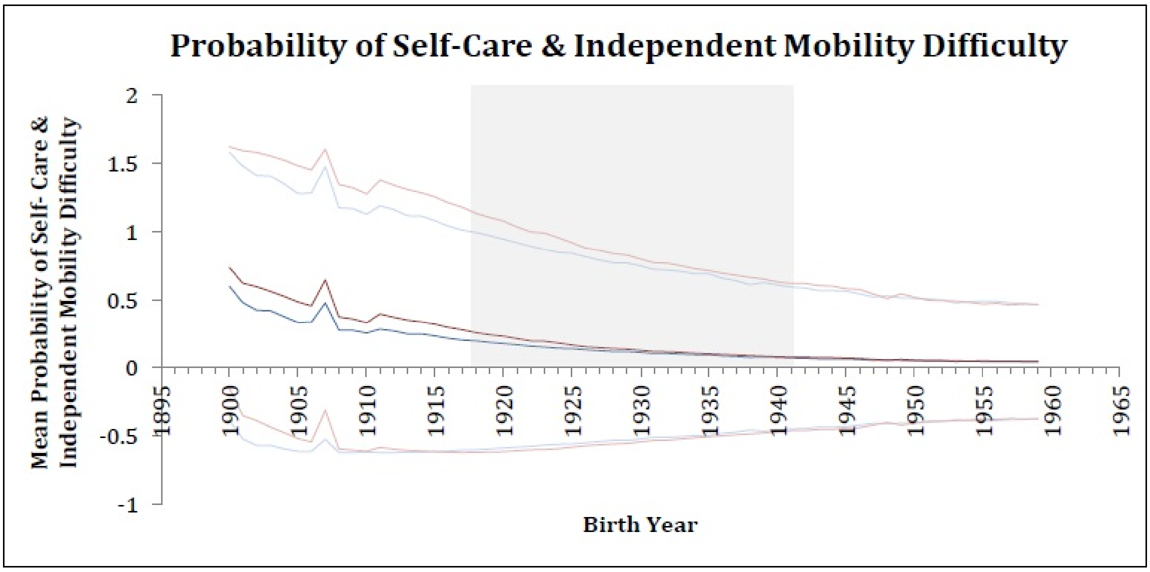

Figure 3.15: Average Later-Life Outcomes by Birth Year and Sex: Probability of Self-Care & Independent Mobility Difficulty

grant farm labor camps in Northern California (discussed in further detail in Section 5.2.3) and the qualitative literature suggest may have also suffered from the Dust Bowl (Nealand, 2008). The use of states beyond those strictly in the Great Plains or those hardest hit by the Dust Bowl allows me to exploit a greater degree of variation in shock severity. Such variation is necessary for differences-in-differences analysis.

Birth Year – The individual's year of birth (identified using "BIRTHYR" in IPUMS). In this paper, only those born in the years 1900 to 1959, inclusive, are included in the sample. Since individuals born between 1918 and 1941, inclusive, are the cohort of interest, the inclusion of individuals born before and after this period allows for differences-in-differences estimation against those who could not possibly have been exposed to the Dust Bowl.

Census Year – The census year in which the individual's data are reported (identified using "YEAR" in IPUMS). In this paper, the 5% sample of the US census for 1980, 1990, and 2000 are used. These census waves are chosen in part for the specific variables they report which other censuses do not (such as the highest grade achieved, or a standardized veteran status), as well as to enable capture adult outcomes that are relatively stable (such as those for schooling and fertility, which an individual may not yet have completed if using earlier censuses) (Bhalotra & Venkataramani, 2012).

State Trends – A linear time trend for each birth state in the sample (constructed using "BPL" and "BIRTHYR" in IPUMS).

3.4 Results

I begin by testing the impact of Dust Bowl exposure on later-life outcomes by sex. Table 3.3 lists the values for β_1 from Equation 3.1 for each of the later-life outcomes considered. I find that individuals exposed to the Dust Bowl as children suffered permanent damage to human capital and wellbeing on a scale that is both statistically and economically significant, with magnitudes similar to those found in analogous studies of early-life interventions such as Bhalotra & Venkataramani (2012) and those reviewed in Almond & Currie (2011a).

Under the baseline specification, given in column (1) for men and column (2) for women, I find that exposed women have a 1.17 percentage point lower probability of completing college, and a 0.47 percentage point higher probability of living in poverty, defined as living at or below the census poverty line (see Section 3.3.2 for further detail). The poverty effects for men are even starker: exposed men have a 0.80 percentage point greater likelihood of living in poverty than men who were not exposed, and receive $15.05 more in welfare payments. Furthermore, exposed men have a 1.10 percentage point greater chance of experiencing physical disability.

In column (3) for men and column (4) for women, the percentage change in farm values between 1930 and 1940 takes the place of erosion as the measure of Dust Bowl severity. The percentage change in farm value is a useful exposure proxy since it does not penalize states that may naturally experience poor land quality or higher levels of pre-1930 erosion, traits that the baseline model would attribute to the Dust Bowl. Here I find similar effects on poverty and welfare payments. However, a number of additional impacts are also statistically significant, most notably, disability: exposed men see a 1.12 percentage point greater chance of experiencing self-care and independent mobility difficulty, while exposed women experience 1.45, 0.94, and 1.29 percentage point increases in cognitive, vision and hearing, and self-care and independent mobility disabilities, respectively. Exposed women also see a 3.71 percentage point greater chance of high school completion than their unexposed counterparts. This seemingly counterintuitive finding (which also appears in the results in Tables 4.1-5.7 concerning agriculture, developmental stage, and public spending) may in fact reflect a drop in the opportunity cost of secondary schooling as farms, and thus, the demand for child farm labor in regions where it was not strictly essential, collapsed.

I discuss this result in greater detail in Section 4.4.

In column (5) for men and column (6) for women, when the sum of the magnitudes of all drought events during the period 1930 to 1940 takes the place of erosion in the baseline equation, the signs are as in columns (1) through (4), although the magnitudes are slightly larger (for a shift from the least to the most drought-affected state in the sample), and the effects on a greater number of outcomes are statistically significant.

The results are intuitive, and are consistent with the literature on childhood shocks (Almond, 2006; Almond et al., 2007, 2009; Banerjee et al., 2010; Ó Gráda, 2011): the Dust Bowl had negative consequences for health, human capital, and indicators of wellbeing, increasing poverty and disability, and decreasing ages at marriage and college completion. In upcoming chapters I test hypotheses as to the mechanisms by which the Dust Bowl produced the effects discussed here; indeed, I show that many of the Dust Bowl's adverse effects–for instance, lower rates of postsecondary education–stem from low incomes and developmental complications in early life.

Lastly, the magnitudes of impact I find are in line with many studies on early-life shocks and interventions. For instance, Almond (2006) finds a roughly 1-2 percentage point higher chance of work disability among men exposed to the 1918 flu pandemic, a figure similar to the increases in disability I find. Disability effects of such magnitude also accord with the statistically insignificant estimates in Cutler et al. (2007). My estimates of the reduction in the probability of college completion (roughly 1-3 percentage points) and the increase in disability rates (roughly 1-2 percentage points) are also similar to the magnitudes of effects found in Bhalotra & Venkataramani (2012), which tracks the impact of a pneumonia vaccine introduction.[10] These studies suggest that the magnitudes of human capital effects found here are feasible—neither so large

[10]Since Bhalotra & Venkataramani (2012) studies a beneficial intervention, the signs are generally the opposite of those I find. Surprisingly, I also find positive effects on high school completion similar both in sign and magnitude to those in Bhalotra & Venkataramani (2012).

as to be implausible, nor so small as to be economically unimportant. For further discussion of effect magnitudes, see Bhalotra & Venkataramani (2012, p. 32), and Almond & Currie (2011a), the latter of which provides a survey of conceptually and methodologically similar studies.

Although I have provided above some context for understanding the size of the effects of the Dust Bowl on human capital, it should be noted that it is difficult to draw meaningful comparisons between the magnitudes of human capital effects found in this and in other studies. In large part, this is because of the lack of comparability between diverse and often unique or idiosyncratic human capital interventions: for instance, what does it actually mean to say that a disease epidemic and a drought have similar effects, or that a school-building program and a new vaccine invention do? Even where events may be more intuitively comparable—say, in the case of this thesis, where the Dust Bowl may more reasonably parallel a famine—the ability to place findings in useful context also relies on the comparability of outcomes.

For instance, studies on the Dutch Hunger Winter, an acute famine in 1944 Netherlands, are conceptually similar to this one in that they track the effects of prenatal famine exposure in an industrialized Western society on health outcomes in middle and old age. However, the outcomes they test for are too distinct to meaningfully compare. These studies are concerned with outcomes such as adult obesity and heart disease (e.g. Ravelli et al. 1999; Roseboom et al. 2000; Stein et al. 2007). My data, however, do not allow me to examine clinical outcomes such as these, nor do theirs allow them to engage with educational attainment or poverty in the way that this thesis does. Other studies on famine are even less similar in research approach and historical and geographic context, as in the case of Almond et al. (2007), which examines the Great Famine in China. Their focus is on mortality and changes in the cohort sex composition. Although outcomes here may be in the same category (e.g. education), they nevertheless lack comparability (e.g. literacy vs schooling completion).

Thus, it is perhaps most relevant to determine whether the effects found in this thesis are large enough to be "intrinsically" important. Indeed, the magnitudes of the effects found here would certainly be economically meaningful to individuals, particularly when taking into account the average levels of human capital presented in Table 3.2. For instance, a 1+ percentage point reduction in the probability of college completion is large in a cohort whose average college completion rate is roughly 15%, and in an era where college graduates could expect a lifetime earnings premium of roughly 43%[11] over a high school graduate (Vandenbroucke, 2015). Furthermore, although I do not attempt to estimate them here, it might be expected that the general equilibrium effects of (for example) a 7% reduction in the availability of college graduates would be deemed meaningful to the larger economy.

3.5 Robustness

In this section I perform a series of checks to test the robustness of the findings presented thus far. I find that the results on the later-life impact of early-life exposure to the Dust Bowl endure in the face of conservative corrections for multiple comparisons, the inclusion of a variety of controls, and alternative estimation approaches.

The results discussed above refer to coefficients that are statistically significant at the standard levels. However, many results, particularly those for college completion and disability, also survive the much stricter standards imposed by a Bonferroni correction for multiple comparisons. Table 3.3 in this chapter, as well as the key tables in upcoming chapters (Tables 4.1, 4.2, 4.3, 4.4, 5.6, and 5.7) use daggers to denote the strength of a coefficient's Bonferroni-corrected statistical significance.[12]

[11]The 43% calculation in Vandenbroucke (2015) is based on the expected lifetime earnings of white men in the U.S. born in 1930.

[12]The Bonferroni correction, used to manage familywise error rates, is one of the simplest and most popular adjustments for the multiple comparisons issue, but it is also one of the most conservative. It aims to reduce the incidence of false positive results when running large numbers of similar tests, since as the set of tests gets larger, so too do the chances of obtaining significant results merely by

Table 3.3: Impact of Childhood Exposure to the Dust Bowl on Later-Life Outcomes

	Baseline		**Δ Farm Values**		**Drought**	
	(1)	(2)	(3)	(4)	(5)	(6)
Outcomes	Men	Women	Men	Women	Men	Women
Age at First Marriage	-0.1320	-0.0034	-0.2490*	-0.1520	-0.00347***,†††	-0.00278***,†††
	(0.1390)	(0.1230)	(0.1280)	(0.1210)	(0.00036)	(0.00040)
Children Ever Born		-0.2060		0.0659		0.00147*
		(0.1320)		(0.1150)		(0.00081)
Probability of Completing High School	0.0169	0.0307	0.0236	0.0371**	0.00021**	0.00043***,†††
	(0.0152)	(0.0192)	(0.0165)	(0.0176)	(0.00009)	(0.00009)
Probability of Completing College	-0.0107	-0.0117**	-0.0000	-0.0068	0.00005	-0.00007
	(0.0075)	(0.0054)	(0.0091)	(0.0055)	(0.00008)	(0.00005)
Welfare Income	15.05*	16.54	22.83***,†††	15.37**	0.21500***,†††	0.05660
	(9.1470)	(10.2000)	(6.8840)	(6.4140)	(0.04650)	(0.12100)
Probability of Poverty	0.0080*	0.0047*	0.0111*	0.0036	0.00008***,†	0.00004
	(0.0042)	(0.0028)	(0.0058)	(0.0065)	(0.00003)	(0.00003)
Probability of Cognitive Disability	0.0065	-0.0054	0.0030	0.0145**	0.00010***,†	0.00012***,†††
	(0.0069)	(0.0062)	(0.0102)	(0.0066)	(0.00004)	(0.00003)
Probability of Physical Disability	0.0110***,†	-0.0021	0.0069	-0.0144	-0.00001	-0.00022***,††
	(0.0041)	(0.0110)	(0.0102)	(0.0091)	(0.00005)	(0.00008)
Probability of Vision & Hearing Difficulty	-0.0027	-0.0023	0.0080	0.0094**	0.00010*	-0.00002
	(0.0066)	(0.0050)	(0.0067)	(0.0037)	(0.00005)	(0.00003)
Probability of Self-Care & Independent Mobility Difficulty	-0.0003	-0.0015	0.0112*	0.0129***,†††	0.00005***,††	-0.00002
	(0.0039)	(0.0029)	(0.0048)	(0.0043)	(0.00002)	(0.00004)

*** p<0.01, ** p<0.05, * p<0.1 uncorrected; † † † p<0.01, †† p<0.05, † p<0.1 Bonferroni-corrected; Note: Table reports $treated_b \times erosion_s$ coefficients. Each row in the left-hand column refers to the regression's dependent variable, while the remaining column headings indicate the definition of Dust Bowl severity (i.e. $erosion_s$) used. All regressions are estimated by OLS and include controls for race; birth year, birth state, and census year fixed effects; and state trends. Standard errors, clustered by birth state and birth year, are reported in parentheses below each coefficient. To aid in interpreting the drought interaction coefficient, the minimum, maximum, and standard deviation for drought variable in the full sample are as follows: 40.350, 166.650, 38.837.

The results are robust to the definition of Dust Bowl severity–that is, the alternate thresholds of the erosions term in Equation 3.1 (not reported). These results also survive alternate definitions of the timing of Dust Bowl exposure ($treated_b$ in Equation 3.1), such as those discussed in Section 4.2, that narrow the treatment period from childhood as a whole to specific developmental age ranges spent during the Dust Bowl period.

Specifications including controls for age (to account for life cycle stage effects) and veteran status (to account for possible G.I. Bill benefit effects on postsecondary education and later-life income/wealth Altschuler & Blumin, 2009) produce similar

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chance (Abdi, 2007). (For instance, in running a set, or "family," of 10 similar regressions evaluated at the 0.05 significance level, there would be a $1 - (1 - 0.05)^{10} = 0.41$ or 41% chance of observing at least one significant result by chance, even if none of the 10 tests are actually significant.) The correction is simple to implement, and sets the significance cutoff at α/n; thus, with a family of 10 tests evaluated at a 0.05 level of significance, the null hypothesis would be rejected for p-values less than $0.05/10 = 0.005$.

However, this technique and others like it are not without some concerns worth noting. For instance, although it may reduce false positive results, this approach also reduces power and increases the chances of false negatives (Rothman, 1990), which in this context would have the practical consequence of identifying only the most catastrophic adverse effects of the Dust Bowl on human capital, while perhaps failing to discover (less stark but nevertheless important) damage to health and wellbeing that might otherwise be detected and remedied.

The approach also suffers from a lack of scholarly consensus as to what constitutes a family of tests, meaning that assessments of significance can vary widely based on the discretion of the researcher (Hochberg & Tamhane, 1987; Bender & Lange, 1998, 2001). Particularly harmful to the aims of the correction, it could incentivize the exclusion by researchers of tests which enlarge the family size without producing strong results. (For contrast, pursued to an absurd but logical extreme in efforts to err on the safe side, the family size could potentially be expanded to include all tests in the study, or even those not considered by the researcher, but which are theoretically possible; such an extreme but theoretically defensible approach would of course render most analysis meaningless, which is why some scholars recommend reserving such adjustment for multiple inference only for confirmatory rather than exploratory analysis (Bender & Lange, 1998).) In this study, I treat each distinct specification in which only the outcome variable differs across regressions as a family of tests, such that for women the family typically consists of 10 tests corresponding to the 10 later-life outcomes reported for women, and for men, the family consists of 9 tests since for men, fertility outcomes are not reported in IPUMS.

Lastly, the issues with Bonferroni correction are exacerbated if the family of tests are not independent (Bender & Lange, 2001; Abdi, 2007). This is especially relevant to this study, where theory, intuition, and the secondary literature suggest that human capital outcomes may be correlated, and indeed, where some outcomes may be inputs to others (e.g., nutritional status is an outcome unto itself while also being an input in the production of educational attainment; failure to complete college is both the outcome of a capability formation process, but also contributes to poverty in adulthood).

In light of these conceptual and methodological concerns, I calculate and report Bonferroni-corrected significance merely as a robustness exercise, but focus the discussion on results at the standard levels of significance.

results to those in the baseline. The results of these regressions are presented in Tables 3.4-3.5. The results are also robust to the inclusion of controls for early-childhood economic circumstances at the state-year level; these regressions are discussed in further detail in Section 6.2.2.

Lastly, the results are robust to alternate methods of estimation, including the one-way clustering of standard errors, estimation by probit where relevant to the outcome, and the removal of state trends. These specifications are less rigorous than the ones chosen for the bulk of the analysis presented in this thesis, and accordingly result in a greater number of statistically significant findings. These results are reported in Table 3.6.

For specifications including controls for the individual's veteran status, this variable was defined as a binary dummy taking the value of 1 if the individual has ever served in the U.S. armed forces on active duty, and 0 if not. The variable was constructed based on the variable "VETSTAT" in IPUMS (Ruggles et al., 2010) such that only individuals explicitly listed as veterans are deemed to be veterans; those whose veteran status is unknown are counted as non-veterans. For all the census waves used in this study, women are included in this definition of military service.[13]

[13]Note that veteran status is not used as a standard control in the baseline primarily because it amounts to a "bad control" in the sense that it may be endogenous to levels of human capital and/or to the Dust Bowl shock: the fact that it plausibly both influences and is influenced by the individual's human capital and wellbeing (which may in turn be affected by the Dust Bowl) makes its inclusion problematic (Angrist & Pischke, 2009).

For instance, individuals may be positively selected into military service due to mental/intellectual and physical fitness requirements for service (Bound & Turner, 2003). As such, all else equal, veterans may have higher levels of health and human capital on average than non-veterans. (They may also have been negatively selected on the basis of educational deferments if drafted (Bound & Turner, 2003; Altschuler & Blumin, 2009), or on the basis of poverty, low levels of human capital, and poor civilian employment prospects if enlisting voluntarily.) These predictions are of course complicated by the fact that these entry requirements were likelier to hold for veterans who enlisted voluntarily, rather than in those who were drafted; and deferments, vice-versa. Indeed, there is great diversity in the likely experiences of current/former military personnel in my sample, given that individuals in my sample may have served in contexts as distinct as World War II, the Korean War, the Vietnam War (for individuals in the post-Dust Bowl control group), or in peacetime. On the other hand, veterans may have more damage to wellbeing than non-veterans if wartime experiences resulted in physical injuries or mental health issues that could impact observed levels of human capital and wellbeing directly, as well as indirectly, for instance, through poor employment prospects due to disability or chronic ill health. That the Dust Bowl could, by affecting early-life

Table 3.4: Impact of Childhood Exposure to the Dust Bowl on Later-Life Outcomes: Robustness Checks – Controls, Men

	Age		**Veteran**	
	(1a)	(1b)	(2a)	(2b)
Outcomes	Treated × Erosion	Age	Treated × Erosion	Veteran
Age at First Marriage	-0.1330	0.0744***	-0.1770	1.0690***
	(0.1390)	(0.0135)	(0.1590)	(0.1340)
Probability of Completing High School	0.0170	-0.0112***	0.0112	0.1280***
	(0.0153)	(0.0023)	(0.0132)	(0.0150)
Probability of Completing College	-0.0107	-0.00471**	-0.0106	-0.0039
	(0.0075)	(0.0019)	(0.0074)	(0.0136)
Welfare Income	15.04	2.97**	16.51*	-39.62***
	(9.1470)	(1.3710)	(9.7770)	(4.6510)
Probability of Poverty	0.0080*	0.0000	0.0096*	-0.0431***
	(0.0042)	(0.0011)	(0.0049)	(0.0058)
Probability of Cognitive Disability	0.0065	0.0074***	0.0068	-0.0129***
	(0.0069)	(0.0001)	(0.0069)	(0.0027)
Probability of Physical Disability	0.0110***	0.0162***	0.0111***	-0.0055
	(0.0041)	(0.0001)	(0.0040)	(0.0037)
Probability of Vision & Hearing Difficulty	-0.0027	0.0082***	-0.0027	-0.0035**
	(0.0066)	(0.0001)	(0.0067)	(0.0017)
Probability of Self-Care & Independent Mobility Difficulty	-0.0003	0.0062***	0.0003	-0.0194***
	(0.0039)	(0.0006)	(0.0041)	(0.0027)

*** p<0.01, ** p<0.05, * p<0.1 uncorrected; Note: Table reports $treated_b \times erosion_s$ coefficients and, where relevant, the coefficient on the key control variable. Each row in the left-hand column refers to the regression's dependent variable, while the remaining column headings indicate the robustness check performed. All regressions are estimated by OLS except where explicitly stated otherwise, and include controls for race; birth year, birth state, and census year fixed effects; and state trends. Standard errors, clustered by birth state and birth year except where explicitly stated otherwise, are reported in parentheses below each coefficient. Where coefficients do not appear, this is because the regression in question is not relevant (e.g., because there is no need to control for age in regressions containing outcomes from a single census wave, in which birth year already captures age effects; it is not relevant to estimate regressions with continuous outcomes by probit.)

Table 3.5: Impact of Childhood Exposure to the Dust Bowl on Later-Life Outcomes: Robustness Checks – Controls, Women

	Age		**Veteran**	
	(1a)	(1b)	(2a)	(2b)
Outcomes	Treated × Erosion	Age	Treated × Erosion	Veteran
Age at First Marriage	-0.0033	0.0650***	-0.0042	1.8100***
	(0.1230)	(0.0136)	(0.1250)	(0.2020)
Children Ever Born	-0.2060	0.0339***	-0.2060	-0.4080***
	(0.1320)	(0.0066)	(0.1330)	(0.0342)
Probability of Completing High School	0.0307	-0.0112***	0.0307	0.1200***
	(0.0193)	(0.0012)	(0.0191)	(0.0090)
Probability of Completing College	-0.0117**	-0.0019*	-0.0117**	0.0468***
	(0.0054)	(0.0011)	(0.0054)	(0.0121)
Welfare Income	16.54	4.34	16.57	-36.22***
	(10.2200)	(3.4140)	(10.2000)	(4.8280)
Probability of Poverty	0.00466*	-0.0002	0.0047*	-0.0315***
	(0.0028)	(0.0022)	(0.0028)	(0.0041)
Probability of Cognitive Disability	-0.0054		-0.0054	0.0092***
	(0.0062)		(0.0062)	(0.0030)
Probability of Physical Disability	-0.0021		-0.0022	0.0213***
	(0.0110)		(0.0110)	(0.0047)
Probability of Vision & Hearing Difficulty	-0.0023		-0.0023	0.0103***
	(0.0050)		(0.0050)	(0.0028)
Probability of Self-Care & Independent Mobility Difficulty	-0.0015	0.0084***	-0.0015	-0.0020
	(0.0029)	(0.0009)	(0.0029)	(0.0031)

*** p<0.01, ** p<0.05, * p<0.1 uncorrected; Note: Table reports $treated_b \times erosion_s$ coefficients and, where relevant, the coefficient on the key control variable. Each row in the left-hand column refers to the regression's dependent variable, while the remaining column headings indicate the robustness check performed. All regressions are estimated by OLS except where explicitly stated otherwise, and include controls for race; birth year, birth state, and census year fixed effects; and state trends. Standard errors, clustered by birth state and birth year except where explicitly stated otherwise, are reported in parentheses below each coefficient. Where coefficients do not appear, this is because the regression in question is not relevant (e.g., because there is no need to control for age in regressions containing outcomes from a single census wave, in which birth year already captures age effects; it is not relevant to estimate regressions with continuous outcomes by probit.)

Table 3.6: Impact of Childhood Exposure to the Dust Bowl on Later-Life Outcomes: Robustness Checks – Birth State Clustering, Probit, & Trend Removal

	Men			**Women**		
	(1)	(2)	(3)	(4)	(5)	(6)
Outcomes	Birth State Clustered SE	Probit MFX	No Trends	Birth State Clustered SE	Probit MFX	No Trends
Age at First Marriage	-0.1320		-0.2230*	-0.0034		-0.03390
	(0.1350)		(0.1350)	(0.1210)		(0.1090)
Children Ever Born				-0.2060		-0.2460*
				(0.1330)		(0.1290)
Probability of Completing High School	0.0169	0.0049	0.0339*	0.0307*	0.0211*	0.0444
	(0.0151)	(0.0108)	(0.0206)	(0.0185)	(0.0118)	(0.0281)
Probability of Completing College	-0.0107	-0.0138**	-0.0157**	-0.0117**	-0.0113**	-0.0137***
	(0.0072)	(0.0065)	(0.0077)	(0.0053)	(0.0057)	(0.0053)
Welfare Income	15.05*		2.40	16.54*		2.12
	(8.6330)		(2.4870)	(8.5940)		(10.3400)
Probability of Poverty	0.0080**	-0.0023	-0.0025	0.0047*	-0.0077	-0.0048
	(0.0040)	(0.0031)	(0.0041)	(0.0025)	(0.0056)	(0.0043)
Probability of Cognitive Disability	0.0065	0.0000	-0.0096	-0.0054	-0.0119***	-0.0213***
	(0.0067)	(0.0049)	(0.0077)	(0.0063)	(0.0045)	(0.0078)
Probability of Physical Disability	0.0110**	0.0075***	0.0013	-0.0021	-0.0023	-0.0159
	(0.0047)	(0.0028)	(0.0119)	(0.0112)	(0.0078)	(0.0202)
Probability of Vision & Hearing Difficulty	-0.0027	-0.0043	-0.0111**	0.0023	-0.0043	-0.0092
	(0.0063)	(0.0032)	(0.0053)	(0.0054)	(0.0031)	(0.0058)
Probability of Self-Care & Independent Mobility Difficulty	-0.0003	-0.0018	-0.0074	-0.0015	-0.0019	-0.0100
	(0.0032)	(0.0023)	(0.0070)	(0.0031)	(0.0034)	(0.0092)

*** p<0.01, ** p<0.05, * p<0.1 uncorrected; Note: Table reports $treated_b \times erosion_s$ coefficients. Each row in the left-hand column refers to the regression's dependent variable, while the remaining column headings indicate the robustness check performed. All regressions are estimated by OLS except where explicitly stated otherwise, and include controls for race; birth year, birth state, and census year fixed effects; and state trends except where explicitly stated otherwise. Standard errors, clustered by birth state and birth year except where explicitly stated otherwise, are reported in parentheses below each coefficient. Where coefficients do not appear, this is because the regression in question is not relevant (e.g., because there is no need to control for age in regressions containing outcomes from a single census wave, in which birth year already captures age effects; it is not relevant to estimate regressions with continuous outcomes by probit.)

Figures 3.3 and 3.5).[16]

Results are similar when Equation 3.1 is estimated by probit rather than by OLS. Similarly, results are also robust to alternate means of clustering and to the removal of trends. The results of these regressions are presented in Table 3.6. Under these less rigorous specifications—first, clustering by birth state alone, and second, removing state trends—the signs and magnitudes of β_1 remain intuitive and similar to those in the baseline. As might be expected of these less conservative specifications, many more coefficients are statistically significant: most notably, those on high school completion (effects large and positive) and age at marriage (effects large and negative).

In the face of these extensive robustness exercises, it is clear that the Dust Bowl was indeed a meaningful shock that severely influenced the human capital and later-life welfare of those exposed.

3.6 Conclusion

In this chapter, I have shown evidence that the Dust Bowl had statistically significant adverse effects on the adult outcomes of cohorts exposed to the disaster as children. The effects are economically meaningful both to individuals as well as to the wider economy. In particular, I find that both exposed men and women are likelier to suffer from a variety of disabilities and to live in poverty, and receive higher entitlement payments, than their unexposed counterparts. Exposed men get married younger than those not exposed, while exposed women are less likely to have completed college. Lastly, both exposed men and women experience an increase in the chances of having completed high school,[17] a result which may stem from a drop in the opportunity cost

[16] Cunfer (2005, Ch. 6) for contrast, finds greater consonance between drought and dust storms at the county level. It should be noted, however, that in addition to analyzing Dust Bowl severity at a lower level of aggregation than that necessitated by my study, he also uses a different sample region and a different measure of drought from that used here.

[17] Although it may appear surprising that a cohort who enjoyed higher rates of high school completion would go on to enjoy lower rates of college completion, I discuss in Section 4.2 and again in

of secondary schooling following the collapse of agriculture, and with it, the demand for child farm labor.[18] These effects are robust to a variety of checks, including alternative measures of Dust Bowl severity (e.g. total drought magnitude and change in farm values over the 1930s). The results presented here suggest that in failing to address the human costs of the Dust Bowl, the literature has long underestimated the full toll of this historical event.

Having now established that the Dust Bowl scarred individuals' human capital and wellbeing, I turn to the question of precisely *how* this event affected capability formation. In the upcoming Chapters 4 and 5, I move beyond the baseline analysis presented here to investigate the pathways and mechanisms through which the Dust Bowl affects later-life outcomes, both in terms of the environmental insult itself, and in terms of strategic responses to it. In these chapters, I report results of tests in many cases based on new data and empirical approaches.

Firstly, in Section 4.2, I test whether the severity of adverse effects is linked to the developmental stage at which children were exposed to the Dust Bowl. I do this by disaggregating the treatment measure to account for the timing and duration of exposure to the Dust Bowl. This analysis sheds light on the pathways by which specific capabilities are likely formed, as well as provides insight into sensitive periods for the development of these capabilities.

Secondly, in Section 4.3, I test for the influence of agriculture-dependence on the Dust Bowl's impacts. I use farm population-weighted erosion measures test the hypothesis that individuals born in more agriculture-dependent states experienced worse adverse effects of Dust Bowl exposure—that is, that the income channel effects of the Dust Bowl mattered. I also use farm status interactions test whether individuals born in states with greater levels of farm-dependence experienced worse Dust Bowl

Sections 4.4 and 7.2 how this seeming inconsistency arises from the mechanism by which, and the developmental stage during which, each capability appears to be impacted.

[18] A number of other results are insignificant here, but similar in sign and magnitude to those presented elsewhere in this thesis.

effects than those in less agricultural states. Here, the aim is to test whether individuals growing up in farming states may have experienced greater initial insults, lower compensatory investments, or other constraints on recovery due to the Dust Bowl's particular impact on agricultural communities.

Thirdly, in Section 4.4, I probe the seemingly counterintuitive and beneficial effect of the Dust Bowl on high school completion, a result first introduced in this baseline chapter. In order to establish whether this finding is primarily driven by changing time use decisions in older children, where agricultural collapse may have led to a decline in the opportunity cost of secondary schooling, I test for how child labor intensity prior to the crisis modifies the Dust Bowl's effect on high school completion. Similarly, I test for child labor intensity-driven gradients in contemporaneous school enrollment at the state and county levels.

Fourthly, in Section 5.2, I test whether the Dust Bowl cohort may have used contemporaneous or eventual migration as a margin of adjustment to the Dust Bowl shock, and whether there were socioeconomic gradients in the migration probability of those exposed. I also test whether migration may have contributed to insult remediation,for instance, by exposing migrants to a superior environment for recovery.

Fifthly, in Section 5.3, I test whether fertility responded to the Dust Bowl, whether generally or differentially by maternal characteristics. Here, and similar to the analysis above concerning migration, the aim is to examine whether endogenous fertility responses may have been a coping mechanism by which households mitigated—or even avoided—the adverse effects of the Dust Bowl on child health and intra-household resource distribution.

Lastly, in Section 5.4, I test for the effects of public spending on recovery primarily associated with the New Deal in ameliorating the Dust Bowl's adverse impacts on children's health, nutrition, and education. Here, I test for the biological capacity for recovery from early-life insults to human capital, and examine the means by which compensatory investment in the form of New Deal-era spending may have achieved such remediation.

Chapter 4

Heterogeneity & Mechanisms

Figure 4.1: Venus Barnett Trying to Grow Vegetables for a Second Time after a Windstorm Blew the First Seedlings Away, 1942 (Eisenstaedt, 1942)

4.1 Overview

I now turn to the pathways and mechanisms through which the Dust Bowl affects later-life outcomes, by testing for heterogeneity in individuals' exposure to and experiences of this environmental insult. First, I test whether the severity of adverse effects is linked to the developmental stage at which children were exposed to the Dust Bowl, especially where these sensitive periods may differ by capability. Second, I test for the influence of agriculture-dependence in modifying the Dust Bowl's later-life human capital and wellbeing impacts. Lastly, I test whether the beneficial secondary schooling effects of Dust Bowl exposure are driven by spatial variation in child labor demand.

4.2 Developmental Stage

4.2.1 Background

A key feature of the theoretical framework presented in Chapter 2 is that the effects of human capital interventions, whether positive or negative, may be non-linear over an individual's life-course.[1] Furthermore, the associated literature, along with intuition, proposes that different development processes govern different human capabilities, and at different stages in development: for instance, the development of healthy metabolism may have a different sensitive or critical period, and be affected by different environmental factors, than the development of self-control, or, as Cunha et al. (2010) find, cognitive skills are formed (and are therefore more malleable) early on, while the production of noncognitive skills is roughly stable across the life cycle. As such, it is useful in order to better understand the technology of human capi-

[1] In fact, empirical evidence overwhelmingly suggests that the effects of these interventions actually *are* non-linear across periods, not just that they may be (Cunha et al., 2010; Conti & Heckman, 2014)

tal formation to subdivide the childhood period to yield narrower bands of plausible Dust Bowl exposure. These allow for the testing of when in childhood an adverse shock is likeliest to be harmful to later-life wellbeing. Additionally, by separating distinct developmental periods (e.g., fetal, neonatal, schooling-age) and comparing and contrasting which capabilities appear to be affected by exposure in each sub-period, this approach also helps clarify the pathways by which specific capabilities may be developed and impacted.

In this section, I use age-specific duration-weighted measures of Dust Bowl exposure to isolate sensitive periods in the development of the human capital outcomes of interest. As suggested by theory and the empirical evidence to date, I find that many later-life outcomes are overwhelmingly shaped in earlier periods in childhood—in particular, during fetal development. In particular, the effects on post-secondary education, poverty outcomes, and disability are most severely adverse when Dust Bowl exposure occurred *in utero* and in the first year of life; these adverse effects are less pronounced or are even insignificant when exposure was later in childhood. For contrast, fertility and high school completion are affected later on in childhood. These findings indicate that the developmental stage at Dust Bowl exposure matters. Meanwhile, the contrasts in the timing of sensitive phases by outcome—such as the surprising lack of congruence in college and high school completion development trajectories—provide new insight into the possible mechanisms by which each capability is formed. For instance, in this example it appears that success in post-secondary education is likely governed by cognitive capabilities shaped prenatally, while secondary education poses fewer or less binding cognitive demands and instead is shaped by local labor market conditions in later childhood. Such analysis provides insight that could be used to increase the efficiency and effectiveness of policies targeting early-childhood development, both in normal growth and post-crisis recovery contexts.

4.2.2 Methods & Data

To pinpoint the effect of the age at which the Dust Bowl shock occurs, I estimate Equation 3.1 substituting the proportion of a given age band spent during the Dust Bowl period for the original $treated_b$ term. Thus, where all those aged -1 to 12 at any point between 1930 and 1940 would have previously been counted as treated on a binary basis, now each individual receives a duration-weighted measure of exposure during each development stage, defined as follows: -1 to 0 (*in utero*/early infancy), 1 to 3 (infancy), 4 to 6 (early childhood), 7 to 9 (prime school age), and 10 to 12 (early adolescence).

Accordingly, the new $treated_b$ term is constructed as the proportion of the given age range (-1 to 0, 1 to 3, 4 to 6, 7 to 9, and 10 to 12) spent in (i.e., that coincides with) the Dust Bowl period of 1930 to 1940. For example, an individual born in 1941 spent 50% of the -1 to 0 age range in the Dust Bowl, while an individual born in 1940 spent 100% of the same age range in the Dust Bowl, and an individual born in 1942 spent 0% of this age range in the Dust Bowl. Similarly, an individual born in 1919 would have spent 66.7% of the 10 to 12 age range but 0% of the 7 to 9 age range in the Dust Bowl timeframe. Individuals who were never children during the period 1930-1940 would by this logic, and as in the baseline, receive a value of 0 for $treated_b$ in all specifications. This variable is thus a measure of the developmental stage or age at plausible exposure to the Dust Bowl, weighted more heavily for those that spent a greater proportion of the developmental stage during the Dust Bowl period. All other aspects of the data and regressions are as in the baseline approach described in Chapter 3.

4.2.3 Results & Discussion

In Table 4.1 I report the β_1 values from these of regressions for men, and in Table 4.2 for women. Patterns by outcome and developmental stage, with 95% confidence intervals, are also plotted in Figures 4.2-4.20. I find that age at exposure matters, both to the severity and significance of adverse effects. Indeed, disentangling effects by developmental stage allows me to clarify plausible pathways of impact that the baseline regressions obscure.

For men, adverse effects on poverty, disability, and age at marriage outcomes are worst amongst those exposed *in utero* and in infancy.[2] In particular, welfare receipts rise amongst men exposed in these early development stages by $14.15-16.29 relative to those unexposed in those stages. Meanwhile, chances of physical disability rise by 1.69-1.89 percentage points, and men exposed in these stages get married 0.19-0.21 years younger than those unexposed. Adverse effects appear to dampen as the age of exposure rises,[3] except notably in disability outcomes; in the early adolescence period, shocks that in early childhood produced greater likelihoods of physical disability instead produce lower likelihoods of physical and vision and hearing disabilities.[4] These findings are consistent with the idea that congenital health defects (rather than, say, schooling disruptions) may be responsible for poor later-life outcomes and that these may act through disability, or even directly, to produce lower adult socioeconomic status.

Secondary schooling appears to be the exception to the infant exposure pattern.

[2]Notably, these negative effects on age at marriage for the treated cohorts are found above and beyond the national trend in declining ages at marriage during the period (see Figure 3.6). Since these results occur in the absence of lower rates of poverty, it is difficult to interpret these age-of-marriage decreases as a positive outcome, for instance, as the result of a man having become better financially able to start a new family and household, or better able to attract a marriage partner, earlier in life. Instead, this finding could perhaps be interpreted as an attempt by these men to build an identity and signify adulthood through marriage or family where opportunities to define oneself through one's work or socioeconomic standing may have been elusive.

[3]These findings are consistent with theory in Heckman (2007), Cunha & Heckman (2008), and Almond & Currie (2011a).

[4]It is unclear precisely why Dust Bowl exposure would result in a beneficial reduction in disability when exposure was in later childhood. It is possible that the collapse of child labor in agriculture, which will be discussed in Section 4.4 with respect to secondary schooling, drives this result as well. In particular, it may be the case that older children who might have otherwise worked outdoors in this age range doing strenuous or hazardous work did so less during the Dust Bowl due to the absence of opportunities for child labor in agriculture. As a result, it is possible that their later-life health would stand to benefit from their having avoided hard physical labor in later childhood.

