DISSERTATION

ESSAYS IN THE ECONOMICS OF CARE

Submitted by

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ABSTRACT

ESSAYS IN THE ECONOMICS OF CARE

The Build Back Better legislation (H.R. 5376) currently being debated in Congress represents the first major attempt to build a care infrastructure that heavily invests in children and families, recognizing the value of care and care workers. The legislation (1) promotes recruitment, education, training, retention, and career advancements of direct care workers by providing competitive wages, benefits, and other support services to the direct care workforce; (2) establishes an entitlement program to provide qualifying families the opportunity to obtain high-quality child care; (3) allows states, almost entirely federally funded for the first three years, to provide universal preschool to 3- and 4-year-olds; (4) establishes universal paid family leave; (5) provides infrastructure grants to improve child care safety; (6) supplies child care wage grants for small businesses; (7) provides child care allowances as part of trade adjustment programs for workers; (8) makes permanent the expansion of the Child and Dependent Care Tax Credit provided by the American Rescue Plan Act of 2021; and (9) establishes payroll tax credit for child care workers and tax credits for caregiver expenses. In their own way, each chapter of this dissertation speaks to policies outlined in this legislation.

In Chapter 1, titled *The Role of Care Policy in Procyclical Child Mortality*, I investigate the impact of the business cycle on child mortality. I conceptualize care as being supplied by three sectors—household, private, and public—and argue that public investment and provision insulates children from cyclical fluctuations in the quantity and quality of care provided. I then hypothesize that, in so far as the care mechanism mediates procyclical child mortality, children who are most likely to be the beneficiaries of generous care policy will be less exposed to the mortality risks of economic boom. Employing a sample of 21 OECD countries over the period 1960-2015, I show that procyclical mortality is null for children 5 to 9 years of age. This is the age group for which all OECD countries in my sample have universal, publicly provided

care—i.e., primary education. Among children 0 to 4 years old, however, economic expansions are associated with increased risk of mortality. I then show that procyclical mortality among the 0- to 4-year-old age group is attenuated, and even disappears, in increasingly generous care policy environments.

In Chapter 2, titled *The Contemporaneous Mortality Benefits of the Head Start Program*, I investigate the impact of Head Start on child mortality. Though widely perceived as a schooling program focused on cognitive development, I argue that the “whole child" services provided by Head Start act as a *de facto* investment into the health and safety of poor children. The Head Start Expansion and Quality Improvement Act of 1990 led to considerable variation in program funding across localities. Further, program age requirements meant that increases in funding were largely directed toward the enrollment of 3- and 4-year-old children. Employing a sample of 50 large labor market areas over the period 1983 to 2007, I estimate log-log and log-linear fixed-effect mortality regressions and find that, relative to 1- to 2-year-olds, increases in Head Start funding are associated with reductions in 3- and 4-year-old mortality, all else equal. Then, utilizing that fact that children must also be poverty-eligible for Head Start, I show that the potential mortality benefits of Head Start are pronounced in relatively poor and disproportionately Black communities, as expected.

In Chapter 3, titled *Revisiting the Wages of Virtue and the Relative Pay of Care Work*, I extend and update previous research by investigating the relative pay of care work in the National Longitudinal Survey of Youth 1997. Research in feminist and labor economics provide several theoretical rationale as to why workers in care occupations might receive lower wages. I employ three separate measures of care work and show the continued existence of wage penalties among nurturant care occupations, while there appears to be no wage penalty for workers in reproductive care occupations, all else equal. Testing for heterogeneous care penalties across the occupational skill distribution, I find that the wage penalty for nurturant care work increases in relatively high-skill occupations among men. Alternatively, the wage penalty for nurturant care work is null, if not a slight wage premium, in relatively high-skill occupations among women. I explore potential explanations for the inconsistent behavior of these estimated care penalties across gender, such as occupational crowding and selection via occupational segregation, or sorting. The findings of this chapter have important implications for care penalty literature and motivate potential avenues of future research.

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DEDICATION

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Chapter 1

The Role of Care Policy in Procyclical Child Mortality

1.1 Introduction

Since Ruhm (2000), which found that death rates in the United States rise when the unemployment rate falls, a number of studies have set out to expose the underlying mechanisms that give rise to procyclical mortality. The most prominent hypothesized mechanism is that better labor market opportunities increase the opportunity cost of time and, consequently, reduces individual-level investments in health and well-being. There is some evidence in this direction. For instance, Ruhm (2000) shows that obesity and smoking are procyclical and that diet and exercise improve when the unemployment rate rises. Others noted, however, that procyclical mortality obtains in populations that are only indirectly linked to the labor market. Dehejia and Lleras-Muney (2004) found that infant health outcomes and unemployment are positively linked and, at the other end of the age spectrum, Stevens, Miller, Page, and Filipski (2015) found that procyclical mortality obtains among those ≥ 65 years of age, particularly among those in elderly care facilities. Employing data on employment levels in skilled nursing facilities, the latter study concluded that cyclical fluctuations in the quality of elderly care is another potential mechanism underlying procyclical mortality.

This chapter seeks to contribute to the investigation of mechanisms underlying procyclical mortality. In particular, I explore the role of child care and child care policy in mediating the mortality effects of economic boom that fall on children. I conceptualize care as being supplied by three sectors—household, private, and public. A number of studies suggest that the quantity and quality of household care that children receive is likely to fluctuate counter-cyclically, falling as parents reallocate time from unpaid labor within the household to paid labor in the market (Aguiar, Hurst, & Karabarbounis, 2013; Bauer & Sonchak, 2017; Dehejia & Lleras-Muney, 2004). Others have shown that employment in the early child care sector is characterized by low compensation and high turnover, so that the average quality of care that children receive in the private sector may fluctuate opposite the business cycle (D. Blau, 2001; Brown & Herbst, 2021; Stevens et al., 2015). Taken together, these two strands of literature suggest that care for and supervision of children is likely to fluctuate countercyclically—falling in economic boom and rising in bust. At a young age, however, children in the majority of developed economies transition from care that is largely supplied by a combination of household and private sector services, to publicly provided care—e.g., universal primary education (OECD, 1999, 2015). The provision of public sector goods and services are relatively stable compared to private sector employment and, therefore, the quantity and quality of care provided by the public sector is less likely to fluctuate in relation to the business cycle (Kopelman & Rosen, 2016). I hypothesize that the mortality of children who are the beneficiaries of public care and public care policies will be the least likely to fluctuate in relation to the business cycle. Alternatively, those children that rely on a combination of household and market care will be more exposed to the mortality risks of economic boom.

To empirically implicate the hypothesized insulation effects of the public care sector, I utilize variation in age-specific mortality rates across 21 advanced OECD countries over the period 1960–2015. I find that the mortality of 0- to 4-year-olds—children that, on average, rely on a combination of household and market care in most advanced economies—fluctuates procyclically, while the effect of macroeconomic fluctuations on the mortality 5- to 9-year-olds—children who, on average, are the beneficiaries of universal, publicly-provided care—is null. This result is robust to alternative measures of macroeconomic fluctuations, the use of fine age groups, numerous sample restrictions, and alternative specifications. This first set of analyses, however, is agnostic to the fact that the public provision of, and investment in, early childhood care varies substantially across the OECD countries, as does paid parental leave policy and public spending on families in general. Taking this into account, I employ variation in formal care enrollments, paid parental leave generosity, and public spending on families across countries and find that estimates of procyclical 0- to 4-year-old mortality are attenuated, and even extinguished, in countries with more generous care policy.

The rest of the chapter is organized as follows. Section (1.2) provides a conceptual framework for procyclical child mortality. Section (1.3) describes the data employed and outlines the generalized empirical model. Section (1.4) presents and discusses the empirical results. Section (1.5) concludes.

1.2 Why Procyclical Child Mortality?

Conventional wisdom suggests that, on average, health outcomes should improve and mortality rates fall in economic good times. After all, individuals are more likely to be employed and, therefore, are better able to access the goods and services required to maintain their, and their dependents', health. This may be particularly true in the US where a large majority of the population receives health insurance coverage through an employer provided plan. Influential studies by Brenner (1971, 1975, 1979, 1987) used aggregate time-series data to show that a number of health measures—e.g., admits to mental health hospitals, infant mortality rates, and deaths due to cardiovascular disease, cirrhosis, suicide, and homicide—did vary countercyclically. However, the use of aggregate time-series data in these studies led to a number of technical flaws (Ruhm, 2005).

The use of more robust empirical techniques in the last 20 years has now led to an opposite, and counterintuitive, consensus among health economists—deaths seem to rise in economic boom. The seminal paper in what is now termed the "procyclical mortality" literature was Ruhm (2000)'s study that exploited US state-level variation in mortality rates over the 1972-1991 period. The use of panel data and fixed-effect methods in this study allowed for the control of time-invariant factors that are spuriously correlated with economic conditions across locations—a technical issue that plagued previous time-series studies. Using the unemployment rate as a proxy for macroeconomic conditions, Ruhm (2000) found that a 1% decrease in the state unemployment rate is associated with a 0.5% increase in all cause mortality. Since, the general result of procyclical mortality has been found to obtain for a number time periods, using a variety of data sources, and for a number of, mostly developed, economies—France (Buchmueller, Jusot, & Grignon, 2007), Germany (Neumayer, 2004), Spain (Granados, 2005), the United States (Ionides, Wang, & Granados, 2013; Strumpf, Charters, Harper, & Nandi, 2017), Mexico (Gonzalez & Quast, 2010), Europe generally (Tapia Granados & Ionides, 2017) and OECD countries (Gerdtham & Ruhm, 2006).

There are number of theories as to why mortality fluctuates procyclically. First, leisure time declines during economic upturns, making it more costly to undertake health-producing activities—such as exercise—that are time-intensive (Xu, 2013). Similarly, the time price of medical care will rise if individuals working more hours find it harder to schedule medical appointments. Second, health may be an input into the production of goods and services. Most directly, hazardous working conditions, job-related stress, and the physical exertion of employment may have negative effects on health, particularly when job hours are extended during short-run economic expansions (Asfaw, Pana-Cryan, & Rosa, 2011; Colman & Dave, 2013; Davies, Jones, & Nuñez, 2009). Continuing in a similar vein, non work-related accidents are likely to become more common as the economy expands. For instance, He (2016) showed that the fall in motor vehicle fatalities during the Great Recession is almost entirely due to a decrease in the risk of driving rather than exposure to the amount of driving—e.g., a higher likelihood of a fatal collision with a large commercial vehicle. Some have also pointed to migration flows as a potential contributing factor to procyclical mortality, since these flows can import disease and bring new residents that are unfamiliar with roads or medical infrastructure. Lastly, increases in commercial vehicle travel and traffic congestion can increase the amount of harmful pollutants in the air, negatively affecting health outcomes in economic boom (Brunekreef & Holgate, 2002; Davis, Laden, Hart, Garshick, & Smith, 2010).