Table 4.1: Impact of Childhood Exposure by Age Band: Men

Outcomes	**-1 to 0** (1)	**1 to 3** (2)	**4 to 6** (3)	**7 to 9** (4)	**10 to 12** (5)
Age at First Marriage	-0.2100*	-0.1930*	-0.0743	0.0390	0.0368
	(0.1130)	(0.1140)	(0.1130)	(0.1140)	(0.1250)
Probability of Completing High School	-0.0052	0.0105	0.0188	0.0342***,†	0.0361**
	(0.0139)	(0.0133)	(0.0124)	(0.0127)	(0.0167)
Probability of Completing College	-0.0110	-0.0088	-0.0072	-0.0022	-0.0024
	(0.0103)	(0.0082)	(0.0065)	(0.0058)	(0.0063)
Welfare Income	16.29**	14.15*	5.76	8.37	5.42
	(6.7940)	(8.3700)	(7.6730)	(8.3060)	(8.6060)
Probability of Poverty	0.0039	0.0031	0.0040	0.0043	0.0065
	(0.0032)	(0.0037)	(0.0037)	(0.0036)	(0.0040)
Probability of Cognitive Disability	0.0116	0.0026	-0.0059	-0.0114	-0.0121
	(0.0076)	(0.0083)	(0.0073)	(0.0076)	(0.0095)
Probability of Physical Disability	0.0169***,†††	0.0189***,†††	0.0062**	-0.0054	-0.0164**
	(0.0042)	(0.0045)	(0.0027)	(0.0084)	(0.0082)
Probability of Vision & Hearing Difficulty	0.0067	0.0081	-0.0010	-0.0114	-0.0154*
	(0.0054)	(0.0068)	(0.0080)	(0.0080)	(0.0086)
Probability of Self-Care & Independent Mobility Difficulty	0.0025	-0.0024	-0.0078**	-0.0057	-0.0051
	(0.0041)	(0.0041)	(0.0038)	(0.0042)	(0.0043)

*** p<0.01, ** p<0.05, * p<0.1 uncorrected; † † † p<0.01, †† p<0.05, † p<0.1 Bonferroni-corrected; Note: Table reports $treated_b \times erosion_s$ coefficients. Each row in the left-hand column refers to the regression's dependent variable, while the remaining column headings indicate the developmental stage that defines $treated_b$. All regressions are estimated by OLS and include controls for race; birth year, birth state, and census year fixed effects; and state trends. Standard errors, clustered by birth state and birth year, are reported in parentheses below each coefficient.

Table 4.2: Impact of Childhood Exposure by Age Band: Women

Outcomes	**-1 to 0** (1)	**1 to 3** (2)	**4 to 6** (3)	**7 to 9** (4)	**10 to 12** (5)
Age at First Marriage	-0.0757	-0.0384	0.0329	0.0574	0.0768
	(0.1150)	(0.1290)	(0.1250)	(0.1320)	(0.1050)
Children Ever Born	-0.1650	-0.2410*	-0.2680*	-0.2380*	-0.1840*
	(0.1030)	(0.1310)	(0.1380)	(0.1320)	(0.1110)
Probability of Completing High School	-0.0128	-0.0007	0.0203	0.0489**	0.0639**
	(0.0090)	(0.0129)	(0.0178)	(0.0221)	(0.0278)
Probability of Completing College	-0.0154***,†††	-0.0136***,†††	-0.0144**	-0.0104	-0.0048
	(0.0040)	(0.0035)	(0.0063)	(0.0065)	(0.0064)
Welfare Income	14.90**	14.19	10.85	13.47	12.00
	(7.1030)	(9.3610)	(9.0000)	(11.0700)	(11.3800)
Probability of Poverty	0.0073***,†	0.0051*	0.0010	0.0023	0.0005
	(0.0028)	(0.0028)	(0.0031)	(0.0024)	(0.0033)
Probability of Cognitive Disability	0.0082	0.0000	-0.0070	-0.0153***,†††	-0.0208***,†††
	(0.0053)	(0.0060)	(0.0049)	(0.0043)	(0.0023)
Probability of Physical Disability	0.0128*	0.0054	-0.0119	-0.0191**	-0.0252**
	(0.0071)	(0.0079)	(0.0095)	(0.0094)	(0.0100)
Probability of Vision & Hearing Difficulty	0.0011	0.0029	-0.0008	-0.0043	-0.0051
	(0.0042)	(0.0038)	(0.0040)	(0.0050)	(0.0058)
Probability of Self-Care & Independent Mobility Difficulty	0.0057*	-0.0002	-0.0075**	-0.0104**	-0.0099*
	(0.0032)	(0.0030)	(0.0032)	(0.0049)	(0.0060)

*** p<0.01, ** p<0.05, * p<0.1 uncorrected; † † † p<0.01, †† p<0.05, † p<0.1 Bonferroni-corrected; Note: Table reports $treated_b \times erosion_s$ coefficients. Each row in the left-hand column refers to the regression's dependent variable, while the remaining column headings indicate the developmental stage that defines $treated_b$. All regressions are estimated by OLS and include controls for race; birth year, birth state, and census year fixed effects; and state trends. Standard errors, clustered by birth state and birth year, are reported in parentheses below each coefficient.

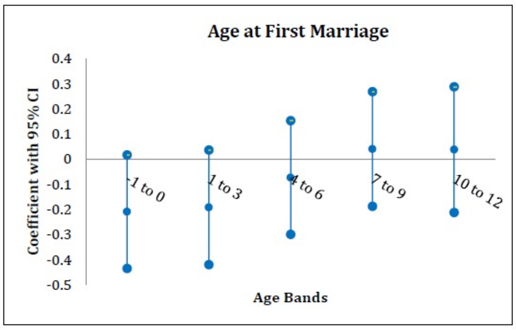

Figure 4.2: Impact of Exposure to the Dust Bowl by Development Stage on Later-Life Outcomes: Age at First Marriage–Men

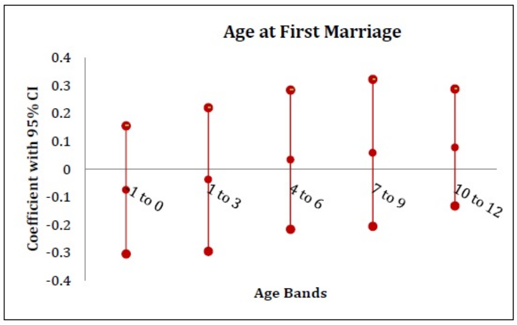

Figure 4.3: Impact of Exposure to the Dust Bowl by Development Stage on Later-Life Outcomes: Age at First Marriage–Women

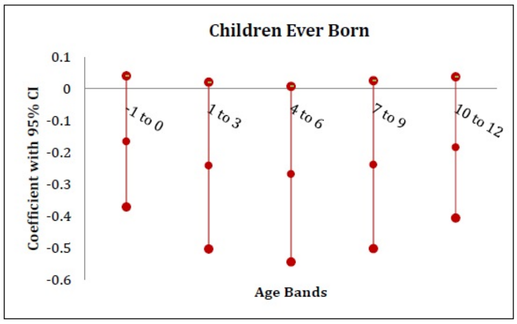

Figure 4.4: Impact of Exposure to the Dust Bowl by Development Stage on Later-Life Outcomes: Children Ever Born–Women

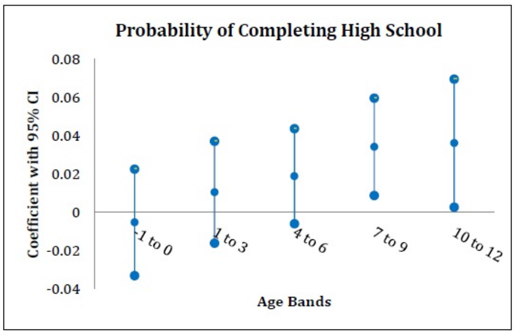

Figure 4.5: Impact of Exposure to the Dust Bowl by Development Stage on Later-Life Outcomes: Probability of High School Completion–Men

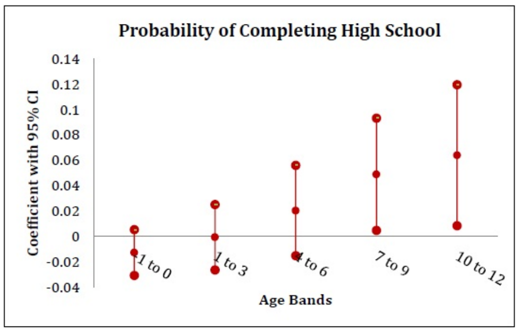

Figure 4.6: Impact of Exposure to the Dust Bowl by Development Stage on Later-Life Outcomes: Probability of High School Completion–Women

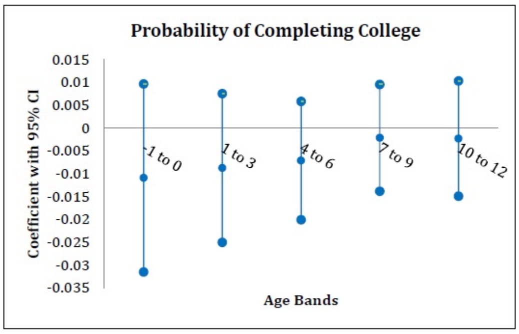

Figure 4.7: Impact of Exposure to the Dust Bowl by Development Stage on Later-Life Outcomes: Probability of College Completion–Men

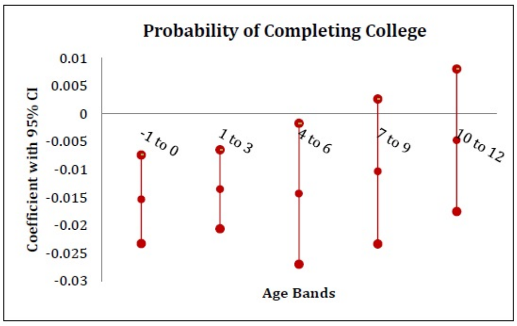

Figure 4.8: Impact of Exposure to the Dust Bowl by Development Stage on Later-Life Outcomes: Probability of College Completion–Women

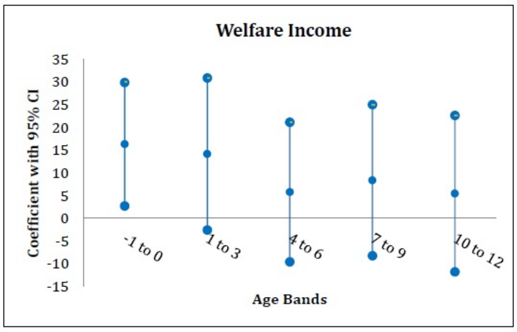

Figure 4.9: Impact of Exposure to the Dust Bowl by Development Stage on Later-Life Outcomes: Welfare Income–Men

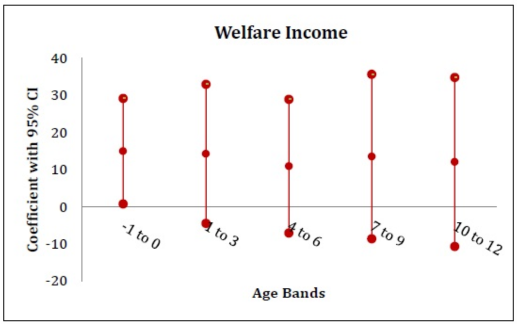

Figure 4.10: Impact of Exposure to the Dust Bowl by Development Stage on Later-Life Outcomes: Welfare Income–Women

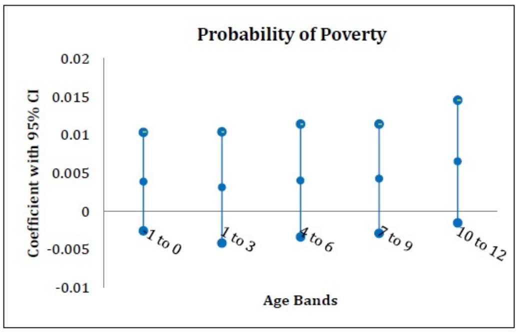

Figure 4.11: Impact of Exposure to the Dust Bowl by Development Stage on Later-Life Outcomes: Probability of Poverty–Men

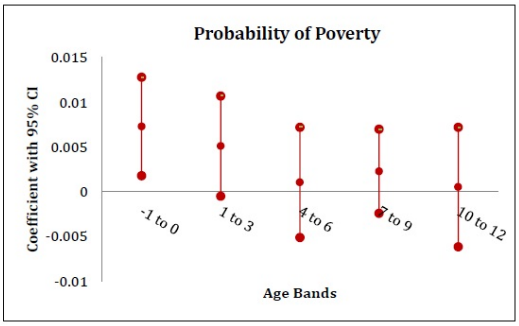

Figure 4.12: Impact of Exposure to the Dust Bowl by Development Stage on Later-Life Outcomes: Probability of Poverty–Women

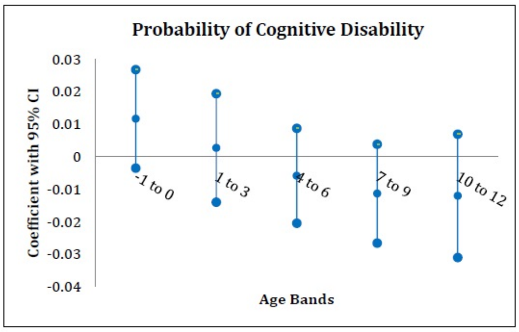

Figure 4.13: Impact of Exposure to the Dust Bowl by Development Stage on Later-Life Outcomes: Probability of Cognitive Disability–Men

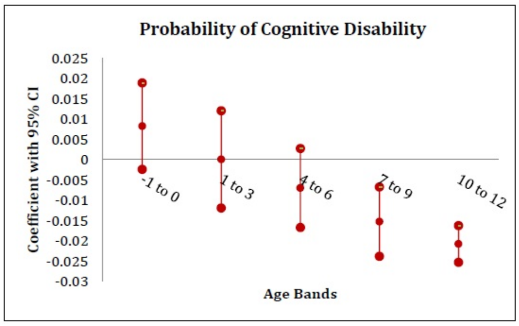

Figure 4.14: Impact of Exposure to the Dust Bowl by Development Stage on Later-Life Outcomes: Probability of Cognitive Disability–Women

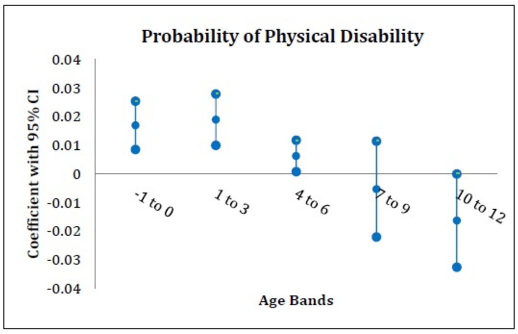

Figure 4.15: Impact of Exposure to the Dust Bowl by Development Stage on Later-Life Outcomes: Probability of Physical Disability–Men

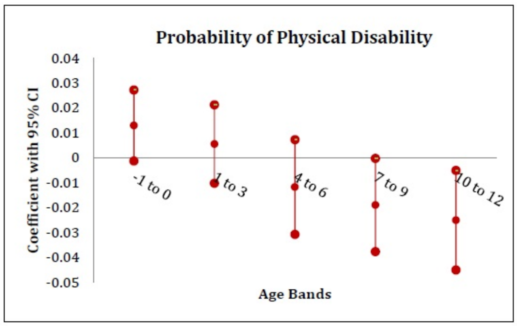

Figure 4.16: Impact of Exposure to the Dust Bowl by Development Stage on Later-Life Outcomes: Probability of Physical Disability–Women

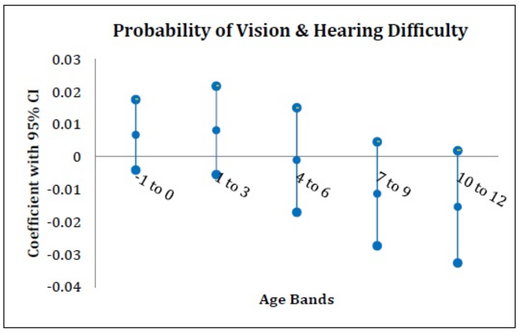

Figure 4.17: Impact of Exposure to the Dust Bowl by Development Stage on Later-Life Outcomes: Probability of Vision & Hearing Difficulty–Men

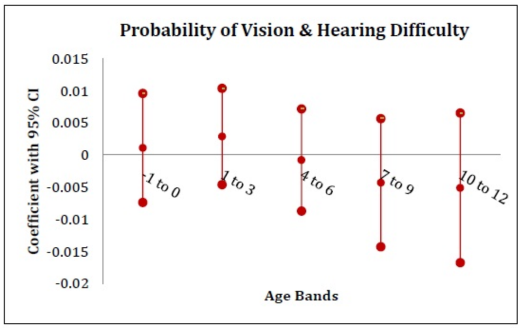

Figure 4.18: Impact of Exposure to the Dust Bowl by Development Stage on Later-Life Outcomes: Probability of Vision & Hearing Difficulty–Women

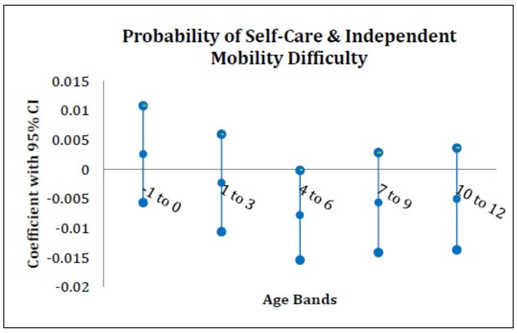

Figure 4.19: Impact of Exposure to the Dust Bowl by Development Stage on Later-Life Outcomes: Probability Self-Care & Independent Mobility Difficulty–Men

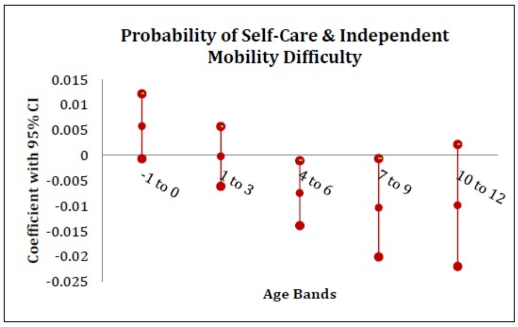

Figure 4.20: Impact of Exposure to the Dust Bowl by Development Stage on Later-Life Outcomes: Probability of Self-Care & Independent Mobility Difficulty–Women

Those exposed in late childhood—that is, school age and beyond—enjoy better outcomes than their unexposed counterparts in high school completion: men exposed between the ages of 7 and 9 see a 3.42 percentage point greater chance of finishing high school; amongst men exposed between 10 and 12, the increase in the probability of high school completion is 3.61 percentage points. Although it seems natural that a shock at school-going age would affect schooling, the finding of *positive* outcomes on schooling may seem less intuitive. However, the age at impact for this outcome, together with the results on farm-dependence that will be presented in Section 4.3, suggest that secondary schooling rates may have risen as the collapse of agricultural incomes reduced chances for child labor and decreased the opportunity cost of schooling. I investigate this explanation of the secondary schooling result in greater detail in Section 4.4.

For women, fertility outcomes are negatively affected across developmental stages. Exposed women experience a 0.18 to 0.27-child reduction in fertility, with the greatest adverse impact occurring in the 4 to 6 age range. That fertility reduction effects are greatest well before puberty suggests early damage to reproductive health, perhaps through poor nutrition or endocrinal maldevelopment. This interpretation, that fertility reductions were involuntary and were likely the result of biological constraints that remained latent until later-life, is corroborated by the fact that exposed women had neither the higher marriage ages nor the higher college completion rates associated with the deferral of childbearing in favor of long-term career planning (Goldin & Katz, 2002), nor did they experience income levels in adulthood which might have induced them to voluntarily restrict fertility. This finding of a non-negligible decline in fertility is particularly striking given the nationwide post-war "baby boom" trend in fertility experienced by the generation born in the 1920s and 1930s (see Figure 3.7 in Section 3.3.2).

Meanwhile, as with men, it is in early childhood that exposed women's college completion, poverty, and disability effects are worst. Women exposed between the ages -1 and 6 had a 1.36-1.54 percentage point lower chance of completing college, with the greatest adverse effects accruing to those exposed *in utero*. A similar pattern follows for probability of poverty (0.73 percentage points higher for those exposed -1 to 0 and 0.51 percentage points higher for those exposed 1 to 3) and for physical disability and self-care and independent mobility difficulty (1.28 percentage points and 0.57 percentage points higher for those exposed *in utero*, respectively). As was the case for men, chances of disability decrease among those exposed to the Dust Bowl in later childhood, and the greatest positive effects on secondary schooling occur amongst those exposed as schooling-age children: an increase of 4.89 percentage points in the probability of high school completion amongst women exposed between ages 7 and 9, and an extraordinary 6.39 percentage point increase among those exposed between 10 and 12. Notably, the timing and direction of high school completion and college completion outcomes are mismatched in a way that suggests these outcomes are less related, whether mechanically or in terms of the underlying capabilities required for the attainment of each credential, than might intuitively be expected of two outcomes in the same human capital category, education.[5]

The findings presented here suggest that many capabilities are fundamentally shaped during the prenatal period, and are consistent with the idea congenital health defects are responsible for many of the poor later-life outcomes observed. While disability outcomes may be intuitively understood to result from the Dust Bowl's effects on fetal and infant nutrition and health, we might, for contrast, expect poor adult socioeconomic outcomes to result from poor labor market readiness, perhaps due to deficits in schooling attributable to shocks at school-going age. However, given that the greatest adverse impacts for poverty and age at marriage outcomes are found in early childhood, the results suggest that poor health and development complications may be to blame, perhaps acting through disability, or even directly, to produce lower adult socioeconomic status.

Thus, for both sexes, the preponderance of damage where exposure was in early childhood suggests that the pathway of impact is largely through subverted capability formation.[6]

4.2.4 Conclusion

By disaggregating the childhood exposure period, the analysis presented in this section sheds light on the technology of human capital formation. In particular, these findings provide policy-relevant insight into the timing of and plausible biological processes governing capability development. I find that the development stage in which children experienced the Dust Bowl shock matters, both to the severity of the later-life

[5]It is also worth noting that since high school credentials would have been one of the requirements for college entry, the adverse college results I find exist in a cohort whose qualifications on this dimension of college eligibility were actually better than amongst the unexposed. As such, the adverse effects of the Dust Bowl on college outcomes may be underestimated. Likelier still, these college results suggest that a high school diploma is not by itself a signal of the skills and resources necessary for post-secondary success. For further discussion of these and other plausible pathways of impact revealed by this analysis, see Section 7.2.

[6]For contrast, the positive effects observed for secondary schooling in later childhood, discussed in Section 4.4, implicate labor markets and access to public services.

insult experienced, as well as to the specific capabilities impacted. For the majority of outcomes considered here, including age at marriage, college completion, welfare income, poverty, and disability, the adverse effects of Dust Bowl exposure are largest for those who experienced the shock *in utero* and in early childhood. For women, fertility outcomes suffer when exposure was in mid-to-late childhood—notably, before the typical onset of puberty, which suggests earlier or perhaps more fundamental problems in reproductive development. In contrast, beneficial effects of Dust Bowl exposure on high school completion are found in those exposed in later childhood.

The prevalence of adverse outcomes for those exposed in earlier stages of development coincides with the theory outlined in Section 2.3 and provides empirical support of the fetal origins hypothesis. Specifically, these findings confirm that for many capabilities, the prenatal and early-infancy periods are formative stages in development, stages in which capability formation is thus especially sensitive to adverse shocks.[7] Interventions occurring in these early stages go on to exert disproportionate influence on subsequent human capital development. The implications of these and related findings in the specific context of the theoretical model are discussed in further detail in Section 7.2

Furthermore, the stage in which a given capability is affected is suggestive of that capability's development process and of the mechanisms by which the Dust Bowl may have undermined this process. For instance, the fact that fertility is adversely affected before puberty suggests that fertility reductions were involuntary and likely the result of biological complications in the development of reproductive systems. Meanwhile, the fact that high school completion is affected much later in childhood suggests that high school attainment may have more to do with time use decisions

[7]This finding also implies that children likely received the brunt of the Dust Bowl's adverse effects regardless of whether they later migrated. If, as these results suggest, the intrauterine environment is overwhelmingly responsible for adult outcomes, this would mitigate concerns around the duration of Dust Bowl exposure and the possibility of mis-measurement of Dust Bowl severity due to childhood migration. This issue is discussed in greater depth in Chapter 6.

and access to schooling than biologically-determined student ability, in contrast to another and much earlier-affected schooling outcome, college completion.

These results also help us understand the interrelationships between capabilities and developmental periods. For example, findings such as those placing the most severe effects of Dust Bowl exposure on both college completion *and* adult poverty in the prenatal and early infancy period are suggestive of self productivity in human capital investments. They support the notion that higher levels of human capital/wellbeing in one period (in this case, post-secondary schooling attainment) produce higher levels of human capital/wellbeing (in this case, adult income) in the subsequent period. Here, the co-location of these two distinct outcomes in the individual's developmental timeline suggests damage to both outcomes may be driven by the same developmental deficits (e.g. subverted cognitive development), or that one may be driven by the other (e.g. cognitive deficits produce poorer college completion outcomes, which become an input to the production of adult poverty status).[8] In this way, these results also suggest cross-pollination between distinct capabilities both within and across periods. Namely, as in this case, college completion may be its own outcome as well as an input into the production of other outcomes such as adult socioeconomic status. These results also show plausible evidence of dynamic complementarity, or the notion that investments in a subsequent stage raise the return to earlier investments. Here, for instance, the failure of higher rates of high school completion on their own to result in better adult socioeconomic status suggest that perhaps the latter could not be achieved without the attainment of a college credential to unlock the earnings potential of a high school degree.

In Section 7.2 I return to these results in a more holistic manner, placing them in the context of the dynamic model of human capital formation presented in Section

[8]Similarly, with such early-life cognitive deficits (i.e. lower stage 1 endowments), it is possible that subsequent investments, such as those in secondary schooling, are less productive (in this case, in terms of adult income) than they otherwise might be had the earlier-stage endowment been higher.

2.3. Nevertheless, it is clear from the findings presented in this section that the effects of an adverse shock like the Dust Bowl depend both on the timing of the shock in an individual's development trajectory, as well as on the specific outcome being considered. Importantly, adult human capital is not a linear function of earlier inputs, a finding which has meaningful implications for child welfare policies.

4.3 Farm

4.3.1 Background

Agriculture suffered greatly during the Dust Bowl. The blow to farms was substantial, and the degree to which agriculture was affected varied greatly. In my sample, for instance, the ratio of the value of the state's farmland in 1940 to that in 1930, calculated from the data in Hornbeck (2012), ranges from 0.40 to 0.80, with the majority of states' farm value in 1940 standing at 60-70% of that in 1930. The distribution of farm destruction (in terms of the percentage of the population impacted, as presented in Column 2 in Table 3.1) was generally much more concentrated, with most states in the sample finding less than 30% of their populations living in high-farm-value-loss counties. In states such as Nebraska and South Dakota, for contrast, destruction was much more uniform, with roughly 90% of the population living in areas affected by significant farm losses.

The decimation of farms was in large part an environmental phenomenon. Wind erosion, sheet erosion, and gullying were widespread (Soil Conservation Service, 1935; Hornbeck, 2012); by 1938 the Soil Conservation Service estimated that 40% of the region had experienced "serious" wind erosion damage, with the average acre in the region losing over 480 tons of fertile topsoil (Hansen & Libecap, 2004). The soil left behind was greatly depleted of organic matter and fertilizing nutrients such as nitrogen, phosphoric acid, and potash (Hansen & Libecap, 2004), reducing its productive capacity (Wallace, 1938). Arable agriculture was made especially difficult given the combination of high temperatures, drought, and reduced quality and quantity of topsoil and ground cover (Wallace, 1938; Worster, 1979). Naturally, this destruction of the land led to significant financial hardship: Bennett (1939), cited in Hansen & Libecap (2004), estimated productivity losses from erosion at $400 million per year. Hornbeck (2012, p.1481), too, reports sharp and substantial decreases in the value of farmland and in agricultural revenue over the 1930s. Rates of unemployment rose in farming communities over the same period (Wallace, 1938; Levine, 2009), adding to the glut of underemployed labor that had already existed on farms (Wallace, 1938, p. 47). The ripple effect was large in communities dependent on agriculture; here, "everyone in the community, from schoolteachers to implement dealers, from railroad workers to dentists" depended on the farmer and felt the pain of agricultural collapse (Worster, 1979, p. 120). Regarding the distressed farm families of the Plains, Gove Hambidge wrote in a contemporary report, "the poverty and lack of opportunity in problem areas needs no proof" (Wallace, 1938, p. 14).

While they might give a sense of the scale of destruction, the *relentlessness* of the adversity faced by farmers may not come across from the sober figures outlined above. Indeed, farmers were beset by problems large and small that from qualitative accounts can seem especially ghoulish. For instance, on top of the terror they inspired and the crops they smothered and uprooted, dust storms also created static electricity so strong that it could stall motor vehicles such as cars and combines, kill wheat sprouts, and prevent the germination of seeds (Hurt, 1981). Of course, these were petty annoyances compared to the financial and psychological stresses faced by the owners of livestock.

The "cattle kills" sponsored by the Drought Relief Service's Emergency Cattle Purchase Program of 1934[9] accomplished several objectives. Firstly, they represented

[9] An earlier livestock culling program run by the Agricultural Adjustment Administration in 1933 had focused on pigs.

a largely successful attempt by the government to rein in the supply of beef (including preventing the market from being overrun by inferior, drought-stricken cattle), and by doing so, to raise livestock income, which was falling sharply over the 1930s (Worster, 1979; Hurt, 1981).[10] Secondly, they served the purpose of allowing farmers to upgrade their livestock by simultaneously relieving them of sickly cattle and providing them the necessary funds to make new farm capital investments (Hurt, 1981). Thirdly, these culls offered an arguably humane way to put starving animals, which were suffering from drought-induced feed shortages, out of their misery (Hurt, 1981). Fourthly, as part of ongoing work relief programs, they created jobs in packing and canning (Hurt, 1981). Lastly, they provided a source of food relief for distressed families as that meat which was deemed edible was distributed by the Federal Surplus Relief Corporation to the unemployed free of charge (Worster, 1979; Hurt, 1981).

In the less than one year that the 1934 drought purchase program was in place, the Department of Agriculture bought 8.3 million head of cattle and paid out $111.7 million to American cattlemen (Worster, 1979, p. 113). Following an official inspection, cattle that were deemed unfit for human consumption were immediately shot and buried (Worster, 1979). Oral histories provided by childhood survivors of the Dust Bowl in Burns (2012) suggest that these slaughters, where a community's emaciated and unproductive cattle were gathered to a central pit and shot, were traumatic events for farmers who viewed their livestock as family, and their inability to sustain these animals as an identity-shaking failure.[11] Those animals deemed fit by the inspectors

[10]Worster (1979) outlines a series of other strategies which were attempted but which were unsuccessful in addressing the beef price issue, including an Agricultural Adjustment Administration proposal for production cutbacks that was voted down by cattle growers, and restrictions on whatever minimal beef was imported from abroad. Meanwhile, Hurt (1981) discusses other support offered to livestock owners, such as Farm Credit Administration loans for the purchase of feed.

[11]Peck et al. (2002) find a similar phenomenon amongst farmers in the case of a 2001 foot-and-mouth disease outbreak in the United Kingdom. There, farmers in affected areas reported higher levels of psychological morbidity relative to those unaffected by the outbreak. These adverse psychological effects appear to stem both from emotional pain due to the slaughter of cattle (a "post-traumatic experience," per Peck (2005)), as well as from more general distress over income losses.

were sent on to be slaughtered, packed, canned, and fed to the poor. In each case, livestock owners were compensated based on the type, age, and condition of their stock (Worster, 1979). The program was widely regarded as a success, although its existence did not prevent thousands of cattle from being sold for a pittance on the open market (Hurt, 1981).

Although the preceding paragraphs have referred primarily to the experiences of ranchers raising cattle for beef and dairy, these livestock management issues also faced farmers using draft stock in arable agriculture—perhaps all the more acutely for their relative poverty and the greater difficulty they faced in coaxing a living from the parched land (Worster, 1979). Indeed, the Dust Bowl caused problems for livestock that went beyond just a reduction in the price they could fetch as beef; it also depleted farmers' capital stock and reduced the productive capacity of what remained. Dust suffocated cattle, blinded them until they fell and were buried in sand drifts, and ground their teeth down to the gums as they grazed on gritty forage (Worster, 1979). Death, ill health, and starvation were not the only factors draining a farmer's stock: once the dust drifts rose high enough to cover fences,[12] many horses and cattle simply wandered away (Worster, 1979).

As if in mockery of farmers' wasting cattle, environmental pests—including grasshoppers, Mormon crickets, cattle parasites, and jackrabbits—were thriving (Worster, 1979). Whether because the drought improved breeding conditions or drove them into the region in search of scarce grass, the population of jackrabbits in particular had "proliferated beyond imagination" during the Dust Bowl (Hurt, 1981). This jackrabbit boom was pernicious, and exacerbated the serious damage wrought by these pests under even normal circumstances: it was suggested that in a regular year, a single jackrabbit could do roughly $10 of damage to farms (for reference, roughly

[12]Of course, the obliteration of property boundaries would have caused its own set of difficulties; whether in the case of land or of livestock, it seems that the dust made defending one's property a futile proposition.

2% of per capita income in Kansas in 1930 (Bureau of Economic Analysis, 2012)), and likely more in drought years such as those of the 1930s. They destroyed crops and competed with livestock for food, with many rabbits stripping the bark off of new trees and digging crops such as alfalfa out by the roots (Kansas Emergency Relief Commission, 1936).

In one 1936 film outlining some of the progress made under various New Deal-associated projects in Kansas, it was estimated that in the short time since these jackrabbit extermination projects had begun there, over 2 million jackrabbits had been killed or captured by 98,000 people in 13 counties in Western Kansas alone (Kansas Emergency Relief Commission, 1936). Efforts like these were estimated by some cattlemen to have saved feed for 200,000 head of cattle (Kansas Historical Society, 2015).

Entire communities gathered of a Sunday to take part in these drives; they created mile-wide circles to corral the rabbits, which were then driven into makeshift pens where they were clubbed to death (Hurt, 1981). Archival video footage of these events is simultaneously absurd and chilling (Kansas Emergency Relief Commission, 1936). Successful as these drives may have been in thinning the jackrabbit population, the macabre spectacle of swinging clubs and crying rabbits would stay with some young onlookers forever (Egan, 2006). Once these culls were completed, the jackrabbits that were killed became food for human consumption (until, it seems, fear over "rabbit fever," a bacterial disease transmitted by rabbits and wild rodents, curbed this practice (Kansas Historical Society, 2015)) or were ground up for tankage and animal feed; those that were captured were sent to other states (Kansas Emergency Relief Commission, 1936).

With such a surreal set of challenges, it must have seemed to Dust Bowl farmers that they were beset by adversity on all sides. However, in light of the large-scale destruction of their livelihoods, any damage to farm capital, difficulty defending crops, and uncertainty-triggered psychological stress of the sort described above were the least of their worries.[13] Instead, and most important to the analysis presented in this thesis, farmers who may have already enjoyed low standards of living (Wallace, 1938) faced destitution during the Dust Bowl as agricultural yields, productivity, property values, and employment fell. These facts in turn would position farm children poorly: both for the Dust Bowl shock itself, as well as for recovery.

4.3.2 Methods & Data

I take two main approaches to assessing the role of agriculture in the Dust Bowl's impact on later-life wellbeing.

Firstly, I use a measure of farm population-weighted erosion to gauge the impact of the Dust Bowl through agricultural livelihoods. This strategy replaces the general population-weighted term $erosion_s$ in Equation 3.1 with the proportion of the state's farm population in 1930 living in high-erosion counties in the individual's birth state. This variable is constructed using 1930 county farm population figures and county-level erosion classifications from the U.S. Census (Hornbeck, 2012), and may be interpreted as the probability that those engaged in agriculture in a given state experienced high erosion. Thus, it serves as a measure of the severity of the Dust Bowl for agriculture-dependent communities.

Regressions using this definition of Dust Bowl exposure test the hypothesis that the Dust Bowl only mattered insofar as farmers were exposed—if a county hit by the Dust Bowl contained no farmers, it would not be considered exposed to the Dust Bowl, and would not contribute to the state's erosion level. This measure of erosion allows me to show that the effects of the Dust Bowl mattered via the income (rather than, say, only by the respiratory hazard) channel.

Although individuals' own farm status in childhood is unknown, this measure is

[13] Indeed, it is likely these factors contributed less to undermining health and human capital formation than did the massive shocks to farm income.

still helpful since even non-farmers in farm regions would have found their fortunes tied to the success or failure of agriculture; for instance, shop owners or local creditors serving a largely agricultural clientele would surely suffer from the collapse of farming incomes (Burns, 2012), while the destruction of farming livelihoods would also have an impact on local tax bases and thus public services for all in a given community, regardless of occupation (Worster, 1979).

Secondly, I interact a measure of a state's degree of dependence on agriculture with the standard measure of Dust Bowl exposure to determine how farm-dependence mediated the Dust Bowl shock.

In this approach, I create a binary dummy, farm-dependence ($farm_s$ in Equation 4.1 below), which takes the value of 1 if the individual is born into a state where the farm population as a proportion of total population in the individual's birth state is above the sample average, and 0 if below average. As with the earlier measure of farm population, this variable is constructed using 1930 county farm population and general population figures from the U.S. Census (Hornbeck, 2012). This variable is a proxy for the state's dependence on agricultural livelihoods, and is interacted with the baseline equation's standard $treated_b \times erosion_s$ term to test whether agriculture-dependence changes effect of the Dust Bowl shock. As a measure of farm-dependence, this variable serves as an alternative to weighting erosion by the farm population.

For this analysis, I add an interaction term to the baseline Equation 3.1 below as follows:

$$\begin{aligned} h_{ibst} = \alpha + \beta_1 \times treated_b \times erosion_s + \beta_2 \times farm_s \times treated_b \times erosion_s \\ + x_{ibst}{}' \psi + \theta_s + \lambda_b + \eta_t + \gamma_s + u_{ibst} \end{aligned} \tag{4.1}$$

As was discussed with respect to $treated_b$ and erosions in Equation 3.1, here the un-interacted $farm_s$ term is absorbed by the state fixed effect, θ_s.

The aim of this approach is to test how farm-dependence modified the Dust Bowl shock, for instance, by reducing the levels of compensating investment received by farming-state individuals relative to that received by their non-farming-state peers due to state-level gradients in the returns to human capital investment.

4.3.3 Results & Discussion

In Table 4.3, I report the results of tests for differences in the Dust Bowl's impact by states' farm status for men, and in Table 4.4, for women. I find that agriculture-dependence exacerbates the adverse effects of the Dust Bowl, although in many cases these farming effects are statistically insignificant.

Column (1) in Table 4.3 for men and column (1) in Table 4.4 for women present the β_1 values for regressions in which the $erosion_s$ variable in Equation 3.1 is defined as the proportion of the state's farm (rather than overall) population living in high erosion counties.

Under this specification, exposed women see a fertility reduction of 0.35 children ever born, as well as a 2.09 percentage point decrease in the probability of completing college, the latter outcome nearly twice as severe as in the baseline. Exposed women are 0.77 percentage points likelier to be living in poverty than their unexposed counterparts; exposed men, 1.27 percentage points likelier. Exposed men similarly have a 1.73 percentage point increase in the probability of physical disability, a figure roughly 50% more severe than the in the baseline.

Columns (2a) and (2b) for men in Table 4.3 and columns (2a) and (2b) for women in Table 4.4 report the β_1 and β_2 coefficients, respectively, for regressions in which a binary variable for agriculture dependence (defined as 1 where the proportion of the state's population engaged in agriculture in 1930 is above average for the sample), per Equation 4.1 above, is interacted with the original $treated_b \times erosion_s$ term.

The statistically significant adverse effects here are similar to those in column (1). I find that agriculture-dependence made the effects of Dust Bowl exposure more

Table 4.3: Impact of Dust Bowl Exposure by Mechanism: Agriculture – Men

	Farm Pop-Weighted Erosion	**Farm Interaction**	
	(1)	(2a)	(2b)
Outcomes	Treated × Erosion	Treated × Erosion	Farm × Treated × Erosion
Age at First Marriage	-0.2190	-0.1320	0.0000
	(0.2110)	(0.1350)	(0.1090)
Probability of Completing High School	0.0318	0.0090	0.0292*
	(0.0211)	(0.0108)	(0.0168)
Probability of Completing College	-0.0153	-0.0103	-0.0014
	(0.0120)	(0.0071)	(0.0050)
Welfare Income	21.47	17.29**	-8.12
	(15.2200)	(8.0930)	(16.0200)
Probability of Poverty	0.0127*	0.0103**	-0.0084
	(0.0070)	(0.0051)	(0.0089)
Probability of Cognitive Disability	0.0114	0.0041	0.0065
	(0.0107)	(0.0056)	(0.0124)
Probability of Physical Disability	0.0173***,††	0.0097***,††	0.0044
	(0.0062)	(0.0034)	(0.0106)
Probability of Vision & Hearing Difficulty	0.0010	-0.0031	0.0011
	(0.0106)	(0.0070)	(0.0065)
Probability of Self-Care & Independent Mobility Difficulty	-0.0012	0.0004	-0.0025
	(0.0061)	(0.0034)	(0.0054)

*** $p<0.01$, ** $p<0.05$, * $p<0.1$ uncorrected; † † † $p<0.01$, †† $p<0.05$, † $p<0.1$ Bonferroni-corrected; Note: Column 1 reports $treated_b \times erosion_s$ coefficients for specifications in which erosion severity is weighted by county-level farm population rather than general population. Column 2a reports $treated_b \times erosion_s$ coefficients (main effect) and Column 2b reports $farm_s \times treated_b \times erosion_s$ coefficients (interaction effect) for specifications that interact the baseline treatment term with the state's proportion of population engaged in farming. All regressions are estimated by OLS and include controls for race; birth year, birth state, and census year fixed effects; and state trends. Standard errors, clustered by birth state and birth year, are reported in parentheses below each coefficient.

Table 4.4: Impact of Dust Bowl Exposure by Mechanism: Agriculture – Women

	Farm Pop-Weighted Erosion	**Farm Interaction**	
	(1)	(2a)	(2b)
Outcomes	Treated × Erosion	Treated × Erosion	Farm × Treated × Erosion
Age at First Marriage	-0.0560	0.0241	-0.1010
	(0.1860)	(0.1110)	(0.0778)
Children Ever Born	-0.3450*	-0.1220	-0.3110
	(0.1990)	(0.0857)	(0.2140)
Probability of Completing High School	0.0473	0.0264*	0.0160
	(0.0297)	(0.0152)	(0.0294)
Probability of Completing College	-0.0209***,††	-0.0110**	-0.0023
	(0.0074)	(0.0048)	(0.0010)
Welfare Income	23.26	21.51**	-18.20
	(17.2300)	(9.5760)	(13.8800)
Probability of Poverty	0.0077*	0.0068*	-0.0080*
	(0.0043)	(0.0038)	(0.0046)
Probability of Cognitive Disability	-0.0083	-0.0033	-0.0074
	(0.0102)	(0.0039)	(0.0134)
Probability of Physical Disability	-0.0035	-0.0079	0.0208
	(0.0187)	(0.0134)	(0.0211)
Probability of Vision & Hearing Difficulty	-0.0046	-0.0032	0.0031
	(0.0084)	(0.0042)	(0.0112)
Probability of Self-Care & Independent Mobility Difficulty	-0.0022	-0.0031	0.0059
	(0.0048)	(0.0028)	(0.0060)

*** p<0.01, ** p<0.05, * p<0.1 uncorrected; † † † p<0.01, †† p<0.05, † p<0.1 Bonferroni-corrected; Note: Column 1 reports $treated_b \times erosion_s$ coefficients for specifications in which erosion severity is weighted by county-level farm population rather than general population. Column 2a reports $treated_b \times erosion_s$ coefficients (main effect) and Column 2b reports $farm_s \times treated_b \times erosion_s$ coefficients (interaction effect) for specifications that interact the baseline treatment term with the state's proportion of population engaged in farming. All regressions are estimated by OLS and include controls for race; birth year, birth state, and census year fixed effects; and state trends. Standard errors, clustered by birth state and birth year, are reported in parentheses below each coefficient.

severe (i.e. larger in magnitude), although its contribution (β_2) to the overall impact is generally smaller than the simple effect of Dust Bowl exposure (β_1), and in many cases is statistically insignificant. One notable exception is in the results for men's high school completion; here, the farm-state exposed see especially large and statistically significant gains in schooling relative to the non-farm-state exposed. This finding lends further support to the idea, first discussed in Chapter 3, that the sudden loss of opportunities for child labor in farming drove an increase in the rates of secondary schooling. This hypothesis, and the relationship between schooling and child labor in agriculture, is investigated in Section 4.4. Together, these results suggest that individuals growing up in non-farm states may have sustained a smaller loss in their pre-shock human capital endowment, been less credit-constrained and thus more able to invest in recovery, received higher rates of compensating investment to aid in their recovery from the shock, or experienced some combination of these factors, than did those in states dependent on agriculture.

As alluded to above, the more intensely adverse effects here experienced by farm-state children can be read in four main ways, complicating interpretation.

Firstly, they may be seen as a more dramatic initial insult due to the Dust Bowl shock, under the assumption that farm children were poorer and more susceptible than their non-farm counterparts (i.e., and in the language of the Heckman (2007) theoretical framework described in Section 2.3), because the first-stage (pre-shock) human capital investment was lower for these children, thus lowering their endowment at the end of the pre-shock period by a greater degree for an equivalent-sized erosion shock).[14] Fishback & Thomasson (2014) find such nonlinearities in the effects of early-life income on later-life wellbeing, although their analysis does not pertain specifically to a farming context.

[14]Because children who are more poorly endowed (e.g. in health or nutritional status) at the time of an adverse shock such as the Dust Bowl would likely find themselves on a steeper portion of the human capital production function than their better-endowed peers, they would accordingly suffer a worse blow to human capital from a same-sized shock.

Secondly, it is possible that the Dust Bowl shock here represented by erosion was actually *larger* in farming communities than elsewhere, since erosion likely undermined farming incomes more so than other livelihoods.[15] That is, for a same-sized erosion shock, the actual shock associated with the Dust Bowl (which is a function of erosion and of agriculture-dependence) may be larger for farming communities than for non-farming communities. This may be not only because the "Dust Bowl" as a shock incorporates, for the former group, both a direct health hazard component *and* an income shock component (whereas for the latter group the Dust Bowl shock consists only/primarily of direct health hazards), but also because the income shock component of the Dust Bowl shock may be considerably larger at a given level of erosion for farming communities than for their non-farming counterparts.

Thirdly, they may be interpreted, as Bhalotra & Venkataramani (2012) and Almond et al. (2009) do, as a poorer investment response in children who might enjoy low returns to human capital investments in future years as a result of the collapse of their chief industry, farming, even where damage to these children's endowments might have been equivalent to the initial damage sustained by those in less farm-dependent states.[16] Indeed, I find that those in less farm-dependent states experienced less severe adverse outcomes, consistent with the idea that they received compensating rather than reinforcing investments in human capital.[17] These findings are striking since much of the literature on human capital upholds a reinforcing-investments explanation of shock response, even while the multi-stage model of human capital formation outlined in Section 2.3 allows for the possibility of compensating investments,[18] es-

[15]Note, however, that as mentioned earlier, agriculture-dependent states likely had a larger local multiplier of Dust Bowl exposure since the economic success of non-farmers in these communities may have depended on the success of farmers. Thus, the Dust Bowl shock may adversely affect a greater proportion of the population in farming than in non-farming states.

[16]Per Bhalotra & Venkataramani (2012), who find that racial barriers in access to institutions lower returns to human capital in African-Americans, thus reducing responsive investments by parents.

[17]Alternately, it is possible to conclude that individuals in non-farm states merely had their shock reinforced to a lesser degree.

[18]Almond et al. (2009) and Bhalotra & Venkataramani (2012) find empirical evidence of reinforcing investments; Heckman (2007) and Almond & Currie (2011a) discuss the possibility of compensatory

pecially in early childhood, when these investments are most likely to be productive and efficient.[19]

Lastly, and especially pertinent to the case of the 1930s, which saw the Dust Bowl and the resultant collapse of family farming on top of the macroeconomic effects of the Great Depression, it is worth noting that households may have faced significant and binding credit constraints. As such, the failure of farm households to make compensatory investments need not be taken as evidence of a strategic decision to reinforce the lowered child endowment. Instead, low incomes may have allowed little scope for re-channeling funds intended to be spent on meager, subsistence consumption towards post-shock investments, limiting the possibility of investment in health and education even where the parental will to invest existed.[20]

investments where investments in different periods are substitutable.

[19]It is difficult to definitively support the idea of compensatory household-level investments in non-farm communities without further data on intra-household resource allocation or on differences (e.g. by child type and earning potential, local returns to human capital, etc.) in parental incentives to invest. However, even if we choose to reject the interpretation that the difference between farm-state and non-farm-state individuals' later-life outcomes is evidence of household-level compensating investment by parents, the most salient conclusion arising from this interpretation—that some degree of remediation is possible, given compensating investments—still stands, and will be discussed in Section 5.4 with reference to the effects of public expenditure on Dust Bowl-era relief programs.

Nevertheless, it should be noted that under these circumstances, the conclusion that some recovery may be possible when resources are applied to an insult says nothing about the *efficiency* of such investments—it merely concerns the biological possibility. For instance, the failure of parents to make investments in children who *do* experience recovery at the hands of state investment may be evidence that parents are not accurately able to gauge the returns to such investment. Here, it is possible that the state has superior insight over households. Alternately, it may be evidence of parental credit constraints, even where households might have preferred to respond to (and were able to correctly evaluate) investment incentives. In both of these scenarios, remediating investment may be efficient.

However, it is also possible that New Deal expenditure on recovery was actually inefficient. In this scenario, it is possible that parents both accurately estimated the returns to investment in recovery *and* had the means to fund such investment, but that they chose not to invest in recovery because such investment was inefficient, at least on a private basis. Here, if children's recovery was a political rather than an economic aim and/or an incidental consequence of large-scale expenditure on other programs, or if health was conceived as a human right (i.e. if equity concerns trumped ones regarding efficiency), the state may have chosen to invest in recovery even if it were known to be an inefficient course of action.

It is of course also possible that the public returns to remediating investments were higher than the private ones, spurring state rather than household-level recovery spending.

[20]Here, as will be discussed in Section 5.4, New Deal loans and cash transfers appear to have helped alleviate the household constraint.

4.3.4 Conclusion

In this section, I have provided evidence that the Dust Bowl, through agriculture, had strong effects on later-life human capital and wellbeing. The results presented here show that individuals exposed in more agriculturally-dependent states, or in which a greater proportion of farmers were exposed, on the whole experienced considerably worse outcomes than those less agriculturally dependent. They also show that the effects of the Dust Bowl on human capital persist even when discounting the exposure of non-farmers (i.e., when excluding those individuals whose exposure would have mattered primarily through the direct health hazards) when measuring Dust Bowl severity. I find that individuals born in states where the level of "agricultural exposure" was higher experience higher relative rates of poverty. Exposed men also experience higher relative rates of disability, while exposed women experience both lower relative rates of college completion and large relative reductions in fertility. Agriculture-dependence similarly amplified the Dust Bowl's (positive) effects on high school completion for men. Together, the findings presented in this section make a case that the income channel, more so than, say, direct dust hazards, likely drove this cohort's early-life disadvantage. By suggesting that the Dust Bowl acted fundamentally as an income shock, and clarifying the relative importance of income and other channels, this analysis contributes to our understanding of how and why the Dust Bowl impacted health.

4.4 Child Labor

4.4.1 Background

The results presented thus far show that the Dust Bowl increased the probability of high school completion for those exposed in childhood. These beneficial effects on secondary schooling may seem especially counterintuitive in light of the wellbeing reductions observed in the other later-life outcomes. For instance, one might expect the Dust Bowl's effects on college completion and high school completion to move in the same direction since both are educational outcomes and since college enrollment usually depends on high school completion, such that a rise in the latter might be expected to also result in a rise in the former.[21] Leaving aside a more holistic discussion of the contrasts in college and high school outcomes, along with their implications for understanding capability formation, for Chapter 7, the aim of this section is to examine in greater depth the beneficial and anomalous secondary schooling result. In particular, and given that child labor and schooling are often seen as competing demands on a child's time, I test how childhood schooling relates to local child labor demand in the context of the Dust Bowl's adverse agricultural shock.

[21] Of course, high school and college each place different cognitive and financial demands on individuals, which may mean that although both pertain to schooling, high school completion and college completion are in fact two fundamentally different outcomes, each driven by a distinct set of considerations. The evidence presented in Section 4.2, for instance, suggests that the sensitive periods for each outcome are rather different, with effects on high school completion appearing in those exposed later in childhood, and effects on college completion occurring much earlier—even *in utero*. These differences by developmental stage of exposure suggests that the pathways, mechanisms, and possibly even biological processes by which high school completion is affected or "produced" are distinct from those for college completion.

On a practical level, low incomes may have placed constraints on college attendance in a way that would not have been a barrier to high school attendance. This is especially likely since in this period, secondary schooling was free and widely available (Goldin, 1998), while college was both expensive and exclusive, with institutions such as Harvard charging roughly $400 in annual undergraduate tuition fees alone throughout the 1930s (Keller, 2001, p. 143), or roughly 91% of the average per capita personal income in 1930 in the 15 states in my sample (Bureau of Economic Analysis, 2012). (Although tuition at other elite institutions such as the University of Pennsylvania were similar to those cited for Harvard (Frazier Lloyd, 2003), fees at state universities within the Dust Bowl region would have been much more modest. For instance, tuition in this period at the University of Oklahoma was $3.50 per credit hour for in-state students and $5 for those from out of state. Assuming the bare minimum of 12 credit hours per semester for two semesters at in-state rates, this would amount to tuition fees totaling $84, which while being significantly lower than fees in the Ivy League, would still have represented a substantial proportion of an individual's income at the time, at roughly 23% of the average per capita personal income in Oklahoma in 1930 (Bureau of Economic Analysis, 2012; Barajas Harp, 2015, p. 52).)

More importantly, low levels of cognitive and noncognitive skills may have barred prospective students from college, which had strict admissions requirements and would have been more intellectually demanding, but not from high school, which was open to all. Indeed, in the present day, Heckman (2007) contends that such deficits in child ability are a more salient barrier to college attendance than household credit constraints.

There are two possible predictions for how child labor demand on the eve of the Dust Bowl might mediate the effects of the shock on secondary schooling: one in which regions with higher pre-shock child labor demand result in higher rates of secondary schooling, perhaps due to a sharp drop in the opportunity cost of such schooling as opportunities for child labor in farming collapse; and one in which regions with higher pre-shock child labor demand result in rates of secondary schooling no higher than in less child labor-intensive regions, either because these former regions are not hard-hit by the shock, or because they are so reliant on child labor that they cannot or will not release child labor in the face of an adverse shock to agriculture. Below, I provide new empirical evidence which, along with the evidence presented thus far in the thesis and in the secondary literature, helps shed light on which of these two scenarios appears to be at play during the Dust Bowl.

In the first scenario, the demand for child labor is sharply reduced by the Dust Bowl shock. As agriculture is destroyed, the demand for child labor in farming drops—and along with it, the opportunity cost of secondary schooling. Indeed, there are a number of facts—drawn from theory, historical context, the secondary literature, and my own results thus far—that taken as a whole suggest the plausibility of this hypothesis.

Firstly, child labor in agriculture was widespread on the eve of the Dust Bowl, meaning that there may have been scope for the destruction of farms to significantly disrupt children's time use patterns. In 1930, for instance, Whaples (2005) reports that 74.5% of U.S. boys and 61.5% of U.S. girls aged 10-15 worked in agriculture. Although legal restraints on this sort of work existed on paper, these appear to have had little effect on reducing child farm labor in practice. For instance, such work continued despite (or often, alongside) compulsory schooling laws. Clay et al. (2012) show that these laws, well in place in all states by the 1930s, could be poorly enforced, released children seasonally for work, and often had explicit exemptions for children living in poor households, those leaving school specifically for work, and those able to prove achievement of some set academic standard.[22] Child labor laws, which are generally seen as effective and even necessary complements to compulsory schooling laws, did little over the period 1880-1910 (Moehling, 1999). Although Manacorda (2006) finds a stronger link between children's legal work eligibility and schooling rates by 1920, this result is based exclusively on urban rather than rural agricultural children.

Even new Depression-era child labor laws did little to discourage child work on farms. Aimed primarily at regulating child labor in factories and urban settings, legislation such as the 1938 Fair Labor Standards Act not only arrived late in the "treatment" period under study here, but also contained generous exemptions for child labor in agriculture, particularly on family farms (United States Department of Labor, 2007, 2013). This law already contained liberal provisions for child work in agriculture for children as young as 12 (and even below), provided such work took place outside of school hours. However, stipulations such as those indicating that "minors under the age of 16 may not be employed during school hours unless employed by their parent or a person standing in place of their parent" (United States Department of Labor, 2007) effectively did away with any pretense of ring-fencing school hours, at least in family or subsistence farming contexts. Clearly, child farm labor was more a fact of life than an exceptional occurrence.

Secondly, the demand for farm labor generally, including that done by children, dropped precipitously as a result of Dust Bowl drought, wind storms, and erosion. As has been discussed in some detail in Sections 2.1.1 and 4.3.1, farms suffered widespread damage during the Dust Bowl, sustaining substantial losses in agricultural output, revenues, productivity, and land values. Accordingly, amidst the larger unemployment crisis, unemployment in agriculture rose particularly sharply. Simi-

[22]Based on these facts, a fuzzy regression discontinuity design may be appropriate for a future paper examining the relationship between agricultural child labor and schooling.

larly, amongst the employed, the share of all workers engaged in agriculture declined between 1930 and 1940, from 12.4% to 10.0% for farm owners and tenants, and 8.6% to 7.1% for unskilled farm laborers as the occupational distribution shifted in favor of higher-skill, white-collar, and service jobs (Levine, 2009). Young people suffered the brunt of the increases in unemployment, as the number of unemployed 14- to 19-year-olds rose by 255% between 1930 and 1940 (Levine, 2009). None of this is to suggest, however, that the supply of child labor was necessarily diminished. If poverty is a major driver of child labor, as has been shown in both historical and contemporary developing-country contexts (Basu & Van, 1998; Hindman, 2002; Edmonds & Pavcnik, 2005; Bhalotra, 2007; Humphries, 2010), it is likely that households needed the income generated by their children immediately during the Great Depression and Dust Bowl more than ever. However, thanks to the environmental decimation of farms, cultivation became a futile if impossible endeavor, and the opportunities for children to contribute to household subsistence were simply not there.

Thirdly, there is no clear evidence of a decline in the supply of secondary schooling during this period. Worster (1979, pp. 125-127) provides anecdotal evidence that in the especially hard-hit Cimarron County, Oklahoma, local public services were "thrown into disarray" by tax delinquency, a shrinking tax base, and changing priorities in public goods provision. Although he notes that schoolteachers' salaries fell as a result of shrinking tax revenues, he attaches a caveat to this statement by indicating that teachers "made out better than most dirt farmers" (Worster, 1979, p. 126), both because they received warrants redeemable for their salaries when tax revenues became available, and because they were able to sell these warrants at a discount to gain ready access to cash. There is no indication whether, on a larger and more systematic scale, such disruptions led to teacher layoffs or school closures; indeed, Worster (1979) credits the influx of federal New Deal funding for saving Cimarron County from further such interruptions to services. Thus, it is unclear to what degree school availability and quality suffered during the Dust Bowl throughout the region under study. What is clear is that the demand for schooling remained and perhaps even grew. Egan (2006, p. 99), for instance, provides anecdotal evidence that the lack of profitable farming work freed children up for other pursuits, namely, school. These children, particularly teens, reasoned that they may as well attend school since they no longer had anything better to do with their time. This sentiment in turn had the potential to raise both intensive and extensive margins of school attendance.

Fourthly, the findings presented so far in this study also support this interpretation of the drivers of the high school result. Firstly, the Dust Bowl's beneficial effects on high school completion are largest and most significant among those exposed later in childhood, at working-age/school-age: as shown in Section 4.2, it is the age range of 7-12, when children may plausibly have traded time in school off for time spent on the family farm, that appears to have been a sensitive period in the production of secondary schooling outcomes. This stands in contrast to other outcomes, including college completion, which manifest earlier in childhood and even prenatally, and which thus are likelier to result from developmental complications than, say, labor market conditions. Secondly, the effects are larger for farm-state individuals: as was observed in Section 4.3, being born in a farm state exacerbates the Dust Bowl's effects on later-life outcomes—of particular interest here, leading to even sharper increases in an individual's probability of high school completion than those seen in the baseline analysis. Together, these results make a reasonable case for a link between farm labor opportunities and secondary schooling.

Lastly, the secondary literature shows that this phenomenon, wherein adverse agricultural shocks result in better schooling outcomes for children, is not unprecedented in other historical contexts. For instance, Baker (2015) finds that the advent of the boll weevil, an agricultural pest attacking cotton, reduced the demand for child labor in cotton production, and so raised the rate of school enrollment among African-

American children in 1909-1922 Georgia. Similarly, Greenbaum (2009) shows that the presence of the boll weevil (along with other factors weakening the demand for child labor in cotton, such as the strengthening of child labor laws and an overall decline in and increasing mechanization of cotton production) resulted in intensive (i.e., higher school quality) and extensive (i.e., higher educational attainment) improvements in schooling in cotton-intensive regions. Both studies find that these adverse shocks to cotton production had the beneficial result of narrowing racial inequalities in the U.S. South. Of particular relevance to the analysis presented in this section, however, these studies show clearly how child-labor-intensive agricultural production can suppress schooling, particularly in poor communities.[23]

Together, these facts—particularly those results suggesting larger spikes in high school completion in farm states, and those locating the largest high school completion gains amongst individuals exposed to the Dust Bowl in later childhood—build a circumstantial case for a mechanism by which a fall in the demand for child labor during the Dust Bowl incentivized and freed up time for secondary schooling. If this

[23]Although these historical studies focus on commercial or wage-based farming, there are many studies in developing countries which link the demand for children's work on family farms to schooling outcomes. For instance, in another of my studies, Arthi & Fenske (2016), I find that for children in late 1930s Nigeria, schooling is explicitly de-prioritized relative to work on the family farm; as a result, few children in the sample attend school. Moving from historical to contemporary Africa, I find that in Tanzania, family farm labor remains extremely prevalent in school-age children: on average in households surveyed weekly and in person that 86% of girls and 91% of boys between the ages of 10 and 19 worked in family farming at some point in the season, with many of them doing so regularly either consistently throughout the season or in multi-week blocks several times during the season. In many cases, this precludes children's school attendance altogether: in this same sample, only 47% of all girls and 59% of all boys aged 10-19 report being in school in the endline; of those ever engaged in family farm labor, the figures are 49% and 59 %, and in weeks they are engaging in own-household agricultural labor, their chances of reporting that they attended school are 0.2% and 0.4%, respectively (Arthi et al., 2016b). This suggests that agricultural shocks may increase schooling on both intensive and extensive margins, although the latter may be likelier for older children. Additionally, it is worth noting that the obligation to work in family farming may have been a stronger force keeping children out of school than even the cash incentives to work in hired agricultural labor: Bhalotra & Heady (2003) find that children in land-rich households (i.e. children in households with farms and so a greater demand for household farm labor) in Pakistan are more likely to work on their family's farm, less likely to attend school, and, attend fewer hours of school conditional on attendance, relative to their land-poor peers. It seems likely, then, that in a region like the Great Plains where children were more active in family farming than in paid agricultural labor, it might take the complete destruction of these farms to free up children's time for schooling.

"opportunity cost of schooling" mechanism holds, we would expect children born in places where child labor was more intensively used on the eve of the Dust Bowl to see the sharpest rises in schooling.

However, this is only one possible way in which child labor demand may modify the Dust Bowl shock. In the second scenario, it could be the case that the Dust Bowl does not much change the demand for child labor. Here, regions with higher pre-Dust Bowl child labor demand might be expected, given the Dust Bowl shock, to have secondary schooling outcomes that are no better than their Dust Bowl-stricken counterparts in lower child labor demand regions—whether because the Dust Bowl shock was less severe (i.e. did not completely decimate agriculture) in the regions with the highest demand for child labor, or because those regions which were very reliant on child labor were motivated to hold onto this labor despite the Dust Bowl.

Indeed, in the new empirical results presented in this section, I find support for the second scenario described above, in which places which use children intensively in farming continue to do so in the face of an adverse agricultural shock. It appears that children exposed to the Dust Bowl in regions with higher child labor demand prior to the Dust Bowl are on the whole less likely than the Dust Bowl-exposed cohort in less child labor-intensive regions to have completed high school, albeit not statistically significantly so. Put another way, pre-Dust Bowl child farm labor demand does not seem to drive the beneficial high school completion effects of Dust Bowl exposure observed elsewhere in this thesis, results that instead may be a compositional artifact of a sample in which small-scale family and subsistence farming dominates large-scale wage labor in cotton production, or may stem from correlated income effects. Nevertheless, I find evidence that tradeoffs of the like outlined in scenario one above, between child farm labor and schooling, existed for non-white children, who were historically most likely to be engaged in such labor. These results, for contrast, accord with the logic of the changing-opportunity-cost hypothesis presented earlier in this thesis as well as with the secondary literature, and suggests that race may be the more interesting story in future investigations of the knock-on effects of agricultural blight in the U.S.

Thus, the totality of the evidence presented in this section, earlier in this thesis, and in the larger literature—is mixed on the issue of how child labor in agriculture plays a role in secondary schooling and in children's time use tradeoffs. Specifically, it suggests heterogeneity between the regions in which child labor in agriculture appears to be less intensive, more responsive to shocks, and centered on family farms; and the regions in which child (wage) labor is an essential and intensively used input to the production of cash crops.

4.4.2 Methods

To test for gradients in secondary schooling by the degree to which child farm labor was used prior to the Dust Bowl, I take two main approaches.

4.4.2.1 Long-Term Analysis

The first approach builds on the standard baseline approach, and interacts the $treated_b \times erosion_s$ term with a continuous measure of the statewide demand for child labor in agriculture as follows:

$$\begin{aligned} h_{ibst} = \alpha + \beta_1 \times treated_b \times erosion_s + \beta_2 \times childlabor_s \times treated_b \times erosion_s \\ + \beta_3 \times childlabor_s \times treated_b + x_{ibst}{}' \psi + \theta_s + \lambda_b + \eta_t + \gamma_s + u_{ibst} \end{aligned} \quad (4.2)$$

The variable $childlabor_s$ takes on three alternative definitions as outlined below in Section 4.4.3, and proxies the demand for child labor in agriculture in the individual's state of birth. Briefly, these are two alternative indices of child labor demand in agriculture based on the exogenously given crop endowment, and a measure of mechanization in agriculture. All other variables and features of the specification are as given in the baseline Equation 3.1: $treated_b \times erosion_s$ is the measure of Dust Bowl exposure; x_{ibst} represents a vector of controls; and θ_s, λ_b , and η_t represent birth-state fixed effects, birth-year fixed effects, and census-year fixed effects, respectively, while γ_s represents state trends; and standard errors are clustered by birth state and birth year. As elsewhere in this study, the (effectively time-invariant) state-level variable $childlabor_s$ is excluded, since it is absorbed by the state fixed effect.

There are a number of reasons why non-white children may be expected to have been especially active in child labor, including higher levels of household poverty and lower returns to investment in human capital due to institutional barriers (Bleakley, 2007; Bhalotra & Venkataramani, 2012). Indeed, the secondary literature is rife with evidence of differentials by race in rates of child labor in farming, particularly in the Cotton South (e.g. Greenbaum, 2009; Baker, 2015). As such, while Equation 4.2 controls for race, in Equation 4.3 I take the additional step of interacting the child's race with the treatment terms to determine whether race modifies the effect of living in regions with a higher demand for child labor. That is, this analysis contains the quadruple-interaction of interest (that of race with child labor intensity with the usual spatial and temporal variables on Dust Bowl exposure), along with all relevant triple and double interactions. Thus, Equation 4.3 is estimated as follows:

$$\begin{aligned} h_{ibst} = \alpha + \beta_1 \times treated_b \times erosion_s \\ +\beta_2 \times nonwhite_{ibst} \times childlabor_s \times treated_b \times erosion_s \\ +\beta_3 \times childlabor_s \times treated_b \times erosion_s + \beta_4 \times nonwhite_{ibst} \times treated_b \times erosion_s \\ +\beta_5 \times nonwhite_{ibst} \times childlabor_s \times treated_b + \beta_6 \times childlabor_s \times treated_b \\ +\beta_7 \times nonwhite_{ibst} \times childlabor_s + \beta_8 \times nonwhite_{ibst} \times treated_b \\ +\beta_9 \times nonwhite_{ibst} \times erosion_s + \beta_{10} \times nonwhite_{ibst} + \theta_s + \lambda_b + \eta_t + \gamma_s + u_{ibst} \end{aligned} \tag{4.3}$$

Here, β_2 is the coefficient of interest, with these coefficients reported in Table 4.8. Since non-white children are likelier to have been engaged in farm labor, non-white children living in regions where the demand for child labor was stronger might have, under the "opportunity cost of schooling" scenario outlined in Section 4.4.1, have faced a sharper drop in the opportunity cost of secondary schooling when the Dust Bowl struck. The results presented in Section 4.4.4 support this prediction.

4.4.2.2 Short-Term Analysis

The second empirical approach tests for the contemporaneous or short-term schooling response to the Dust Bowl as mediated by the local demand for child labor. In particular, this analysis investigates the effects of Dust Bowl exposure and child labor demand on a child's probability of school attendance as reported in the 1920-1940 U.S. Censuses. The benefit of using data on the short term is that they allow for analysis to move to the more granular county level, since county of enumeration is listed and can plausibly be used as the "county of exposure" to Dust Bowl conditions and local child labor intensity in childhood.

I use the county-level analysis as the primary approach to testing short-term educational outcomes. The basic specification used in this analysis is as follows, and as before, is estimated by OLS, separately for girls and boys:

$$\begin{aligned} a_{ibct} = \alpha + \beta_1 \times treated_t \times erosion_c + \beta_2 \times childlabor_c \times treated_t \times erosion_c \\ +\beta_3 \times childlabor_c \times treated_t + {x_{ibct}}'\psi + \theta_c + \lambda_b + \eta_t + \gamma_c + u_{ibct} \end{aligned} \tag{4.4}$$

The variable a_{ibct} is a binary dummy that takes the value of 1 if the child reports attending school in the given reference period.[24] In contrast to the schooling outcomes considered thus far—i.e., high school completion and college completion—this variable

[24]For details of this variable's construction, see Section 4.4.3.

allows me to test the contemporaneous effects of the Dust Bowl on education since it represents an intermediate or short-term schooling choice.

$Treated_t$ is a binary dummy which takes the value of 1 if census year is 1940 since it is the children enumerated in 1940 whose school attendance behavior is likely influenced by exposure to the Dust Bowl in the several years prior.

$Erosion_c$ is an area-weighted measure of erosion severity in the child's county of birth, and represents the proportion of the county area deemed high erosion.

As above, the variable $childlabor_c$ represents the demand for child labor in agriculture in the child's county of birth.

Here, the controls contained in the vector x_{ibct} include race, household size, and age. By controlling for household size, I account for likely differences by household size in poverty and the supply of child labor. By controlling for age, I account for the fact that younger children may be more likely to attend school than older children, whose time may have been allocated in other activities, including farm labor.

θ_c, λ_b, and η_t represent birth county fixed effects, birth year fixed effects, and census year fixed effects, while γ_c represents linear county trends. Standard errors are clustered at the birth county level.

In further regressions concerning school attendance, I also include interactions with race as above in Equation 4.3, as well as interactions of the relevant Dust Bowl exposure and child labor terms with the child's household farm status (taken from the IPUMS variable "FARM", which classifies a household as a farm household if any household member works in agriculture). I also vary the set of controls used, the definition of the child labor demand variable used,[25] and the definition of the school

[25]The proxies for the demand for child labor used in this study are discussed in detail in Section 4.4.3. I use the index of labor intensity of agricultural production in all five of the top crops (corn, wheat, oats, hay, and cotton) as the primary measure of the demand for child labor. Alternate proxies of the demand for child labor include an index of labor intensity of agricultural production based on the region's chief crop (where, as with the index based on all five crops' labor requirements, a higher value indicates a greater labor requirement and thus a greater demand for child labor) and a measure of mechanization in agriculture defined as the value of all farm capital per member of the farm population (a measure for which a higher value indicates more capital-intensive agricultural

attendance variable.[26]

In this analysis using county-level measures of Dust Bowl exposure and child labor demand (in contrast to in the other contemporaneous Dust Bowl analyses pursued elsewhere in this thesis, which use the 1920-1950 censuses), I restrict the data to the 1920-1940 censuses, since the county information in the 1950 census is incomplete and does not allow for comprehensive matching with county-level child labor demand variables.[27]

Here, I treat individuals as if the county of enumeration is also the county of birth.[28] This method of assigning the birth county assumes childhood migration did not take place, an assumption that is a relatively conservative one since the 1930s saw some of the lowest rates of migration in the region in the 20th Century Rosenbloom & Sundstrom (2004), and one that may be justified since it enables county-level analysis that better reflects the local conditions determining schooling behavior. In particular, since individuals in this analysis are observed in childhood/early adolescence rather than in later life (that is, a much shorter period after birth), this assumption is more reasonable here than it would have been in the baseline analysis. Indeed, it appears

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production and thus theoretically a lower demand for child labor). For the indices of labor intensity in agriculture, the expected sign of the β_2 coefficient in Equation 4.4 is positive since a higher pre-Dust Bowl demand for child labor should imply a sharper drop in the opportunity cost of schooling and thus a sharper increase in school attendance. When using the mechanization measure, since higher demand for child labor is represented by lower values of the mechanization variable, β_2 should be negative.

[26]In this analysis, the main definition used is school attendance where all those missing responses to the schooling question are assumed to have not attended school in the reference period. The alternate definition treats missing values as missing, and so drops any individual who did not provide an explicit "yes" or "no" answer to the school attendance question.

[27]In order to maintain parallelism with the overall study, I also perform this analysis using state-level Dust Bowl exposure and child labor demand variables and individual-level school attendance data from the 1920-1950 U.S. Censuses (not reported). In this analysis, all variables which in Equation 4.4 are given at the county level are instead given at the state level; e.g. area-weighted birth-county Dust Bowl severity becomes population-weighted birth-state Dust Bowl severity, birth-county fixed effects become birth-state fixed effects, and linear birth-county trends become linear birth-state trends. Unlike the single-clustered standard errors in Equation 4.4, here standard errors are clustered as in the baseline by both birth state and birth year.

[28]Thus, "county of birth" in the equation above in actuality refers to the county of birth as assumed from county of enumeration, for all children born in and subsequently enumerated in one of the 15 states in my sample.

that reasonably few of these children moved during childhood: only 18% of these children were enumerated in a state other than their birth state.[29] Thus, barring the possibility that children may have moved counties within the state of birth,[30] it seems that the assumption that the enumeration county is also the birth county is plausible.[31]

4.4.3 Data

4.4.3.1 Child Labor Intensity

I take two approaches to measuring how intensively child labor was used in agriculture across the states in my sample. The first, which operates on the principle that child labor is more likely to be used in the cultivation of labor-intensive crops (since child labor generally tends to be used more intensively in more labor-intensive and unskilled industries (Dinopoulos & Zhao, 2007)), is to create an index of the labor intensity in agriculture using data on labor intensity by crop and data on crop suitability. The second, based on the idea that regions which are less capital-intensive in their agricultural production may still need to rely on child labor in order to keep labor

[29]Roughly 52% of these movers (or roughly 9% of the total children in the IPUMS 1920-1950 sample born in the 15 states in my sample) were enumerated in a state other than their state of birth, but still within my 15-state sample; these children remain in my county-level analysis, although for them, the county exposure values assigned are explicitly known not to be those of the county of birth. Since I am concerned with children who spent the entirety of their childhood in my sample region, rather than restricting my analysis to Great Plains-born never-movers, I choose not to exclude the children described above from the analysis since they may represent a selected sub-set of Great Plains-born children. Of course, it is possible to conduct this same analysis for just those children born in the 15-state sample and who were enumerated in the state of birth, but given the negligible number of children for whom this set of assumptions assigns the incorrect birth-county information, and given the lack of knowledge about when during childhood the move took place, this approach is likely reasonable.

[30]This issue of intra-state migration might be problematic in states where the spatial distribution of erosion, or of the demand for child labor, was especially unequal, and may contribute to measurement error attenuating the coefficients reported here.

[31]The interaction of childhood migration status in several specifications reported in Section 4.4.4 also allows me to separate effects by individuals for whom this county-of-birth assignment represents mis-measurement of Dust Bowl severity and child labor intensity from those for whom the assumption is plausible.

costs down, exploits the level of mechanization in local agriculture.[32]

Crop Suitability & Labor Intensity

To construct the first of these spatially-varying measures of the likely pre-Dust Bowl intensity of child labor use in agriculture, I combine spatially-invariant data on the labor intensity associated with the production of several key crops in the U.S. in the 1930s with spatially-varying data on agro-climatic crop suitability for each of these crops.

I focus on five main crops: wheat, corn, cotton, oats and hay. Together, these crops represent 72% of the dollar value and 85% of the acreage of all crops in 1929 (Baker & Genung, 1938).[33] For wheat, corn, cotton, and hay, I calculate the man-hours per unit output from the 1925-1929[34] averages of man-hours per acre harvested and yield

[32]Although the assumption that places with a higher exogenously endowed labor intensity in agriculture ought to be most prone to mechanize in order to conserve on labor is theoretically fair, this logic does not appear to hold in practice in this context. Table 4.5 shows a strong negative correlation at the state level between the level of mechanization and the level of labor intensity in agriculture as given by the exogenous crop endowment. It also shows a strong positive correlation between state-level mechanization and economic activity as measured by retail sales per capita. (Correlations at the county level are extremely weak amongst all variables in Table 4.5.) While this set of correlations says nothing about the direction of any possible causal relationship (i.e. it makes no claim as to whether mechanization leads to strong economic performance, or whether richer states could afford to mechanize), it is suggestive in light of the negative relationship between labor demands in agriculture and mechanization, that mechanization may have more to do with the absence of credit constraints than with the need to conserve labor. Accordingly, it may be most reasonable in this context to interpret mechanization as a sign that the region currently has little demand for child labor, whatever the reasons for its initial drive towards mechanization; rather than as a sign that the region has a high demand for child labor, hence its attempts at mechanization (whether or not sufficient to obviate the need for child labor).

[33]The remaining proportion of total crop value and crop acreage are highly fragmented, and include vegetables, potatoes, tobacco, apples, oranges, barley, sorghums, dry beans, sweet potatoes, grapes, peaches, sugar beets, lemons, strawberries, flaxseed, mixed grains, and rice (Baker & Genung, 1938).