A number of the theories discussed above are helpful in explaining why the mortality of labor market participants may increase in economic boom, but what about non-employed elderly persons and children? Some of the theoretical explanations given above extend to dependents quite easily, such as the time cost story. During economic expansion parents, or the primary caretakers of children, may reduce the quantity of care and supervision they provide to their children as they reallocate time from unpaid household to paid market labor (Gough & Killewald, 2011; Voßemer & Heyne, 2019). This story may also obtain for elderly persons who rely on unpaid care and supervision from family members. Additionally, the increased risk of fatal motor vehicle accidents and pollution induced fatality also obtains for both children and the elderly.

Two related studies have focused on cyclical fluctuations in health for young children and the elderly. Similar to Ruhm (2000), Dehejia and Lleras-Muney (2004) employ US state-level unemployment rates, as well as data on parents and infants from the Vital Statistics Natality over the 1975–1999 period, to investigate whether fluctuations in infant health and mortality are driven by (1) a compositional effect—i.e., who is having children—or (2) changes in a mother's pre- and post-neonatal care behaviors—i.e., how children are being cared for—over the business cycle. Using a Beckerian framework (Becker, 1960, 1965, 1992) the authors argue that a decrease in wages leads to an income effect that outweighs the substitution effect for high wage earners—reduce "consumption" of children—and a substitution effect that outweighs the income effect for low wage earners—increase "consumption" of children—so that there will be a selection, or compositional, effect on fertility. Further, the substitution effect of a wage decrease, for both high and low wage earners, allows for time-intensive health activities which should improve infant health outcomes and decrease mortality in economic downturns, all else constant. Thus, both the quantity and quality of household investments in children may be negatively influenced by cyclical substitution effects (Aguiar et al., 2013; Bauer & Sonchak, 2017; Cunha & Heckman, 2007; Heckman, 2015). Ultimately, Dehejia and Lleras-Muney (2004) find that cyclical selection into fertility by socioeconomic status varies by race, with less-educated single Black mothers being *less* likely to have children during recessions while less-educated White mothers being *more* likely to have babies during recessions. Alternatively, the authors unambiguously find when unemployment is high, neonatal and postneonatal mortality decline, and all mothers tend to increase their use of prenatal care.

Focusing on health outcomes for the elderly, Stevens et al. (2015) implicate the quality of private sector care in contributing to procyclical mortality. While they don't provide a strong theoretical framework for their investigation, the authors posit a labor market tightness story where the quantity and average quality of care for the elderly declines in economic boom and increases in bust. This is particularly plausible among direct care workers who, at least historically, have been relatively low-paid, low-skilled workers for which there has been substantial shortages in economic expansions (Goodman, 2006; Yamada, 2002). Using state-level yearly unemployment rates constructed from the Current Population Survey (CPS) and place-specific mortality rates from the Vital Statistics' place of death data, the authors find that fluctuations in the number of elderly deaths occurring in care homes is highly correlated with changes in unemployment rate, while fluctuations in the number of elderly deaths that occur outside of care homes is not. Corroborating their initial findings, Stevens et al. (2015) also find that procyclical mortality for the elderly is accentuated in states with a higher percentage of elderly persons living in nursing homes. Finally, and to fully implicate the role of private sector care quality in procyclical mortality for the elderly, the authors use Online Survey, Certification and Reporting (OSCAR) data over the 1990–2006 period to show that employment levels in skilled nursing facilities are highly correlated with the business cycle—increasing in bust.

These two studies suggest that procyclical fluctuations in the mortality of dependents, such as children, is likely to be driven by a combination of factors including (1) fluctuations in the quantity and quality of care received within the household and (2) fluctuations in the quantity and quality of care received in the market (Aguiar et al., 2013; Bauer & Sonchak, 2017; D. Blau, 2001; Brown & Herbst, 2021; Dehejia & Lleras-Muney, 2004; Stevens et al., 2015). Moreover, the combination of household and market effects suggest that children may be “doubly squeezed" in economic boom, as families find it hard to procure high-quality care at the very same moment that they are unable to allocate their time to unpaid care labor within the home. However, there is an additional care-providing sector, particularly for children, which may affect cyclical fluctuations in health outcomes and mortality, but has not yet been investigated in the procyclical mortality literature—the public care sector.

In most developed economies, young children—at the age of 5 or 6 years old in the majority of OECD countries (OECD, 1999, 2015)—transition from a combination of household and private sector care, to publicly provided care—i.e., primary education. Universal and publicly provisioned child care insulates children from the “double squeeze" of economic boom since parents are ensured access to high-quality child care as they reallocate time between household and market activities. In addition to publicly provided care, public subsidization of care through spending on families may stabilize demand for child care over the course of the business cycle. The stabilization of demand for child care in the private sector has the potential to counteract countercyclical fluctuations in the availability and quality of care (Brown & Herbst, 2021). Care policies such as paid parental leave also alleviate constraints faced by families when determining the allocation of time between paid market activities and unpaid care for dependents within the household. Whether it be through public provision of care or through public investment in families, care policy has the potential to mediate the negative external effects of economic boom that fall on children.

1.3 Methods

1.3.1 Data and Measurement

The data employed in the empirical analysis that follows come from three main sources. Age-specific mortality data are obtained from the Human Mortality Database (HMD, 2019). The HMD is a joint project sponsored by teams at the University of California at Berkeley and the Max Planck Institute for Demographic Research in Germany which provides publicly available, detailed, and comparable national mortality data for 41 countries or areas. The mortality rates used in the empirical analysis are constructed from the country-specific 1x1 (age-by-year) HMD life tables. In particular, *Deaths* and the *Exposure to Risk Population* counts are used to generate the mortality measure of interest, which is the all-cause mortality rate. The primary analysis aggregates child mortality data into two groups: 0- to 4-year-olds and 5- to 9-year-olds. Age in the HMD is defined according to the "age last birthday" rule which determines that the population aged x at time t refers to all persons in the age range $[x, x+1)$ at exact time t, or on January 1st of calendar year t. For example, the 4-year-old age group in 1990 refers to all persons in the age range [4,5) on January 1st of 1990. This is important to note given that age-specific mortality rates supply the strategy for identifying the insulating effects of public care on cyclical child mortality. In addition to the empirical estimates that make use of HMD mortality data described above, I also employ data from the World Health Organization's (WHO) Mortality Database to test the robustness of the results to the use of an alternative data source (WHO, 2019).

Table 1.1: Means and Standard Deviations for Major Variables

	OECD Sample (N=1,003)	
Population		
0- to 4-year-olds	2,599,182	(4,349,438)
5- to 9-year-olds	2,661,732	(4,423,520)
Deaths		
0- to 4-year-olds	7,233.37	(15,573.39)
5- to 9–year-olds	697.39	(1,402.7)
Mortality		
Crude Mortality Rate: 0- to 4–year-olds	232.55	(184.05)
Crude Mortality Rate: 5- to 9-year-olds	23.50	(14.45)
Employment		
Unemployment Rate (%)	6.38	(4.15)
Employment-Population Ratio (%)	43.42	(5.76)
Additional Explanatory Variables		
Real GDP per Capita	26,591.79	(11,293.54)
Female Labor Force Participation Rate (%)	58.75	(13.50)
Government Consumption (% of GDP)	16.44	(3.44)

Sources: HMD, 1960-2015; OECD ALFS, 1960-2015; PWT, 1960-2015.
Notes: Standard deviations in parentheses. Crude mortality rates are calculated as deaths per 100,000. Trends in child mortality across the sample of 21 OECD countries over the 1960-2015 period are displayed in Appendix A Figure A.1.

Measures of the business cycle as it relates to employment are drawn from the OECD's Annual Labor Force Statistics Database (OECD, 2019a). The primary measure of the business cycle is a country's unemployment as a percentage of the civilian labor force, as this is standard in the procyclical mortality literature. Additionally, civilian employment as a percentage of the population, or the employment-population ratio, is employed as an additional labor market measure of the business cycle. Additional explanatory variables include the natural log of real GDP per capita and public expenditure as a share of GDP, measures which are collected form the Penn World Tables (Feenstra, Inklaar, & Timmer, 2015), as well as female labor force participation which is drawn from the OECD's Annual Labor Force Statistics (OECD, 2019a).

The sample of analysis is an unbalanced panel of 21 OECD countries over the 1960–2015 period. Appendix Table A.1 shows how the total 1,003 country-year observations are distributed across countries in the sample. Coverage over the period is substantial for all but three countries, resulting in an average of 47 observations per country.[1] Table 1.1 provides summary statistics of variables over the period of analysis. Appendix A Figure A.1 plots the age-specific crude mortality rates for each country over the 1960-2015 sample period, showing substantial declines and convergence in child mortality across OECD countries. Specifically, the sample average mortality rate fell 90% and 85% for 0- to 4-year-olds and 5- to 9-year-olds, respectively, from 1960 to 2015. The bold, black line in each panel of Figure A.1 indicates the trend in child mortality for the United States. Child mortality rates have declined less dramatically in the United States relative to other OECD countries in our sample—78% for 0- to 4-year-olds and 76% for 5- to 9-year-olds—so that as of 2015 the United States performed the worst out of all sample countries in surviving children through the first decade of their lives. The country with exceptionally high child mortality rates at the beginning of the sample period, as well as an exceptional trend to convergence by roughly 2000, is Portugal.

[1] There is a lack of female labor force participation data for Greece, Iceland, and Switzerland.

1.3.2 Empirical Model

The empirical strategy outlined below utilizes variation in age-specific mortality rates across 21 OECD countries over the period 1960–2015. Specifically, I estimate fixed-effects semi-log mortality functions of the form

$$\ln M_{it}^{j} = \beta^{j} E_{it} + X_{it}\gamma^{j} + \alpha_{i}^{j} + \delta_{t}^{j} + \varepsilon_{it}^{j} \tag{1.1}$$

where $\ln M_{it}^{j}$ is the natural log of mortality for age group j and the subscripts i and t index country and year. E_{it}, the variable of interest, is a measure of the business cycle as it relates to employment—e.g., the unemployment rate in most instances—so that the coefficient β^{j} captures the effect of a one percentage point increase in the unemployment rate on the natural log mortality rate of children in age group j. X_{it} is a vector of time-varying controls which attempt to capture other potential changes in observables that codetermine child mortality rates. These controls include the natural log of real GDP per capita, the female labor force participation rate, and public spending as a percentage of GDP. Time-invariant determinants of mortality rates across countries—e.g., possible geographic, cultural, and institutional influences—are controlled for through country fixed effects, α_{i}^{j}, while time-varying shocks and general trends in child mortality common to all countries—e.g., advancements in medical technologies—are controlled for by year fixed effects, δ_{t}^{j}. I also test the inclusion of country-specific time trends, $\alpha_i \times T$, as is common in the literature.[2]

I implement Panel Corrected Standard Errors (PCSE) estimation, which is often employed in cross-sectional time-series settings and allows correction for groupwise heteroskedasticity ($\sigma_i \neq \sigma$) as well as contemporaneous correlation of disturbances across panels ($\sigma_{it} \neq 0$). I also test specifications that allow for first-order autocorrelation in the disturbances—i.e., Cor(ε_{it},$\varepsilon_{i,t-1}$)$\neq$ 0—through both a general (ρ) and country-specific (ρ_i) AR(1) process.

[2]The inclusion of the country-specific time trends may be redundant since both real GDP per capita and the female labor force participation rate are trend-like variables that are included as controls in the model.

I focus my analysis on the mortality of 0- through 9-year-olds. In particular, I investigate heterogeneous effects of the business cycle on the mortality of 0- to 4-year-olds relative to 5- to 9-year-olds. Children in the 0- to 9-year-old age range are common in their need for care and supervision—especially at the 4- to 5-year-old age cutoff—but they differ with respect to where the majority of the care and supervision they receive takes place. The majority of children aged 0- to 4-years-old receive care through a combination of household and private sector while 5- to 9-year-old children, at least in the majority OECD economies, are cared for through the public sector—e.g., universal, primary schooling (OECD, 1999, 2015). I expect that children most dependent on the household and private sectors for care and supervision will more intensely experience the negative external effects of economic boom. Empirically, I expect that $\beta^j < 0$ for $j \in [0,5)$ and $\beta^j \approx 0$ for $j \in [5,10)$. Further, one might expect that procyclical child mortality may be driven by the most vulnerable of the younger age group, 0- to 2-year-olds for instance. My strategy to investigate this is to estimate the Eq. 1.1 model described above using finer, one-year age groups, investigating the discontinuity at the 4- to 5-year-old and 5- to 6-year-old cutoff.

While it is true that the majority OECD economies provide universal, publicly provided care to children around the age of 5 or 6 years old, there is substantial variation in the public provision of, and investment in, care for children under the age of 5 years old. This variation supplies a second strategy for identifying the attenuating effects of care policy on procyclical child mortality. Specifically, I employ data on formal care enrollments, paid leave generosity, public spending on families, as well as a composite measure that is inclusive of all three. Using these measures as proxies of the relative generosity of care policy environments, and focusing on children 0 to 5 years old, I estimate Eq. 1.1 models for subsamples of relatively ungenerous and generous countries. The expectation being that countries with more generous care policy environments will better insulate young children—i.e., those below the age of 5 to 6 years old—from the negative external effects of economic boom. Empirically, I expect that $\beta^j_{Ungenerous} < 0$ for $j \in [0,5)$ and $\beta^j_{Generous} \approx 0$ for $j \in [0,5)$.

1.4 Results

1.4.1 Procyclical Child Mortality

Table 1.2 summarizes the main results of estimating Eq. 1.1, showing that procyclical mortality obtains for young children ages 0 to 4 years old (Panel A) but not for children aged 5 to 9 years old (Panel B) who are the beneficiaries of publicly provided care. Specifically, column (1) presents the fixed-effects estimate of the unemployment rate on the natural log of age-specific child mortality when no other model covariates are included. The coefficient estimates of -0.019 in column (1) of Panel A suggests that a 1% point decrease in the unemployment rate is associated with a net 1.9% increase in 0- to 4-year-old mortality. This 1% point fall in unemployment represents a 15.6% decrease in unemployment from the sample average of 6.4%, generating an unemployment elasticity of 0- to 4-year-old mortality around 0.12. Employing the sample averages, the estimated effect equates to an all-cause mortality increase of 4.4 deaths per 100,000, which amounts to 114 deaths total for the average OECD country in the sample.

In columns (2)–(4), I introduce additional determinants of child mortality so that column (4) provides the estimation of the complete model described by Eq. 1.1. Here, a 1% point decrease in the unemployment rate is estimated to increase the mortality of 0- to 4-year-olds by 2.1%, an unemployment elasticity of 0- to 4-year-old mortality around 0.13. The results produced by specifications that allow for general and country-specific AR(1) processes are presented in columns (5) and (6), respectively. In column (7), the estimated effect of the unemployment rate on mortality is approximately reduced by half upon the inclusion of country-specific linear time trends. Here, a 1% point fall in the unemployment rate is estimated to increase the mortality of 0- to 4-year-olds by 1%, compared to the estimated effect of roughly 2% in columns (1)-(6).

I consider two possible explanations of this result. First, in so far as countries with steeper declines in child mortality also have higher unemployment rates on average, then the inclusion of country-specific time trends expunges the estimated coefficient of downward bias generated by differences in trends across countries which are potentially correlated with unemployment rates. I find some, albeit weak, evidence in favor of this explanation in the data. Specifically,

Table 1.2: Estimated Effects of the Unemployment Rate on *Ln* Mortality Rate

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A: Dependent variable is the natural log of the 0- to 4-year-old mortality rate								
Unemployment Rate	-0.019***	-0.027***	-0.026***	-0.021***	-0.021***	-0.018***	-0.010***	-0.020***
	(0.003)	(0.002)	(0.002)	(0.002)	(0.003)	(0.003)	(0.001)	(0.001)
Ln GDP per Capita		-0.783***	-0.776***	-0.906***	-0.776***	-0.542***	-0.399***	-0.550***
		(0.049)	(0.049)	(0.048)	(0.086)	(0.089)	(0.054)	(0.046)
Female LFPR (%)			0.003***	0.004***	0.003**	0.004**	0.004***	-0.000
			(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Government Consumption (% of GDP)				-0.021***	-0.020***	-0.016***	-0.006***	-0.001
				(0.002)	(0.004)	(0.003)	(0.002)	(0.002)
Panel B: Dependent variable is the natural log of the 5- to 9-year-old mortality rate								
Unemployment Rate	0.001	-0.002	-0.001	-0.001	-0.001	0.005	-0.000	0.001
	(0.003)	(0.002)	(0.002)	(0.003)	(0.004)	(0.003)	(0.003)	(0.002)
Ln GDP per Capita		-0.310***	-0.308***	-0.329***	-0.327***	-0.223**	-0.175	0.11
		(0.058)	(0.059)	(0.063)	(0.085)	(0.088)	(0.108)	(0.087)
Female LFPR (%)			0.001	0.001	0.001	0.002	0.000	-0.001
			(0.001)	(0.001)	(0.002)	(0.002)	(0.002)	(0.002)
Government Consumption (% of GDP)				-0.003	-0.002	-0.002	0.006	0.009**
				(0.003)	(0.004)	(0.003)	(0.004)	(0.004)
Year FEs	X	X	X	X	X	X	X	
AR(1)					X			
Country-Specific AR(1)						X		
Country-Specific Time Trend							X	X

Sources: HMD, 1960-2015; OECD ALFS, 1960-2015; PWT, 1960-2015.
Notes: All models include country FEs. Panel corrected standard errors in parentheses. Levels of statistical significance are indicated by * $p < 0.1$, ** $p < 0.05$, and *** $p < 0.01$.

countries that experienced larger year-by-year reductions in mortality seemed to have higher unemployment rates, on average, over the sample period—though this correlation fails a test of statistical significance at all conventional levels of confidence. The second explanation considers the result given in column (8), where the exclusion of year fixed effects returns the estimated effect of the unemployment rate on mortality to a magnitude more similar to those given in columns (1)–(6). This result suggests that the presence of country-specific time trends may allow the year fixed-effects to better absorb year-by-year fluctuations in mortality that are (1) common to all countries and (2) correlated with fluctuations in the unemployment rate.

In column (1) of Panel B, the estimated effect of the unemployment rate on 5- to 9-year-old mortality is 0.001 and statistically indistinguishable from zero, suggesting that mortality for this age group is unaffected by short-term macroeconomic fluctuations. This result of acyclical mortality is robust to the addition of controls in columns (2)–(4), allowing for first-order autocorrelation in the disturbances in columns (5) and (6), and the inclusion of country-specific time trends in columns (7) and (8). As hypothesized, then, the results presented in Table 1.2 show that procyclical mortality obtains among the 0- to 4-year-old age group—children who more heavily rely on a combination of household and private sector care—but not among 5- to 9-year-old age group—children who are the beneficiaries of universal, publicly provided care.

Turning to the additional explanatory variables, the estimated coefficient for the natural log of real GDP per capita, introduced in column (2), is interpreted as an elasticity—i.e., % change in mortality given by a % change in real GDP per capita. In general, increases in income reduce mortality across both age groups, though the magnitude of the estimated effect is larger for younger children. Specifically, the income elasticity reported column (2) of Panel A indicates that a 1% increase real income per capita is estimated to reduce 0- to 4-year-old mortality by 0.78%, while income elasticity reported in column (2) of Panel B indicates that a 1% increase real income per capita is estimated to reduce 5- to 9-year-old mortality by 0.31%. Female labor force participation is estimated to be a statistically significant determinant of mortality for 0- to-4-year-olds, the age group that has traditionally relied on the unpaid care work of women, while the effect of female labor force participation on the mortality of 5- to 9-yer-olds is estimated to be zero. In column (3) of Panel A, the coefficient estimate of 0.003 suggests that a 1% point increase in the female labor force participation rate is associated with a 0.3% increase in 0- to 4-year-old mortality. This 1% point increase corresponds to a 1.7% increase in the female labor force participation rate from the sample average of 58.8%, generating a FLP elasticity of 0- to 4-year-old mortality of 0.18.[3] Lastly, government spending as a share of GDP is introduced in column (4). Given the cyclical nature of public spending it is no surprise that the coefficient estimate behaves similarly to that on the unemployment rate. A 1% point increase in government spending as a share of GDP is estimated to decrease 0- to 4-year-old mortality by 2.1%. Again, making use of the sample average, the implied public spending elasticity of 0- to 4-year-old mortality given by column (5) of panel A is -0.34.