[34]I use the labor intensity by crop in 1925-1929 so as to capture the production technology available and likely demand for child labor on the eve of the Dust Bowl. Reassuringly, for the five crops I examine, the labor requirements in cultivation remain remarkably constant during the Dust Bowl period (United States Census Bureau, 1975). Labor and capital requirements can, of course, change over time endogenously in response to technological advances and changes in relative factor prices. Indeed, agricultural production had become much less labor-intensive over the late 19th and early 20th Centuries. For instance, the labor requirements in wheat production in the U.S. (measured in man-hours per 100 bushels) fell by roughly 80% between 1800 and 1925-1929, and following a plateau during the 1930s, would continue to fall sharply again beginning in the 1940s (United States Census Bureau, 1975).

per acre given in United States Census Bureau (1975).[35] For oats, I calculate the man-hours per unit output from Cooper et al. (1916), which is based on production averages from 1902-1912.[36] For all five crops, price per unit output in 1929 is taken from United States Census Bureau (1975).[37] Following Fouka & Schlaepfer (2014), I then calculate the labor share of income for each crop, relative to the labor share of income in corn cultivation;[38] those crops for which the relative labor share of income is higher are considered more labor-intensive. By this calculus, cotton—widely recognized as a tremendously laborious crop, and one in which child labor was commonly used (Greenbaum, 2009; Baker, 2015)—was clearly the most labor-intensive of the five staple crops considered here, at over twice the labor-intensity of corn production, and roughly 3.6-4.5 times the labor intensity of producing hay, wheat, and oats.

For each, I then calculate a measure of local crop suitability using gridded data from the FAO-GAEZ project (Food and Agriculture Organization of the United Nations, 2012),[39] which provides grid-cell-level data on the agro-climatic suitability for each crop.[40] This index of suitability for cultivation by grid cell combines information

[35]Series K 445-485 in United States Census Bureau (1975, pp. 500-501) gives the man-hours per acre harvested and yield per acre for corn for grain, cotton, hay, and wheat.

[36]In contrast to the figures for corn, wheat, cotton, and hay, which were developed from national data, the labor requirements figures for oats given in Cooper et al. (1916) are estimated based on a sample of Minnesota farms; for further details of sampling strategy and representativeness, see Cooper et al. (1916).

[37]The figures used come from Series 502-516 in United States Census Bureau (1975, pp. 510-512), and are the "census figures" given for corn for all purposes, all wheat for grain, oats for grain, and hay. For cotton, appearing in Series 550-563 United States Census Bureau (1975, pp. 517-518), the census figure version of the data is not given, so the non-census figure has been used, with prices calculated for bales of gross weight 500 lb.

[38]Calculating relative labor share of income between crops rather than the absolute labor share of income for each crop obviates the need for detailed wage data, as the wage term drops out altogether when the calculations are made relative. Thus, in the absence of more detailed data on both wages and task-level man-hours breakdowns by crop and region, this approach rests on the assumption that wages are the same within and across crops—that is, in all types of agricultural tasks, across all crops, and across all regions.

[39]These data are frequently used in modern-day studies of developing countries, and have also been widely used in a variety of historical studies including Nunn & Qian (2011) and Durante (2010).

[40]FAO-GAEZ provides data on maize (corn), wheat, oats, cotton, and alfalfa (which I use as my source of hay suitability; alfalfa is one of the primary sources of American hay, and although other sources of hay in the U.S. include plants such as timothy and clover (Cooper et al., 1916), the

on a variety of agro-climatic characteristics, including soil quality and temperature. Since the data are based on low-input rain-fed agriculture in the absence of irrigation and other technological intervention, they should be considered the cell's plausibly exogenous agricultural "endowment" of the crop in question.[41] I then assign each county[42] the crop suitability value associated with its centroid. For easier comparability, and since FAO-GAEZ suitability scales (i.e. yield potential values) are different for each crop, I divide the county's suitability for a given crop by that crop's maximum suitability value in the sample, such that each county's suitability runs on a scale of 0 to 1, where 1 is the maximum suitability for that crop in any of the counties in the 15 states in the sample.

The labor intensity index based on all five crops[43] is then constructed as the sum of each of the five crops' relative labor intensity weighted by that crop's contribution toward the county's total standardized crop suitability.[44] The resulting county suitability index is then divided by the maximum value for the county index in the sample, such that all counties have a labor intensity value which runs from 0 to 1. As an alternate measure of labor intensity, I also construct an analogous index of labor

FAO-GAEZ database does not provide crop suitability estimates for these.

[41]Note that my analysis considers only the exogenous distribution of crops over space, not the actual crop production take-up or realized yields by region. The latter would of course be endogenous to considerations such as the cost of labor and capital inputs, global commodity prices, etc.

[42]I use 1929 historical county boundaries, in order to remain consistent with the labor requirements and price data.

[43]The data available for each of the measures necessary for the calculation of the county-level index of labor intensity are not strictly parallel, and as such do not allow me to consistently account for differences in crop varieties (e.g. spring wheat vs winter wheat, tame hay vs wild hay, alfalfa vs timothy seed vs clover seed) or purpose (e.g. corn for fodder vs corn for silage vs corn for grain). As such, in each stage of the calculation I use the best figures available, even while these may not always be defined with perfect consistency.

[44]The total standardized crop suitability is the simple sum of each of the five crops' standardized suitabilities. Note that here, each crop's relative contribution to total standardized suitability should not be construed as a comparison between crops in the sense that I am not comparing whether a county is better at producing hay than it is at producing oats—say, in terms of caloric yields or dollar value of output. Rather, what each crop's contribution to the county suitability value does is to suggest how much better a county is at producing each crop, relative to all other counties' suitability in that crop. Thus, each crop's contribution to the total standardized suitability value establishes, say, continuing the hay vs oats example above, how much better at producing hay a county is relative to all other counties' hay suitability than it is at producing oats relative to all other counties' oat suitability.

intensity in which only the labor intensity of the county's chief crop (that is, the crop in the county with the highest standardized suitability value) is taken into account.

In order to facilitate comparability with the baseline analysis, which is conducted at the state level, I also construct an analogous state-level crop suitability index based on area-weighted averages of county-level crop suitabilities.

Mechanization in Agriculture

As a second proxy for the demand for child labor in agriculture, I use the level of mechanization in farming on the eve of the Dust Bowl. The logic of this approach is that those counties that are more highly mechanized should use less child labor in their agricultural production. This approach is valid from both the supply and the demand sides of the child labor question. On the child labor supply side, if we believe that poverty compels child labor (Basu & Van, 1998; Edmonds & Pavcnik, 2005) on small family farms such as those of the Great Plains, then the very communities that rely on child labor for subsistence are those that are also most likely to be credit-constrained, thereby limiting their ability to invest in farm capital such as machinery and livestock. On the child labor demand side, if we believe that the choice to mechanize is dictated by the desire to conserve on labor (say, because cultivation is particularly labor-intensive, labor is expensive,[45] and capital is cheap), then those communities that mechanize may demand less farm labor more generally, including child farm labor.[46]

I calculate a county's level of mechanization per capita as the sum of the value of all farm capital (i.e., the value of farm machinery and equipment averaged across the known 1920 and 1930 values, and the value of livestock from 1920, all in 1910 dollars)

[45] Hornbeck & Naidu (2014), for example, show that farm mechanization in the American South was a response to labor scarcity arising from black out-migration following the Great Mississippi Flood of 1927.

[46] In his survey of child labor in American history, Hindman (2002) provides examples of occupations such as coal mining, glass manufacture, and newspaper sales, in which child labor substituted for, and was eventually replaced by, machinery.

divided by the farm population in 1930, using the data given in Hornbeck (2012). As with the labor intensity measures described above, I also create state-level versions of the mechanization variable to enable analysis at the state level, per the baseline approach.

4.4.3.2 Dust Bowl-Era Schooling

Lastly, I take data on contemporaneous schooling outcomes from the 1920-1950 1% samples of the U.S. Census (Ruggles et al., 2010). Here, the outcome variable of interest is individual-level school attendance, taken from the IPUMS variable "SCHOOL".[47] I keep individuals aged 5 to 20, inclusive,[48] and create two versions of the school attendance variable. In one version, I treat all missing responses to the attendance question as if the child was not attending school in the specified period,[49] and in another version, those values remain missing and the children with missing values are thus dropped from the analysis. A drawback of this analysis is that the reference period for assessing school attendance is rather short, and ranges from 1 month (in 1940) to 6 months (in 1930). As such, the reference period may reflect the seasonality of the agricultural cycle, as well as potentially be too short to capture consistent schooling patterns if the individual's schooling patterns during the reference period are different from those earlier in the years, or from those over the past several years—a particular concern for the 1940 census, which occurs at the tail end

[47]Each of the relevant census waves asks about school attendance during a slightly different reference period, due to differences in each wave between the census date and the reference date; in 1920, respondents were asked if they attended or were enrolled in school in the past 4 months; in 1930, 6 months; in 1940, 1 month; and in 1950, 2 months. Census enumerators asked individuals aged 5-21 the school attendance questions, but also recorded affirmative answers for those outside these ages who explicitly reported attending school (Ruggles et al., 2010).

[48]This age range is chosen since it nests within the age range for which census enumerators would have asked all respondents the school attendance question. Since this age range coincides with typical ages for primary and secondary schooling, and since the enumeration methodology suggests that there was some expectation that individuals within this age range would attend school, a negative answer to this attendance question should constitute a meaningful comment on non-attendance.

[49]The soundness of this approach is supported by the enumeration methodology outlined in Ruggles et al. (2010).

of (rather than at the peak of) the Dust Bowl and so may fail to capture spikes in school attendance that coincide with the worst agricultural losses.[50] Thus, alternative measures such as the number of months spent in school in the past year, or the highest grade attained, might give a better sense of patterns in schooling than school attendance, which serves as more a snapshot of schooling at a point in time. However, these more precise and descriptive measures of educational patterns are only available in censuses from 1940 on, and so do not allow for pre-Dust Bowl comparisons. As such, school attendance may be the best available variable in large-scale and comprehensive data to test for short-term schooling responses. In order to control for as well as to test for the shock-modifying effects of individual and household characteristics, I also use the IPUMS data to create individual-level variables such as race, sex, age, childhood migrant status, household farm status, and household size.

4.4.3.3 Interrelationships: Child Labor Demand, Economic Conditions, and Erosion

To validate my approach to measuring the demand for child labor on the eve of the Dust Bowl, I calculate correlations between the indices of labor intensity and the variables concerning the value of farm capital, which stands in for the degree of mechanization in agriculture. I also include correlations between these variables and my primary measure of Dust Bowl severity, Dust Bowl-era erosion, as well as economic conditions on the eve of the Dust Bowl, as represented by retail sales per capita in 1929.[51] These correlations are presented at the state and county levels in Table 4.5.

[50] Although differences by census waves in the enumeration methods should be accounted for in the census fixed effects, the shortness of the reference period in 1940, the year in which respondents would be deemed treated by the Dust Bowl in this analysis, and this census's position at the tail end of the Dust Bowl, may mean that enrollment probabilities drawn from this census might be underestimates of children's actual enrollment patterns over the entire Dust Bowl period, or during its peak.

[51] Here, retail sales per capita is used in place of personal income per capita since the latter is not readily available at the county level.

Table 4.5: Correlation Between Labor Intensity in Agriculture & Degree of Mechanization

	State Level					**County Level**				
	Farm Capital PF	Labor Intensity, All Crops	Labor Intensity, Chief Crop	Erosion	Retail Sales PC, 1929	Farm Capital PF	Labor Intensity, All Crops	Labor Intensity, Chief Crop	Erosion	Retail Sales PC, 1929
State Farm Capital Per Farm Pop	1.0000	-0.7795	-0.5320	-0.1526	0.7750					
State Labor Intensity, All Crops	-0.7795	1.0000		0.2095	-0.7136					
State Labor Intensity, Chief Crop	-0.5320		1.0000	-0.2702	-0.5069					
State Erosion	-0.1526	0.2095	-0.2702	1.0000	0.1473					
State Retail Sales PC, 1929	0.7750	-0.7136	-0.5069	0.1473	1.0000					
County Farm Capital Per Farm Pop						1.0000	0.0747	0.0967	-0.0165	0.0335
County Labor Intensity, All Crops						0.0747	1.0000		0.0086	-0.0877
County Labor Intensity, Chief Crop						0.0967		1.0000	-0.1577	-0.0498
County Erosion						-0.0165	0.0086	-0.1577	1.0000	-0.0252
County Retail Sales PC, 1929						0.0335	-0.0877	-0.0498	-0.0252	1.0000

From Table 4.5, it is clear that at the state level, the level of mechanization in agriculture and the labor intensity of agriculture are highly negatively correlated, indicating that those states in which there were high labor requirements in agriculture may have indeed resorted to child labor to meet these requirements, rather than substituting farm capital for labor. At the county level, however, the correlation is positive and extremely weak. High exogenously-given labor requirements in agriculture seem unlikely to have driven the level of mechanization; instead, the strong and positive association between mechanization and retail sales in 1929 at the state level suggests that either richer states may have faced fewer financial constraints to investment in farm capital, or that higher rates of mechanization in agriculture contributed to their relative economic success. In either case, it seems reasonable to interpret the mechanization variable not as an endogenous response to high labor demands, but rather, as an indicator of lower current demands for labor more generally, and thus also for child labor, in agriculture. Higher values of the crop-suitability-based measures of child labor demand, for contrast, should be interpreted as raising the pre-Dust Bowl demand for child labor, particularly given their state-level association with lower levels of mechanization and poorer local economic performance—the latter of which is often associated with child labor prevalence (as in studies such as Edmonds & Pavcnik 2005; Bhalotra 2007 and Humphries 2010).

In all cases, erosion seems to have little systematic relationship with levels of mechanization or with the labor requirements in farming. This suggests that it is not the case that places that had higher child labor demand in agriculture just so happened to be untouched by the Dust Bowl. Specifically, it seems unlikely that cotton regions which relied heavily on child labor in agriculture continued to rely on child labor during the Dust Bowl exclusively because cotton fields were not adversely affected by the shock.

Furthermore, the finding of little correlation between Dust Bowl erosion and child labor intensity is especially reassuring in light of debates in the literature, touched upon in Chapter 2, over whether Dust Bowl erosion may have been endogenous. Proponents of the plausible endogeneity of erosion posit that Plains farmers may have pursued irresponsible and expedient cultivation practices because of settlement policies that encouraged suboptimal farm size (as in Hansen & Libecap (2004)) as well as credit constraints that restricted their ability to scale up production and to mechanize. Although it certainly appears from Table 4.5 that poorer regions (regions with lower per capita retail sales on the eve of the Dust Bowl) were less likely to be highly mechanized in agriculture, there is little relationship between economic performance and erosion. Furthermore, given the historical prevalence of machinery-pooling schemes by which small farmers in this and agriculturally similar regions shared equipment in order to enjoy economies of scale (Olmstead, 1975; United States Department of Agriculture, 1989; Bogue, 2001; Landis, 2001; Stofferahn, 2004; Kimmel, 2007), it is possible that the actual use of farm capital was greater than these mechanization figures would suggest, even in—and perhaps especially in—poorer regions.[52] As such, on the basis of the evidence presented here, at least on the county and state levels, if not at the individual farm level, it seems not to be the case that poverty and low levels of farm mechanization created erosion.

4.4.4 Results & Discussion

Changes in the chances of high school completion in the long term, or of contemporaneous school attendance in the short term, could reflect schooling changes on either or both of the extensive and intensive margins. For instance, if agricultural collapse led to the collapse of child farm labor demand with it, this may have led children who were previously un-enrolled to attend school, just as it may have pushed students already intermittently attending to attend more consistently and/or for a longer pe-

[52]Since equipment-pooling is unlikely to have occurred across county or state lines, however, this is unlikely to pose a threat to accurate measurement of county and state mechanization.

riod. Conversely, children's schooling patterns may not have changed substantially if the Dust Bowl drove children off of farms and into schools only where child labor was less intensive and thus more expendable.

In this section, I test for gradients by pre-Dust Bowl child farm labor demand in long- and short-term schooling outcomes amongst individuals exposed to the Dust Bowl as children. I also examine whether the effects on schooling are more pronounced in non-white and farm children, that is, children who at any level of child farm labor demand may be likelier to engage in such work.

4.4.4.1 Long-Term Analysis

The results of the long-term analysis described above, which expands on the baseline model by interacting the Dust Bowl exposure term with measures of pre-Dust Bowl child labor demand in agriculture, are presented in Table 4.6 for men, and Table 4.7 for women. In each specification, the usual Dust Bowl exposure coefficient is listed in the "a" column, with the coefficient on the interaction between child labor intensity and Dust Bowl exposure listed in the "b" Column. Here, if a Dust Bowl-induced drop in the demand for child farm labor (and through it, a drop in the opportunity cost of secondary schooling) were the factor driving the increase in the probability of high school completion amongst those exposed to the Dust Bowl, we would expect individuals born in states where child labor in agriculture was more prevalent prior to the Dust Bowl to experience greater increases in high school completion. We would also expect other outcomes not to be affected by the pre-Dust Bowl child labor demand (i.e. not to have significant interaction coefficients), since these outcomes are not hypothesized to have been affected by tradeoffs in a child's time use.[53] For

[53] Given that child labor intensity at the state level is strongly negatively correlated with poverty, however, this may be an unreasonable expectation. Indeed, these regressions may be manifesting the effects of growing up in an impoverished state, thus making changes in outcomes other than high school completion plausible for reasons that have little to do with the collapse of child farm labor demand.

contrast, if a region is extremely reliant on child labor in farming prior to the Dust Bowl, we might expect little change in schooling outcomes as agricultural employers retain child workers rather than releasing them to other uses of time.

The results of a similar later-life analysis, based on adding further interactions for race, are presented in Table 4.8. Here, the table presents only the coefficients on the quadruple-interacted term which represents the effect of being a non-white individual who was exposed to the Dust Bowl in a state which more intensively used child labor in agriculture prior to the Dust Bowl, relative to that experienced by a white individual exposed to the Dust Bowl in a state with the same child labor intensity profile. Here, given the greater prevalence of agricultural labor amongst non-white children, we might expect non-white children to have had a starker drop in the opportunity cost of schooling relative to their white peers, and so to enjoy sharper increases in the probability of high school completion.

In the "interaction" columns of regressions (1) and (2) in Table 4.6, it appears that there is no statistically significant gradient in the probability of high school completion by child labor intensity of agricultural production. Although statistically insignificant, the sign of the interaction coefficients is negative, suggesting that boys exposed to the Dust Bowl in states with a greater labor requirements due to the exogenously-given crop endowment may have had lower chances of completing high school than their exposed counterparts in less child-labor-prone states, a finding that is consistent with the idea that child labor-reliant regions may have been more motivated to hold onto young workers given a shock, than were less child labor-intensive regions. Notably, the coefficient on the "main" Dust Bowl exposure term in these specifications remains large and positive, as in earlier sections of this thesis, and suggests that contrary to the evidence presented up to this point in support of an explanation hinging on a fall in the opportunity cost of schooling, the collapse of child labor in agriculture may *not* in fact be the driver of the rise in high school completion in the Dust Bowl cohort.

Table 4.6: Later-Life Impact of Dust Bowl Exposure by Mechanism: Child Labor – Men

	CLI-All		**CLI-Chief**		**Mech**	
	(1a)	(1b)	(2a)	(2b)	(3a)	(3b)
Outcomes	Treated × Erosion	Child Labor × Treated × Erosion	Treated × Erosion	Child Labor × Treated × Erosion	Treated × Erosion	Child Labor × Treated × Erosion
Age at First Marriage	0.4050	-0.6300	0.0973	0.0377	-0.3450**	0.00024
	(0.4230)	(0.5030)	(0.1440)	(0.1930)	(0.1380)	(0.00016)
Probability of Completing High School	0.1640*	-0.1780	0.0764***	-0.2870	-0.0211	0.00004*
	(0.0897)	(0.1090)	(0.0217)	(0.0134)	(0.0239)	(0.00002)
Probability of Completing College	-0.0182	0.0087	0.0589***	-0.2980	0.0044	-0.00002
	(0.0375)	(0.0486)	(0.0033)	(0.0171)	(0.0165)	(0.00001)
Welfare Income	20.18	-6.84	19.60*	-58.85	16.68	-0.00316
	(46.3600)	(58.4700)	(11.5700)		(11.8200)	(0.01240)
Probability of Poverty	0.0572*	-0.0597	0.0364***	-0.1530***	0.0059	0.00000
	(0.0305)	(0.0368)	(0.0056)		(0.0065)	(0.00001)
Probability of Cognitive Disability	-0.0825	0.1080*	0.0594	-0.2540	0.0328**	-0.00003**
	(0.0506)	(0.0636)	(0.0057)		(0.0157)	(0.00001)
Probability of Physical Disability	-0.0678***	0.0954***	0.0061	0.0376	0.0306***	-0.00002**
	(0.0262)	(0.0246)			(0.0051)	(0.00001)
Probability of Vision & Hearing Difficulty	-0.0305	0.0334	-0.0071	-0.0290	0.01800	-0.00002**
	(0.0386)	(0.0482)			(0.0123)	(0.00001)
Probability of Self-Care & Independent Mobility Difficulty	-0.0007	0.0004	-0.0056	0.0065	0.0066	-0.00001
	(0.0228)	(0.0258)			(0.0074)	(0.00001)

*** p<0.01, ** p<0.05, * p<0.1; Note: Tables report $treated_b \times erosion_s$ coefficients (main effect) and $interaction \times treated_b \times erosion_s$ coefficients (interaction effect) for specifications that interact the baseline treatment term with the relevant measure of the state's measure of child labor intensity (CLI-All refers to the child labor intensity index based on all crops, CLI-Chief refers to that based on the chief crop, and Mech refers to mechanization in agriculture as measured by the total value of farm capital per member of the farm population). All regressions are estimated by OLS and include the relevant two-way interactions as well as controls for race; birth year, birth state, and census year fixed effects; and state trends. Standard errors, clustered by birth state and birth year, are reported in parentheses below each coefficient. Child labor intensity variables are continuous; to aid in interpreting the interaction coefficient for these regressions, the minimum, maximum, and standard deviation for the relevant variables in the full sample are as follows: CLI-All (0.502, 1.000, 0.178); CLI-Chief (0.222, 1.000, 0.266); Mech (291.486, 3005.363, 750.518).

Table 4.7: Later-Life Impact of Dust Bowl Exposure by Mechanism: Child Labor – Women

	CLI-All		**CLI-Chief**		**Mech**	
	(1a)	(1b)	(2a)	(2b)	(3a)	(3b)
Outcomes	Treated × Erosion	Child Labor × Treated × Erosion	Treated × Erosion	Child Labor × Treated × Erosion	Treated × Erosion	Child Labor × Treated × Erosion
Age at First Marriage	0.4790	-0.5650	0.0031	0.8540*	-0.0537	0.00007
	(0.5610)	(0.6660)	(0.1630)	(0.4380)	(0.1980)	(0.00020)
Children Ever Born	0.1990	-0.5010	0.0111	-1.6600***	-0.3670	0.00015
	(0.7580)	(0.9660)	(0.1740)	(0.1150)	(0.3050)	(0.00022)
Probability of Completing High School	0.1080	-0.0957	-0.0022	0.0808	0.0086	0.00002
	(0.0965)	(0.1130)	(0.0222)		(0.0216)	(0.00002)
Probability of Completing College	0.0791***	-0.1090***	0.0096	-0.0646	-0.0394***	0.00003***
	(0.0305)	(0.0374)			(0.0099)	(0.00001)
Welfare Income	108.80**	-111.70	69.87	-213.70	3.90	0.01150
	(47.6100)	(68.5600)			(26.3900)	(0.01830)
Probability of Poverty	-0.0265	0.0372	0.0256***	-0.1250	0.0185***	-0.00001***
	(0.0257)	(0.0290)	(0.0019)		(0.0024)	(0.00000)
Probability of Cognitive Disability	0.0273	-0.0400	-0.0093	-0.0202	-0.0112	0.00000
	(0.0311)	(0.0387)	(0.0122)		(0.0121)	(0.00001)
Probability of Physical Disability	-0.0667	0.0791	-0.0064	0.0710	0.0083	-0.00001
	(0.0496)	(0.0631)	(0.0161)		(0.0148)	(0.00001)
Probability of Vision & Hearing Difficulty	0.1120***	-0.1380***	-0.0010	-0.0065	-0.0420***	0.00004***
	(0.0192)	(0.0224)	(0.0151)	(0.0639)	(0.0101)	(0.00001)
Probability of Self-Care & Independent Mobility Difficulty	0.0402	-0.0502	0.0237***	-0.1090	-0.0111*	0.00001
	(0.0369)	(0.0425)	(0.0059)		(0.0057)	(0.00001)

*** p<0.01, ** p<0.05, * p<0.1; Note: Tables report $treated_b \times erosion_s$ coefficients (main effect) and $interaction \times treated_b \times erosion_s$ coefficients (interaction effect) for specifications that interact the baseline treatment term with the relevant measure of the state's measure of child labor intensity (CLI-All refers to the child labor intensity index based on all crops, CLI-Chief refers to that based on the chief crop, and Mech refers to mechanization in agriculture as measured by the total value of farm capital per member of the farm population). All regressions are estimated by OLS and include the relevant two-way interactions as well as controls for race; birth year, birth state, and census year fixed effects; and state trends. Standard errors, clustered by birth state and birth year, are reported in parentheses below each coefficient. Child labor intensity variables are continuous; to aid in interpreting the interaction coefficient for these regressions, the minimum, maximum, and standard deviation for the relevant variables in the full sample are as follows: CLI-All (0.502, 1.000, 0.178); CLI-Chief (0.222, 1.000, 0.266); Mech (291.486, 3005.363, 750.518).

Table 4.8: Later-Life Impact of Dust Bowl Exposure by Mechanism: Child Labor & Race

	Men			Women		
	(1)	(2)	(3)	(4)	(5)	(6)
Outcomes	Non-White × CLI-All × Treated × Erosion	Non-White × CLI-Chief × Treated × Erosion	Non-White × Mech × Treated × Erosion	Non-White × CLI-All × Treated × Erosion	Non-White × CLI-Chief × Treated × Erosion	Non-White × Mech × Treated × Erosion
Age at First Marriage	5.9760	7.1020***	-0.00128*	3.9780	4.8920***	-0.00134
	(5.7660)	(1.7160)	(0.00074)	(4.1230)	(0.3430)	(0.00091)
Children Ever Born				2.3930	7.0320	-0.00011
				(2.7450)	(161.6000)	(0.00054)
Probability of Completing High School	-0.2340	-1.0690	-0.00022	-0.4450	-1.6890	-0.00017
	(0.5710)	(0.0318)	(0.00014)	(0.6270)	(0.0826)	(0.00016)
Probability of Completing College	0.1200	0.3910	-0.00005	0.0278	0.0036	-0.00002
	(356.5000)	(0.0773)	(0.07120)	(0.0871)	(0.0513)	(0.00004)
Welfare Income	398.50	412.70	0.00009	63.81	1,722.00***	0.19400
	(0.2340)	(0.0711)	(0.00006)	(858.7000)	(0.0393)	(0.17300)
Probability of Poverty	0.0138	-0.0276	0.00000	-0.0834	0.1970**	0.00005
	(0.1940)		(0.00005)	(0.4450)	(0.0413)	(0.00009)
Probability of Cognitive Disability	-0.0573	-0.1660	0.00001	-0.5610***	-0.1320	0.00009**
	(0.3130)		(0.00003)	(0.1760)		(0.00004)
Probability of Physical Disability	-0.3650	-0.9590***	-0.00002	-0.5130	-0.2700***	0.00009***
	(0.3690)		(0.00007)	(0.2110)		(0.00003)
Probability of Vision & Hearing Difficulty	0.3310	-0.3740***	-0.00013*	-0.0068	-0.2250***	-0.00003
	(0.2080)		(0.00004)	(0.2930)		(0.00004)
Probability of Self-Care & Independent Mobility Difficulty	-0.4960**	-0.0250	0.00010**	-0.4780	0.0939**	0.00011*
						(0.00006)

*** p<0.01, ** p<0.05, * p<0.1; Note: Tables report the $nonwhite_{ibst} \times childlabor_s \times treated_b \times erosion_s$ coefficients for specifications that interact the baseline treatment term with the relevant measure of the state's measure of child labor intensity (CLI-All refers to the child labor intensity index based on all crops, CLI-Chief refers to that based on the chief crop, and Mech refers to mechanization in agriculture as measured by the total value of farm capital per member of the farm population) and with the individual's race (non-white is a binary variable taking the value of 0 if the individual is white, and 1 if not). All regressions are estimated by OLS and include all relevant interactions as well as controls for race; birth year, birth state, and census year fixed effects; and state trends. Standard errors, clustered by birth state and birth year, are reported in parentheses below each coefficient. Child labor intensity variables are continuous; to aid in interpreting the interaction coefficient for these regressions, the minimum, maximum, and standard deviation for the relevant variables in the full sample are as follows: CLI-All (0.502, 1.000, 0.178); CLI-Chief (0.222, 1.000, 0.266); Mech (291.486, 3005.363, 750.518).

Higher rates of mechanization appear to drive high school completion increases amongst Dust Bowl-exposed children: a one standard deviation increase in the state farm capital per farmer yields a 3.00 percentage point increase in a Dust Bowl-exposed child's chances of completing high school. As above with the crop-based labor intensity measures, this result contradicts the prediction that places which had lower pre-Dust Bowl demand for child labor in agriculture (that is, places with higher levels of farm mechanization, assuming enough mechanization took place to obviate the need for child labor) would have experienced less stark a drop in the opportunity cost of secondary schooling once the Dust Bowl had destroyed local farms; in these as opposed to more child-labor-intensive places, the Dust Bowl would have had less of an impact on the demand for child labor.

Of course, given the strong positive correlation between state economic circumstances and levels of mechanization in agriculture, it is possible that these results reflect income rather than child labor effects, e.g. because richer households are better able to cope with adverse shocks. If income were driving these results, it might also explain why outcomes other than high school completion are affected in specifications interacting Dust Bowl exposure with child labor demand. For instance, it might explain why a one standard deviation increase in child labor demand (itself strongly negatively correlated at the state level with retail sales per capita in 1929) as given in regression (1) results in a 1.70 percentage point increase in the probability of physical disability, or why mechanization (which is strongly positively correlated with state retail sales per capita in 1929), given in regression (3) is shown to attenuate several adverse effects of Dust Bowl exposure.

A similar pattern holds in the results for women, presented in Table 4.7; here, as for men, child labor demand as given by agro-climatic crop endowments does not significantly modify the effect of the Dust Bowl, although unlike for men, neither does child labor demand as measured by the level of mechanization. Here, the pattern in which crop-suitability-based measures of child labor demand intensify the Dust Bowl's adverse effects, and in which mechanization-based measures attenuate it, persists. These findings, like the ones above, suggest that places that were highly reliant on child labor found it more difficult to cease employing children than those places where child labor may have been more casual, flexible, or family farm-based. They also may imply that the measures of child labor intensity used here may in fact reflect the effect of childhood economic circumstances, suggesting a gradient in either or both of a child's pre-shock vulnerability and a child's post-shock capacity for recovery based on childhood income. This finding would not only be intuitive, but it would also accord with the conceptual framework in Section 2.3 and empirical results in Fishback & Thomasson (2014) which support the existence not only of income gradients, but also of nonlinear effects of adverse shocks based on initial income.

For contrast with the overwhelming empirical evidence that Dust Bowl-era child labor demand changed little in the places that relied on it most, the results on race fit much more intuitively with a scenario in which the loss of opportunities for child labor in agriculture—perhaps in places where there is relatively little or quite flexible child work in agriculture—results in the mitigation of the Dust Bowl's adverse long-range effects. While high school completion results remain negative and insignificant in all specifications, being non-white at a given level of child labor intensity and Dust Bowl exposure confers an advantage in age at first marriage and the probabilities of cognitive, physical, vision and hearing, and self-care and independent mobility disabilities.[54] The attenuation of adverse effects of Dust Bowl exposure on disability may be, as discussed with respect to later-childhood disability effects in Section 4.2, the result of a collapse in child farm labor demand leading to a removal of children (in this case, especially the more intensively-working non-white children) from the

[54]However, race does not beneficially modify Dust Bowl exposure and child labor intensity for all outcomes: poverty outcomes appear worse for non-whites than whites in a given Dust Bowl exposure and child labor regime.

sorts of strains and hazards of hard physical labor outdoors.

4.4.4.2 Short-Term Analysis

I now turn to the results on contemporaneous or shorter-term schooling responses, presented in Table 4.9 for men, and in Table 4.10 for women. In these tables, each cell represents a single regression whose dependent variable is the probability of attending school during the reference period, and the coefficient reported is the coefficient of primary interest as specified in the row text. The results are almost completely identical whether missing school attendance information is coded as an explicit lack of attendance or dropped, as well as whether or not age is included as a control. In keeping with the patterns observed in the results on longer-range schooling outcomes, for men, pre-Dust Bowl child labor demand (the series of coefficients presented in row 2) has effects on school attendance that are not only insignificant, but also negative in sign. Given the weak relationships at the county level between mechanization-based and crop suitability-based measures of child labor demand, as well as between these and local economic conditions, it is perhaps less surprising here than in the state-level analysis above that coefficients bear the same sign irrespective of the measure of child labor demand used. As before, it appears more plausible given this evidence that regions with very high pre-Dust Bowl child labor demand (for instance, those regions whose agricultural economy was driven by extremely child labor-intensive cotton) may not have allowed the degree of child labor-secondary schooling tradeoff that was possible where child labor in farming was less essential.

For women, these results are also negative, but statistically significant. For instance, a one standard deviation increase in the all-crop labor intensity index corresponds to a 8.11 percentage point *decrease* in the likelihood a Dust Bowl-exposed girl attended school in the reference period, relative to her Dust Bowl-exposed counterpart in a state with lower pre-Dust Bowl child labor intensity in agriculture.

Here, it is possible the extremely short reference period plays a role in the results: if Dust Bowl-induced changes in schooling behaviors were not persistent, then answers given by children in 1940, when the worst of the Dust Bowl was over and when behavior had had time to re-adjust to the new labor landscape, about schooling choices made in the past several weeks, may fail to reflect work-school tradeoffs made in the Dust Bowl's trough.

For both men and women, the results for specifications interacting a child's actual household farm status with both Dust Bowl exposure and pre-Dust Bowl child labor demand in the county of birth indicate that at a given level of child labor demand, a Dust Bowl-exposed child's farm status made that child if anything *less* likely to be found attending school than its non-farm peers, once again supporting the hypothesis that farmers were keen to hold onto child labor where it was more intensively used, over the one in which steeper collapses in child labor demand lead to sharper changes in the opportunity cost of schooling. Indeed, farm-children, whose engagement in child labor prior to the Dust Bowl ought to have been higher than their non-farm peers and who thus, according to the latter hypothesis, stood to see the sharpest drop in the opportunity costs of schooling, experience a stunning 24.30 to 26.40 percentage point lower probability of attending school in the reference period. As above, it should be noted that school attendance as reported in the census (the only meaningful education variable available to measure short-term responses), may make a poor proxy for the real outcome of interest: enrollment and attendance patterns throughout the Dust Bowl.

As above in the long-term analysis, the results on race coincide more naturally with an explanation that places child labor tradeoffs at the heart of time use decisions. Here, non-white children, who were historically likelier to be engaged in farm labor than their white counterparts, saw dramatic increases in the likelihood of attending school during the reference period. These results suggest, as do Baker (2015) and

Table 4.9: Impact of Dust Bowl on School Attendance: County-Level – Men

Specification	**Attending School** (1)	**Attending School** (2)	**Attending School** (3)	**Attending School** (4)
Dust Bowl (Coefficient Reported: $Treated \times Erosion$)	-0.05370*	-0.05370*	-0.05370*	-0.05370*
	(0.02990)	(0.02990)	(0.02990)	(0.02990)
Child Labor (Coefficient Reported: $CLI - All \times Treated \times Erosion$	-0.45200	-0.45200	-0.45200	-0.45200
	(0.33200)	(0.33200)	(0.33200)	(0.33200)
Child Labor (Coefficient Reported: $CLI - Chief \times Treated \times Erosion$	-0.12300	-0.12300	-0.12300	-0.12300
	(0.08760)	(0.08760)	(0.08760)	(0.08760)
Child Labor (Coefficient Reported: $Mech \times Treated \times Erosion$	-0.00017	-0.00017	-0.00017	-0.00017
	(0.34000)	(0.16200)	(0.34000)	(0.16200)
Race (Coefficient Reported: $Nonwhite \times CLI - All \times Treated \times Erosion$	2.24300***	2.24300***	2.24300***	2.24300***
	(0.67300)	(0.67300)	(0.67300)	(0.67300)
Race (Coefficient Reported: $Nonwhite \times CLI - Chief \times Treated \times Erosion$	0.13600	0.13600	0.13600	0.13600
	(0.11300)	(0.11300)	(0.11300)	(0.11300)
Race (Coefficient Reported: $Nonwhite \times Mech \times Treated \times Erosion$	0.00006	0.00006	0.00006	0.00006
	(0.00022)	(0.00021)	(0.00022)	(0.00021)
Farm (Coefficient Reported: $Farm \times CLI - All \times Treated \times Erosion$	0.08560	0.08560	0.08560	0.08560
	(0.26800)	(0.26800)	(0.26800)	(0.26800)
Farm (Coefficient Reported: $Farm \times CLI - Chief \times Treated \times Erosion$	-0.2640***	-0.2640***	-0.2640***	-0.2640***
	(0.07970)	(0.07970)	(0.07970)	(0.07970)
Farm (Coefficient Reported: $Farm \times Mech \times Treated \times Erosion$	-0.00001	-0.00001	-0.00001	-0.00001
	(0.00006)	(0.00006)	(0.00006)	(0.00006)
Observations with Missing School Attendance Are:	Dropped	Dropped	Coded No	Coded No
Age Control:	No	Yes	No	Yes

*** p<0.01, ** p<0.05, * p<0.1; Note: Tables report the coefficient specified in the row title; thus, each cell rather than each column represents a distinct regression. CLI-All refers to the child labor intensity index based on all crops, CLI-Chief refers to that based on the chief crop, and Mech refers to mechanization in agriculture as measured by the total value of farm capital per member of the farm population. Nonwhite is a binary variable that takes the value of 1 if the individual is nonwhite, and 0 if not. Farm is a binary variable that takes the value of 1 if the individual lives in a farm household, and 0 if not. All regressions are estimated by OLS and include all relevant interactions as well as controls for age (where stated), race, and household size; birth year, birth county, and census year fixed effects; and county trends. Standard errors, clustered by birth county, are reported in parentheses below each coefficient. Child labor intensity variables are continuous; to aid in interpreting the interaction coefficient for these regressions, the minimum, maximum, and standard deviation for the relevant variables in the full sample are as follows: CLI-All (0.222, 1.000, 0.108); CLI-Chief (0.222, 1.000, 0.290); Mech (77.992, 33100000.000, 965666.500).