1.4.2 Sensitivity Tests and Robustness

Employment-Population Ratio

Here, I test the use of an alternative, less stationary labor market measure of the business cycle, the employment-population ratio, and re-estimate the Eq. 1.1 models presented in Table 1.2. Appendix A Table A.2 presents the results of this exercise. The results are qualitatively identical to those given in Table 2.2. That is, procyclical mortality obtains for 0- to 4-year-olds but not for 5- to 9-year-olds. Column (4) of panels A and B present the preferred specification. The statistically significant coefficient estimate of 0.027 suggests that a 1% point increase in the employment-population ratio is associated with a 2.7% increase in 0- to 4-year-old mortality, all else constant. For reference, a 1% point increase in the employment-population ratio above the sample average of 43.4% corresponds a 2.3% increase in employment or an implied employment elasticity of 0- to 4-year-old mortality of 1.17. Across the columns of Panel B I observe a well

[3]There appears to substantial covariation between the female labor force participation and the unemployment rate—see Appendix A Figure A.2. The coefficient estimate for the female labor force participation rate grows in magnitude and becomes more precisely estimated—i.e., the standard error falls—when estimating the Eq. 1.1 model net of the unemployment rate.

estimated null effect of changes in the employment-population ratio on 5- to 9-year-old mortality, all else constant.

Fine Age Groups

In this section I test the result of procyclical mortality for finer age groupings, making use of the 4- to 5-year-old and 5- to 6-year-old age cutoff, when nearly all children in our sample of OECD countries become the beneficiaries of universal, publicly-provided care—i.e., primary education (OECD, 1999, 2015). Recall that age in the HMD is defined according to the "age last birthday" rule, which determines that the population aged x at time t refers to all persons in the age range $[x, x+1)$ at exact time t, or on January 1st of calendar year t. So, the "4-year-old" group in any given year will actually be representing a large number of children who "turn 5" in that year. Therefore, what the HMD calls the "4-year-old" age group, I call the "4- to 5-year-old" age group. This subdivision of the 0- to 9-year-old group generates 10 fine age groupings to estimate over—0- to 1-year-olds, 1- to 2-year-olds, 2- to 3-year-olds, and so on, up to the 9- to 10-year-old age group.

I estimate the preferred Eq. 1.1 model for these 10 age groups with the expectation being evidence of procyclical mortality for each age group up until the 4- to 5-year-old group and evidence of acyclical mortality for the 5- to 6-year-old age group and above. The results of this exercise are shown in Figure 1.1, which plots the estimated effect of the unemployment rate on the natural log of mortality for each age group. I observe robust statistical evidence of procyclical child mortality up through the 4- to 5-year-old age group. Moving from the 0- to 1-year-old to the 4- to 5-year-old age group, the estimated effect of the unemployment rate on child mortality falls by roughly 48% while the intervals of confidence surrounding these estimates appear to expand. The behavior of these estimates suggest that procyclical child mortality may be accentuated for those with the greatest need for care and supervision—i.e., the most vulnerable. As mentioned previously, there is also substantial variation in the public provision of, and investment in, care for children under the age of 5 years old across the OECD countries in the sample. The attenuation of procyclical mortality estimates with age among

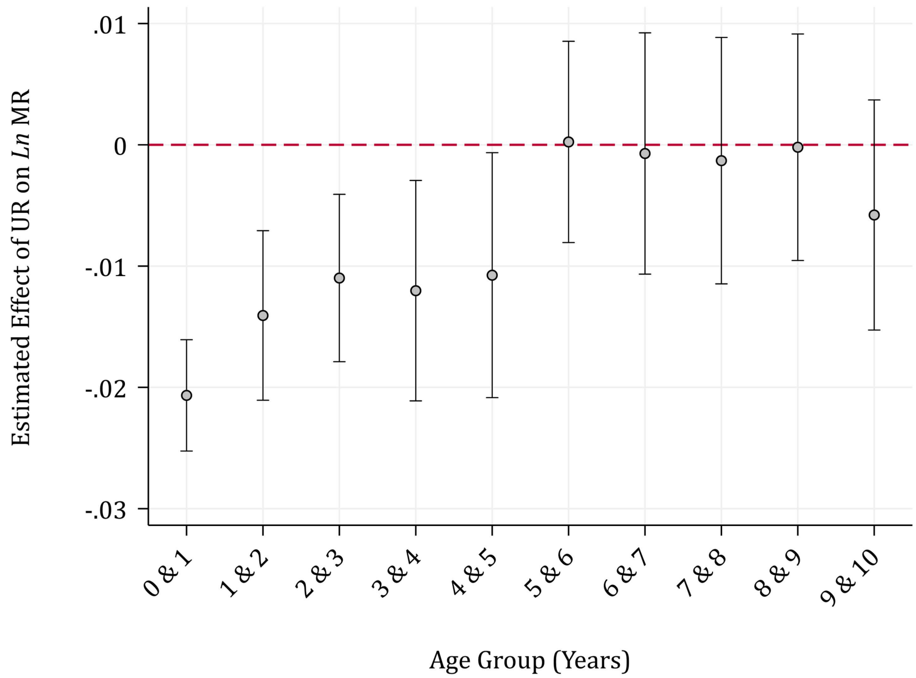


Figure 1.1: Estimated Effect of the Unemployment Rate (UR) on *Ln* Mortality Rate (MR) by Fine Age Group

children 0 to 4 years old, then, may be a reflection of the fact that a number sample countries investment heavily in early childhood care and education—for 3- and 4-year-olds, in particular.

Then, as hypothesized, I observe a discontinuity in the evidence of procyclical mortality at the 4- to 5-year-old age cutoff. Specifically, a 1% point fall in the unemployment rate is estimated to increase the mortality of the 4- to 5-year-old age group by 1.1%, an estimate that is significant at 95% level of confidence, while the same movement in the unemployment rate is estimated to have no effect on the mortality of the 5- to 6-year-old age group—i.e., the estimated effect is 0.000. Moving beyond the 5- to 6-year-old age group, I find no evidence of procyclical mortality for older children. These results provide substantial evidence in support of the hypothesized insulation effects of public care, as well as providing evidence that the result of procyclical mortality for the 0- to 4-year-old age group is not entirely driven by the youngest and most vulnerable.

Sample Restrictions

In this section I report the results of several tests that place various restrictions on the sample. First, I explore multiple restrictions of the period of analysis, estimating our preferred specification of Eq. 1.1. Table 1.3 displays the results of this exercise, reporting the estimated coefficient of interest which is the estimated effect of the unemployment rate on the natural log of the mortality rate for 0- to 4-year-olds and 5- to 9-year-olds, respectively. The first column of this table re-presents the results given in column (4) of Table 3—the results generated through the full sample 1960-2015—where a 1% point fall in the unemployment rate is estimated to increase the mortality of 0- to 4-year-olds by 2.1%. Moving across the columns of Table 1.3, I verify that the result of procyclical 0- to 4-year-old mortality is not a function of the 1960-2015 time period. In fact, the size and standard errors of the coefficient estimates remain relatively unchanged when the period of analysis is reduced to 1970-2015, 1980-2015, and 1990-2015. Similarly, the estimated null effect for 5- to 9-year-olds holds across sample periods, though the magnitude of the marginally negative coefficient estimates increases across the columns of Table 1.3.

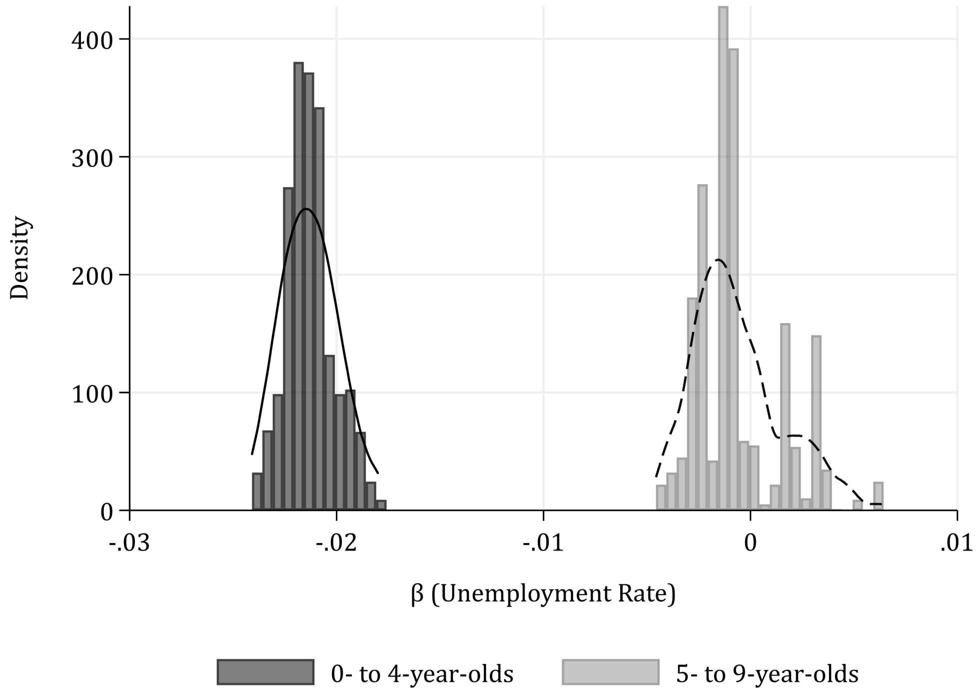
Table 1.3: Estimated Effect of the Unemployment Rate on *Ln* Mortality Rate Over Various Sample Period Restrictions

	1960-2015 (1)	1970-2015 (2)	1980-2015 (3)	1990-2015 (4)
0- to 4-Year-Olds	-0.021***	-0.022***	-0.022***	-0.021***
	(0.002)	(0.002)	(0.002)	(0.003)
5- to 9-Year-Olds	-0.001	-0.001	-0.003	-0.006
	(0.003)	(0.003)	(0.003)	(0.004)
N	1,003	878	708	520

Sources: HMD, 1960-2015; OECD ALFS, 1960-2015; PWT, 1960-2015.
Notes: Estimates are generated by PCSE estimation with country and year fixed effects, as well as the full set of controls from Table 1.2, so that column (1) above corresponds to column (4) of Table 1.2. Standard errors in parentheses. Levels of statistical significance are indicated by * $p < 0.1$, ** $p < 0.05$, and *** $p < 0.01$.