Table 4.10: Impact of Dust Bowl on School Attendance: County-Level – Women

Specification	**Attending School** (1)	**Attending School** (2)	**Attending School** (3)	**Attending School** (4)
Dust Bowl (Coefficient Reported: $Treated \times Erosion$)	-0.07250**	-0.07250**	-0.07250**	-0.07250**
	(0.03180)	(0.03180)	(0.03180)	(0.03180)
Child Labor (Coefficient Reported: $CLI - All \times Treated \times Erosion$	-0.75100**	-0.75100**	-0.75100**	-0.75100**
	(0.34100)	(0.34100)	(0.34100)	(0.34100)
Child Labor (Coefficient Reported: $CLI - Chief \times Treated \times Erosion$	-0.12100	-0.12100	-0.12100	-0.12100
	(0.08990)	(0.08990)	(0.08990)	(0.08990)
Child Labor (Coefficient Reported: $Mech \times Treated \times Erosion$	-0.00014**	-0.00014***	-0.00014**	-0.00014***
	(0.00006)	(0.00005)	(0.00006)	(0.00005)
Race (Coefficient Reported: $Nonwhite \times CLI - All \times Treated \times Erosion$	2.53600***	2.53600***	2.53600***	2.53600***
	(0.48900)	(0.48900)	(0.48900)	(0.48900)
Race (Coefficient Reported: $Nonwhite \times CLI - Chief \times Treated \times Erosion$	0.14800	0.14800	0.14800	0.14800
	(0.10200)	(0.10200)	(0.10200)	(0.10200)
Race (Coefficient Reported: $Nonwhite \times Mech \times Treated \times Erosion$	0.00027	0.00027	0.00027	0.00027
	(0.00026)	(0.00026)	(0.00026)	(0.00026)
Farm (Coefficient Reported: $Farm \times CLI - All \times Treated \times Erosion$	0.37100	0.37100	0.37100	0.37100
	(0.30900)	(0.30900)	(0.30900)	(0.30900)
Farm (Coefficient Reported: $Farm \times CLI - Chief \times Treated \times Erosion$	-0.24300***	-0.24300***	-0.24300***	-0.24300***
	(0.09030)	(0.09030)	(0.09030)	(0.09030)
Farm (Coefficient Reported: $Farm \times Mech \times Treated \times Erosion$	0.00000	0.00000	0.00000	0.00000
	(0.00005)	(0.00005)	(0.00005)	(0.00005)
Observations with Missing School Attendance Are:	Dropped	Dropped	Coded No	Coded No
Age Control:	No	Yes	No	Yes

*** p<0.01, ** p<0.05, * p<0.1; Note: Tables report the coefficient specified in the row title; thus, each cell rather than each column represents a distinct regression. CLI-All refers to the child labor intensity index based on all crops, CLI-Chief refers to that based on the chief crop, and Mech refers to mechanization in agriculture as measured by the total value of farm capital per member of the farm population. Nonwhite is a binary variable that takes the value of 1 if the individual is nonwhite, and 0 if not. Farm is a binary variable that takes the value of 1 if the individual lives in a farm household, and 0 if not. All regressions are estimated by OLS and include all relevant interactions as well as controls for age (where stated), race, and household size; birth year, birth county, and census year fixed effects; and county trends. Standard errors, clustered by birth county, are reported in parentheses below each coefficient. Child labor intensity variables are continuous; to aid in interpreting the interaction coefficient for these regressions, the minimum, maximum, and standard deviation for the relevant variables in the full sample are as follows: CLI-All (0.222, 1.000, 0.108); CLI-Chief (0.222, 1.000, 0.290); Mech (77.992, 33100000.000, 965666.500).

Greenbaum (2009), that race may be the most important lens by which to understand time use tradeoffs between child farm labor and schooling in 20th Century America. Indeed, the results on race presented here bear further investigation in future work, especially where race-differential institutional access and returns to schooling may have mediated schooling takeup in the face of agricultural crisis.

4.4.5 Conclusion

In this section, I have shown that a high pre-Dust Bowl demand for child labor resulted in a less pronounced increase in childhood schooling, consistent with the hypothesis that children may not have been released to other uses of time where their labor was most essential to agricultural production. Nevertheless, there is mixed support overall for the hypothesis that increases in high school completion amongst those exposed to the Dust Bowl as older children may be attributable to a fall in the opportunity costs of schooling: in the analysis taking child labor intensity into account, the results specific to race are somewhat supportive of this scenario, while those on child labor intensity more generally are not. It is unclear, then, whether the root of schooling increases observed earlier in this thesis is not, in fact, in childhood time use tradeoffs. It may well be the case that there is considerable heterogeneity by crop type, farm ownership or tenancy status, or local economic conditions—or even non-linearity in the way child labor intensity modifies the Dust Bowl shock, with, for instance, a positive effect of labor intensity on schooling only for very low levels of pre-Dust Bowl child labor intensity. That is, the baseline findings on the Dust Bowl's beneficial effects on secondary schooling may be the result of this thesis's focus on low child labor-intensity areas. What is clear is that given the disproportionate involvement of non-white children in agricultural labor in American history, race plays an important role in assessments of schooling and child labor choices, and should be given greater attention in future analysis. Here, results contribute to a recent and growing literature linking agricultural disruptions to a reduction in race-based inequalities in human capital.

4.5 Conclusion

This chapter provides evidence of considerable heterogeneity in the Dust Bowl cohort's experience of the shock—for instance, by the developmental timing of exposure, by farm-dependence, and by the interaction of race and child farm labor demand. Specifically, the analysis presented here shows that those exposed to the Dust Bowl earlier in childhood development, particularly prenatally, experience some of the worst human capital insults as adults. Here, the specifics of the capabilities impacted and the timing of exposure also shed light on when and how specific capabilities may be formed, and the mechanisms by which they can be affected by adverse health and income shocks. Analysis also indicates that farm-state individuals often suffered more intensely than their non-farm counterparts, a result that could be driven by a number of factors, most notably, a more severe initial insult followed up by constraints on compensating investment. Lastly, in tests of hypotheses examining the potential for tradeoffs between child farm labor and schooling, I find that regions with high labor intensity in agriculture were less likely to release child workers to other uses of time. There is little evidence that a Dust Bowl-driven reduction in the demand for child labor in agriculture drove schooling except potentially in places where there was very low child labor demand to begin with. Nevertheless, it appears that for Dust Bowl-exposed nonwhite children in states with high pre-Dust Bowl child farm labor demand, the Dust Bowl, and with it, the collapse of this labor demand, resulted in intuitive improvements in wellbeing. Together, the results presented in this chapter illuminate the channels—e.g. fetal development and adverse income shocks—through which childhood Dust Bowl exposure scarred later-life human capital.

Chapter 5

Mitigation & Recovery

Figure 5.1: Work Progress Administration Workers Building a Farm-to-Market Road, Tennessee, 1936 (Unknown, 1936a)

5.1 Overview

In this chapter, I test for how individuals, households, and policy may have responded to the Dust Bowl shock in an attempt to mitigate its adverse effects on wellbeing. I also test whether these coping strategies and remediating investments were effective in aiding recovery. First, I test whether migration, whether generally or selectively by household characteristics, and whether contemporaneously or subsequently, may have acted as a mechanism by which individuals in the Dust Bowl cohort limited their Dust Bowl exposure and sought recovery. I also investigate the effectiveness of migration as a possible remediation mechanism. Next, I test whether the Dust Bowl induced changes in fertility that may have constituted a voluntary and endogenous response to low incomes and adverse environmental conditions. I also examine whether there were socioeconomic differences in those who may have used this adjustment strategy. Lastly, I investigate the possibility of recovery given compensating investments in human capital. I do this by testing for the influence of New Deal and related public expenditure in ameliorating the Dust Bowl's harmful effects.

5.2 Migration as an Adjustment Mechanism

Perhaps no work of art or scholarship has done more to shape popular understandings of the Dust Bowl than John Steinbeck's *The Grapes of Wrath.* In it, the Joads, a distressed family of Oklahoma tenant farmers, leave the ruination of the Great Plains behind to try for a more dignified future in California's Central Valley (Steinbeck, 1939). Accordingly, the "Okies," of which the Joads were the foremost example, have become the enduring icon of the Dust Bowl (Worster, 1979). This image of the Dust Bowl migrant has gained such traction in the public imagination—perhaps in part because of its visibility, its poignancy and evocativeness, or its enshrinement in the art and literature of John Steinbeck, Woody Guthrie, Dorothea Lange, and Arthur

Rothstein—that the idea of massive out-migration from the Plains is often taken for granted as a matter of fact. However, as recent work has shown (Long & Siu, 2014), and as I also find below, the outsize cultural resonance of this image is perhaps out of step with its actual scope and importance as a historical phenomenon. Nevertheless, as one of the key themes associated with the Dust Bowl (and indeed, as one of the bugbears of cohort studies (Katusic et al., 1998)), the impact of migration bears examination.

Migration could confound the analysis presented thus far in a number of ways. For instance, migration during the treatment window may result in the incorrect measurement of the duration and severity of Dust Bowl exposure. Similarly, migrant selectivity by characteristics such as household education, income, occupation, etc., which influence both an individual's sensitivity to shocks and their capacity for recovery, could bias results. Lastly, migration could expose individuals to substantially different environments for recovery.

As just outlined, there are a number of ways in which Dust Bowl-driven migration, whether generally or selectively by household status, may affect the interpretation of this thesis's main results on the later-life effects of early-life Dust Bowl exposure. Accordingly, migration will be discussed in the context of possible confounding factors in Chapter 6. However, as will be the focus of this section, migration may also be viewed as an adversity-fleeing and/or recovery-seeking response by individuals and households to health hazards and income shocks. Indeed, this is the obvious intuition behind the popular image of Dust Bowl migration.

In this context, migration becomes a possible margin of adjustment to wellbeing shocks, rather than just a phenomenon that threatens inference. Recent work in economic history such as Arthi et al. (2016a) and Hanlon (2015) has engaged with this idea of migration as a purposeful mechanism by which individuals adjust to environmental shocks and localized recessions.[1]

In the analysis below, I examine whether the Dust Bowl spurred childhood and eventual adult migration, whether generally or differentially by groups more or less able to avail themselves of migration as a shock-mitigation strategy. I find limited evidence that migration was used as a strategy to limit or compensate for childhood Dust Bowl exposure; instead, Dust Bowl-cohort children are significantly less likely to migrate during childhood. However, I demonstrate a gap in migrant and non-migrant outcomes in adulthood that suggests, in the absence of childhood migrant selectivity, that migration may indeed have been a means by which some measure of recovery was achieved, whether intentionally or unwittingly.[2]

5.2.1 Childhood Migration

Migration out of the birth state during childhood could have been a mechanism by which parents sought to mitigate their children's exposure to the Dust Bowl crisis. It is thus useful to establish whether such migration was prevalent, whether it was associated with Dust Bowl severity, and whether its timing was likely to allow for meaningful reductions in exposure. I also examine which sorts of children may have had better access to such a coping strategy. Accordingly, I test whether exposure to the Dust Bowl drives childhood migration, as well as whether there is evidence of migrant selectivity in those individuals migrating as children.

[1]Hornbeck (2012), too, discusses the role of population declines (due to changing migration patterns) as a means for economic adjustment; however, he frames this as a mechanism for regional agricultural and macroeconomic recovery rather than as an individual or human phenomenon.

[2]Unfortunately, it is not possible here to disentangle possible selectivity by health status, income, etc. in migrant status where migration took place later in life, from the remediating effects of the receiving-state environment, or from a decrease in the duration or severity of childhood dust exposure. However, given the apparent lack of selectivity in childhood migrant status and the fact that Dust Bowl exposure does not drive adult migration probability, it seems most likely that an improvement in the post-migration environment account for the migrant/non-migrant gap in adult outcomes.

5.2.1.1 Methods & Data

I use data on children (that is, individuals aged 12 and under only) born in my 15 states of interest in the 1% IPUMS samples of the 1920, 1930, 1940, and 1950 US Censuses (Ruggles et al., 2010) to test for whether Dust Bowl exposure and/or household characteristics influenced a child's likelihood to have migrated before the age of 12. Similar to the method employed in the baseline specification (given in Equation 3.1), I test for whether children born between 1918 and 1941, inclusive (that is, children aged -1 to 12 during the Dust Bowl) were likelier to be enumerated before age 12 in a state other than their birth state. I interact the standard Dust Bowl treatment term with measures of household income, socioeconomic status, and farm status to test whether certain household characteristics were associated with patterns in who migrated during childhood.

To test for differences in likelihood of having migrated out of the birth state as a child, I estimate the following regression, by OLS:

$$\begin{aligned} k_{ibst} &= \alpha + \beta_1 \times treated_t \times erosion_s \\ &+\beta_2 \times householdcharacteristic_{ibst} \times treated_t \times erosion_s \\ &+\beta_3 \times householdcharacteristic_{ibst} \times treated_t \\ &+\beta_4 \times householdcharacteristic_{ibst} \times erosion_s \\ &+\beta_5 \times householdcharacteristic_{ibst} \\ &+{x_{ibst}}'\psi + \theta_s + \lambda_b + \eta_t + \gamma_s + u_{ibst} \end{aligned} \tag{5.1}$$

where k_{ibst} is a binary dummy which takes the value of 1 if child i, born in year b in state s and enumerated in census year t, currently (in the 1920-50 Censuses) aged 12 or below, is living in a state other than the birth state at the time of the census, and 0 if not.[3] This outcome variable allows for testing the impact of the Dust Bowl

[3]The variable is constructed based on "BPL" and "STATEFIP" in IPUMS such that individuals for whom BPL $\neq$ STATEFIP are deemed childhood migrants.

on children's likelihood of migration, as well as that of household characteristics on determining who may have migrated out of the sample during childhood.

$Treated_t$ is a binary dummy which takes the value of 1 if the child was born between 1918 and 1941, inclusive.

$Erosion_s$ is, as in the baseline regressions, a population-weighted measure of erosion severity in the child's birth state.

$Householdcharacteristic_{ibst}$ is a measure of the child's household income or socioeconomic circumstances; several alternative versions of socioeconomic and household status are used, including Occscore, SEI, PRESGL, and farm status. Occscore, a continuous occupational income index which serves as a proxy for actual household income before income was recorded in the Census, is constructed by IPUMS (Ruggles et al., 2010) as the median 1950 salary attached to the household head's stated occupation. (The IPUMS variables SEI, an index of socioeconomic status, and PRESGL, an occupational prestige index, are also used to proxy for household income and socioeconomic status; see Ruggles et al. (2010) for further details of these variables' construction.) Farm status, in regressions such as these, which use the 1920-50 U.S. Censuses (Ruggles et al., 2010), is a binary dummy that identifies households listed in the Census as farm households, i.e. at least one household member is engaged in an agricultural occupation. The column headers in Table 5.1 identify which of these interacted variables has been used in the specification.

x_{ibst} represents a vector of individual- and household-level controls, including sex, race, age, household farm status (for regressions using income variables and where, thus, farm status has not already been included as a control), and income (for regressions using farm status, where income has not already been included as a control). In regressions on farm status, the income variable control chosen is Occscore.

θ_s, λ_b, and η_t represent census state fixed effects, birth year fixed effects, and census year fixed effects, while γ_s represents state trends. Standard errors are clustered at the birth state and birth year levels.

The specification above is also estimated and reported in the table under the header of "Dust Bowl" as follows:

$$k_{ibst} = \alpha + \beta_1 \times treated_t \times erosion_s + x_{ibst}{}' \psi + \theta_s + \lambda_b + \eta_t + \gamma_s + u_{ibst} \qquad (5.2)$$

As in the farm status regressions, here, Occscore is the income control used. Results are reported in Table 5.1.

5.2.1.2 Results & Discussion

Column 1 of Table 5.1 shows that Dust Bowl-exposed children (i.e. children who were enumerated in 1940 and experienced full Dust Bowl severity) were roughly 14 percentage points less likely to be found in a state other than their birth state. That is, they were less likely than their unexposed counterparts to have moved during childhood. This (weakly significant) result suggests that Dust Bowl-affected households did not systematically attempt to flee the crisis; indeed, they were more likely to have stayed at the site of treatment. Was this because these households were unwilling or unable to avail themselves of this mitigation strategy? Examining heterogeneity in the Dust Bowl cohort's childhood migration propensity sheds light on the latter issue.

Accordingly, regressions 2-6 in the table add an interaction between measures of the child's known household socioeconomic status and the usual Dust Bowl exposure term. The purpose of this approach is to ask whether children were differentially selected for childhood migration based on household status, e.g., whether children in richer households were more likely to move during childhood, perhaps because they were better able to afford to do so. Of course, it could alternatively be that poorer children, whose households had less to lose and fewer assets tying them to the land

Table 5.1: Impact of Household Status on Decision to Migrate During the Dust Bowl: Migration Analysis on Children

	Dust Bowl	**Occscore**		**SEI**		**PRESGL**		**Farm**		**Farm**	
	(1)	(2)		(3)		(4)		(5)		(6)	
Outcomes	Treated × Erosion	Treated × Erosion	Occscore × Treated × Erosion	Treated × Erosion	SEI × Treated × Erosion	Treated × Erosion	PRESGL × Treated × Erosion	Treated × Erosion	Farm × Treated × Erosion	Treated × Erosion	Farm × Treated × Erosion
Probability of Childhood Migration	-0.1440* (0.0857)	-0.1780	-0.01500	-0.1950	-0.00454 (0.00951)	-0.2490	-0.00363 (0.00768)	-0.0242 (0.0180)	0.02100 (0.01890)	0.4320** (0.1840)	-0.61700*** (0.13500)

*** p<0.01, ** p<0.05, * p<0.1; Note: Column 1 reports the $treated_b \times erosion_s$ coefficient. Columns 2-6 report $treated_b \times erosion_s$ coefficients (main effect) and $characteristic_{ibst} \times treated_b \times erosion_s$ coefficients (interaction effect) for specifications that interact the baseline treatment term with the child's household income/socioeconomic status measure. All regressions are estimated by OLS and include two-way interactions and controls for race, age, sex, farm status, and household income; birth state, birth year, and census year fixed effects; and state trends. Standard errors, clustered by birth state and birth year, are reported in parentheses below each coefficient. Column 6 adds a control for OCCSCORE to the farm regressions. Farm status is a binary variable, while the other household status terms are continuous; to aid in interpreting the interaction coefficient for the remaining household status variables, the minimum, maximum, and standard deviation are as follows: Occscore (3, 80, 6.060); SEI (6, 92, 7.494); PRESGL (14.7, 81.5, 6.507).

economically and psychologically, were more likely to migrate. Instead, I find no evidence of either type of selectivity: it does not appear that household income or socioeconomic status modified the Dust Bowl's effects on the probability of migration during childhood.

The same holds for farm-household children when not separately accounting for proxies of household income, per Column 5: here, it appears that farm children are no likelier to move than their non-farm counterparts. However, in Column 6, once controlling for a household income proxy, OCCSCORE, it appears that Dust Bowl-exposed farm children are if anything much less likely to have moved during the Dust Bowl than their non-farm but nevertheless Dust Bowl-exposed counterparts, consistent with the idea that smallholder farmers may have ignored push factors (i.e. felt irrationally tied to their land and assets), and/or that they may have faced few pull factors (i.e. there was little non-agricultural employment for displaced farmers nearby, and the move to the nearest plausible job may have been a very long-distance one). For such farmers, it seems, migration was an unattractive or inaccessible strategy for mitigating environmental damage to health.

Indeed, although popular narratives of the Dust Bowl such as Steinbeck's *The Grapes of Wrath* might suggest otherwise (Steinbeck, 1939), farmers were often resistant to moving. In their report on the contribution of "traditional attitudes and institutions" to causing the Dust Bowl, L.C. Gray, John B. Bennett, Erich Kraemer, and W.N. Sparhawk note that

> a recent survey of an infertile cut-over area in Louisiana (*370*) revealed that of 92 relief families living in the open country, containing an able-bodied male member between the ages of 15 and 50, and receiving the major portion of their subsistence from agriculture, only 37 families, or 40 percent, were willing to move from the area, even if they were to be assisted in disposing of their present holdings and in acquiring better farm

> lands or greater opportunities for work in some other locality (Wallace, 1938, p. 127).

Such a choice is suggestive of a perhaps sentimental attachment to the land, and appears galling in the face of total agricultural collapse. Nevertheless, it is supported by the analysis presented here, as well as that in Long & Siu (2014).

Together, these results shed light on how individuals adjusted to the Dust Bowl shock. In this case, they seem to suggest that Dust Bowl-exposed households in fact failed to use migration during childhood as a strategy for minimizing harm to their children. That even richer households failed to avail themselves of this potential coping mechanism, and that a large proportion of farm households stated their intent to refuse hypothetical government relocation assistance, suggests that income constraints did not drive these households' migration decisions.

5.2.2 Eventual Migration

The analysis above suggests that those exposed to the Dust Bowl as children were unlikely to have cut short their exposure window, nor to have exposed themselves to a superior environment for recovery during the childhood period. However, is it possible that subsequent migration may have improved exposed individuals' prospects for recovery?

To test this, I broaden the period during which migration may have taken place and look for evidence of systematic differences in the adult outcomes of migrants and non-migrants.[4] As in the childhood migration analysis above, I also examine whether Dust Bowl exposure drives the likelihood of migrating from the state of birth (where this migration may have either taken place during the childhood treatment window,

[4]Note that it is impossible from this data to discern when in their life-course individuals who migrated away from their home state did so, or even to tell if individuals now found in their home state may have out-migrated at some point before returning. As such, the individuals I refer to in this section as migrants (non-migrants) are actually more accurately termed "eventual migrants" ("plausible non-migrants").

or later on in life). Such analysis establishes the prevalence of Dust Bowl-induced migration, and suggests its scope as a possible mechanism for recovery.[5]

Accordingly, I use the data from my baseline analysis to test whether eventual migrants exposed to the Dust Bowl (that is, individuals found in adulthood to be living in a state other than their birth state; see below for a discussion of this variable and its interpretation) have statistically significantly different later-life outcomes than their exposed counterparts enumerated as adults in their birth state. I also test for the effect of Dust Bowl exposure on the likelihood of being an eventual migrant.

Accordingly, I use the data from my baseline analysis to test whether eventual migrants exposed to the Dust Bowl (that is, individuals found in adulthood to be living in a state other than their birth state; see below for a discussion of this variable and its interpretation) have statistically significantly different later-life outcomes than their exposed counterparts enumerated as adults in their birth state. I also test for the effect of Dust Bowl exposure on the likelihood of being such an eventual migrant.

5.2.2.1 Methods & Data

To test for differences in adult outcomes by migrant status, I estimate the following regression, by OLS:

$$\begin{aligned} h_{ibst} = \alpha + \beta_1 \times treated_b \times erosion_s + \beta_2 \times migrant_{ibst} \times treated_b \times erosion_s \\ + \beta_3 \times migrant_{ibst} \times treated_b + \beta_4 \times migrant_{ibst} \times erosion_s + \beta_5 \times migrant_{ibst} \\ + {x_{ibst}}' \psi + \theta_s + \lambda_b + \eta_t + \gamma_s + u_{ibst} \end{aligned} \tag{5.3}$$

where h_{ibst}, as in the baseline Equation 3.1, is the individual-level human capital outcome of interest for the individual i born in year b in state s and enumerated in

[5]The implications of possible differences in migrant and non-migrant outcomes in adulthood also sheds light on the likelihood that Dust Bowl exposure is systematically mismeasured, a matter discussed in Chapter 6.

year t.

$Treated_b$ is a binary dummy which takes the value of 1 if the individual was born between 1918 and 1941, inclusive.

$Erosion_s$ is, as in the baseline regressions, a population-weighted measure of erosion severity in the individual's birth state.

$Migrant_{ibst}$ is a binary dummy that takes the value of 1 if the individual is enumerated (as an adult) in a state other than their birth state at the time of the census, and 0 if not.[6] Although the age at which migration took place is unknown, and although it is possible that even those who are currently found living in their birth state migrated out at some point before returning, this variable serves as a rough proxy for individuals who may have migrated out of the sample during childhood. It should be noted that differences in adult outcomes by this measure of migrant status may conflate several possible effects: first, the effects of shortening the duration of childhood treatment by the Dust Bowl and/or of lessening its severity; second, the effects of moving to a state that offers different conditions for remediation (e.g. access to schools, jobs, health care, etc.) than the birth state; and third, the effects of migrant selectivity in the post-childhood period by characteristics such as the individual's levels of health, education, or income.

x_{ibst} represents a vector of controls, including race.

θ_s, λ_b, and η_t represent census state fixed effects, birth year fixed effects, and census year fixed effects, while γ_s represents state trends. Standard errors are clustered at the birth state and birth year levels. Each equation is estimated separately for men and women.

I also estimate the effect of Dust Bowl exposure on the likelihood of being an

[6]This variable is constructed based on "BPL" and "STATEFIP" in IPUMS such that individuals for whom BPL $\neq$ STATEFIP are deemed migrants.

eventual migrant, by OLS:

$$migrant_{ibst} = \alpha + \beta_1 \times treated_b \times erosion_s + {x_{ibst}}' \psi + \theta_s + \lambda_b + \eta_t + \gamma_s + u_{ibst} \quad (5.4)$$

All details of this regression are as specified for Equation 5.3 above. Results are reported in Table 5.2.

5.2.2.2 Results & Discussion

If eventual migrants and non-migrants have systematically different outcomes, this could be evidence of selectivity in migrants, and/or of exposure to distinct remediating environments. Since the analysis above regarding children seems to rule out selectivity in migration during childhood, unless we have reason to believe adult migrants were selected by adult characteristics unrelated to earlier Dust Bowl exposure,[7] we can set the former explanation aside and focus on the latter.

Regressions 1 and 3 in Table 5.2 show that early-life Dust Bowl exposure did not drive the probability of eventual migration. These results show that the chances of eventual migration for Dust Bowl-exposed men and women are no different than those of their unexposed counterparts: although the coefficients on Dust Bowl exposure are small and positive, they are statistically insignificant.

We might also be concerned whether the outcomes for eventual migrants were systematically different from those who did not eventually migrate out of the state of birth. If this were the case, these differences might have arisen from a change in the post-exposure environment. Regressions 2 and 4 present the coefficients for men and women, respectively, of Dust Bowl exposure and of Dust Bowl exposure interacted with the individual's migrant status. Here, exposed male migrants' outcomes

[7]Of course, it is possible that this is the case. For instance, among individuals equally damaged by Dust Bowl exposure, it is possible that the more ambitious (i.e. "higher-quality") individuals may be likelier to out-migrate, and that the same characteristics which led them to leave the birth state also drove them to be healthy and successful in later life.

Table 5.2: Impact of Exposure to the Dust Bowl on Later-life Outcomes: Migration Analysis on Adults

	Men			Women		
	Dust Bowl	**Migrant Interaction**		**Dust Bowl**	**Migrant Interaction**	
	(1)	(2)		**(3)**	(4)	
Outcomes	Treated × Erosion	Treated × Erosion	Migrant × Treated × Erosion	Treated × Erosion	Treated × Erosion	Migrant × Treated × Erosion
Probability of Eventual Migration	0.0160			0.0194		
	(0.0288)			(0.0292)		
Age at First Marriage		-0.0688	-0.0894		0.1060	-0.2290**
		(0.1470)	(0.0649)		(0.1230)	(0.0944)
Children Ever Born					-0.2430	0.0870
					(0.1650)	(0.1020)
Probability of Completing High School		0.0113	-0.0018		0.0425*	-0.0373
		(0.0178)	(0.0108)		(0.0221)	
Probability of Completing College		-0.0253**	0.0179		-0.0119***	-0.0023
		(0.0114)	(0.0141)		(0.0011)	
Welfare Income		22.72**	-17.62		21.22	-8.97
		(9.3270)			(13.2600)	(16.1000)
Probability of Poverty		0.0081***	0.0068		0.0050	0.0016
		(0.0029)				(0.0046)
Probability of Cognitive Disability		0.0053	0.0038		0.0010	-0.0143***
		(0.0074)	(0.0043)		(0.0052)	(0.0007)
Probability of Physical Disability		0.0124***	-0.0018		-0.0030	0.0019
		(0.0038)	(0.0041)		(0.0141)	(0.0114)
Probability of Vision & Hearing Difficulty		-0.0013	-0.0039		0.0036	-0.0125**
		(0.0066)	(0.0078)		(0.0047)	(0.0058)
Probability of Self-Care & Independent Mobility Difficulty		0.00265	-0.00538		0.00479**	-0.0129**
		(0.0060)	(0.0082)		(0.0019)	(0.0057)

*** p<0.01, ** p<0.05, * p<0.1; Note: Columns 1 & 4 report $treated_b \times erosion_s$ coefficients. Columns 2 & 4 report $treated_b \times erosion_s$ coefficients (main effect; i.e., that for Dust Bowl-exposed non-migrants) and $migrant_i \times treated_b \times erosion_s$ coefficients (interaction effect; i.e., that for Dust Bowl-exposed eventual migrants) for specifications that interact the baseline treatment term with the individual's lifetime migrant status. All regressions are estimated by OLS and include two-way interactions and controls for race; birth year, birth state, and census state fixed effects; and state trends. Standard errors, clustered by birth state and birth year, are reported in parentheses below each coefficient.

for many human capital indicators (such as the probability of college completion and the probability of physical, vision and hearing, and self-care and independent mobility disabilities) and are shown to be slightly better than those who were exposed but remained in their birth state, but not statistically significantly so. For women, the evidence that exposed eventual migrants experienced better human capital outcomes in adulthood than exposed non-migrants is stronger. Although it appears that exposed female migrants may have gotten married even earlier than those who did not migrate, on the whole the results show that exposed women appeared to have benefited from out-migration. To wit, their chances of cognitive, vision and hearing, and self-care and independent mobility disabilities are 1.25 to 1.43 percentage points lower than those of exposed non-migrant women.

The source of this eventual-migrant advantage is unclear. Given the findings above that Dust Bowl exposure did not drive eventual migration, it is unlikely that this differential effect by migrant status is a result of migrants having received a smaller Dust Bowl "treatment" effect—say, because their period of childhood treatment was cut short, or the severity of their exposure reduced, by migration. Instead, it appears that individuals who migrated away from the state of birth may have benefited from better resources for recovery in their new home states, such as better schools, hospitals, and labor market opportunities. Of course, it is also possible that post-childhood, eventual migration was driven by selection on some unobservable characteristic indicative of individual quality. For instance, even though more severe Dust Bowl exposure is not found to have more intensely driven migration, it is possible that amongst individuals experiencing the same erosion regime, those who were of "better quality" in a human capital sense (e.g. wealthier, healthier, or merely more motivated) may have been more likely to out-migrate, causing positive selection into eventual migrant status.[8]

[8]It is worth noting that the analysis presented above in Section 5.2.1 suggests that at least in childhood, there was no positive selection into migration by observable measures of "child quality."

5.2.3 Migrant Farm Labor Camps

If the popular image of the Dust Bowl Okie was shaped by Steinbeck, then Steinbeck's own view of the migrant "exodus" was born of his observations at the Arvin/Weedpatch federal "Migratory Labor Camp" in Kern County, California. Although the quantitative analysis presented above suggests that the Dust Bowl did not systematically drive individuals—let alone, farmers like the Joads—West to such camps, it is worth briefly examining the receiving side of the migration question, to understand just what sort of remediating environment migrants may have been able to access.[9]

The Arvin/Weedpatch camp was one of many such facilities built in the 1930s throughout California and Arizona by the Resettlement Administration and the Farm Security Administration to handle the influx of migrant agricultural workers to the West. Although this influx had begun well before the 1930s, and came primarily from regions outside the Great Plains, such as the southwestern South (Nealand, 2008), the need for these camps in the 1930s stemmed from the peculiarities of the Depression and the Dust Bowl: while many earlier migrants had tended to leave after the harvest, 1930s-era migrants were not seasonal migrants, and often stayed on in the West for lack of better options (Nealand, 2008).

Accordingly, their presence in communities in California and Arizona strained local public services and became a source of conflict. This was the case certainly *before* the building of such camps, when locals needed to be convinced that their new neighbors were not "degenerate" or "uncivilized" and would be less trouble if offered orderly, sanitary, and humane accommodation (Nealand, 2008)[10]—but tension

In the analysis on adults, it is impossible to test for selection into migration by observable characteristics such as income or health in childhood, adolescence, or young adulthood because these variables are not available in the census. Any indication we have of these individuals' wealth, health, and education is that measured in adulthood, rendering it impossible to determine whether these characteristics drove or were the result of the migration decision.

[9]As I note below, not all migrants would have migrated to California or to migrant labor camps like this one. Accordingly, Weedpatch represents just one type of environment to which Dust Bowl migrants may have been exposed.

[10]For instance, camp manager Tom Collins writes in his December 21, 1935 weekly report that

with local residents continued even *after* the advent of this more organized system for managing migrant labor. For instance, archival sources from the Weedpatch files document frequent arguments with neighboring mainstream communities over whether and how to fund the attendance of camp children in local public schools (Gray, 1937; Stockton, 1937).

Steinbeck's guide to life at Arvin was Thomas E. Collins, the camp manager. Although Steinbeck spent considerable time observing and interviewing Collins and the camp residents, he drew most heavily on Collins' official reports to build his account of migrant farm labor. Collins' meticulous weekly reports, which are now held at the National Archives in San Francisco, were unusual in their casual tone and colorful, quasi-ethnographic descriptions of camp life. Many real-life anecdotes, individuals, and dialects represented in these reports can be found, barely fictionalized, in *The Grapes of Wrath*; indeed, in an appendix entitled "Correlations between events, subjects or items in the novel Grapes of Wrath and in RG 96 FSA Regional Office Camp Admin Files, Reports of Arvin/Weedpatch Migrant Labor Camp 1935-1936," Nealand (2008) links over 40 distinct features of these historical administrative documents to Steinbeck's work of literature.

These weekly government reports also provide information that allows me to corroborate my analysis, and the picture of the Dust Bowl that it paints, from the migrant "recipient" side. For instance, the rosters of new tenants indicate that although Oklahomans make up a large share of camp residents, a great number of camp residents are nevertheless drawn from Arkansas and Missouri (as shown in a sample headcount in Collins (1936a)), states which are not typically thought of as the locus of the Dust Bowl, but which the population-weighted erosion measures presented in Figure 3.3 show were actually more severely (and uniformly) hit in human terms than states like Texas or Oklahoma.

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the camp would soon become a haven for ten families who had just been notified by their employer that they would no longer be permitted to squat on his land (Collins, 1935a).

Similarly, the weekly sanitary reports show that rates of serious illness in the camp were not unusually high[11] (Collins, 1936d), suggesting as does the analysis presented in this section that migrants were unlikely to be negatively (if at all) selected. The reports also boast robust camp sanitation, health facilities, and educational programs (Collins, 1935b, 1936b,c; Unknown, 1936b), consistent with my finding that eventual migrants' better outcomes likely stemmed from an improvement in the post-migration environment. Of course, such evidence must be interpreted cautiously: Arvin/Weedpatch was not the only migrant farm labor camp available to new settlers, not every Dust Bowl migrant even settled in such a camp, and not every camp resident was driven to the West by the Dust Bowl. Nevertheless, these archival sources provide a window into who migrated long-distance, and what sorts of labor market, schooling, and public health conditions they may have faced when they left the Plains.

Indeed, the data most valuable to an attempt to quantitatively corroborate the findings presented in this thesis, and to further explore the dynamics of Dust Bowl exposure and recovery, would have been data on children's "intermediate" outcomes, such as educational attainment or anthropometric health both at entry into the camp, and after some time spent in camp. However, the surviving archival sources contain frustratingly little on these counts. For instance, although some measures of schooling exist in the camp records held by the National Archives, these are scarce, and almost exclusively pertain to school attendance (e.g. Farm Security Administration (1942)) rather than to educational attainment or academic performance.

An even greater loss to researchers is the absence of systematic individual-level health information. Although documentary evidence of an active camp clinic exists, one in which, for example, children were regularly checked and measured against

[11]For instance, the first time Collins has any illnesses to report is over a month after the camp's opening, on January 25, 1936. In what is by then a community of 73 individuals, he reports only 5 cases of any sort of illness, as follows: one case of pneumonia, one liver complaint, one pregnancy, one instance of ptomaine poisoning from canned tomato juice, and one instance of "complications resulting from eating improper food too soon after appendectomy" (Collins, 1936d).

growth standards,[12] it appears that only a blank growth chart (Collins, 1936e), left as a template of the sort of individual-level health information the camp collected as standard practice, survives. There is no clear de-accession record indicating when and how these documents were disposed of, but archivists speculate that these must have been destroyed in the 1970s, when the camp records were acquired by the National Archives, because they were deemed not to be of historical interest (Personal correspondence with Marisa Louie, archivist, NARA San Francisco).

Had these records survived, it would not only have been possible to better establish the anthropometric and general health—for instance, as indicated by stunting, underweight, or high morbidity—of children who migrated out of the Great Plains at the time of camp entry; it would also have allowed tests of catch-up growth that would have shed light on levels of deprivation and recovery processes that could contribute to and clarify the adult outcomes observed in the census. Such intermediate health measurements would be especially useful in understanding what is a non-linear growth or human capital production process, and one where the response of later-childhood and adult health depend on the timing and severity of deprivation. As such, the presence of intermediate and longitudinal data of this sort would allow for a more nuanced examination of human capital formation than studies such as Stein et al. (2007), which study adult height and weight differentials in those exposed to famine prenatally, and unfortunately can say little about health plasticity and short-term outcomes.

5.2.4 Conclusion

Although migration remains the prominent theme in popular depictions of the Dust Bowl, new research is challenging many of the misconceptions surrounding this phenomenon—using systematic analysis to shed light on who migrated during the catas-

[12] In fact, the opening image for Chapter 6 is an archival photograph from Arvin/Weedpatch depicting precisely this activity: that of camp children having their height and weight taken.

trophe, from where, to where, in what numbers, and under what circumstances. The evidence presented here, however, suggests that migration—whether in childhood or subsequently—was less prevalent as a coping strategy than popular images of the 1930s might lead us to believe. Eventual migration, however, does appear to have conferred an advantage on the recovery of migrants, suggesting not only that migration may have been a viable mechanism for adjustment for individuals who chose this option, but also that recovery may be possible given an improvement in material and environmental conditions. Indeed, these results suggest that a policy of evacuation and assisted relocation (had contemporary officials chosen to pursue it and had Plains residents been amenable) could have been one approach to fostering human recovery from the Dust Bowl, and may have likely resulted in higher levels of human capital and wellbeing for this cohort.[13]

5.3 Endogenous Fertility Responses

It is possible that voluntary changes in childbearing behaviors may have been yet another mechanism by which individuals adjusted to the Dust Bowl shock and attempted to mitigate its adverse effects. Specifically, it is possible that Dust Bowl-era women may have deliberately restricted their fertility during the crisis in order to avoid exposing any as-yet unborn child to the health hazards and deprivation associated with the Dust Bowl, and/or in order to avoid overburdening their households, whose incomes were already constrained by the shock. It is also possible that such fertility changes may have been heterogeneous by maternal socioeconomic status. To

[13] Hornbeck (2012) alludes to a similar strategy by which policymakers could have potentially accelerated the agricultural and macroeconomic recovery of the Plains, although in his view the point is to remove people from the area less for their own benefit, but more for the benefit of those remaining on Plains farms and in Plains labor markets. Given the complexity of such a proposal for sending and receiving communities, it would be worth investigating this policy counterfactual in future research, for instance, by comparing the costs, benefits, and feasibility (not just in terms of politics, but also in terms of the receiving labor and housing markets) of such a plan with recovery efforts actually undertaken via the New Deal.

wit, work on crisis events such as famine and macroeconomic busts indicates that often in such periods, only those who can afford to provide for children will choose to continue bearing them (Dehejia & Lleras-Muney, 2004; Ó Gráda, 2011).

Despite such work, questions of fertility selection and of endogenous fertility responses to shocks have received relatively little attention, but are nevertheless important both to accurately estimating the effects of a shock like the Dust Bowl at the economy-wide, cohort, and individual levels (Pitt & Rosenzweig, 1989; Pitt, 1997), and to better understanding how, if at all, individuals adjust contemporaneously to health and income shocks.

For instance, if the Dust Bowl induced reductions in fertility, whether across the board or differentially by maternal socioeconomic status, this fertility response is important in that first, it may reflect a voluntary shock-mitigation strategy.[14]

As above in the discussion of migration, in this section, I focus on this aspect of the fertility response to the Dust Bowl: namely, whether changes in fertility acted as a mechanism of adjustment to this environmental event. However, as alluded to above, such a fertility response is also important in that it may change the composition of births and/or average cohort health status in ways that complicate the interpretation of the results presented earlier in this thesis regarding the later-life wellbeing impact of early-life Dust Bowl exposure. Accordingly, Section 6.4, I separately address the implications of any Dust Bowl-driven changes in fertility for the interpretation of my larger findings.

Although of course the possibility of involuntary fertility restriction exists, and is discussed in Section 6.4, here I ask whether potential Dust Bowl-era mothers had fewer children as a possible means of coping with the Dust Bowl shock, and whether any

[14]Of course, it is possible that if fertility reductions occurred, these were not a choice, but rather represented an involuntary biological consequence of the shock. Indeed, it would be impossible to tease apart involuntary from voluntary fertility reductions using the available data. In this instance, the question is moot, given that the effects of the Dust Bowl on fertility would be expected to be negative regardless of the channel, and given that using multiple alternative strategies I find no evidence of a Dust Bowl-driven reduction in contemporaneous fertility.

subset of these potential mothers was more or less likely to adopt such a mitigation strategy. As a further check, I test Dust Bowl-era fertility adjustments as inferred from cohort size in single-age bands, and find no significant effect of childhood exposure to the Dust Bowl on cohort size. Together, the results of these tests suggest fertility selection has no effect on the findings presented thus far regarding the adult outcomes of children exposed to the Dust Bowl.

Accordingly, I test whether the Dust Bowl reduced fertility and whether it did so selectively in ways that would shed light on who among these potential mothers was better able to cope with the shock, and who was more prone to modify their fertility choices in response to it. Testing for fertility selection into my sample I find no statistically significant differences in fertility in response to the Dust Bowl by maternal socioeconomic status, nor do I find that Dust Bow-era birth rates were driven by Dust Bowl exposure. As a final check, I infer possible Dust Bowl-era fertility adjustments from cohort size in single-age bands, and find no significant effect of childhood Dust Bowl exposure on cohort size. Together, these results suggest that individuals did not pursue a shock-mitigation strategy of fertility adjustment.

5.3.1 Dust Bowl-Era Births

First, I test for Dust Bowl-induced changes in the birth rate at an aggregated level.

5.3.1.1 Methods & Data

I use yearly state-level crude birth rates over the period 1915-1960 from US Vital Statistics (Public Health Service, 1947; National Center for Health Statistics, 1968) for each of the 15 states in my sample to test for whether the Dust Bowl cohort had significantly different (for instance, lower) birth rates than those not exposed.

To test for differences in birth rates across cohorts, I estimate the following regression, by OLS:

$$c_{st} = \alpha + \beta_1 \times treated_t \times erosion_s + \theta_s + \lambda_t + \gamma_s + u_{st} \tag{5.5}$$

where c_{st} is the crude birth rate in state s in year t.

$Treated_t$ is a binary dummy which takes the value of 1 if the year is 1930-40, inclusive. In order to test when birth rates may have begun to respond to Dust Bowl conditions, $treated_t$ is also alternatively defined as 1 if the year is 1931-40, 1932-40, and on until $treated_t$ takes the value of 1 only if the year is 1940; results for each of these definitions of $treated_t$ are reported in Table 5.3 under column headers referring to how $treated_t$ has been defined in that specification.

$Erosion_s$ is, as in the baseline regressions, a population-weighted measure of erosion severity in state s.

As in the baseline, θ_s and λ_t represent state fixed effects and year fixed effects, while γ_s represents state trends. Standard errors are clustered at the state level. Results are reported in Table 5.3.

5.3.1.2 Results & Discussion

The coefficients for the Dust Bowl exposure term appear in Table 5.3, with each of the column headers 1-11 referring to the set of years deemed treated by the Dust Bowl. The intent of varying the treatment window is to examine whether there was a distinct point during the Dust Bowl period when fertility patterns changed, whether involuntarily (e.g. due to famine amenorrhea or maternal ill health, per Ladurie (1969)) or, as is the focus here, voluntarily due to individual or household choices. The results in Table 5.3 indicate that Dust Bowl exposure was not a statistically significant driver of birth rates.[15]

[15]The failure of the Dust Bowl to significantly affect birth rates at the state level could be in part because of the effects of New Deal expenditure. For instance, in Fishback et al. (2007), city-level analysis indicates that although fertility rates were depressed during the 1930s (a fact accounted for

Table 5.3: Impact of Dust Bowl Exposure on State Birth Rates

	1930-40 (1)	1931-40 (2)	1932-40 (3)	1933-40 (4)	1934-40 (5)	1935-40 (6)	1936-40 (7)	1937-40 (8)	1938-40 (9)	1939-40 (10)	1940 (11)
Crude Birth Rate	0.0518	0.0455	0.0415	-0.1400	-0.3610	-0.5300	-0.5600	-0.5460	-0.5360	-0.4170	-0.4890
	(0.4770)	(0.5430)	(0.6220)	(0.7550)	(0.7450)	(0.7050)	(0.7020)	(0.8570)	(0.9330)	(0.9300)	(1.1290)

*** $p<0.01$, ** $p<0.05$, * $p<0.1$; Note: Table reports $treated_t \times erosion_s$ coefficients. The left-hand column refers to the regression's dependent variable, while the remaining column headings refer to the way the $treated_t$ term is defined (e.g. for Column 1, $treated_t = 1$ if the year is 1930-40, inclusive). All regressions are estimated by OLS and include state and year fixed effects and state trends. Standard errors, clustered by state, are reported in parentheses below each coefficient.

However, the sign of the coefficients suggests a turning point in 1933, after which birth rates decline. Coinciding as this turning point does with the trough of the Depression and the advent of the Dust Bowl's most severe storms, it is possible, had these coefficients been significant, to interpret this decline in birth rates as the result of households adapting to circumstances which were not only worsening, but which clearly now would not be resolved quickly. Nevertheless, with such imprecisely estimated coefficients, it is not possible to say that the Dust Bowl caused any observed decline in fertility. Instead, it appears that the Dust Bowl did not create an endogenous fertility response.

5.3.2 Selectivity of Dust Bowl-Era Births by Maternal Characteristics

Perhaps the stability in birth rates during the Dust Bowl is partly a product of heterogeneity in fertility.[16] Accordingly, and in order to establish what types of mothers, if any, were likelier to deviate from pre-Dust Bowl fertility trends in response to the shock, I test whether maternal socioeconomic status modifies the probability of a Dust Bowl birth.

5.3.2.1 Methods & Data

I use data on ever-married women enumerated in my 15 states of interest in the 1% IPUMS samples of the 1920, 1930, 1940, and 1950 US Censuses (Ruggles et al., 2010) to test for fertility selection into my sample. Similar to the method employed in baseline specification (given in Equation 3.1), I test for whether ever-married women enumerated in 1940 had a lower probability of having had children in the preceding

in my analysis by the inclusion of year fixed effects and trends), relief spending staved off further declines and by the late 1930s brought these rates back on trend as households were enabled to return to pre-Depression-era family planning behaviors.

[16] As mentioned above, this could be because of heterogeneous preferences or because of heterogeneous health endowments.

10 years (that is, that women in 1940, for whom the preceding 10 years coincided with the Dust Bowl period), than those ever-married women for whom the past 10 years were not spent in the Dust Bowl. I interact the Dust Bowl treatment term with measures of household income, socioeconomic status, and farm status to test whether there was heterogeneity in Dust Bowl-era childbearing patterns by maternal characteristics.

To test for differences in likelihood of having borne children in the past 10 years across time, I estimate the following regression, by OLS:

$$
\begin{aligned}
p_{ibst} = \alpha + \beta_1 \times treated_t \times erosion_s \\
+\beta_2 \times householdcharacteristic_{ibst} \times treated_t \times erosion_s \\
+\beta_3 \times householdcharacteristic_{ibst} \times treated_t \\
+\beta_4 \times householdcharacteristic_{ibst} \times erosion_s \\
+\beta_5 \times householdcharacteristic_{ibst} \\
+{x_{ibst}}' \psi + \theta_s + \lambda_b + \eta_t + \gamma_s + u_{ibst}
\end{aligned}
\tag{5.6}
$$

where p_{ibst} is a binary dummy which takes the value of 1 if woman i born in year b and enumerated in state s and census year t reported having any children aged 10 or under.

$Treated_t$ is a binary dummy which takes the value of 1 if the census year is 1940.

$Erosion_s$ is, as in the baseline regressions, a population-weighted measure of erosion severity in the state in which the woman is enumerated.

$Householdcharacteristic_{ibst}$ is a measure of the woman's household income or socioeconomic circumstances; several alternative versions of socioeconomic and household status are used, including Occscore, SEI, PRESGL, and farm status. (See Section 5.2.1.1 for a details of these variables' construction.) Results are reported in Table 5.4, with the column headers referring to which of these interacted variables has been used in the specification.

x_{ibst} represents a vector of individual- and household-level controls, including race, age, household farm status (for regressions using income variables and where, thus, farm status has not already been included as a control), and income (for regressions using farm status, where income has not already been included as a control). In regressions on farm status, the income variable control chosen is Occscore.

θ_s, λ_b, and η_t represent census state fixed effects, birth year fixed effects, and census year fixed effects, while γ_s represents state trends. Standard errors are clustered at the census state and birth year levels. Results are reported in Table 5.4.

5.3.2.2 Results & Discussion

The evidence provided in Section 5.3.1 established that the Dust Bowl did not cause women to have significantly fewer children in aggregate over the 1930s. However, analysis using aggregated vital statistics do not allow for the testing of changes in the composition of Dust Bowl-era mothers, particularly where the composition of mothers may depend on maternal characteristics that also influence the child's initial human capital endowment and subsequent development environment. Here, for contrast, it is possible to examine the role of selectivity into Dust Bowl motherhood. These results indicate that there is in fact no clear or statistically significant selection into Dust Bowl-era fertility by maternal socioeconomic status.

Table 5.4 presents the coefficients on standard Dust Bowl exposure at the individual level, as well as the coefficient for this exposure term interacted with individual measures of a mother/potential mother's socioeconomic status. For all four definitions of maternal socioeconomic status, the interaction is insignificant. For the income-proxying measures, especially, the magnitude of effects are minuscule; for instance, a one-standard-deviation increase in a Dust Bowl-exposed woman's occupational income score only decreases her probability of having had a child in the preceding 10 years by 0.70 percentage points relative to her poorer Dust Bowl-exposed counterpart. The direction of the association is also unstable across definitions of maternal socioeconomic status, making a clear relationship between maternal status and fertility difficult to identify, particularly when the literature on poverty and fertility shows that there is empirical precedent for both.

Together, these results indicate no significant differences in fertility by maternal/household characteristics. Thus, among Dust Bowl-stricken potential mothers, it does not appear that one socioeconomic group was more or less vulnerable than another to involuntary fertility changes, nor does it appear that one socioeconomic group was more or less likely to employ voluntary fertility restrictions as a Dust Bowl coping mechanism. Perhaps, in contrast to the famine examples provided earlier, the Dust Bowl was less acute and so did not spur changes in fertility behavior. It is also possible that as a result of this low-grade deprivation, health damage remained latent and did not attract the attention of individuals who otherwise may have wished to remediate this harm.

5.3.3 Cohort-Size Approach to Demographic Accounting

As a final check, I test for differences in birth cohort size, an approach similar to that used in Jayachandran (2009) to infer infant mortality due to pollution, and one which is used here to test for the net demographic effect of the Dust Bowl.

5.3.3.1 Methods & Data

I use data from the 1920-1950 decennial censuses (Ruggles et al., 2010) to test for possible changes in cohort size by single-year age band. That is, I ask whether there are more or fewer children of a given age in the Dust Bowl-exposed cohort than there are children of that same age in non-Dust Bowl-exposed cohorts. I weight the sample of data provided in Ruggles et al. (2010) to produce the actual population cohort size

Table 5.4: Impact of Household Status on Decision to Bear Children During the Dust Bowl

	Occscore		**SEI**		**PRESGL**		**Farm**	
	(1a)	(1b)	(2a)	(2b)	(3a)	(3b)	(4a)	(4b)
Outcome	Treated × Erosion	Occscore × Treated × Erosion	Treated × Erosion	SEI × Treated × Erosion	Treated × Erosion	PRESGL × Treated × Erosion	Treated × Erosion	Farm × Treated × Erosion
Probability of Having Had a Child in the Past 10 Years	0.0489**	-0.00069	0.0356	0.00020	0.0360	0.00017	0.0454	-0.01090
	(0.0221)	(0.00080)	(0.0261)	(0.00030)	(0.0447)	(0.00100)		(0.04110)

*** p<0.01, ** p<0.05, * p<0.1; Note: Table reports $treated_b \times erosion_s$ coefficients (main effect) and $characteristic_{ibst} \times treated_b \times erosion_s$ coefficients (interaction effect) for specifications that interact the baseline treatment term with the woman's household income/socioeconomic status measure. All regressions are estimated by OLS and include two-way interactions and controls for race, age, household farm status, and income; census state, birth year, and census year fixed effects; and state trends. Standard errors, clustered by census state and birth year, are reported in parentheses below each coefficient. Farm status is a binary variable, while the other household status terms are continuous; to aid in interpreting the interaction coefficient for the remaining household status variables, the minimum, maximum, and standard deviation are as follows: Occscore (3, 80, 10.081); SEI (4, 96, 21.632); PRESGL (9.3, 81.5, 13.327).

by birth year, birth state, race, and sex. I then estimate, for individuals of the same age for each single-year age band from ages 0 to 12, the following equation by OLS:

$$\textit{ln(cohort size)}_{grsb} = \alpha + \beta_1 \times treated_b \times erosion_s + x_{grsb}{}'\psi + \theta_s + \lambda_b + \gamma_s + u_{grsb} \quad (5.7)$$

Here, $\textit{ln(cohort size)}_{grsb}$ represents the log of the size of the cohort containing individuals of sex g and race r born in year b in state s.[17]

The term $treated_b \times erosion_s$ refers to whether an individual was exposed to the Dust Bowl. As in the baseline, $treated_b$ is equal to 1 for those individuals aged -1 to 12 at any time during the Dust Bowl (1930-1940, inclusive). Similarly, $erosion_s$ represents the erosion intensity in the child's birth state over the Dust Bowl period.

x_{grsb} represents a vector of controls which here includes the race and the sex of the cohort in question.

Lastly, θ_s and λ_b represent birth-state fixed effects and birth-year fixed effects, respectively, while γ_s represents state trends.

Note that age, which would normally have been included for a cohort study using multiple censuses, has been omitted as a control since by design, only those individuals of the same age have been included in a given regression. Census year dummies have been omitted since, in single-year age bands, these are equivalent to the birth-year fixed effects already included. Standard errors are clustered at the birth-state level.

I also repeat the above analysis using the year before birth rather than the year of birth as the temporal component of exposure. The results of this analysis are

[17] Since I restrict the sample in each regression to individuals of the same age in their respective census (that is, I run separate regressions consisting only of individuals aged 0, only of individuals aged 1, and so on up to age 12) the combination of the census wave and the birth year determine the age. For instance, the regression comparing sizes of different cohorts of 9-year-olds includes the birth years 1911 (9 years old in the 1920 census), 1921 (9 years old in the 1930 census), 1931 (9 years old in the 1940 census), and 1941 (9 years old in the 1950 census). Of these four groups, the 1921, 1931, and 1941 birth cohorts are deemed treated by the Dust Bowl, per the definition of Dust Bowl treatment given in Section 3.2.

presented in Table 5.5.

5.3.3.2 Results & Discussion

Row 1 of Table 5.5 indicates that the Dust Bowl exposure in the year of birth was not a significant driver of cohort size. Although reductions in cohort size may conflate reductions in fertility with increases in mortality, I interpret these cohort size reductions, in light of the other fertility and mortality results, as evidence of likely voluntary decisions on the part of exposed mothers to forgo or defer childbearing. Changes in cohort size due to migration by potential parents and thus changes in the location of births should be captured by the inclusion of state trends, unless these population declines/increases were temporary. Furthermore, if the migration of potential parents were to affect cohort size, I would expect if anything a widening in the gap between Dust Bowl-and non-Dust Bowl cohort sizes as those who would have given birth in Dust Bowl states instead give birth elsewhere. (A similar widening of the cohort-size gap would be expected inter-temporally, if births deferred during the Dust Bowl occurred at higher rates after the end of the crisis.) However, given the limited evidence for the Dust Bowl cohort of interstate migration in childhood; a post-Dust Bowl baby boom; increases in stillbirth rates, infant mortality rates, or later-life mortality rates (discussed below in Section 6.5); or decreases in birth rates, the lack of significant cohort-size effects suggests that during the Dust Bowl, and in contrast to the acute famines referenced earlier in this section, individuals and households did not in fact attempt to mitigate the shock by restricting fertility.

When exposure is measured by the year before birth as given in row 2, however, I find that the 1940 and 1936 birth cohorts are significantly smaller than their unexposed age 0 and age 4 counterparts, respectively. A discussion of the implication of these results for the interpretation of later-life health effects is given in Section 6.4. However, from the perspective of endogenous fertility responses, these results, like

Table 5.5: Impact of Dust Bowl Exposure on Cohort Size by Age

	Age 0	Age 1	Age 2	Age 3	Age 4	Age 5	Age 6	Age 7
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Log Cohort Size (YOB Exposure)	-0.5210	-0.0054	-0.0043	-0.0782	-0.0232	-0.1300	0.3210	-0.0906
	(0.4350)	(0.2150)	(0.3600)	(0.2410)	(0.2090)	(0.1750)	(0.3260)	(0.1410)
Log Cohort Size (YOB-1 Exposure)	-0.4760*	-0.0648	0.0335	-0.2700	-0.2890***	-0.2640	0.2520	0.1610
	(0.1650)	(0.2340)	(0.1710)	(0.2190)	(0.2380)	(0.0780)	(0.2460)	(0.2800)

	Age 8	Age 9	Age 10	Age 11	Age 12
	(9)	(10)	(11)	(12)	(13)
Log Cohort Size (YOB Exposure)	0.3180	0.1550	-0.0394	-0.2170	-0.0020
	(0.2580)	(0.3810)	(0.2450)	(0.5320)	(0.3460)
Log Cohort Size (YOB-1 Exposure)	0.2350	0.3900	-0.5720	-0.1730	-0.5540
	(0.3050)	(0.3770)	(0.5000)	(0.2350)	(0.4730)

*** $p<0.01$, ** $p<0.05$, * $p<0.1$; Note: Table reports the $treated_b \times erosion_s$ coefficient, e.g. the effect of Dust Bowl cohort membership for children of a given age on log cohort size, relative to the log cohort size of children of the same age in cohorts not exposed to the Dust Bowl. Column headers refer to the single-year age band for which the regression has been run. Row 1 reports results where $treated_b$ is based on whether the individual's year of birth falls within the Dust Bowl period; Row 2 reports results where $treated_b$ is based on whether the year before the individual's year of birth falls within the Dust Bowl period (i.e. they sustained *in utero* exposure). All regressions are estimated by OLS and include birth-state and birth-year fixed effects and birth-state trends, as well as controls for cohort race and sex. Standard errors, clustered by state, are reported in parentheses below each coefficient.

the insignificant ones regarding a post-nadir turning point in birth rates, suggest that it may have taken parents some time to assess the crisis, and for these assessments to be reflected in their family planning choices.[18]

5.3.4 Conclusion

The analysis above on individual mothers has shown no evidence of differential fertility by maternal socioeconomic status, at a given level of Dust Bowl exposure. Meanwhile, tests on vital statistics data indicate that birth rates stayed on trend for Dust Bowl-era mothers. A similar finding holds when examining possible changes in cohort size. These results suggest not only that individuals did not voluntarily reduce their fertility during the crisis, but also that if there was some malnutrition threshold below which potential pregnancies would have failed to take place biologically, this threshold was not met. Simply, there is no evidence that the Dust Bowl drove changes in fertility, let alone that fertility adjustment was a systematic and strategic response to the Dust Bowl shock.

5.4 The New Deal & Recovery

5.4.1 Background

New Deal-era programs helped a Depression-ravaged nation get back on its feet. Beginning in 1933 with President Franklin D. Roosevelt's First Hundred Days, a patchwork of evolving policies was formed to cope with the worsening crisis. What the approach may have lacked in coherent strategy it made up for in sheer scale and ambition: dozens of distinct programs were created, and substantial shares of GNP spent, to address a diversity of needs (Fishback & Wallis, 2013). Relief agencies

[18]Alternately, it could be that the Dust Bowl's adverse health and income effects only became severe enough to result in infertility later on in the 1930s.

like the Works Progress Administration targeted the jobless but employable, while the Social Security Administration provided assistance to those who could not work and the Federal Emergency Relief Administration and targeted both; meanwhile, the Public Works Administration stimulated employment and local economic activity, the Agricultural Adjustment Administration regulated and supported farm production, and the Soil Conservation Service provided technical assistance to farmers while cataloging the Dust Bowl's devastation in the very maps from which the erosion data in this thesis is drawn (Soil Conservation Service 1935; Fearon 2007, Chs. 2 & 5). While a good many of these programs were phased out during or shortly after the 1930s, the legacy of many Depression-era recovery initiatives remains to this day: for instance, in the modern American welfare state as well as in infrastructure projects from the Hoover Dam to LaGuardia Airport to the large-scale modernization and electrification of Appalachia through the Tennessee Valley Authority (Fishback & Wallis, 2013; Fishback, 2016).

Recovery efforts also took place on a local basis, sometimes anticipating the shape of federal relief projects soon to follow. Some grassroots relief programs, such as Kansas's initiatives to resettle the unemployed on small subsistence farms, were eventually folded into the New Deal; others, such as attempts to institute barter economies, were obviated by federal programs (Fearon, 2007, Ch. 2). Indeed, the sort of coordination of federal, state, and local funds for public service provision strategies ushered in by the New Deal would become "a fundamental feature of the fiscal structure of the country" (Fishback & Wallis, 2013, p. 296).

Given the scope and scale of New Deal and related expenditure, it is possible that in attributing any recovery experienced by Dust Bowl children to possible compensatory household investments in human capital, I may be ignoring the direct and indirect roles of state expenditure on Dust Bowl relief—e.g., cash assistance, farm loans, or the building of schools, hospitals, and roads. I thus test for whether the

New Deal attenuated Dust Bowl impacts. I find that New Deal and related expenditure was indeed effective in mitigating the adverse effects of the Dust Bowl, a result which suggests that remediation of early-life insults is possible. This study is, to my knowledge, one of the only studies in the early-life health literature providing empirical evidence of such remediation, and one of the few to consider the contemporaneous policy response to disaster.

5.4.2 Methods & Data

As in the regressions on farm-dependence, I interact measures of total state-level per capita public spending over the period (Fishback et al., 2003)[19] with the measure of treatment as follows:

$$\begin{aligned} h_{ibst} = \alpha + \beta_1 \times treated_b \times erosion_s + \beta_2 \times publicspending_s \times treated_b \times erosion_s \\ + \beta_3 \times publicspending_s \times treated_b + {x_{ibst}}' \psi + \theta_s + \lambda_b + \eta_t + \gamma_s + u_{ibst} \end{aligned} \tag{5.8}$$

The $publicspending_s$ term above alternately takes the value of three key categories of spending, defined as follows and corresponding to the column headers in Tables 5.6 and 5.7:

New Deal – total per capita New Deal expenditure over the period in the individual's birth state. This category of spending includes expenditures such as those on the construction of roads and public buildings.

Relief – total per capita relief expenditure over the period in the individual's birth state as a part of New Deal and related programs. This category of spending includes

[19] See Fishback et al. (2003) for details of the variables' construction, sources of the underlying data, and detailed discussion of New Deal-era programs.

expenditures such as those on Works Progress Administration, Federal Emergency Relief Administration, and Public Assistance grants.

Loans – total per capita loan expenditure over the period in the individual's birth state as a part of New Deal and related programs. This category of spending includes expenditures made in aid of farms and housing.

For further detail on each spending category, see Fishback et al. (2003).

In order to trust the results of this analysis, it is necessary to demonstrate the plausible exogeneity of New Deal and related spending. I find these public spending variables to be correlated neither with observable birth-state characteristics such as erosion[20] nor with the residuals of the adult outcome regressions.[21] The apparent exogeneity of New Deal allocations is consistent with Fishback et al. (2007) and Fishback et al. (2003), who suggest that funding may not have been allocated purely by need; rather, such spending may have in part been used to curry political favor in swing states. Additionally, I find using annual state-level spending data for a variety of highly granular New Deal programs (Fishback, 2015; Fishback & Kachanovskaya, 2015) that there is little correlation between spending and contemporaneous or lagged values of state drought or income per capita, except in the case of a few very narrow categories of relief spending (not reported).

It is worth noting that while such exogeneity in spending is of course useful in that it enables clean identification of the effects of investments in recovery, it is neither desirable nor optimal from a policy standpoint. That is, it would be unfortunate for spending to be exogenous—say, to Depression or to Dust Bowl severity—if the goal of such spending were to repair the damage to child human capital induced by these

[20]For instance, the correlation between total state expenditure per capita and state erosion are -0.0670 for New Deal spending, 0.0935 for relief spending, and -0.2822 for loans.

[21]The correlations here are exceptionally low for all regressions and categories of spending, and are never in excess of 0.10 in absolute value.

events. An important implication of this is that the true treatment-on-the-treated effects of this recovery spending are likely to be larger than those estimated here.[22]

5.4.3 Results & Discussion

The results from these regressions are reported in Tables 5.6 (for men) and 5.7 (for women). Public expenditure associated with the New Deal is found to attenuate the adverse effects of Dust Bowl exposure in large and statistically significant ways.

Relief spending in particular appears to mitigate the effect of Dust Bowl exposure: a one standard deviation increase in per capita relief spending raises exposed men's ages at marriage by 1.21 years, reduces welfare income by $60.13, and reduces work disability, vision and hearing disability, and self-care and independent living disability by 0.89, 4.49, and 2.05 percentage points, respectively. Exposed women similarly see a 0.84-year rise in age at marriage given the same increase in per capita relief spending, although they also experience a puzzling 8.8 percentage point increase in physical disability.

Per capita loans similarly ameliorate adverse impacts on male college completion and female work disability, albeit to a smaller degree: a one standard deviation increase in per capita loans is associated with a 0.66 percentage point greater chance of completing college, and the same increase in spending contributes to 0.52 percentage point lower rates of work disability amongst exposed women.

New Deal expenditures, meanwhile, are associated with higher probabilities of high school completion among both exposed men (5.93 percentage points for a one standard deviation increase in spending) and exposed women (3.26 percentage points). Given that loans and New Deal spending are associated with higher probabilities of high school completion amongst the exposed, but relief spending has the opposite effect, perhaps spending was most effective on improving secondary schooling outcomes

[22] A counterfactual estimation of the recovery potential of this spending, given strategically rather than randomly targeted investment, is a matter for future research.

Table 5.6: Impact of Dust Bowl Exposure by Mechanism: Public Spending – Men

	New Deal		**Relief**		**Loans**	
	(1a)	(1b)	(2a)	(2b)	(3a)	(3b)
Outcomes	Treated × Erosion	New Deal × Treated × Erosion	Treated × Erosion	Relief × Treated × Erosion	Treated × Erosion	Loans × Treated × Erosion
Age at First Marriage	-0.2590	0.00075	-3.9740***,†††	0.05290***,†††	-0.0758	-0.00173
	(0.2420)	(0.00211)	(1.1640)	(0.01600)	(0.3420)	(0.00389)
Probability of Completing High School	-0.1510***,†††	0.00125***,†††	0.3260***,††	-0.00422***,††	-0.0880***,†††	0.00148***,†††
	(0.0309)	(0.00025)	(0.1110)	(0.00146)	(0.0250)	(0.00031)
Probability of Completing College	-0.0227	0.00010	0.0012	-0.00017	-0.0313**	0.00028*
	(0.0162)	(0.00010)	(0.0889)	(0.00115)	(0.0148)	(0.00016)
Welfare Income	7.18	0.07430	202.80***,†††	-2.58600***,†††	1.79	0.26000
	(20.9600)	(0.17400)	(54.6100)	(0.73700)	(21.3600)	(0.26000)
Probability of Poverty	-0.0154	0.00018*	0.0768	-0.00095	-0.0070	0.00024
	(0.0124)	(0.00009)	(0.0536)	(0.00071)	(0.0121)	(0.00017)
Probability of Cognitive Disability	0.0075	0.00000	0.0987	-0.00126	0.0080	-0.00004
	(0.0265)	(0.00020)	(0.0676)	(0.00088)	(0.0145)	(0.00011)
Probability of Physical Disability	0.0377*	-0.00020	0.0132	-0.00003	0.0404***,††	-0.00041**
	(0.0226)	(0.00018)	(0.0676)	(0.00093)	(0.0138)	(0.00017)
Probability of Vision & Hearing Difficulty	-0.0178	0.00013	0.1350*	-0.00191**	-0.0060	0.00007
	(0.0289)	(0.00022)	(0.0721)	(0.00096)	(0.0195)	(0.00024)
Probability of Self-Care & Independent	0.0205	-0.00014	0.0631**	-0.00088**	0.0080	-0.00007
Mobility Difficulty	(0.0140)	(0.00011)	(0.0256)	(0.00035)	(0.0059)	(0.00008)

*** p<0.01, ** p<0.05, * p<0.1 uncorrected; † † † p<0.01, †† p<0.05, † p<0.1 Bonferroni-corrected; Note: Tables report $treated_b \times erosion_s$ coefficients (main effect) and $publicspending_s \times treated_b \times erosion_s$ coefficients (interaction effect) for specifications that interact the baseline treatment term with the relevant measure of the state's total per capita New Deal and related spending. All regressions are estimated by OLS and include two-way interactions and controls for race; birth year, birth state, and census year fixed effects; and state trends. Standard errors, clustered by birth state and birth year, are reported in parentheses below each coefficient. Public spending variables are continuous; to aid in interpreting the interaction coefficient for these regressions, the minimum, maximum, and standard deviation for the relevant variables in the full sample are as follows: New Deal (104.351, 289.141, 47.396); Relief (41.708, 128.980, 23.143); Loans (58.462, 167.357, 23.787).

Table 5.7: Impact of Dust Bowl Exposure by Mechanism: Public Spending – Women

	New Deal		**Relief**		**Loans**	
	(1a)	(1b)	(2a)	(2b)	(3a)	(3b)
Outcomes	Treated × Erosion	New Deal × Treated × Erosion	Treated × Erosion	Relief × Treated × Erosion	Treated × Erosion	Loans × Treated × Erosion
Age at First Marriage	0.4630**	-0.00360*	-2.6710***,†††	0.03680***,†††	0.1970	-0.00337
	(0.2230)	(0.00199)	(0.6960)	(0.00965)	(0.2730)	(0.00248)
Children Ever Born	0.0257	-0.00164	1.0060*	-0.01690*	-0.2030	0.00078
	(0.4590)	(0.00342)	(0.5990)	(0.00916)	(0.3330)	(0.00295)
Probability of Completing High School	-0.0594	0.00069**	0.5130***,†††	-0.00662***,†††	-0.0623*	0.00139***,†††
	(0.0397)	(0.00035)	(0.1200)	(0.00158)	(0.0327)	(0.00038)
Probability of Completing College	-0.0276	0.00011	-0.0800	0.00095	-0.0335**	0.00029
	(0.0304)	(0.00023)	(0.0608)	(0.00080)	(0.0158)	(0.00019)
Welfare Income	-23.97	0.30500	-12.93	0.40400	-9.51	0.37800*
	(38.3300)	(0.23500)	(135.7000)	(1.72600)	(17.2800)	(0.19800)
Probability of Poverty	0.0067	-0.00001	-0.0035	0.00010	0.0162*	-0.00015
	(0.0210)	(0.00017)	(0.0538)	(0.00073)	(0.0086)	(0.00011)
Probability of Cognitive Disability	-0.0060	0.00002	0.1290**	-0.00186**	-0.0150	0.00020
	(0.0230)	(0.00018)	(0.0586)	(0.00083)	(0.0176)	(0.00021)
Probability of Physical Disability	0.0036	-0.00006	-0.2780***,†††	0.00379***,†††	0.0363	-0.00059
	(0.0165)	(0.00017)	(0.1040)	(0.00146)	(0.0328)	(0.00042)
Probability of Vision & Hearing Difficulty	-0.0387*	0.00027	0.0388	-0.00055	-0.0134*	0.00021
	(0.0221)	(0.00017)	(0.0517)	(0.00066)	(0.0069)	(0.00013)
Probability of Self-Care & Independent Mobility Difficulty	-0.0232*	0.00017	-0.0293	0.00038	-0.0003	0.00002
	(0.0133)	(0.00010)	(0.0629)	(0.00084)	(0.0084)	(0.00012)

*** p<0.01, ** p<0.05, * p<0.1 uncorrected; † † † p<0.01, †† p<0.05, † p<0.1 Bonferroni-corrected; Note: Tables report $treated_b \times erosion_s$ coefficients (main effect) and $publicspending_s \times treated_b \times erosion_s$ coefficients (interaction effect) for specifications that interact the baseline treatment term with the relevant measure of the state's total per capita New Deal and related spending. All regressions are estimated by OLS and include two-way interactions and controls for race; birth year, birth state, and census year fixed effects; and state trends. Standard errors, clustered by birth state and birth year, are reported in parentheses below each coefficient. Public spending variables are continuous; to aid in interpreting the interaction coefficient for these regressions, the minimum, maximum, and standard deviation for the relevant variables in the full sample are as follows: New Deal (104.351, 289.141, 47.396); Relief (41.708, 128.980, 23.143); Loans (58.462, 167.357, 23.787).

where it was targeted at agricultural recovery and infrastructural investment.

Whatever the mechanism, state spending aimed at Depression and Dust Bowl recovery appears to have been effective in compensating for early-life insults and diminishing the worst Dust Bowl impacts on human capital—in particular those on disability and schooling. Indeed, given that New Deal spending was exogenous to Dust Bowl severity and may have been allocated to individuals of varying exposure and need somewhat incidentally, it is likely that my findings here are underestimates of the potential for remediation and the return on post-shock investment. Instead, each dollar spent might have been even more productive in recovery had spending been targeted to affected individuals (or even to strategic subgroups of those affected) through interventions specifically designed to boost maternal and children's health and nutrition.

It is important to note as first touched upon in Section 4.3.3 when contrasting public and private recovery strategies, that while the results presented in this section concerning recovery indicate that the biological possibility for remediation exists given this level of investment, they do not say anything about whether these investments were efficient or whether they ought to have been undertaken from an economic perspective. Indeed, per the framework outlined in Section 2.3, while it is possible that these investments may have been an efficient response by the state to the Dust Bowl shock, based on the public returns to such human capital investment as the state assessed them, it is entirely possible that recovery expenditure through New Deal programs was actually inefficient.

One source of possible inefficiency arises from the fact that the recovery of children from Dust Bowl-induced health and educational insults was not the priority of the New Deal and related programs. Rather, the effects of this spending on human capital recovery may have been an *incidental* consequence of these programs, rather than the specific aim. Indeed, although some schools, hospitals, and sanitary facilities were built using public health service resources, few of the major spending programs were aimed explicitly at ameliorating injury to children's health, whether generally or that specifically due to Dust Bowl damage. Instead, interventions were most often targeted at lowering unemployment, supplementing household incomes, and managing the recovery of the farming industry (Fishback & Kachanovskaya, 2015). Such interventions would of course have the consequence of improving household economic circumstances in a way that empirical work has shown would have beneficial effects on health, education, and wellbeing. However, they would not have been designed with the specifics of the human capital production function in mind.[23] Furthermore, if as the secondary literature suggests and my own calculations have indicated, funding allocations were largely uncorrelated with Dust Bowl and Depression severity (and may have in fact been influenced by outside factors such as political concerns), it increases the likelihood that the levels and recipients of investment were not efficiently chosen. To put it simply, it is perhaps unsurprising that when faced with an enormous influx of funding through a variety of channels, children's recovery was facilitated.

It is also possible that efficiency may not have been an aim of such spending. If the recovery of children was a political aim rather than the concern of a government investing in its future workforce and tax base, the state may have intentionally chosen an otherwise economically inefficiently large level of investment to achieve these goals. Similarly, if the state's concern was for equity in health, or for health as a basic human right, then they may have chosen to ignore the possible inefficiency of investments in recovery. Nevertheless, these results suggest that the capacity for remediation from a disaster of this scale does exist given the right circumstances, even if it is unclear whether the level of investment in this case is justified by the returns.

[23]In fact, it would in fact be quite difficult even in a back-of-the-envelope sense to estimate the true costs of recovery and to judge these against the economic value of such recovery. This is especially challenging given the need to disentangle the spending associated with, and effects of, this complexly interacting and complementary bundle of programs, in order to isolate the spending components directly responsible for the recovery observed here.

5.4.4 Conclusion

In this section, I have shown that New Deal and related public spending on recovery strongly mitigated the adverse impacts of early-life Dust Bowl exposure. For instance, increased expenditure per capita served to raise the exposed cohort's college completion rates and lower their rates of disability, relative to their exposed peers who received less expenditure. In some cases, the beneficial effects of spending appear to have been large enough to cancel out the Dust Bowl's insult to the human capital endowment. These findings indicate that the biological capacity for remediation exists if policy responses are as timely, substantial, and mutually reinforcing, as in the case of the New Deal. They also suggest that had investments been targeted more strategically than they appear to have been in this historical context, both the full potential for remediation and the productivity of a dollar spent on recovery may be quite a bit higher than observed here.

Given these strong findings on public spending and the remediation of early-life insults, I plan in future research to disaggregate New Deal spending by program using newly-digitized annual state- and program-level data made available in Fishback & Kachanovskaya (2015) and Fishback (2015) (here used only in checks of spending exogeneity). Such analysis would allow for an exploration of the role specific policies played in the remediation of early-life insults. In particular, it would be possible using this data to test the relative effectiveness of cash transfers (which would have alleviated what appear to be binding household constraints on compensatory health investments) and broader infrastructural spending (which would have improved access to public services while also raising the return to human capital investment) on short- and long-term human capital outcomes. Similarly, fully exploiting the spatial richness in the Fishback et al. (2003) total spending data (here aggregated at the state level to match individuals' birth states) might yield fruitful analysis of the effects of intra-state spending allocations, which state and local governments may have been better able to target, on contemporaneous human capital outcomes which can be observed at the county level. Both of these avenues of research would allow for more sophisticated analysis of the mechanics of recovery.

5.5 Conclusion

This chapter has demonstrated that the available strategies for insult mitigation and remediation—e.g. migration, fertility restriction, compensatory investment—were not always pursued. However, and importantly, where these were taken up, the evidence presented in this chapter indicates that there did indeed exist some, and at times considerable, capacity for recovery.

For instance, although out-migration and fertility restriction could have offered an effective means of adjusting to the crisis contemporaneously, it appears that those exposed to the Dust Bowl did not take advantage of these insult-mitigation and remediation strategies. However, for those who did eventually migrate, whether the move was a strategic response to health or an incidental change in circumstances, later-life outcomes are better. Barring selection on unobservable migrant quality, this finding suggests that these adult migrants likely benefited from access to superior conditions for recovery in their new home states.

Most saliently, the analysis presented here suggests a role for policy in fostering recovery from adverse early-life shocks to health and income, such as those occasioned by natural disasters or similar crises. Indeed, these findings show that New Deal and related programs were effective in remediating many of the Dust Bowl's worst effects on later-life human capital and wellbeing. By mitigating (perhaps only unwittingly and incidentally) serious damage to human capital that only manifested in the long run, these programs have had a longer-reaching influence than the literature may currently recognize.

Chapter 6

Confounding Factors

Figure 6.1: Parents at the Arvin Migratory Labor Camp Waiting to Have Their Children's Heights and Weights Measured, Weedpatch, California, 1936 (Unknown, 1936b)

6.1 Overview

There are four major phenomena that could be said to have confounded effects here attributed to Dust Bowl exposure: the Great Depression, which could have affected childhood incomes independently of the Dust Bowl; migration from the Great Plains to the American West, which could have affected duration of treatment and sample selection; and selection into (fertility) or out of (mortality) the sample. In this chapter, I outline why these issues are unlikely to undermine the analysis and findings presented in this thesis.