Next, I perform a number of country exclusions to test the effect that the presence of one, two, or three countries might have on the estimate of β. I perform “leave-one-out”, “leave-two-out”, and “leave-three-out” estimation. For the set of 21 advanced OECD countries, there are 21 unique ways to “leave-one-out”, 210 unique ways to “leave-two-out”, and 1,330 unique ways

(A) Estimated Effects of the Unemployment Rate on *Ln* Mortality Rate



(B) z-Scores Associated with Estimated Effects of the Unemployment Rate on *Ln* Mortality Rate

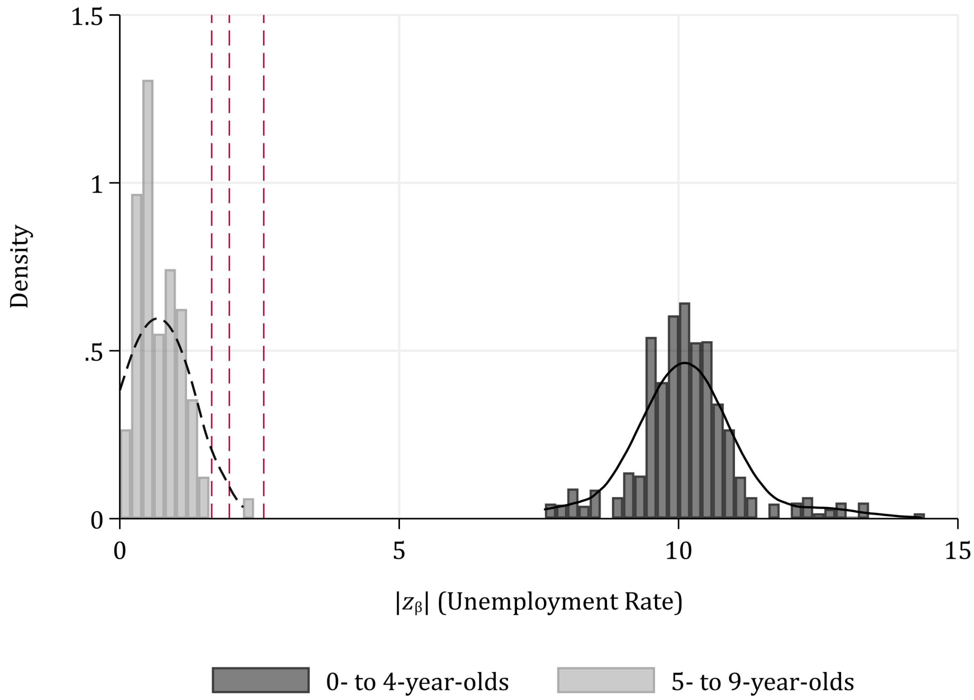


Figure 1.2: Sensitivity of Procyclical Mortality Estimates to Country Exclusion

to “leave-three-out”. This generates a total of 1,561 unique country groupings to estimate over. The coefficient estimates of interest—i.e., the marginal effect of the unemployment rate on the natural log of the mortality rate—generated from the estimation of Eq. 1.1 for each unique country grouping, as well as the z-scores associated with these coefficient estimates, are plotted in the histograms shown in Figure1.2. The vertical lines in panel B of 1.2 indicate the z-scores associated with traditional levels of statistical significance: 1.65 ($p < 0.1$), 1.96 ($p < 0.05$), and 2.58 ($p < 0.01$).

Panel A verifies that the estimate of procyclical 0- to 4-year-old mortality is not due to the presence of a few countries. The coefficient estimates for the 0- to 4-year-old age group range from -0.024 to -0.018 and appear to be normally distributed around a mean of -0.021. All of these coefficient estimates pass a test of statistical significance at the 99% level of confidence as is shown in panel B. On the other hand, panel A verifies the previously discussed result of acyclical mortality for the 5- to 9-year-old age group. The coefficient estimates for this age group range from -0.005 to 0.006 and are, roughly, normally distributed around a mean of -0.0006. Further, panel B shows that only a handful of these coefficient estimates are significantly different from zero. In fact, of the 19 coefficient estimates that are statistically significant at the 95% level of confidence, all of them are greater than zero.

WHO Mortality Data

To further test the sensitivity of these results, I collect additional mortality data from the World Health Organization (WHO) Mortality Database (WHO, 2018). The success of the WHO data is that it reports deaths by underlying cause of death in accordance with the rules of the International Classification of Diseases (ICD). This allows me to test the role of transport related deaths in procyclical child mortality, a cause of death that has been shown to contribute substantially to procyclical mortality for other age groups (Ruhm, 2000). The downfall of the WHO data, however, is its coverage. Specifically, the sample for this test is reduced to an unbalanced panel of 18 advanced OECD countries—Denmark, Greece, and Japan are absent—over the 1996-2015 period, for a total of 259 country-year observations. This small sample disallows the use of

PCSE estimation, which makes use of casewise estimation of the disturbance covariance matrix in the presence of an unbalanced panel, so I employ standard OLS estimation where standard errors are clustered at the country level.

Table 1.4: Comparing Procyclical Mortality Estimates Across HMD and WHO Mortality Data, 1996-2015

	HMD (1)	WHO (2)	WHO (3)
0- to 4-year-olds	-0.014***	-0.015***	-0.014***
	(0.003)	(0.003)	(0.003)
5- to 9-year-olds	-0.006	-0.011	-0.028
	(0.006)	(0.007)	(0.022)
Mortality rate excludes transport related deaths			X

Sources: HMD, 1996-2015; WHO, 1996-2015.
Notes: Estimates are generated via OLS estimation of Eq. 1.1 with country and year fixed effects, as well as the full set of controls presented in Table1.1. For reference, the mean unemployment rate for this sample period is 7.05%. Standard errors in parentheses are clustered at the country-level. Levels of statistical significance are indicated by * $p < 0.1$, ** $p < 0.05$, and *** $p < 0.01$.

Appendix A Figure A.3 displays a scatter plot of HMD mortality rates against WHO mortality rates, showing that the mortality rates supplied by the HMD and WHO sets are nearly identical. Table 1.4 reports the regression results that make use of the new sample and mortality data. The estimates shown in column (1) are generated using the HMD mortality data, as was previously used, but the sample has been reduced to mirror the availability of WHO mortality data. Even for this small sample, I observe qualitatively similar results as discussed previously—procyclical mortality obtains for 0- to 4-year-olds but not for 5- to 9-year-olds. This result obtains in the WHO data as well—column (2)—though the coefficient estimate for 5- to 9-year-olds becomes substantially more negative. Moving to column (3) I find that the coefficient estimate for 0- to 4-year-olds is robust to the removal of transport related deaths, suggesting that the result of procyclical mortality for this age group is not entirely driven by increases in transportation related deaths during economic boom. Interestingly, upon removal the of transport accident deaths, the coefficient estimate for 5- to 9-year-olds becomes substantially more negative, though the standard error of this estimate also grows in size leading to the consistent result of acyclical mortality for 5- to 9-year-olds. For reference, the implied unemployment elasticity of 0- to 4-year-old mortality given by the WHO data over the 1996-2015 period is 0.10.

Asymmetric Effects of BOOM and BUST

Thus far, the empirical models have assumed that the effects of boom and bust on child mortality are symmetric. In other words, the interpretation of the results in Table 1.2 assumed that a 1% increase in the unemployment rate was just as protective as a 1% point fall in the unemployment rate was lethal. In this section, I report the results of empirical tests that relax this assumption. I make use of country-specific standardized unemployment rates, classifying country-year observations according to various standard deviation cutoffs and estimating versions of the flowing equation

$$\ln M_{it}^{j} = \beta^{j} \sum BoomBust_{it} + X_{it}\gamma^{j} + \alpha_{i}^{j} + \delta_{t}^{j} + \varepsilon_{it}^{j} \tag{1.2}$$

where

$$\begin{aligned} \sum BoomBust_{it} = {} & BOOM_{(-\infty,-1.75],it} + BOOM_{(-1.75,-1.25],it} + BOOM_{(-1.25,-0.75],it} + \\ & BOOM_{(-0.75,-0.25],it} + BUST_{[0.25,0.75),it} + BUST_{[0.75,1.25),it} + \\ & BUST_{[1.25,1.75),it} + BUST_{[1.75,\infty),it}. \end{aligned}$$

Here, $BOOM_{(-\infty,-1.75],it}$ is an indicator variable that equals 1 if the unemployment rate in country i at year t is less than or equal to -1.75 standard deviations below the country specific mean. A similar definition applies to all of the other indicator variables in the $BoomBust_{it}$ vector according to the interval given in the subscript of the *BOOM* or *BUST* indicator. Note that the reference group is all country-year observations for which the standardized unemployment rate falls within the -0.25 to 0.25 standard deviation window.