6.2 The Great Depression

The Great Depression remains the largest adverse macroeconomic shock in American history; to this day it dominates discussions of the 1930s. By its nadir in 1933, unemployment had risen nearly eightfold relative to its 1929 value, while real GDP had fallen by over a quarter (Fishback & Thomasson, 2014). Many of those who were unemployed remained so for longer than one year; those who were fortunate enough to keep or find employment were often subject to involuntary reductions in working hours (Levine, 2009). Thus, the Great Depression represented a serious blow to household finances. Indeed, consumption proxies bear this out: the ratio of 1933 to 1929 per capita retail sales varied from an anemic 0.435 in Mississippi to a stronger but nevertheless worrisome 0.791 in South Carolina (Fishback et al., 2001). Accordingly, it is reasonable to expect that the Great Depression precipitated a drop in the quality and quantity of human capital inputs during this period. However, as this thesis proposes, the Dust Bowl also represented a crucial blow to early-life circumstances for those growing up in the 1930s. Worster (1979, p. 24) goes even further, stating of the Dust Bowl, “not even the Depression was more devastating, economically.” Of course, the concurrence of the Dust Bowl and the Depression make disentangling the true source of later-life disparities in health and wellbeing especially challenging. Nevertheless, factors such as differences in the spatial distribution of the Dust Bowl and Depression shocks and the exogeneity of the Dust Bowl as environmental shock can help separate the two events' effects. Using a variety of approaches, I find consistent evidence that the Dust Bowl as an environmental shock had adverse effects on early-life circumstances, and through them, on later-life outcomes, above and beyond those due to the Great Depression as a macroeconomic shock.

6.2.1 The Great Depression & Features of the Model

The baseline model outlined in Section 3.2 by its nature embeds several features that help account for the effects of the Great Depression. In particular, its differences-in-differences construction, and its inclusion of fixed effects and trends, mean that the results presented thus far already incorporate to a degree issues of state-level economic performance.

6.2.1.1 Differences-in-Differences

Firstly, the differences-in-differences model is designed to exploit both spatial and temporal variation in Dust Bowl exposure (Angrist & Pischke, 2009). As such, it tests for the effect of differences in the severity of Dust Bowl exposure even within cohorts in which all individuals were exposed to the Depression.

Thus, as long as Depression severity was relatively uniform across the states in the sample, the β_1 coefficient in the baseline specification isolates the treatment effect of the Dust Bowl above and beyond any effects of the Great Depression. Indeed, the similar ratios obtained across states of state per capita income at various stages of the Depression to 1929 baselines (presented in Table 6.1) suggest that the Depression affected states with similar intensity, relative to each state's pre-Depression income (Bureau of Economic Analysis, 2012).

Table 6.1: Depression Severity by State

State	PCY 1929/31 (1)	PCY 1929/33 (2)	PCY 1929/35 (3)	PCY 1929/37 (4)	PCY 1929/39 (5)	PCY 1929/40 (6)
Arkansas	1.459	1.993	1.495	1.210	1.245	1.196
Colorado	1.343	1.795	1.425	1.189	1.226	1.160
Iowa	1.444	2.279	1.368	1.115	1.225	1.160
Kansas	1.333	2.120	1.471	1.242	1.389	1.251
Louisiana	1.302	1.814	1.419	1.165	1.145	1.129
Minnesota	1.305	1.929	1.332	1.110	1.158	1.145
Missouri	1.266	1.856	1.475	1.224	1.234	1.195
Montana	1.540	1.993	1.250	1.157	1.120	1.048
Nebraska	1.433	2.154	1.454	1.422	1.480	1.341
New Mexico	1.418	1.942	1.388	1.125	1.144	1.080
North Dakota	1.974	2.547	1.401	1.171	1.201	1.080
Oklahoma	1.517	2.055	1.527	1.209	1.303	1.218
South Dakota	1.733	3.128	1.368	1.304	1.227	1.172
Texas	1.380	1.867	1.465	1.139	1.144	1.092
Wyoming	1.411	1.818	1.354	1.111	1.151	1.124

Note: Columns 1-6 refer to the ratio of the per capita personal income in 1929 to that in 1931, 1931, 1933, 1935, 1937, 1939, and 1940, respectively, each in that year's dollars.

Additionally, and importantly, spatial variation in the Dust Bowl was by nature different from that in the Great Depression. Although the Dust Bowl surely contributed to low incomes during the era, and although the Great Depression may have exacerbated the adverse effects of the Dust Bowl in farming communities, Dust Bowl severity and Depression-era incomes are at best very weakly correlated. The correlation between my erosion measures, given in Table 3.1, and per capita personal income in 1929, on the eve of the Depression, is 0.2897. During the 1933 trough of the Depression, this figure is 0.2399, while the end of the Depression in 1940 the correlation is 0.0761. Similarly, Dust Bowl severity and Depression severity (the latter given by the ratio of 1929 to 1933 per capita personal income) is a very weak -0.1271, indicating that if anything, a harsher Dust Bowl experience was associated with a less severe decline in Depression-era income.

6.2.1.2 Fixed Effects & Trends

Second, I control for several fixed effects and trends that account for variation beyond that due to the Dust Bowl. These include birth-year fixed effects, which control for nationwide macroeconomic shocks; birth-state fixed effects, which take into account a state's level of economic development; and state trends, which capture trends in state economic performance and public spending over time. Only if the Depression's effects varied dramatically within a state (for instance, spatially or demographically) might the latter two controls be insufficient to approximate the Depression's effect on individuals. Examination of county-level retail sales per capita for 1929, 1933, 1935, and 1939, a proxy for local economic conditions (Fishback et al., 2001), suggests that intra-state, inter-county variation in these circumstances was reasonably large (the standard deviation in county retail sales per capita ranges from roughly 20-50% of the mean at its peak, with urban and resource-rich states the most spatially unequal), but that this variation dropped considerably during the trough of the Depression as counties' fates became more uniformly poor. Figures 6.2-6.5 give a sense of the variation in county retail sales per capita within states by year, while Figure 6.6 shows how the degree of intra-state variation in per capita retail sales changes over the Depression in each of the sample states. More reassuringly, when accounting for the spatial distribution of population (based on the population distribution in 1930), this intra-state variation becomes less pronounced, although some states clearly enjoyed less intra-state inequality than others: in my sample, states saw 55-76% (58-77%, 53-77%, 53-77%) of their population living in above-median retail sales counties in 1929 (1933, 1935, 1939) and 41-70% (41-71%, 43-69%, 41-71%) of their population living within one standard deviation of the median in 1929 (1933, 1935, 1939). Figures 6.7-6.10 provide a snapshot of the distribution of state population by county retail sales per capita in each of the sample states in each of the years for which such data are available.

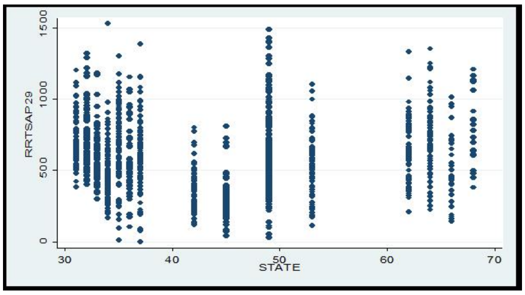

Figure 6.2: Intra-State Variation in Retail Sales Per Capita: 1929

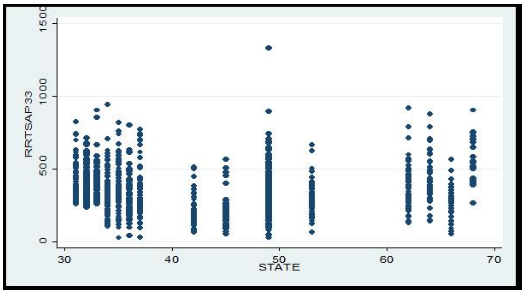

Figure 6.3: Intra-State Variation in Retail Sales Per Capita: 1933

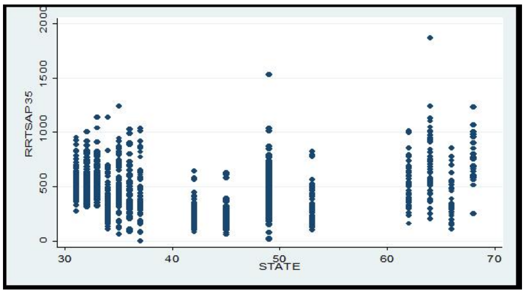

Figure 6.4: Intra-State Variation in Retail Sales Per Capita: 1935

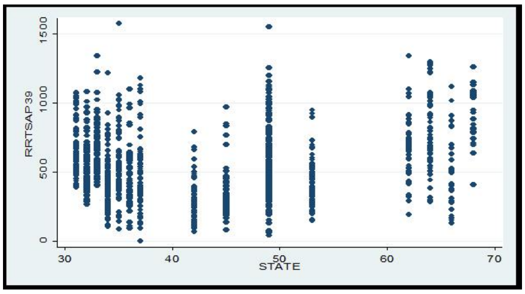

Figure 6.5: Intra-State Variation in Retail Sales Per Capita: 1939

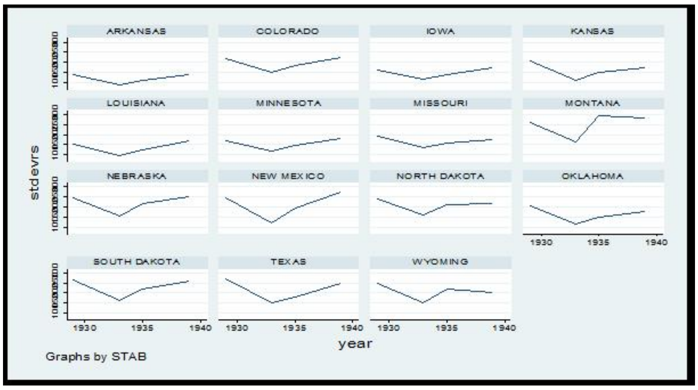

Figure 6.6: Intra-State Variation in Retail Sales Per Capita by State, 1929-1939

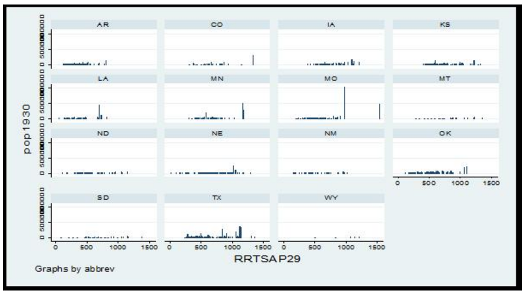

Figure 6.7: Distribution of State Population by Retail Sales Per Capita: 1929

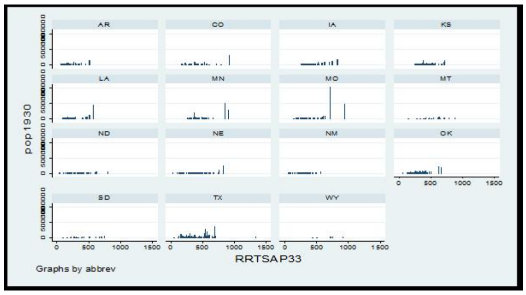

Figure 6.8: Distribution of State Population by Retail Sales Per Capita: 1933

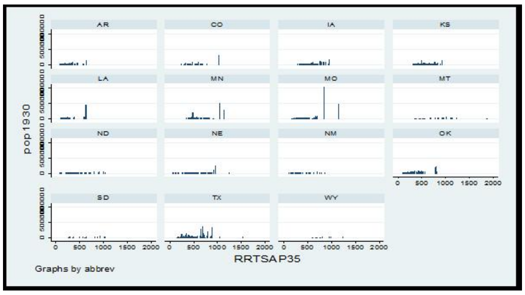

Figure 6.9: Distribution of State Population by Retail Sales Per Capita: 1935

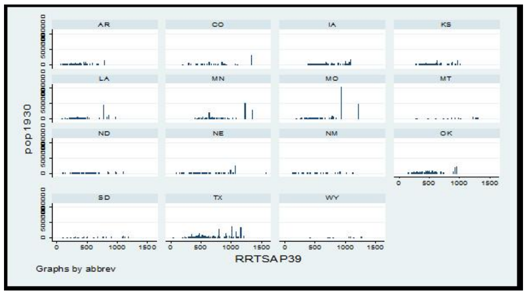

Figure 6.10: Distribution of State Population by Retail Sales Per Capita: 1939

While controls for an individual's actual household income in childhood would be ideal, these figures are not available in the censuses which provide adult outcomes.

6.2.2 Controls for Depression-Era Incomes

As with other studies of the long-range outcomes of individuals born during the early 20th Century (e.g. Cutler et al., 2007; Fishback & Thomasson, 2014), individual-level childhood incomes cannot be recovered from publicly-available census microdata.[1] In

[1]As of this writing, the 1940 US Census is the most recent wave of IPUMS data which has been de-anonymized and which is thus available for linkage to earlier records. In any case, linkage-based analysis using the best techniques available to date would require the exclusion of women from consideration altogether because of difficulties in matching women's pre- and post-marriage surnames. Their exclusion, in turn, would make it impossible to track differences by gender in the consequences of early-life shocks, and it would complicate efforts to establish the fertility effects of such shocks, since the number or children ever born is only given in the census for women. Furthermore, back-linkage to the 1930 and 1940 censuses, even if possible, may not necessarily

the absence of information on individuals' actual childhood household circumstances, I also control for state-level childhood income per capita, meant to proxy for Great Depression effects.[2] I find that although childhood income has an intuitive and significant impact on outcomes such as disability, results on Dust Bowl exposure are robust to the inclusion of these controls.

As discussed in Sections 1.1 and 2.1.1, the Dust Bowl shock can be thought of as having adverse effects through two main channels: firstly, by acting as a direct hazard to (e.g. respiratory) health; and secondly, by lowering incomes, particularly in communities dependent on agriculture. Thus, given the concurrent timing of the Great Depression and the Dust Bowl, it is reasonable to consider the adverse early-childhood circumstances produced by the former as a macroeconomic shock to income completely independent of the environmental shock to income represented by the latter.

Indeed, in an important recent paper, Fishback & Thomasson (2014) find that the per capita personal income in the state and year of birth, a variable they use to proxy childhood incomes during the Depression,[3] has significant and economically

provide accurate income figures, since in these census waves, actual income is not given. Rather, IPUMS imputes income from individuals' reported occupations, e.g. in the "OCCSCORE" variable, which assigns individuals a time-invariant occupational income score, constructed as the median income in 1950 associated with the reported occupation (Ruggles et al., 2010). Indeed, the usefulness of income measures based on occupational classifications has been a source of controversy (Ruggles et al., 2010), particularly in recent studies of social mobility (e.g. Collins & Wanamaker (2015), which seeks to mitigate issues in the traditional income assignment approach by accounting more flexibly for occupational income differences by race and region).

[2]It should be noted that even some measure of the observed Depression severity, as captured in traditionally used variables such as state per capita personal income or per capita retail sales, is likely to reflect agricultural damage wrought by the Dust Bowl. As an environmental event and as measured by erosion, the Dust Bowl was more clearly exogenous.

[3]The per capita personal income variable used in Fishback & Thomasson (2014) should be thought of as a net measure of resources available to individuals during the 1930s, rather than as a measure of the pure macroeconomic Depression shock per se. For instance, this income measure includes relief payments made to households via the New Deal and related program, and so incorporates both elements of the initial shock and elements of the remediation response. More importantly for comparisons with the study presented in this thesis, this personal income measure cannot separate the "pure" Depression shock from income losses due to the Dust Bowl's environmental shock, e.g. drought-, erosion-, and windstorm-induced damage to farms, livestock, individual earning power through illness, etc.

meaningful effects on income and disability in adulthood. They use a nationwide sample of individuals in 1970 and 1980 to show, using data and methods similar to those employed in this study, that those born during the trough of the Depression suffer worse outcomes as adults relative to those born during years of better economic performance. Notably, they find that the adverse effects of early-life income shocks are most pronounced amongst those living in low-income states, consistent both with the larger literature on the public health effects of recessions across differing levels of economic development (e.g. Ruhm, 2000; Chay & Greenstone, 2003; and Dehejia & Lleras-Muney, 2004 in rich-country circumstances, and van den Berg et al., 2006; Banerjee et al., 2010; and Arthi et al., 2016a under developing-country conditions), as well as with the theoretical framework proposed in Heckman (2007) and described in Section 2.3. In such a framework, adult human capital may be a nonlinear function of childhood inputs such as income, and so those children who are more poorly endowed at the time of an adverse shock (i.e. those individuals on a steeper portion of the human capital production function) stand to lose more from a same-sized shock than their better-placed, richer counterparts–even independently of their lower incentives and abilities to later compensate for the shock.

6.2.2.1 Methods & Data

In order to test whether the Dust Bowl had adverse effects on later-life human capital above and beyond those of income shocks attributable to the Great Depression, I add controls for childhood economic circumstances to the baseline specification given in Equation 3.1.

Since information on individuals' actual incomes in childhood is unavailable in current matchable microdata, I use three state-year-level proxies, following Fishback & Thomasson (2014): firstly, state per capita personal income in the birth year; secondly, state per capita personal income in the year before birth, meant to capture *in utero* circumstances; and thirdly, state per capita personal income averaged over childhood (per the definition of childhood used in the baseline analysis, the annual per capita personal income averaged across the years in which the child was aged -1 to 12, inclusive).

Yearly per capita personal income by state is taken from the Bureau of Economic Analysis (2012) for 1929 to 1971; from Easterlin (1957) for 1920; and from Klein (2009) for 1890, 1900, and 1910. Income figures for 1899, 1901-9, 1911-9, and 1921-8 have been exponentially interpolated from the known data. All income figures are given in 1982-4 dollars, per the deflators given in Sahr (2007).

6.2.2.2 Results & Discussion

The results on later-life human capital of Dust Bowl and Depression exposure in childhood are presented in Tables 6.2 for men, and 6.3 for women. In each regression in the table, the first of the two columns (denoted by "a") refers to the standard Dust Bowl exposure coefficient per the baseline analysis, while the second column (denoted by "b") refers to the coefficient on the Depression exposure variable included as a control within the same regression.

Columns 1, 3, and 5 in Table 6.2 show that even upon the inclusion of controls for childhood income (which, for the Dust Bowl cohort, would have included the especially low Depression-era incomes), the results of Dust Bowl exposure on welfare income, the probability of poverty, and the probability of physical disability remain statistically significant as in the baseline results presented in Table 3.3. For all three outcomes, the magnitudes of the coefficients are nearly identical to those found in the baseline. They are also similar across the three specifications presented in Table 6.2 using alternate definitions of early-life income. Although the baseline results survive the inclusion of income controls which, for the Dust Bowl cohort, represent "Depression exposure," it is notable that childhood economic circumstances are not

Table 6.2: Impact of Exposure to the Dust Bowl on Later-life Outcomes: Depression Controls – Men

	Income in Year of Birth		**Income *In Utero***		**Average Income Across Childhood**	
	(1a)	(1b)	(2a)	(2b)	(3a)	(3b)
Outcomes	Treated × Erosion	PC Income	Treated × Erosion	PC Income	Treated × Erosion	PC Income
Age at First Marriage	-0.1590	0.000124***	-0.1610	0.000129***	-0.1540	0.000337***
	(0.1260)	(0.000000)	(0.1280)	(0.000000)	(0.1250)	(0.000100)
Probability of Completing High School	0.01850	-0.000008	0.0184	-.000007	0.0184	-.000031***
	(0.0138)	(0.000000)	(0.0140)	(0.000000)	(0.0115)	(0.000000)
Probability of Completing College	-0.0105	-0.000001	-0.0102	-0.000003	-0.0107	-0.000001
	(0.0075)	(0.000000)	(0.0075)	(0.000000)	(0.0071)	(0.000000)
Welfare Income	15.92*	-0.004380**	16.29**	-0.005830***	15.48*	-0.014300***
	(8.3350)	(0.002000)	(8.1360)	(0.001900)	(8.3170)	(0.005500)
Probability of Poverty	0.0090***	-0.000005***	0.0087**	-0.000003*	0.0084**	-0.000013***
	(0.0034)	(0.000000)	(0.0036)	(0.000000)	(0.0033)	(0.000000)
Probability of Cognitive Disability	0.0074	-0.000005	0.0072	-0.000003	0.0065	-0.000010*
	(0.0068)	(0.000000)	(0.0068)	(0.000000)	(0.0062)	(0.000000)
Probability of Physical Disability	0.0116**	-0.000004*	0.0112**	-0.000001	0.0109**	-0.000015*
	(0.0050)	(0.000000)	(0.0049)	(0.000000)	(0.0046)	(0.000000)
Probability of Vision & Hearing Difficulty	-0.0023	-0.000002	-0.0024	-0.000008	-0.0028	-0.000009**
	(0.0060)	(0.000000)	(0.0061)	(0.000000)	(0.0057)	(0.000000)
Probability of Self-Care & Independent	0.000057	-0.000002*	0.0002	-0.000003***	-0.0002	-0.000005*
Mobility Difficulty	(0.0031)	(0.000000)	(0.0030)	(0.000000)	(0.0033)	(0.000000)

*** p<0.01, ** p<0.05, * p<0.1; Note: Table reports the $treated_b \times erosion_s$ and per capita income coefficients for each regression. Column headers refer to the measure of birth state-birth year per capita income used. All regressions are estimated by OLS and include additional controls for race; birth year, birth state, and census state fixed effects; and state trends. Standard errors, clustered by birth state, are reported in parentheses below each coefficient. To aid in interpreting the coefficients of the per capita income variables, the minimum, maximum, and standard deviation are as follows: Income in Year of Birth (988.636, 7842.472, 1686.206); Income *In Utero* (854.516, 7610.116, 1650.102); Average Income Across Childhood (1292.224, 8893.168, 1878.751).

Table 6.3: Impact of Exposure to the Dust Bowl on Later-life Outcomes: Depression Controls – Women

	Income in Year of Birth		**Income *In Utero***		**Average Income Across Childhood**	
	(1a)	(1b)	(2a)	(2b)	(3a)	(3b)
Outcomes	Treated × Erosion	PC Income	Treated × Erosion	PC Income	Treated × Erosion	PC Income
Age at First Marriage	-0.0312	0.000130***	-0.0340	0.000140***	-0.0159	0.000191***
	(0.1110)	(0.000000)	(0.1120)	(0.000000)	(0.1120)	(0.000100)
Children Ever Born	-0.1960	-0.000052	-0.1950	-0.000052*	-0.2010	-0.000100
	(0.1360)	(0.000000)	(0.1340)	(0.000000)	(0.1400)	(0.000100)
Probability of Completing High School	0.0324*	-0.000009	0.0326*	-0.000009*	0.0330**	-0.000041***
	(0.0178)	(0.000000)	(0.0176)	(0.000000)	(0.0158)	(0.000000)
Probability of Completing College	-0.0132***	0.000007***	-0.0133**	0.000008***	-0.0124***	0.00001*
	(0.0050)	(0.000000)	(0.0052)	(0.000000)	(0.0046)	(0.000000)
Welfare Income	17.18**	-0.003230	17.09**	-0.002620	17.48**	-0.022300***
	(8.4190)	(0.002700)	(8.3850)	(0.003300)	(8.3410)	(0.007100)
Probability of Poverty	0.0055*	-0.000004**	0.0057**	-0.000005*	0.0048*	-0.000003
	(0.0028)	(0.000000)	(0.0029)	(0.000000)	(0.0025)	(0.000000)
Probability of Cognitive Disability	-0.0048	-0.000003	-0.0043	-0.000005**	-0.0053	-0.000003
	(0.0062)	(0.000000)	(0.0060)	(0.000000)	(0.0064)	(0.000000)
Probability of Physical Disability	-0.0027	0.000003	-0.0035	0.000007***	-0.0022	0.000007
	(0.0112)	(0.000000)	(0.0108)	(0.000000)	(0.0112)	(0.000000)
Probability of Vision & Hearing Difficulty	-0.0025	0.000001	-0.0025	0.000001	-0.0024	0.000007
	(0.0056)	(0.000000)	(0.0057)	(0.000000)	(0.0054)	(0.000000)
Probability of Self-Care & Independent	-0.0013	-0.000001	-0.0011	-0.000002	-0.0014	-0.000002
Mobility Difficulty	(0.0031)	(0.000000)	(0.0030)	(0.000000)	(0.0030)	(0.000000)

*** p<0.01, ** p<0.05, * p<0.1; Note: Table reports the $treated_b \times erosion_s$ and per capita income coefficients for each regression. Column headers refer to the measure of birth state-birth year per capita income used. All regressions are estimated by OLS and include additional controls for race; birth year, birth state, and census state fixed effects; and state trends. Standard errors, clustered by birth state, are reported in parentheses below each coefficient. To aid in interpreting the coefficients of the per capita income variables, the minimum, maximum, and standard deviation are as follows: Income in Year of Birth (988.636, 7842.472, 1686.206); Income *In Utero* (854.516, 7610.116, 1650.102); Average Income Across Childhood (1292.224, 8893.168, 1878.751).

irrelevant to adult outcomes. Rather, their effects are frequently statistically significant, with intuitively signed coefficients: for instance, higher incomes in childhood are associated with increases in the age at marriage, decreases in welfare income and in the probability of poverty, and decreases in the probability of a variety of disabilities. These results stand in contrast to the adverse effects of the Dust Bowl, which, for instance, lowers ages at marriage and raises the chances of poverty and disability. The coefficient magnitudes for childhood incomes are reasonably large, as well: for example, a one standard deviation increase in the per capita personal income in the year of birth corresponds to a 0.81 percentage point decrease in a man's chances of poverty in adulthood (a figure similar in magnitude to the causal impact on poverty of moving from no to full Dust Bowl exposure), while the same change in income corresponds to a more modest 0.30 percentage point decrease in a man's probability of self-care and independent mobility difficulty.

The results for women, given in Table 6.3, present a similar story. Here as in the baseline, the effects of Dust Bowl exposure on college completion and the probability of poverty are significant. Here, however, the coefficient on the probability of high school completion (essentially the same in sign and magnitude as in the baseline, and consistent with the sign, magnitude, and significance of this result elsewhere in the thesis) is also statistically significant. This latter result shows that at a given level of early-life income, the Dust Bowl cohort was more likely to complete high school. Consistent with the evidence presented earlier in this thesis, this result suggests that environmental destruction, rather than, say, poverty, may have governed children's time use tradeoffs for the Dust Bowl cohort. As with men, early-life economic circumstances are significantly, meaningfully, and positively associated with women's adult human capital outcomes, particularly age at marriage, the probability of college completion, and the probability of poverty. Where outcomes are comparable, the results presented here accord with the finding in Fishback & Thomasson (2014) that lower levels of early-life income are associated with lower levels of later-life human capital, although their estimates (for example, of the effects on disability) are larger than those found for early-life income (and accounting for Dust Bowl exposure) here.

As discussed above in Section 6.2.2, the inclusion of early-life income as a control is less than ideal, since at least in farming communities, and in the 1930s, these measures are likely to embed the income effects of Dust Bowl exposure in my states of interest. It would appear that no significantly better or purely macroeconomic measure (e.g. retail sales, rate of bank failures) of local Depression intensity exists, insofar as these variables would in their Dust Bowl-era values reflect farming collapse. Here, and from the perspective of establishing whether the Dust Bowl had independent effects on adult human capital in the exposed cohort, it is heartening that erosion offers a cleaner and more clearly exogenous early-life shock. Meanwhile, the elements of the empirical design discussed in Section 6.2.1 help purge effects only attributable to the separate Depression shock from the Dust Bowl exposure coefficients.

The results presented here suggest the Dust Bowl had adverse effects above and beyond those due to the Depression; indeed, it is likely that the Dust Bowl, and not just macroeconomic shocks, contributed to the low incomes experienced in this period by the Great Plains states in the sample. These results also indicate that income, while important, was likely not the only mechanism by which the Dust Bowl produced its adverse effects on later-life human capital.

6.2.3 Conclusion

Although the Great Depression represented a very real adverse shock to incomes in the 1930s, and accordingly had long-reaching effects on adult wellbeing, the Dust Bowl had adverse consequences separate from and in addition to these. In this section, I showed how the baseline empirical strategy by its very design accounts for the effects of the Great Depression, and how as such, its results reflect the impact of the Dust Bowl as an independent and more clearly exogenous shock. Furthermore, I provide evidence that when controls for childhood economic conditions are added (conditions which for the Dust Bowl cohort would capture the blow dealt by the Great Depression), the effect of the Dust Bowl still survives. It should be noted that even some measure of the observed Depression severity, as traditionally captured in variables such as state per capita personal income or per capita retail sales, is likely to reflect agricultural damage wrought by the Dust Bowl. It is thus clear that the Dust Bowl as an independent and environmental shock to early-life circumstances remains an important part of explaining the later-life disadvantage faced by the children of the 1930s.

6.3 Migration

As first touched on in Section 5.2, migration could complicate the interpretation of this thesis's central findings, regarding the later-life effects of early-life Dust Bowl exposure, in several ways. Firstly, migration during the childhood treatment window could lead to incorrect measurement of the duration and severity of Dust Bowl exposure. Similarly, if migrants are selected by household characteristics such as education, income, or occupation, this could bias results since treatment compliance would align with both an individual's sensitivity to shocks and their capacity for recovery. Lastly, as was the focus of Section 5.2, migration could expose individuals to substantially different environments for recovery and so understate the true insult to human capital by conflating the countervailing effects of the original insult and of remediation.

Accordingly, in this section I consider how issues of migration may affect the results presented thus far in this thesis. Specifically, I provide evidence that the plausible effects of migration on health indicate that the Dust Bowl's true effects are likely understated here; that migration is unlikely to have affected childhood treatment; and that there is little evidence of migrant selectivity.

6.3.1 Childhood Migration

Migration out of the birth state during childhood could lead to error in the measurement of the duration and severity of Dust Bowl exposure. It is thus useful to establish whether such migration was prevalent, whether it was associated with Dust Bowl severity, and whether its timing was likely to allow for meaningful reductions in exposure. Migration during childhood may also bias results if the likelihood of migration was a function of household characteristics. Accordingly, I test whether exposure to the Dust Bowl drives childhood migration, as well as whether there is evidence of migrant selectivity in those individuals migrating as children.

6.3.1.1 Results & Discussion

The data and methods for these tests are outlined in Section 5.2.1.1, and the results, as originally discussed in the context of mitigation, are presented in Table 5.1.

As noted in Section 5.2.1.2, Column 1 of Table 5.1 shows that Dust Bowl-exposed children were about 14 percentage points less likely to than their unexposed counterparts to have moved during the childhood period. This (weakly significant) result is consistent with the findings in Rosenbloom & Sundstrom (2004) and Long & Siu (2014) which show that migration rates were actually quite low during the 1930s. Furthermore, it suggests that the underlying assumption I make in this thesis, that birth-state erosion represents the level of erosion to which the individual was exposed for the duration of childhood, is a reasonable one. If anything, it appears that the degree of Dust Bowl exposure is more likely to be accurately measured for Dust Bowl cohort children than for their non-Dust Bowl counterparts.

In regressions 2-6 in Table 5.1, I present the differential likelihood of childhood migration in the Dust Bowl cohort by measures of the child's known household socioeconomic status. The aim here is to examine whether children were differentially selected for childhood migration based on household status in ways that would affect the average "quality" of children remaining in severely exposed regions. I find no evidence that household income or socioeconomic status mediated the Dust Bowl's effects on the probability of migration during childhood. There is limited evidence that farm status may have made Dust Bowl-exposed children less likely to out-migrate, an effect which could potentially lower the average vulnerability of the Dust Bowl cohort if the Dust Bowl inflicted some penalty on farm households of all income levels, simply for being farm households. Indeed, the absence of farm effects when not controlling separately for household income suggests that farm status mattered as separate from income.

Most importantly to the analysis presented in this thesis, it appears that there was no significant out-migration of children during the Dust Bowl, rendering the measures of childhood treatment used here feasible. Similarly, there is at best limited evidence of migrant selectivity of the sort which could misattribute a change in the composition of children treated to Dust Bowl effects.

6.3.2 Eventual Migration

To establish the direction in which migration is likely to bias my results, if at all, it is useful to look for evidence of systematic differences in the adult outcomes of migrants and non-migrants. Furthermore, as in the childhood migration analysis above, it is useful to examine whether Dust Bowl exposure drives the likelihood of migrating from the state of birth (where this migration may have either taken place during the childhood treatment window, or later on in life). Such analysis establishes the prevalence of Dust Bowl-induced migration, and sheds light on the likelihood that Dust Bowl exposure is systematically mismeasured.

As described in Section 5.2.2.1, I use the data from my baseline analysis to test whether eventual migrants exposed to the Dust Bowl have statistically significantly different later-life outcomes than their exposed counterparts enumerated as adults in their birth state, as well as to test whether Dust Bowl exposure affected the likelihood of being an eventual migrant. Results of this analysis are presented in Table 5.2.

6.3.2.1 Results & Discussion

Since we might worry about how migrant selectivity by Dust Bowl exposure would affect the results presented thus far in the thesis, it is useful to show that the Dust Bowl cohort is no more likely to have eventually migrated from their state of birth, and that Dust Bowl exposure does not systematically drive migration behaviors—not just during the childhood period, as considered in Section 6.3.1 above, but even potentially later in the individual's life-course. If rates of out-migration are higher for the Dust Bowl cohort than for others, it could, for instance, suggest that Dust Bowl "treatment" has been mismeasured for this cohort. Regressions 1 and 3 in Table 5.2 indicate that concerns over systematic out-migration as a result of Dust Bowl exposure are unlikely to complicate the interpretation of adult human capital effects. Indeed, these results show that early-life Dust Bowl exposure did not drive the probability of eventual migration.

As first discussed in Section 5.2.2.2, it is also useful to examine whether the outcomes for eventual migrants were systematically different from those who did not eventually leave. If such heterogeneity does exist, these would affect the interpretation of coefficients generated by the analysis presented in earlier chapters, which aggregates migrants and non-migrants. Regressions 2 and 4 show generally significant evidence that Dust Bowl-cohort migrants enjoy better later-life human capital and wellbeing outcomes than their similarly Dust Bowl-exposed but non-migrant counterparts.

In Section 5.2.2.2, I raised several possible explanations for the possible disparity in adult migrant-non-migrant outcomes. However, whatever the source of eventual migrant advantage, these results show that Dust Bowl-exposed eventual migrants experienced better human capital and wellbeing outcomes as adults than their exposed but non-migrant counterparts. Accordingly, the estimates of the adverse effects of Dust Bowl exposure presented earlier in this thesis—for instance, in the baseline analysis of Chapter 3, are if anything conservative. By conflating the poor outcomes of exposed non-migrants with the better ones of exposed migrants, the coefficients on Dust Bowl exposure represent underestimates of the impact on later-life human capital of early-life Dust Bowl exposure—whether because of migration-induced mismeasurement of true Dust Bowl exposure, or because the observed later-life effects incorporate substantial improvements in the recovery environment.

6.3.3 Conclusion

Although migration remains the prominent theme in popular depictions of the Dust Bowl, new research is challenging many of the misconceptions surrounding this phenomenon—using systematic analysis to shed light on who migrated during the catastrophe, from where, to where, in what numbers, and under what circumstances. The evidence presented here, too, suggests that concerns about Dust Bowl-era migration may be overstated. Indeed, I find for several reasons that such migration is unlikely to undermine my results on the Dust Bowl's adverse effects on later-life human capital.

First, features of the empirical design along with the possibility of out-migration mean that the adverse effects presented here are if anything underestimates of the Dust Bowl's impact. Since migration status during childhood is unknown, baseline Equation 3.1 represents an intent-to-treat analysis and should not be interpreted as a measure of actual treatment. Instead, the adverse effects estimated in this study are a lower bound on the true treatment effects of the Dust Bowl. Indeed, regressions disaggregating adult outcomes by (eventual) migrant status indicate that migrants had wellbeing outcomes similar to or better than non-migrants, whether because Dust Bowl exposure was cut short by childhood out-migration, or because the state to which the individual eventually migrated provided better resources to remediate early-life insults. By conflating plausible migrants and non-migrants, my analysis actually understates the adverse effects experienced by exposed non-migrants. Furthermore, I find that childhood Dust Bowl severity has no significant effect on eventual migration status as an adult.

Second, migration is unlikely to have affected childhood treatment. As the analysis presented in Section 4.2 suggests, even individuals migrating as children are unlikely to have escaped the phase in which I find the strongest adverse impacts of exposure: the -1 to 0 age band. The narrowness and precariousness of this window in a family's life cycle makes out-migration during this timeframe unlikely. This, added to the fact that interstate migration rates were actually "unusually" low during the 1930s (Rosenbloom & Sundstrom, 2004), suggests that migration during the treatment window–particularly the most sensitive portion of it–is unlikely to influence exposure measures.

Third, I find little evidence of migrant selectivity. Neither Dust Bowl severity nor measures of household income and socioeconomic status, conditional on Dust Bowl treatment, are significant in determining the likelihood of migration in childhood. The only significant difference is found in Dust Bowl-exposed children from farm households, who were if anything less likely to have migrated before age 12 than the nonfarm exposed, consistent with Long & Siu (2014), who find that farmers were the least likely to have moved during the Dust Bowl. Secondary literature, too, suggests an ambiguous (or even non-existent) relationship between migrant status and individual or household characteristics, such as wealth, which one might expect to heterogeneously affect both Dust Bowl treatment and the response to this shock. For instance, while some accounts suggest that those with the means to migrate did, yet others suggest that those who held greater land and wealth felt tied to these assets and were thus reluctant to leave (Lord, 1938; Wallace, 1938; Worster, 1979; Nealand, 2008; Burns, 2012; Long & Siu, 2014). Together, these findings suggest no systematic difference in migrant and non-migrant characteristics of the like that might introduce selection bias into the results presented here.

6.4 Fertility Selection

As touched upon in Section 5.3, selective fertility may be a concern if less healthy (i.e. the most severely Dust Bowl-exposed) mothers had fewer children, or if, as the literature suggests (Dehejia & Lleras-Muney, 2004; Ó Gráda, 2011), there existed socioeconomically-driven gradients in the fertility of Dust Bowl-exposed women.

If such a phenomenon were at play during the Dust Bowl, I would expect that the individuals most resilient to and best able to compensate for the shock would disproportionately enter my sample, while many of the most vulnerable would never been born at all, thus overstating adult wellbeing and underestimating the adverse effects of the shock. Similarly, Dust Bowl-era mothers may have experienced involuntary reductions in fertility during the crisis due to famine amenorrhea and/or stress amenorrhea, conditions in which severe malnutrition and stress, respectively, prevent women from conceiving (Ladurie, 1969; Ó Gráda, 2000). If this were the case, I would expect that while birth rates may go down, the children who *were* conceived and born would enjoy better health status on average. Consequently, such involuntary reductions in fertility would, like positive fertility selection by maternal socioeconomic status, lead to an underestimation of the true adverse effects of the Dust Bowl on those born into it.[4]

[4]It is worth noting that voluntary and involuntary fertility reductions could combine to widen the wedge between the crisis-time fertility outcomes of rich and poor women. For instance, during a crisis, poor women may become less likely to have children both because their financial circumstances discourage them, and also because even where the will to bear children exists, they are more likely to experience biological constraints on fertility due to malnutrition. For contrast, rich women are

As described in Section 5.3, I test for such fertility selection into my sample and find no statistically significant differences in fertility in response to the Dust Bowl by maternal socioeconomic status. I also examine whether Dust Bow-era birth rates themselves were affected by the shock, and find that birth rates and erosion timing and severity have little relationship: states experienced no significant change in birth rates during the Dust Bowl period, consistent with Cutler et al. (2007) and Fishback et al. (2007). As a further check, I test Dust Bowl-era fertility adjustments as inferred from cohort size in single-age bands, and find no significant effect of childhood exposure to the Dust Bowl on cohort size. Together, the results of these tests suggest fertility selection has no effect on the findings presented thus far regarding the adult outcomes of children exposed to the Dust Bowl.

6.4.1 Dust Bowl-Era Births & Endogenous Fertility Responses

The methodology used to test for Dust Bowl-driven changes in the crude birth rate are outlined in Section 5.3.1.1, and results are presented in Table 5.3.

6.4.1.1 Results & Discussion

As discussed in Section 5.3.1.2, the Dust Bowl was not a significant driver of birth rates, although the New Deal may have had a role in preventing a decline in 1930s births (Fishback et al., 2007). Thus, it appears that the Dust Bowl did not create an endogenous fertility response that would in turn affect the average "quality" (e.g. latent health status) of individual entering the Dust Bowl birth cohort.

less likely to face either of these constraints.

6.4.2 Selectivity of Dust Bowl-Era Births by Maternal Characteristics

Next, in order to establish whether the children entering the Dust Bowl cohort are non-randomly drawn from the population of potential births in ways that would affect both their initial health endowment and the scope for subsequent investments, I consider differences in birth probabilities by maternal socioeconomic status.

6.4.2.1 Results & Discussion

Data and methods for these tests are outlined in Section 5.3.2.1, with the results of this analysis presented in Table 5.4.

For each of the four definitions of maternal socioeconomic status used here, there is no evidence of a socioeconomic gradient in the fertility of Dust Bowl-exposed potential mothers. There is thus no clear relationship between maternal status and fertility here, and no evidence of maternal selectivity.

Together, these results show no evidence for the Dust Bowl cohort of differential selection into birth by maternal/household characteristics. That is, there is no evidence that the composition of realized births in the Dust Bowl cohort was substantially different from that of adjacent cohorts. Thus, it is unlikely that the average initial health endowment of the Dust Bowl cohort was any worse than that of those cohorts which were not exposed. Similarly, it is not likely that this cohort faced intrinsically worse household-level conditions for recovery on average (i.e. as a result of a change in composition of the types of households containing young children) than those unexposed, apart from damage to household resources (likely uniform across socioeconomic status within the cohort) due to the Depression and Dust Bowl.

6.4.3 Cohort-Size Approach to Demographic Accounting

Finally, I infer changes in demography from cohort size as a means of corroborating the analysis presented above on fertility.[5]

6.4.3.1 Results & Discussion

The data and methods for this analysis are outlined in Section 5.3.3.1, and the results are presented in Table 5.5.

In row 1, I find no evidence that the Dust Bowl drove changes in cohort size, consistent with the evidence presented thus far regarding fertility and the probability of migrating children (and so, of migrating mothers), and consistent with the evidence in the coming section on mortality selection.[6] Together, these results suggest a demographic stability that is unlikely to change the composition of my sample in ways that undermine the main results of the thesis.

When exposure timing is narrowed to the year before birth as given in row 2, however, 1940 and 1936 birth cohorts are found to have significantly smaller cohort sizes than their untreated age 0 and age 4 counterparts, respectively. It is ambiguous whether and how these findings would affect the main differences-in-differences results. For instance, if the "missing" 0- and 4-year-olds were drawn from the lower end of the cohort's health/socioeconomic status distribution (say, because marginal individuals are culled), then the average adult health in the Dust Bowl cohort would be overestimated in this thesis (that is, the Dust Bowl's potential for scarring effects would be underestimated). The opposite might be true if the missing individuals in the 0 age band were the result of restricted fertility among high-status mothers. However, neither scenario may be at play if, as is suggested by the analysis above in which there was no heterogeneity in Dust Bowl-exposed mothers' fertility patterns,

[5] Of course, this analysis by its nature also serves to corroborate the results on mortality presented in the next section.

[6] For further discussion, see Section 5.3.3.2.

any cohort size reductions were uniform across the health/socioeconomic status distribution. That said, if gradients in cohort size are driven by gradients in Dust Bowl severity (and even if the probability of a missing child were not driven by household characteristics), then it is possible that the observed cohort size reductions could imply a rise in the Dust Bowl cohort's average health endowment. Nevertheless, in the absence of other evidence of Dust Bowl-induced shifts in demographic patterns throughout the analysis presented in this chapter, these isolated results regarding cohort size are unlikely to substantially affect the interpretation of the larger findings of this thesis.

6.4.4 Conclusion

As discussed in Section 5.3.4, tests on the Dust Bowl-era birth probabilities individual mothers has shown no evidence of a maternal socioeconomic status gradient in fertility for Dust Bowl-exposed potential mothers. Neither is there evidence that the Dust Bowl induced an reduction in the aggregate birth rate, a finding corroborated by tests of cohort size. Given that theory would predict that voluntary and involuntary changes in fertility would move in the same direction, the lack of net change in fertility suggests that neither of these fertility changes took place—at least in ways that were driven by Dust Bowl exposure. As such, it does not appear that the Dust Bowl changed the pre-crisis composition of realized births, nor that it had compositional effects on the average "quality" of child entering the Dust Bowl cohort. Accordingly, it would be reasonable to attribute any reductions in a Dust Bowl child's health endowment to Dust Bowl scarring.

6.5 Mortality Selection

Especially when studying long-range rather than immediate health outcomes, it is crucial to account for the competing effects of scarring (i.e. damage to human capital in survivors as the result of an adverse shock or other deprivation) and selection (i.e. an improvement in average human capital in survivors who are positively selected, as the adverse shock culls the weak) . Works such as Deaton (2007) have shown that it is possible for either effect to dominate the other, and have described the circumstances under which each scenario is likely to manifest.[7] For instance, in poorer countries with inadequate public health infrastructure, adverse shocks in early childhood are likelier than in richer countries to kill individuals in marginal health, leaving behind healthier—in this example, taller—individuals (Deaton, 2007). Thus, if the selection effect dominates, the examination of adult outcomes (in Deaton (2007), heights, an indicator of net nutritional status) may capture only the net rather than the gross effect of the early-life shock, and could lead to erroneous conclusions about the size and even direction of the effects had mortality selection not occurred.

Given that the individuals in the Dust Bowl cohort are aged 39-62 when I observe the first group as adults in the 1980 Census, and are 59-82 by the time I observe the last group in 2000, the sort of survivorship bias described above may be a real concern. That is, it is possible that many of the individuals in poorest health as a result of Dust Bowl exposure may have died at some point before being observed as adults in 1980-2000, a "culling" or "selection" effect that would bias the wellbeing outcomes of individuals in my sample upwards. If such an effect existed, my results would underestimate the true adverse effects of the Dust Bowl on health.

Testing for differences in yearly state-level stillbirth rates and infant mortality rates, I find no statistically significant change in either measure of early-life mortality.

[7] Work such as Hatton (2011) examines these potentially countervailing effects in historical context.

Similarly, I test for whether the Dust Bowl cohort had significantly higher mortality rates at any stage in their life-course (under age 1, 1-4, 5-14, 15-24, 25-34, 45-54, 55-64, 65-74, and 74-85) up to and including my observation period, than those not exposed. I find no significant difference in mortality rates by age bin for the Dust Bowl cohort, except for in the 5-14 age bin, during which boys in the Dust Bowl cohort experienced marginally (0.41 point) lower mortality rates than those not exposed, and in the 55-64 age bin, during which men exposed to the Dust Bowl as children experienced a 3.21-point lower mortality rate than their unexposed counterparts. These findings indicate that mortality selection and survivorship bias are unlikely to influence my results, and that accordingly, the results I have presented throughout this thesis should be interpreted as the result of Dust Bowl scarring.

6.5.1 Stillbirths and Infant Deaths

In addition to the fertility selection effects considered above, positive selection could be driven by mortality, particularly in the perinatal period when those in the most marginal health would be especially vulnerable—perhaps more so than they would be in any subsequent mortality age bin. As such, it is useful to test for changes in stillbirth rates (since these individuals are not otherwise captured in births or deaths data) and in the rate of deaths within the first year of life. Here, consistent with the findings in Fishback et al. (2007), I find that the Dust Bowl did not raise rates of stillbirth or infant mortality.

6.5.1.1 Methods & Data

Firstly, I use yearly state-level stillbirth rates over the period 1915-1960 from US Vital Statistics (Public Health Service, 1947; National Center for Health Statistics, 1968) for each of the 15 states in my sample to test for whether the Dust Bowl cohort had significantly higher stillbirth rates than those not exposed.

To test for differences in stillbirth rates across cohorts, I estimate the following regression, by OLS:

$$b_{st} = \alpha + \beta_1 \times treated_t \times erosion_s + \theta_s + \lambda_t + \gamma_s + u_{st} \tag{6.1}$$

where b_{st} is the stillbirth rate in state s in year t.

$Treated_t$ is a binary dummy which takes the value of 1 if the year is 1930-40, inclusive. In order to test when stillbirth rates may have begun to respond to Dust Bowl conditions, $treated_t$ is also alternatively defined as 1 if the year is 1931-40, 1932-40, and on until $treated_t$ takes the value of 1 only if the year is 1940; results for each of these definitions of $treated_t$ are reported in Table 5.3 under column headers referring to how $treated_t$ has been defined in that specification.

$Erosion_s$ is, as in the baseline regressions, a population-weighted measure of erosion severity in state s.

As in the baseline, θ_s, and λ_t represent state fixed effects and year fixed effects, while γ_s represents state trends. Standard errors are clustered at the state level.

Results are reported in the first row of Table 6.4.

Next, in an analysis that proceeds as the one for stillbirths, I use yearly state-level infant mortality rates over the period 1915-1960 from US Vital Statistics (Public Health Service, 1947; National Center for Health Statistics, 1968) for each of the 15 states in my sample to test for whether the Dust Bowl cohort had significantly higher infant mortality rates than those not exposed.

To test for differences in infant mortality rates across cohorts, I estimate the following regression, by OLS:

$$i_{st} = \alpha + \beta_1 \times treated_t \times erosion_s + \theta_s + \lambda_t + \gamma_s + u_{st} \tag{6.2}$$

where i_{st} is the infant mortality rate in state s in year t.

$Treated_t$ is a binary dummy which takes the value of 1 if the year is 1930-40, inclusive. In order to test when infant mortality rates may have begun to respond to Dust Bowl conditions, $treated_t$ is also alternatively defined as 1 if the year is 1931-40, 1932-40, and on until $treated_t$ takes the value of 1 only if the year is 1940; results for each of these definitions of $treated_t$ are reported in Table 6.4 under column headers referring to how $treated_t$ has been defined in that specification.

$Erosion_s$ is, as in the baseline regressions, a population-weighted measure of erosion severity in state s.

As in the baseline, θ_s, and λ_t represent state fixed effects and year fixed effects, while γ_s represents state trends. Standard errors are clustered at the state level.

Results are reported in the second row of Table 6.4.

6.5.1.2 Results & Discussion

Rows 2 and 3 in Table 6.4 present the coefficients on the treatment effect of the Dust Bowl on stillbirth and birth rates, respectively. As discussed earlier with respect to the results on birth rates, the column headers 1-11 refer to the set of years which are considered to be treated by the Dust Bowl. By varying the treatment window it is thus possible to examine whether stillbirth and infant mortality patterns changed at a given point during the Dust Bowl—say, later in the period, when storms may have become more intense. The coefficients presented here indicate that the Dust Bowl has no significant effect on the stillbirth rate, except for where only 1940 stillbirths are counted as treated by the Dust Bowl. Here, stillbirth rates rise, possibly because 1940 pregnancies were subject both to dust storms and the cumulative effects of erosion over the Dust Bowl period. Infant mortality rates, however, are unaffected by Dust Bowl exposure regardless of the temporal definition of treatment. Thus, it appears that whatever the adverse effects of Dust Bowl exposure on fetal and neonatal health, these were insufficient to push individuals below the survival threshold, and damage to health status may have remained latent for some time. Accordingly, the results on adult outcomes presented in this thesis show evidence of scarring rather than of culling in early life.[8]

6.5.2 Mortality Across Life-Course

Having established no excess infant deaths due to Dust Bowl exposure, I turn my attention to mortality rates during the rest of an individual's life course. The aim, as before, is to establish whether there is evidence that the Dust Bowl cohort is healthier on average by the time they enter my sample as adults, as a result of the premature death of the cohort's weakest members. I find that the Dust Bowl cohort experienced similar mortality patterns to those unexposed.[9]

6.5.2.1 Methods & Data

I use decennial state-level mortality rates from US Vital Statistics 1930-2000 (Public Health Service, 1947; National Center for Health Statistics, 1968, 1974, 1985, 1994; National Bureau of Economic Research, 2006) for each of the 15 states in my sample to test for whether the Dust Bowl cohort had significantly higher mortality rates at any life stage (under age 1, 1-4, 5-14, 15-24, 25-34, 45-54, 55-64, 65-74, and 74-85) up to and including my observation period, than those not exposed.

[8]Indeed, these results are consistent with Almond (2006), in which infant mortality rates exert at best a weak influence on adult outcomes, and in which scarring dominates culling amongst those exposed to the 1918 influenza pandemic *in utero*. More pertinently to the context studied here, my findings accord with Fishback et al. (2007), which finds that the rise in the infant mortality rate relative to the 1915-1917 trend began several years before the Great Depression (and the Dust Bowl). Although the infant mortality rate experienced a brief spike in 1934, New Deal spending served to reduce it thereafter. The timing of the fluctuations in infant mortality, as in the results here, do not suggest that the Dust Bowl drove these changes.

[9]In order for survivorship bias not to affect my interpretation of adult wellbeing outcomes, the mortality rates of the Dust Bowl cohort should be no different than those of any other birth cohort. It should be noted, though, that although a culling effect would undermine my analysis in this thesis, it would of course be an interesting result on its own. Namely, such a result would indicate that the Dust Bowl's effects on health were so severe that they resulted in premature death rather than in scarring.

Table 6.4: Impact of Dust Bowl Exposure on State Stillbirth and Infant Mortality Rates

	1930-40 (1)	1931-40 (2)	1932-40 (3)	1933-40 (4)	1934-40 (5)	1935-40 (6)	1936-40 (7)	1937-40 (8)	1938-40 (9)	1939-40 (10)	1940 (11)
Stillbirth Rate	-0.4010	-0.1340	0.1900	0.4970	0.9440	1.1550	0.6170	0.8530	0.9950	0.3550	3.1980**
	(2.1680)	(2.1540)	(2.2220)	(2.3170)	(2.2360)	(2.1330)	(2.0910)	(1.5110)	(1.4960)	(1.5420)	(1.3750)
Infant Mortality Rate	-0.9520	0.4300	0.4280	-0.0181	0.0726	-1.4360	-0.8610	-2.0950	-2.6190	-2.9030	-1.1550
	(3.0420)	(3.1900)	(3.0220)	(3.0560)	(3.5210)	(3.2890)	(3.1000)	(3.1180)	(3.1670)	(3.2980)	(3.8670)

*** $p<0.01$, ** $p<0.05$, * $p<0.1$; Note: Table reports $treated_t \times erosion_s$ coefficients. Each row in the left-hand column refers to the regression's dependent variable, while the remaining column headings refer to the way the $treated_t$ term is defined (e.g. for Column 1, $treated_t = 1$ if the year is 1930-40, inclusive). All regressions are estimated by OLS and include state and year fixed effects and state trends. Standard errors, clustered by state, are reported in parentheses below each coefficient.

To test for differences in age-specific mortality rates, I estimate the following regression, by OLS:

$$m_{gast} = \alpha + \beta_1 \times treated_t \times erosion_s + \theta_s + \lambda_t + \gamma_s + u_{gast} \qquad (6.3)$$

where m_{gast} is the mortality rate for gender g in age bin a in state s in year t.

$Treated_t$ is a binary dummy which takes the value of 1 if in a given census year, the age bin for which the regression is being run is populated only by individuals who were aged -1 to 12 during the Dust Bowl (that is, the age bin contains only 1918-41 births in the given census year). Regressions using this version of $treated_t$ are referred to in the tables as "age bins narrowly defined" since an age bin only counts as containing the Dust Bowl cohort if all the age bin's members were children during the Dust Bowl. In the specifications labeled "age bins broadly defined", $treated_t$ takes the value of 1 if any individual in that age bin was a child during the Dust Bowl, even if the age bin also contains individuals not exposed to the Dust Bowl as children. For instance, an age bin containing people born 1915-1920 would be categorized as broadly- rather than narrowly-defined $treated_t$, since it contains both people born 1918-1920, who belong to the Dust Bowl cohort, as well as people born 1915-1917, who were not part of the Dust Bowl cohort.

$Erosion_s$ is, as in the baseline regressions, a population-weighted measure of erosion severity in state s.

As in the baseline, θ_s and λ_t represent state fixed effects and year fixed effects, respectively, while γ_s represents state trends. Standard errors are clustered at the state level. The regression is estimated separately for each gender and age bin; results are reported in Table 6.5 for men and Table 6.6 for women.

6.5.2.2 Results & Discussion

Tables 6.5 and 6.6 provide coefficients of the effect of Dust Bowl exposure on mortality rates by age bin for men and women, respectively.

Table 6.5: Impact of Dust Bowl Exposure on Age-Specific Mortality Rates – Men

	Under 1	1-4	5-14	15-24	25-34	35-44	45-54	55-64	65-74	75-84
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Mortality Rate (Narrow)	-16.6700	-0.0812	-0.4060**	0.2680	0.1680	0.2000	-0.0080	0.6280	-1.0090	
	(15.6200)	(1.2540)	(0.1610)	(0.3920)	(0.5370)	(0.3880)	(0.6670)	(0.9980)	(1.2640)	
Mortality Rate (Broad)	-16.6700	-0.0812	0.03440	-0.0902	0.3250	0.0839	-0.1120	-3.2120**	-1.7190	-6.2280
	(15.6200)	(1.2540)	(0.1040)	(0.4540)	(0.6010)	(0.5020)	(0.4400)	(1.1270)	(2.6260)	(4.0260)

*** $p<0.01$, ** $p<0.05$, * $p<0.1$; Note: Table reports the $treated_t \times erosion_s$ coefficient, e.g. the effect of Dust Bowl cohort membership on mortality rates within a given age bin, relative to mortality rates in the same age bin for those not exposed to the Dust Bowl. Column headers refer to the age bin for which the regression has been run. Row 1 reports results where $treated_t$ has been narrowly defined, i.e., $treated_t = 1$ only if the age bin contains only Dust Bowl-exposed individuals; Row 2 reports results where $treated_t$ has been broadly defined, i.e., $treated_t = 1$ if the age bin contains any Dust Bowl-exposed individuals. All regressions are estimated by OLS and include state and year fixed effects and state trends. Standard errors, clustered by state, are reported in parentheses below each coefficient.

Table 6.6: Impact of Dust Bowl Exposure on Age-Specific Mortality Rates – Women

	Under 1 (1)	1-4 (2)	5-14 (3)	15-24 (4)	25-34 (5)	35-44 (6)	45-54 (7)	55-64 (8)	65-74 (9)	75-84 (10)
Mortality Rate (Narrow)	-15.1600	-0.8830	-0.2750	0.1700	0.5020	0.1380	0.1440	0.4390	0.5520	
	(11.3700)	(1.2450)	(0.2020)	(0.4120)	(0.4220)	(0.2720)	(0.3350)	(0.8080)	(1.2880)	
Mortality Rate (Broad)	-15.1600	-0.8830	-0.0862	0.4840	0.5420	0.3630	0.0564	0.6730	0.5020	0.9840
	(11.3700)	(1.2450)	(0.0757)	(0.4530)	(0.4880)	(0.3850)	(0.3890)	(0.9550)	(1.5320)	(2.2290)

*** $p<0.01$, ** $p<0.05$, * $p<0.1$; Note: Table reports the $treated_b \times erosion_s$ coefficient, e.g. the effect of Dust Bowl cohort membership on mortality rates within a given age bin, relative to mortality rates in the same age bin for those not exposed to the Dust Bowl. Column headers refer to the age bin for which the regression has been run. Row 1 reports results where $treated_b$ has been narrowly defined, i.e., $treated_b = 1$ only if the age bin contains only Dust Bowl-exposed individuals; Row 2 reports results where $treated_b$ has been broadly defined, i.e., $treated_b = 1$ if the age bin contains any Dust Bowl-exposed individuals. All regressions are estimated by OLS and include state and year fixed effects and state trends. Standard errors, clustered by state, are reported in parentheses below each coefficient.

Here, if the Dust Bowl were found to have brought forward in time those deaths that would naturally have occurred in a later age bin (that is, if the age distribution of deaths is significantly different for the Dust Bowl cohort), we might interpret it both as an adverse Dust Bowl health effect unto itself, as well as as a contributor (via selection) to possible overestimation of the effects of the Dust Bowl on adult human capital. Per the variable descriptions in Section 6.5.2.1, in the first row I take a conservative approach to the definition of cohort membership within the age bin, and in the second, a looser one. The results show that for both men and women the Dust Bowl does not cause death rates to be significantly out of step (relative to naturally expected age-specific death rates) at any point in the life course. Two small exceptions are found in the results on men, in which Dust Bowl cohort boys in the narrowly-defined 5-14 age bin, and men in the broadly-defined 55-64 age bin which includes the Dust Bowl cohort, experience slightly but significantly better mortality outcomes than their unexposed counterparts. These findings, however, are not systematic and do not compound the worry that I might be capturing selected survivors in my analysis of adult outcomes. Indeed, the results presented here and in the earlier sections in this chapter suggest no Dust Bowl-induced cohort attrition at any stage of life, whether pre-birth, neonatally, or in adolescence and adulthood. Instead, selection effects, if they exist at all, are dominated by scarring effects.

6.5.3 Conclusion

I find no significant effects of the Dust Bowl in attriting the group of individuals who enter my sample, and whom I observe as adults. Indeed, at no point in their life-course does the Dust Bowl cohort experience significantly worse mortality rates than their unexposed peers: not as infants, as would be reflected in rates of stillbirth or infant mortality, and not later on in life as the cohort moves from childhood to adolescence and on into adulthood. That is, it seems that for all its very real hazards, the Dust Bowl never caused (unobserved earlier-life) health stocks to fall below some survival threshold. As such, it would appear that survivorship bias does not play a role in the results presented elsewhere in this thesis.[10] Instead, the Dust Bowl's adverse effects on adult wellbeing should be interpreted primarily as a function of scarring.

6.6 Conclusion

In this chapter, I have used a variety of additional data and approaches, along with existing features of the baseline empirical design and corroboration by primary and secondary sources, to show the robustness of my findings in the face of possible threats to inference. In particular, I examine the ways in which the Great Depression, migration, selective fertility, and selective mortality could complicate my interpretation of the effects of childhood exposure to the Dust Bowl on adult outcomes, if at all. I find that these phenomena do not, in fact, undermine the analysis presented in this thesis.

Specifically, I demonstrate that although early-life exposure to the Great Depression certainly had statistically significant and economically meaningful adverse effects on later-life human capital, the Dust Bowl independently had adverse effects above and beyond these.

Analyzing the potential impact of migration on my results, I also present evidence that my key findings are if anything a lower bound on the true adverse effects of early-life Dust Bowl exposure. Migration is also found to have been unlikely to influence the intensity of childhood treatment, and there is little evidence of selectivity in childhood migration.

Similarly, I find neither a drop in birth rates due to the Dust Bowl, nor differential

[10] In any case, were such bias to exist, it would only render my results underestimates of the true adverse effects of the Dust Bowl on health.

fertility amongst Dust Bowl mothers by maternal socioeconomic status. These results imply no significant differences in the intrinsic “quality” of cohorts analyzed.

Lastly, an analysis of stillbirth rates, infant mortality rates, and later age-specific mortality rates indicates no evidence of cohort-differential culling. Thus, it is unlikely that the Dust Bowl induced a change in cohort composition (that is, a shift in the health distribution of children or of adults, whether due to selective fertility or mortality) that would in turn affect my larger results on the scarring effects of this environmental disaster.

Indeed, in their totality, the results presented in this chapter make a strong case that the Dust Bowl had potent and largely harmful effects on later-life wellbeing for the children that lived through it, that these effects are driven by the scarring effects of deprivation and ill health in early life, and that if anything, these effects may represent underestimates of the Dust Bowl’s consequences for health.

Chapter 7

Conclusion

Figure 7.1: "Children and Sugar Beets," Hall County, Nebraska, 1940 (Harmon, 1940)

7.1 Summary

Using a differences-in-differences approach, I leverage variation in childhood Dust Bowl exposure to explain variation in adult health and socioeconomic status. Testing for the shock's impact at different developmental stages, and for the shock-modifying effects of agriculture-dependence and public spending responses, allows me to identify the channels through which the Dust Bowl had the greatest adverse effect, and the mechanisms by which recovery may have been possible.

Through this analysis, I show that the Dust Bowl had meaningful long-term human capital costs for those exposed as children. For women, fertility and college completion rates fell, while poverty rates rose. For men, age at marriage fell, while poverty rates, welfare receipts, and disability rates rose. Notably, these adverse effects were most severe for those exposed *in utero* and in early childhood, implicating an indirect health pathway—that is, poor prenatal nutrition and health, and through it, disrupted capability development—in the production of these impacts.

For all those exposed, but especially for women, high school completion rates rose; these impacts, for contrast, were greatest amongst children in late childhood and early adolescence during the Dust Bowl, when the tradeoff between child farm labor and formal schooling may have dictated time use. Thus, as indicated both by the secondary literature and by the income-pathway effects shown here among the farm-dependent, when agricultural livelihoods fell off in regions where child labor was less crucial to farming (i.e. non-cotton regions), the opportunity cost of schooling likely fell, prompting increased school attendance but not college attendance, where cognitive ability—which suffered as a result of Dust Bowl exposure—was a barrier to entry.

Results comparing individuals from farm states with their non-farm counterparts suggest not only that exposed farm-state individuals may have suffered a larger blow to the health endowment, but also that the latter households made human capital investments that compensated for rather than reinforced poor endowments. Similarly, policy intervention was effective: New Deal-related expenditure is found to have attenuated the Dust Bowl's adverse effects, demonstrating the capacity for remediation even where perhaps private investment in recovery was inefficient or constrained.

As intent-to-treat rather than treatment-on-the-treated estimates, my results represent the lower bounds on the actual later-life effects of childhood Dust Bowl exposure. This intent-to-treat design stems from a limitation in census data: the absence of information on individuals' circumstances as children. Although adult outcomes cannot be linked to childhood erosion below the state level, making it difficult to accurately gauge an individual's actual Dust Bowl exposure, true erosion exposure may not be as strongly tied to birth county erosion as expected, meaning that the state-level construction of erosion exposure may actually yield two advantages over county-level construction. First, a less localized measure reflects the reality of the considerable intra-state, inter-county migration documented within the Dust Bowl region (Long & Siu, 2014), and second, a state-level measure is likelier to capture downwind externalities (Hansen & Libecap, 2004). Nevertheless, I do take steps to account for the likely level, timing, and duration of exposure. First, I weight erosion by county population, allowing for a more realistic measure of the severity of human exposure within a state. Second, I disaggregate exposure by developmental stage. Additionally, since census data do not allow for analysis of the effects of differences in parental characteristics or household farm and socioeconomic status in childhood, I include state-level proxies for childhood economic circumstances. Together, these strategies attempt to overcome the data's limitations until the expiry of the 72-year rule for this generational cohort allows for back-linkage through censuses to actual, granular childhood residency and household details.

7.2 Findings in Context

Together, the results presented in the preceding chapters—particularly those on agriculture and developmental stage—indicate that the Dust Bowl should be interpreted as an "indirect" and fundamentally economic shock that had permanent health consequences.

However, the results also hint at when and how certain capabilities are developed, thus shedding light on the technology of human capital formation. Interpreted in light of the theoretical framework presented in Section 2.3, for instance, the finding that health outcomes like disability are determined within the first year of life is intuitive enough, given that the fetal programming literature suggests that the weight accorded to earlier stages in the human capital production function is large, allowing earlier developmental stages to disproportionately influence many health outcomes. However, it is striking that this early-life development pathway would also be responsible for outcomes like college completion and poverty, which might more intuitively be associated with deficits in schooling and labor-market preparation attributable to shocks at school-going age. Indeed, given that high school completion rates rise among the exposed, why do college completion rates not rise as well, and poverty levels fall with this acquisition of education? Why does college behave like a health outcome? The answer might be that cognitive and physical impairments accrued *in utero* as a result of maternal poverty and ill-health influenced individuals' ability to enter and complete college, to hold down jobs, and to earn income as adults. That these impairments were not fully remediated by adulthood is consistent with the Heckman (2007) model outlined in Section 2.3, in which investments made further in time from the shock are less productive.

For contrast, increases in high school completion amongst those exposed (a notable exception to the infant exposure pattern more generally observed) suggest that secondary schooling outcomes may depend less on innate ability than on access to opportunity and questions of time use. As discussed briefly with respect to the change-in-farm-value results, these increases in high school completion rates are substantial: for instance, men exposed between the ages of 7 and 12 see a 3.42 to 3.61 percentage point greater chance of finishing high school; for women, the figures are even higher, ranging from an extraordinary 4.89 to 6.39 percentage point increase. Although the idea that a shock at schooling age would affect schooling is intuitive, finding positive outcomes on schooling may appear less intuitive at first glance. Here, as in establishing the Dust Bowl's income-channel effects, farming livelihoods take center stage.

In particular, the bulk of my findings suggest a tradeoff between secondary schooling and child labor in agriculture. Prior to the Dust Bowl's destruction of farms, child farm labor was prevalent. Whaples (2005) shows that in 1930, 74.5% of boys and 61.5% of girls aged 10-15 were employed in agriculture, and Clay et al. (2012) indicate that child labor was extensive even in the face of compulsory schooling laws, which were not strictly enforced. Laws restricting child labor, such as the 1938 Fair Labor Standards Act, contained many exemptions for child labor in agriculture, especially on family farms (United States Department of Labor, 2007, 2013). Thus, the collapse of agriculture in the Great Plains had scope to reduce opportunities for child labor: the practice was widespread but may not have been as essential on family farms growing wheat than in the commercial production of cotton. Child labor and schooling have been shown to be closely related (McNay et al., 2005; Bhalotra, 2007; Clay et al., 2012). For instance, in present-day India, Bhalotra (2007) finds that lowering the opportunity cost of schooling through cash transfers meant to offset the child's wages is essential to reducing child labor and increasing schooling. Poverty may have remained an issue for Dust Bowl households, but for them, the lack of opportunities for labor in agriculture likely drove a drop in the opportunity cost of schooling. Indeed, McNay et al. (2005) find this phenomenon at play where higher rates of girls' schooling in 19th Century Britain are found in regions where opportunities for girls' employment were few.

Together, the timing of the surge, and the fact that the agriculture interaction results show that state farm status amplifies the Dust Bowl's high school completion effect,[1] imply that a reduction in the need for child labor in agriculture following the destruction of farms may have lowered the opportunity cost of secondary schooling for school-age (i.e., working-age) children who now had no more profitable way to spend their time in regions where child labor was not so essential as to drive employers to retain it during the crisis.[2] Of course, increased high school completion also reflects increased public spending[3]—for instance, on school-building, or on other infrastructure that would have raised the returns to education—as a response to the disaster.

Although this wealth of evidence—for instance, from the results on farm households and developmental stage, and from the secondary literature—supports the view that the Dust Bowl raised rates of high school completion by removing opportunities for child farm labor, it should be noted that more targeted analysis on the basis of pre-Dust Bowl child labor demand suggests that places which were especially child labor-intensive, and which as a result may have been loath to release essential workers, saw little change in schooling outcomes relative to regions in which child labor was used less. Indeed, through this analysis, the only straightforward evidence that changes in the opportunity costs of schooling may have driven secondary schooling in the Dust Bowl cohort to support this explanation except in the case of non-white children exposed to the Dust Bowl. Here, their generally more intensive engagement

[1] In general but also more for men than for women: boys were more active than girls in agricultural child labor (Whaples, 2005), such that when farms failed, their opportunity cost of schooling stood to rise more sharply than that of their sisters.

[2] (Egan, 2006, p. 99) provides anecdotal evidence that the lack of profitable farming work during the Dust Bowl freed children up for other pursuits, namely, school.

[3] Fishback et al. (2006) discuss infrastructure projects, such as school construction, completed under the New Deal, as well as the intra-state migration response to these projects.

in child labor in agriculture relative to white children accords intuitively with their observed relative increase in the chances of school attendance in the face of Dust Bowl-induced agricultural crisis. Together, these results suggest that child labor demand may modify the Dust Bowl shock non-linearly, with the baseline secondary schooling results being driven in large part by the low intensity of child labor use in the Dust Bowl sample.

Most saliently to our understanding of human capital formation, the disparity between *how and when* high school and college completion rates are affected highlights the importance of childhood time allocation concerns to primary and secondary school attendance decisions (Moehling, 1999; McNay et al., 2005; Bhalotra, 2007; Clay et al., 2012), and the intellectual capability barriers to college completion (Heckman, 2007). While for present-day households, credit constraints have been found less important than child ability in determining college enrollment (Heckman, 2007), it should be noted that the same low incomes that may have played a role in producing poor college outcomes through fetal malnutrition and thus cognitive impairments *in utero*, may also have produced poor college outcomes through constraints on a parent's ability to afford college.

The results also shed light on the recovery process. For instance, they suggest that eventual migration may have aided recovery, but that contemporaneous out-migration and fertility restriction, contrary to popular belief and the famine literature, respectively, were not used as margins of adjustment to the crisis. More importantly, the results show that public spending (i.e., the state's contribution to post-shock human capital investment at the individual level), helped mitigate the Dust Bowl's adverse effects—but what role might parental investment responses have played?

Here it is useful to compare the adult outcomes of children in more and less farm-dependent states. As was discussed in Section 4.3, the more severe adverse effects on farm-state children found here can be read in several ways, complicating interpretation. Firstly, these effects may be evidence of a more dramatic initial insult due to the Dust Bowl shock. That is, if farm children were poorer (i.e. had lower pre-shock endowments or investments) and were therefore more susceptible to the shock than their non-farm counterparts, then an equivalent-sized Dust Bowl shock would stand to lower their human capital endowment at the end of period 1 by a greater degree. Secondly, it is possible that a given erosion shock actually represented a larger Dust Bowl shock in farming communities than elsewhere, since erosion likely lowered farming incomes more so than it did other livelihoods. Thirdly, they may be interpreted, per Bhalotra & Venkataramani (2012) and Almond et al. (2009), as a poorer investment response in children for whom the damage to the human capital endowment may well have been equivalent to the initial damage sustained by those in less farm-dependent states, but for whom the returns to human capital investments may have been low in comparison, especially following the collapse of their chief industry, farming. As those in less farm-dependent states are found to have experienced less severe adverse outcomes of Dust Bowl exposure, it is plausible that they may have received compensating investments in human capital. Since much of the literature on human capital suggests that adverse shocks are likely to be reinforced by subsequent investments (even though the dynamic model of human capital formation presented in Section 2.3 theoretically allows for compensating investments, particularly in early childhood when these investments are likeliest to be efficient), the empirical evidence presented here regarding the plausibility of household-level compensating investment is noteworthy. Whatever the household's contribution to recovery, the most striking vindication of compensatory investments, remain, however, the results on New Deal spending. These show that considerable biological capacity for remediation does exist.[4]

[4]They also suggest that the New Deal was especially long-reaching through its beneficial effects on individuals' life-course wellbeing. Thus, by focusing on the immediate effects of New Deal programs (and especially given that much of the meaningful damage to health appears to have been latent until adulthood) we may underestimate its larger impact.

The evidence that Dust Bowl effects are mitigated by public spending and possibly even by household-level investment suggests greater inter-temporal elasticity of substitution than might be expected, even if it may still be reasonably low. However, the findings that recovery is incomplete and that those exposed in earlier stages suffer worse outcomes than those struck later on in life, corroborate theories of self-productivity and dynamic complementarity in investment, and show that inter-temporal investments in human capital are not in fact perfectly substitutable. Thus, as Heckman (2007) and Almond & Currie (2011a) suggest, the assumption that investments have linear effects across childhood is unrealistic.

Notably, and particularly relevant to this context, in which the Dust Bowl compounded Depression-era misfortune, households were likely to have faced credit constraints which limited their capacity to invest in recovery. Accordingly, the paucity of compensatory investment by farm-state households may not necessarily reflect a concerted decision to reinforce the lowered child endowment. Instead, low incomes may have limited the possibility of trading off the basic consumption needed for subsistence in favor of post-shock human capital investment, even where parents may have wished to make these investments.[5] On this front, New Deal spending, particularly in the form of cash transfers, appears to have helped alleviate the household constraint.

7.3 Contribution

These results represent an important contribution to our understanding of the Dust Bowl's impacts; namely, they show quantitatively that exposed children suffered long-

[5]Indeed, this appears to be the likeliest explanation for the results on agriculture-dependence, given the finding that New Deal spending was effective in aiding recovery. However, as discussed in Section 4.3.3, it could also be possible that New Deal investments in recovery were inefficiently large, or that the incentives for private investment in recovery were insufficient to motivate parental remediation attempts while the public returns to such investment justified the observed recovery spending.

term and meaningful damage to health and human capital, and that in contrast to studies which have emphasized the prevalence of reinforcing investments (Almond et al., 2009; Bhalotra & Venkataramani, 2012), there is still considerable biological scope for compensating investments to mitigate environmental insults to health.[6] This study also offers policy-relevant findings, for example, adding to the literature urging intervention in child labor as a means of boosting schooling,[7] and substantiating the hypothesis that college-readiness may be largely determined *in utero* and in early childhood and suggesting that postsecondary educational interventions should be targeted accordingly Heckman (2007); Cunfer (2005). Perhaps most intriguing is the finding that economic shocks can have as large an effect on health as shocks directly targeting health, but that this harm can be overcome through targeted policy intervention.

7.4 Conclusion

Americans exposed to the Dust Bowl in early life suffered permanent and meaningful damage to their health, human capital, and wellbeing in later life. They are more likely to experience disability and poverty, less likely to have completed college, and they had fewer children over the course of their lifetimes, all while their unexposed peers gave rise to the famous post-war Baby Boom. Those exposed in earlier developmental stages—particularly *in utero*—were the most sensitive to the Dust Bowl's negative effects on health, and suffer the worst outcomes as adults.

Much of these adverse effects stem from the Dust Bowl's destruction of agricultural livelihoods. This loss of income in turn disadvantaged children's health, nutrition and early-life development, and also constrained parents' ability to invest in their

[6]Whether such investment is efficient and worthwhile for households and/or for the state is a matter of great policy relevance and merits further study.

[7]Either in complement with schooling reforms per Clay et al. (2012) or via alleviation of poverty constraints per Bhalotra (2007); rather than through child labor restrictions, which have been shown in Moehling (1999) to be ineffective in influencing schooling outcomes.

children's recovery even where the will to invest existed–here, it seems, New Deal-era cash transfers helped alleviate this constraint.

However, the collapse of farming was not without a silver lining: high school completion rates went up among those exposed to the Dust Bowl—in some cases by nearly 6.5 percentage points—plausibly as the demand for child labor in farming decreased.

Although the Dust Bowl did long-term harm, public spending through President Roosevelt's New Deal programs helped mitigate Dust Bowl damage, for example, raising rates of college completion among the exposed by nearly 1 percentage point and reducing their rates of disability by over 4 percentage points.

The results presented in this thesis suggest that we have long underestimated the full human cost of this seminal event in US history—but they also provide some hopeful conclusions. In particular, the findings indicate that timely and substantial policy interventions can aid in recovery from natural disasters. They also bring us closer to understanding when and how in a child's development these investments are likely to be most effective.

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