Table 1.5: Testing for Asymmetric Effects of BOOM and BUST on *ln* Mortality Rate

	0- to 4-year-olds	5- to 9-year-olds
	(1)	(2)
BOOM $\in (-\infty, -1.75]$ ($N = 9$)	0.169**	0.129**
	(0.07)	(0.06)
BOOM $\in (-1.75, -1.25]$ ($N = 63$)	0.132***	0.039
	(0.03)	(0.03)
BOOM $\in (-1.25, -0.75]$ ($N = 172$)	0.110***	0.059**
	(0.02)	(0.03)
BOOM $\in (-0.75, -0.25]$ ($N = 143$)	0.011	-0.004
	(0.02)	(0.02)
NORM $\in (-0.25, 0.25)$ ($N = 193$)	–	–
BUST $\in [0.25, 0.75)$ ($N = 153$)	-0.003	0.014
	(0.02)	(0.02)
BUST $\in [0.75, 1.25)$ ($N = 120$)	-0.040**	0.019
	(0.02)	(0.03)
BUST $\in [1.25, 1.75)$ ($N = 100$)	-0.085***	0.017
	(0.02)	(0.02)
BUST $\in [1.75, \infty)$ ($N = 50$)	-0.152***	-0.034
	(0.03)	(0.04)
N	1,003	1,003

Source: HMD, 1996-2015.
Notes: This table reports estimation of Eq. 1.2, which includes country and year fixed effects as well as the full set of controls introduced in Table 1.1. Country-year observations are organized into *BOOM*/*BUST* bins based on country-specific standardized unemployment rates. For example, if a country's standardized unemployment rate was 1 standard deviation below its mean in a given year, that country-year observation is classified as "*BOOM* $\in (-1.25, -0.75]$". All estimated coefficients shown above are relative to the *NORM*, where the *NORM* is all country-year observations in which the unemployment rate was -0.25 to 0.25 standard deviations from the mean. Panel corrected standard errors in parentheses. Levels of statistical significance are indicated by * $p < 0.1$, ** $p < 0.05$, and *** $p < 0.01$.

Table 1.5 reports the estimation of Eq. 1.2, where only the estimated coefficients of the $BoomBust_{it}$ vector are reported. Beginning in the top row of column (1), the coefficient estimate of 0.169 can be interpreted as follows: an unemployment rate that is 1.75 standard deviations above the sample average country's mean is associated with a 18.4% increase in the 0- to 4-year-old mortality rate, relative to the mortality rate when unemployment is within -0.25 to 0.25 standard deviations from the mean. Moving down column (1) we see that 0- to 4-year-old mortality is dose responsive to the size of *BOOM* and *BUST* where the estimated coefficients fall monotonically and the sign of the estimated coefficients flip at the transition from *BOOM* to *BUST*. The coefficient estimate of -0.152 in the final row of column (1) suggests that an unemployment rate that is 1.75 standard deviations above the sample average country's mean is associated with a 14.1% decrease in 0- to 4-year-old mortality, all else equal. Interestingly, there seems to be some evidence that large BOOMs have a positive effect on the mortality of 5- to 9-year-olds, though the estimated coefficients do not show evidence of dose responsivity—e.g., falling, then rising, and switching signs with no consistent relation to changes in the unemployment rate. There is also evidence in favor of asymmetric effects of *BOOM* and *BUST* for the 0- to 4-year-old group. For each corresponding standard deviation grouping the estimated positive effect of *BOOM* is larger, in magnitude, than the negative estimated effect of *BUST*.

Standardized Analysis

To further verify that the result of procyclical 0- to 4-year-old mortality is not entirely driven by (1) the exceptional downward trend in mortality over the sample period or (2) substantial differences in natural rates of unemployment across countries, I perform standardized, univariate regression analysis. I estimate versions of the following equation

$$m_{it}^{j} = \beta^{j} e_{it} + \varepsilon_{it}^{j} \tag{1.3}$$

where m_{it}^{j} is the detrended and standardized mortality rate for age group j and, as before, the subscripts i and t index country and year. e_{it} is a detrended and standardized variable that captures macroeconomic fluctuations for which I explore three different measures: the unemployment rate, the employment-population ratio, and real GDP per capita. I remove the cyclical component of these measures through the use of country-specific Hodrick-Prescott decompositions. These detrended measures are then standardized.

Due to gaps in the data during certain years, across various countries and the need for a consistent time series in applying a Hodrick-Prescott decomposition, the sample period for this test is reduced to the 1980-2015 period. Figure 1.3 plots the sample average, detrended and

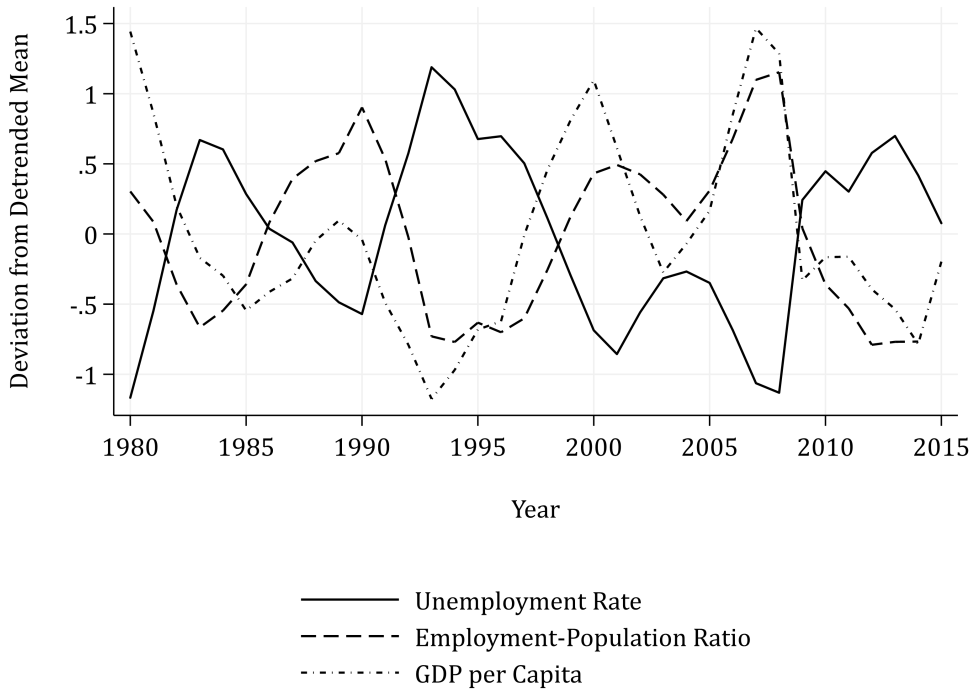


Figure 1.3: Standardized Measures of the Business Cycle for OECD Sample, 1980-2015

standardized measures of the business cycle over the 1980-2015 period. The comovement of these measures overtime provides convincing evidence that each seems to be operating as a proxy for the business cycle. The employment-population ratio and GDP per capita tend to deviate from their mean in the same direction while the unemployment rate deviates contemporaneously in the opposite direction, as expected.

Table 1.6 reports the results of the univariate regression analysis. Beginning in column (1) of panel A, the statistically coefficient estimate of -0.283 suggests that a 1 standard deviation fall in the unemployment rate is associated with a 0.283 standard deviation increase in 0- to 4-year-old mortality. Alternatively, the coefficient estimate of -0.066 in column (1) of panel B, which is statistically indistinguishable from zero at conventional levels of confidence, indicates that movements in the unemployment rate have little to no association with the mortality of 5- to 9-year-olds. Columns (2) and (3) report the results of estimating equation (2) with alternative measures of the business cycle. This exercise produces results qualitatively and quantitatively similar to column (1). In particular, a 1 standard deviation increase in the

Table 1.6: Univariate, Standardized Test of Procyclical Child Mortality, 1980-2015

	(1)	(2)	(3)
Panel A: Dependent variable is the detrended and standardized mortality rate of 0- to 4-year-olds			
ur	-0.283***		
	(0.060)		
emp/*pop*		0.317***	
		(0.051)	
gdp/*pop*			0.236***
			(0.067)
Panel B: Dependent variable is the detrended and standardized mortality rate of 5- to 9-year-olds			
ur	-0.066		
	(0.045)		
emp/*pop*		0.038	
		(0.043)	
gdp/*pop*			0.060
			(0.045)
N	747	724	748

Sources: HMD, 1980-2015; OECD ALFS, 1980-2015; PWT, 1980-2015.
Notes: Standard errors in parentheses. Levels of statistical significance are indicated by * $p < 0.1$, ** $p < 0.05$, and *** $p < 0.01$.

employment-population ratio and GDP per capita is associated with a 0.317 and 0.236 standard deviation increase in 0- to 4-year-old mortality, respectively, while the relationship between the business cycle and the mortality of 5- to 9-year-olds remains null.

1.4.3 Heterogeneous Effects by Care Policy Environment

So far, the identification of the insulation effects of public care has relied solely on the 4- to 5-year-old and 5- to 6-year-old cutoff, ignoring substantial variation in care policy environments across countries. In the section that follows I explore various partitions of the sample according to different proxies of care policy environment. These proxies include enrollments in formal early childhood care and education, paid leave policy, and per capita public spending on families. For each measure, I sort countries into two groups—e.g., relatively “generous" and relatively “ungenerous"—and perform a series of tests, with the expectation that procyclical child mortality will be more pronounced in countries with a relatively ungenerous care policy environment. This exercise is similar to that of Ruhm and Gerdtham (2006), who estimate across country groupings according to the size of social expenditure as a percentage GDP. I am interested in the extent to which procyclical child mortality can be attenuated through care policy. Since the results thus far show that procyclical mortality obtains only in the 0- to 4-year-old age group, I limit my focus to this age group in the analysis that follows and include the 5- to 6-year-old age group as a relevant counterfactual.

Early Childhood Care and Education Enrollments

First, I explore variation in age-specific early childhood care and education (ECCE) enrollment rates across countries. Data on formal ECCE enrollments are drawn from the OECD Family Database (OECD, 2019b) which reports enrollment rates for the year 2016—or the most recent year available in some cases—across four distinct age groups: 0- to 2-year-olds, 3-year-olds, 4-year-olds, and 5-year-olds.[4] Appendix A Table A.3 lists the age-specific ECCE enrollment rates for each country in our sample.[5] Using these measures, countries are sorted into relatively “generous" and relatively “ungenerous" groups according to the sample median—countries with an enrollment rate above the sample median enrollment rate for age group j are placed into the relatively “generous" group, and vice versa. The grouping that results from sorting countries according to this rule can be seen in Appendix A Table A.4. Now, given that the majority of the ECCE enrollment data corresponds to the year 2016, I reduce the sample period to 1990-2015

[4]It is important to note that the formal ECCE enrollments drawn from the OECD Family Database are inclusive of enrollments in both public and private institutions. However, it is likely that countries with high levels of ECCE enrollments are also those with a high levels of public investment into ECCE. To verify this assumption, I collect data on public ECCE expenditure as a percent of GDP from the OECD Social Expenditure Database (OECD, 2019c). The positive correlation between public ECCE expenditures (2015) and ECCE enrollments (2016) is strong among younger age groups and statistically indistinguishable from zero for 5-year-olds—0- to 2-year-olds: $r = 0.721$, $p < 0.001$; 3-year-olds: $r = 0.580$, $p = 0.009$; 4-year-olds: $r = 0.425$, $p = 0.070$; 5-year-olds: $r = 0.153$, $p = 0.532$. The sample size for these correlations is $n = 20$.

[5]There are no data on ECCE enrollments for Canada in the data. Therefore, Canada is excluded from the ECCE analysis.

and note that, since I am unable to observe a continuous measure of formal ECCE enrollments over the period, the country grouping can only proxy for relative enrollment levels.

Table 1.7: Care Policy and the Estimated Effect of the Unemployment Rate on *Ln* Mortality Rate: Formal ECCE Enrollments

	(1)	(2)	(3)	(4)	(5)	(6)
Age group (years):	0 to 1	1 to 2	2 to 3	3 to 4	4 to 5	5 to 6
Panel A: All Countries						
UR	-0.015***	-0.015***	-0.009*	-0.019***	-0.014**	-0.001
	(0.003)	(0.004)	(0.006)	(0.007)	(0.006)	(0.004)
N	496	490	488	483	480	483
Panel B: Relatively "Ungenerous" Countries						
UR	-0.009***	-0.017***	-0.011*	-0.033***	-0.022*	-0.003
	(0.002)	(0.005)	(0.005)	(0.009)	(0.012)	(0.007)
N	256	256	256	242	240	255
Panel C: Relatively "Generous" Countries						
UR	-0.025***	0.004	0.002	-0.001	-0.004	-0.009
	(0.007)	(0.013)	(0.016)	(0.007)	(0.006)	(0.012)
N	240	234	232	241	240	228

Sources: HMD, 1990-2015; OECD ALFS, 1990-2015; PWT, 1990-2015; OECD Family Database, 2016.
Notes: Models include country and year fixed effects as well as the full set of controls introduced in Table 1.1. Panel corrected standard errors in parentheses. Levels of statistical significance are indicated by * $p < 0.1$, ** $p < 0.05$, and *** $p < 0.01$.

Table 1.7 reports the estimation of Eq. 1.1 across six age groups and three country groupings, so that each cell in the table reports the estimated coefficient of interest ($\hat{\beta}$) from one of 18 unique regressions. The estimates in panel A are generated using the full sample of country-year observations generated by 20 OECD countries over the 1990-2015 period. These estimates are qualitatively similar to those shown in Figure 1.1, showing evidence of procyclical mortality for 0- to 4-year-olds and acyclical mortality for 5- to 6-year-olds. Panels B and C report estimates of procyclical mortality for the subsample of relatively "ungenerous" and "generous" countries, respectively. Beginning in column (1), we see that the estimate of procyclical mortality behaves opposite expectation for the youngest age group, with the absolute magnitude of the estimated effect being much larger for the relatively “generous" subsample of countries compared to relatively “ungenerous". While this result is counter expectation, it is not surprising since 0- to 1-year-olds are not often enrolled in formal care and, therefore, are not the population most benefiting from high formal ECCE enrollments. Further, the grouping of countries into relatively “ungenerous" and “generous" for this age group is based on ECCE enrollment data for 0- to 2-year-olds broadly, and, therefore, the grouping may not reflect the appropriate relative care environment for 0- to 1-year-olds.[6] The estimates behave according to expectations in the columns (2) through (6). Procyclical mortality obtains, and is inflated relative to the full sample estimates in Panel B, in the relatively “ungenerous" subsample of countries while evidence of procyclical mortality disappears for 1- to 2-year-olds through 4- to 5-year-olds in panel C.

Paid Leave Policy

Similar to ECCE enrollments, data on paid leave policy across countries in the sample is drawn from the OECD Family Database (OECD, 2019b). The OECD data on paid leave policy spans the 1970-2015 period. The total duration of paid maternity, paternity, and parental leave refers to the total number of weeks which both parents can be on paid leave with job protection after the birth of a child—combining maternity, paternity, and parental leave. And, as is noted in the OECD Family Database, "Data reflect entitlements at the national or federal level only, and do not reflect regional variations or additional/alternative entitlements provided by states/provinces or local governments in some countries." (OECD, 2019b) I rank the paid leave generosity of countries based on the following criterion. In each year of the 1970-2015 period I calculate the median number of weeks with paid maternity, paternity, and parental leave for the sample. Based on these yearly median values, I then indicate whether a country is above or below “median generosity” for that year. For each country in the sample, I then calculate the share of observed years over 1970-2015 period that their paid leave policy was above “median generosity”—see Appendix A Table A.5. Countries that were over the yearly median for the majority of their

[6] A more age-relevant care policy measure for the 0- to 1-year-old age group is likely to be paid parental leave, which is discussed in the following section.

observation years (> 50%) are labeled as relatively "generous" while countries that were under the yearly median for the majority of their observation years are labeled relatively "ungenerous".

Table 1.8: Care Policy and the Estimated Effect of the Unemployment Rate on *Ln* Mortality Rate: Paid Leave

	(1)	(2)	(3)	(4)	(5)	(6)
Age group (years):	0 to 1	1 to 2	2 to 3	3 to 4	4 to 5	5 to 6
Panel A: All Countries						
UR	-0.020***	-0.013***	-0.011***	-0.013***	-0.012***	-0.000
	(0.002)	(0.003)	(0.004)	(0.005)	(0.003)	(0.004)
N	834	828	826	821	818	821
Panel B: Relatively "Ungenerous" Countries						
UR	-0.032***	-0.017***	-0.025***	-0.015*	-0.022**	-0.008
	(0.004)	(0.006)	(0.006)	(0.009)	(0.010)	(0.006)
N	308	308	308	308	308	308
Panel C: Relatively "Generous" Countries						
UR	-0.009***	-0.009**	-0.000	-0.009**	-0.001	0.008
	(0.002)	(0.004)	(0.004)	(0.004)	(0.003)	(0.006)
N	481	475	473	468	465	468

Sources: HMD, 1970-2015; OECD ALFS, 1970-2015; PWT, 1970-2015; OECD Family Database, 1970-2015.
Notes: Models include country and year fixed effects as well as the full set of controls introduced in Table 1.1. Panel corrected standard errors in parentheses. Levels of statistical significance are indicated by * $p < 0.1$, ** $p < 0.05$, and *** $p < 0.01$.

Table 1.8 reports the results of an analysis similar to that presented in Table 1.7, except that countries are now ranked according to their relative generosity of paid leave policy instead of their formal ECCE enrollment levels. The estimates in panel A are generated using the full sample of country-year observations—21 OECD countries over the 1970-2015 period—and they are quantitatively and qualitatively similar to those presented in Figure 1.1 and panel A of Table 1.7. In panels B and C of Table 1.8, estimates of procyclical mortality are accentuated for the subsample of countries with relatively "ungenerous" paid leave policy and attenuated for the sample of relaitvely "generous" countries. As opposed to the estimates reported in Table 1.7, the estimate of procyclical mortality for the 0- to 1-year-old group in Table 1.8 behaves according to expectation across the two subsamples. Specifically, the estimated effect of a 1% point change in the unemployment rate on 0- to 1-year-old mortality is roughly 3 times larger for the subsample of countries with relatively “ungenerous" paid leave policy. This is assuring given that insulation effects of paid leave policy should fall most heavily on infants compared to the insulation effects of formal ECCE enrollments.

Public Spending on Families

Per capita public expenditure on the family is drawn from the OECD Social Expenditure Database (OECD, 2019c). Consistent data for public spending on families begins in 1980, so I reduce the sample period to 1980-2015 for this test. In the OECD Social Expenditure Database, per capita public spending on families includes cash benefits—e.g. family allowances, maternity and parental leave, and other cash benefits—as well as benefits in kind—e.g. early childhood education and care, home help and accommodation, and other in-kind benefits. I rank a country’s generosity of public spending on the family in the same way I ranked paid leave generosity—according to each country’s share of observation years that are above the yearly sample median level of spending—see Appendix A Table A.6. Similar to Tables 1.7 and 1.8, the estimates presented in Table 1.9 show that procyclical mortality is attenuated for 0- to 4-year-olds and, in most cases, is expunged in the subsample of countries with relatively generous per capita public spending on families over the period.

A Composite Measure of Care Policy Environment

I perform one final test to further verify the role of care policy in attenuating procyclical child mortality. Using the OECD data on formal ECCE enrollments for 0- to 2-year-olds and 3- to 5-year-olds, as well as the measures of relative generosity for paid leave policy (Appendix A Table A.5) and public spending on families (Appendix A Table A.6), I employ Principal Components Analysis (PCA) to generate a composite measure of care policy environment. The *care policy PCA score* is generated through the first principal component which accounts for 62% of the variation in the data—see Appendix A Table A.7. Then, using the care policy PCA score as a proxy for the relative generosity of a country’s overall care policy environment, I estimate Eq.

Table 1.9: Care Policy and the Estimated Effect of the Unemployment Rate on *Ln* Mortality Rate: Per Capita Public Spending on Families

	(1)	(2)	(3)	(4)	(5)	(6)
Age group (years):	0 to 1	1 to 2	2 to 3	3 to 4	4 to 5	5 to 6
Panel A: All Countries						
UR	-0.020***	-0.015***	-0.012***	-0.015***	-0.012***	-0.001
	(0.003)	(0.004)	(0.005)	(0.006)	(0.003)	(0.004)
N	658	652	650	645	642	645
Panel B: Relatively "Ungenerous" Countries						
UR	-0.024***	-0.015***	-0.021***	-0.017***	-0.019**	-0.004
	(0.003)	(0.004)	(0.005)	(0.006)	(0.008)	(0.005)
N	320	320	320	320	320	320
Panel C: Relatively "Generous" Countries						
UR	-0.016***	-0.006	0.005	-0.012	0.005	0.007
	(0.003)	(0.008)	(0.010)	(0.008)	(0.006)	(0.012)
N	338	332	330	325	322	325

Sources: HMD, 1980-2015; OECD ALFS, 1980-2015; PWT, 1980-2015; OECD Social Expenditure Database, 1980-2015.
Notes: Models include country and year fixed effects as well as the full set of controls introduced in Table 1.1. Panel corrected standard errors in parentheses. Levels of statistical significance are indicated by * $p < 0.1$, ** $p < 0.05$, and *** $p < 0.01$.

1.1 over 12 country subsamples for each age group in the 0- to 6-year-old age range. I begin with a subsample of the most "ungenerous" countries and transition to a subsample of the most "generous" countries.

The results of this exercise are given in Figure 1.4. Panel A plots estimated effects of a 1% point increase in the unemployment rate on the log mortality rate of 0- to 1-year-olds ($\hat{\beta}$) across 12 different subsamples of countries. From left to right, along the horizontal axis of panel (A), the relative generosity of each country grouping, as measured by the subsample's average care policy PCA score, increases. The point estimates are connected by the solid line and the shaded area surrounding the point estimates plots the corresponding 95% intervals of confidence. Similar to the estimates reported in Tables 1.7, 1.8, and 1.9, procyclical mortality obtains for 0- to 1-year-olds in all countries, though the magnitude is larger, on average, for country groupings that are relatively "ungenerous". The result of procyclical mortality for 0- to 1-year-olds, which

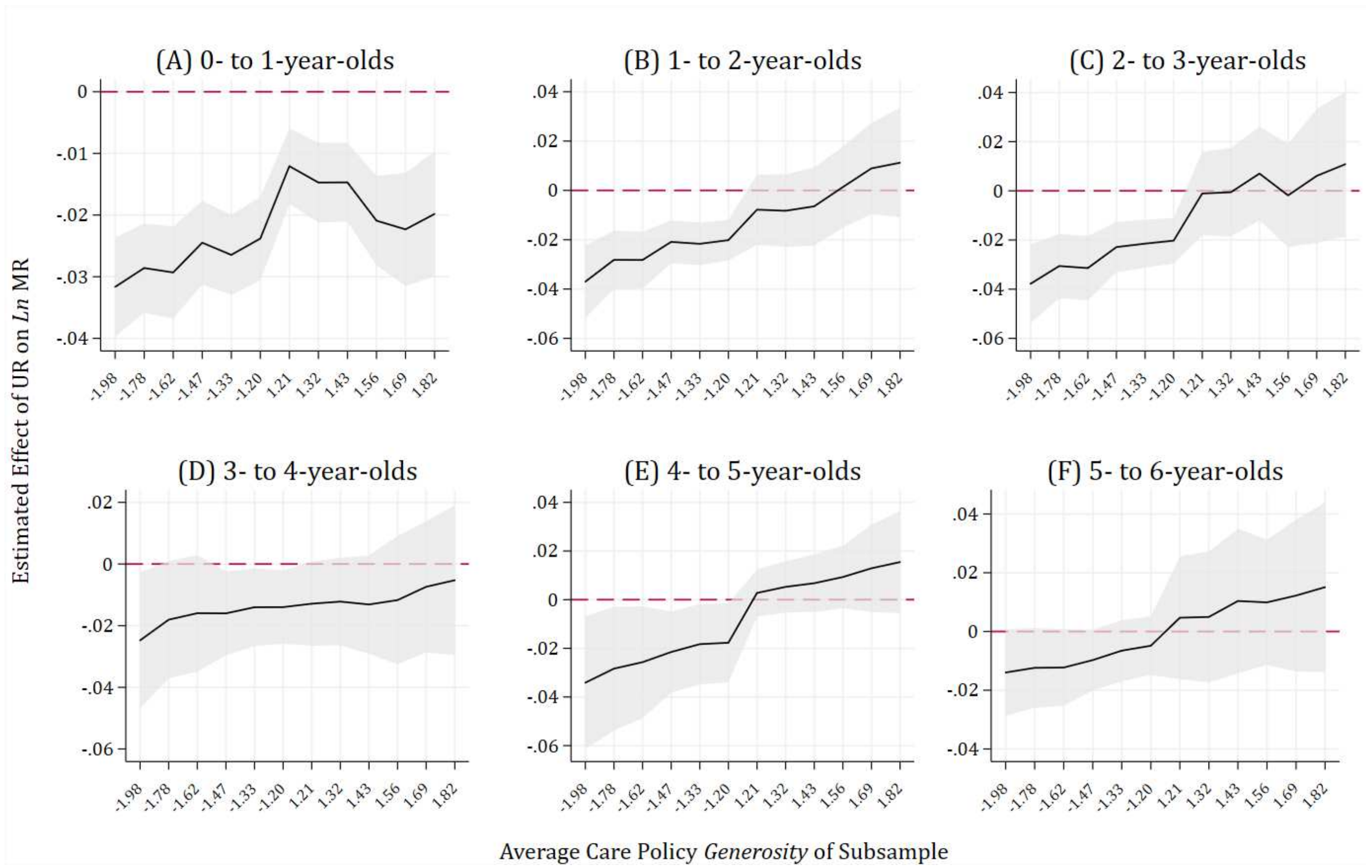


Figure 1.4: Care Policy Environment and Procyclical Child Mortality

is robust to differing care policy environments, may reflect the observations of Dehejia and Lleras-Muney (2004) who find that cyclical fluctuations in infant health outcomes may not be solely attributable to cyclical fluctuations in child care, but also a cyclical fluctuations in fertility decisions that generate selection among types of mothers having children during recessions.

Panels B through E of Figure 1.4 show that estimates of procyclical mortality attenuate in relatively care-generous subsamples. This is indicated by the the upward sloping line connecting the point estimates, as well as the intervals of confidence that overlap zero for more generous country groupings. The estimates of procyclical mortality behave similarly for 5- to 6-year-olds in panel F—i.e., the estimated effect of the unemployment rate on the log mortality rate becomes more positive as the subsample being estimated over becomes more generous. However, all estimates are statistically indistinguishable from zero at the 95% level of confidence which is expected given that, across the majority of OECD countries, 5- to 6-year-olds are the beneficiaries of universal, publicly-provided primary schooling for a substantial portion of each year—see Appendix A Table A.3.

Appendix A Figure A.4 shows the results of this exercise when employing the employment-population ratio in place of the unemployment rate. The results are qualitatively similar, with two exceptions. First, procyclical mortality now appears to be effectively null for 0- to 1-year-olds in relatively “generous" country groupings. This stands in contrast to the results produced via the unemployment rate, where procyclical 0- to 1-year-old mortality appeared to obtain in both ‘the ‘ungenerous" and “generous" samples of countries. Second, there appears to be no impact of care policy environment on procyclical mortality among the 3- to 4-year-old age group. This is somewhat odd given that the estimates produced in all other age groups suggest otherwise. In general, the results produced via the employment-population ratio corroborate the findings produced via the unemployment rate. There is some distinction between the two measures, however, as evidenced by the behavior of the estimates among 0- to 1-year-olds.

1.5 Discussion and Conclusion

This chapter contributes to the investigation of mechanisms underlying pro-cyclical mortality by emphasizing the role of care and care policy in mediating the mortality effects of economic boom that fall on children. I conceptualize care as being supplied by three sectors—household, private, and public. I posit that procyclical child mortality is likely to be driven by a combination of factors that cause countercyclical fluctuations in the quantity and quality of care received within the household and private sectors (Aguiar et al., 2013; Bauer & Sonchak, 2017; D. Blau, 2001; Brown & Herbst, 2021; Dehejia & Lleras-Muney, 2004; Stevens et al., 2015). Alternatively, universal, publicly provided care insulates children from the "double squeeze" of economic boom since parents are ensured access to high-quality child care as they reallocate time between household and market activities. The subsidization of care through spending on families may also stabilize demand for child care over the course of the business cycle and has the potential to counteract countercyclical fluctuations in the availability and quality of care in the private sector (Brown & Herbst, 2021). From this framework, I hypothesize that the mortality of children who are most likely to be the beneficiaries of public care policy will be the least likely to fluctuate in relation to the business cycle.

To empirically implicate the hypothesized insulation effects of the public care sector, I investigate the relationship between short-run macroeconomic fluctuations and age-specific mortality rates across 21 advanced OECD countries over the 1960–2015 period. I find that the mortality of 0- to 4-year-olds—those children who more heavily rely on household and private sector care—fluctuates pro-cyclicaly, while the effect of macroeconomic fluctuations on the mortality 5- to 9-year-olds—the beneficiaries of universal, publicly-provided care—is null. This result is robust to alternative measures of macroeconomic fluctuations, the use fine age groups, numerous sample restrictions, and alternative specifications. Lastly, I explore variation in care policy environments across countries—e.g., formal ECCE enrollments, paid leave generosity, and public spending on families. I find that estimates of pro-cyclical 0- to 4-year-old mortality are attenuated, and even expunged, in countries with relatively generous care policy.

The results of this study have important implications. This study furthers the understanding of mechanisms that give rise to procyclical mortality. Just as Stevens et al. (2015) expose the quality of elderly care as an underlying mechanism of procyclical mortality among the non-employed elderly, this study exposes care as an underlying mechanism of procyclical child mortality and, further, the role of care policy in protecting children from the mortality effects of economic boom. This study also speaks directly to potential benefits of many of the policies proposed in the Build Back Better legislation (H.R. 5376) currently being debated in Congress. Such policies include the subsidization of child care and the rollout of universal preschool for 3- and 4-year-old children, among other substantial care infrastructure investments. The results of this paper show that procyclical child mortality can be attenuated, and even expunged, as a result of such policies and, therefore, save lives.

Chapter 2

The Contemporaneous Mortality Benefits of Head Start Programs

2.1 Introduction

On March 13, 1967, Frank “Pancho" Mansera sat with President Lyndon B. Johnson in the East Room of the White House. Frank was not an important politician or a well-known celebrity. Rather, Frank was a 6-year-old Mexican-American boy from the migrant community of southern San Luis Obispo County who had been selected as the 1966 Head Start Child of the Year. At the time of his enrollment in the inaugural Head Start program, Frank had the cognitive ability of an 18- to 24-month-old child and had not physically grown since he was 2 years of age. In fact, his younger brother of 2 years had already surpassed him in physical stature. Frank was lethargic, bloated, and seemed to be continually sick. Despite their concern, Frank's parents could not afford the private medical care required to diagnose his apparent condition. However, and as is the case for all children in the Head Start program, Frank immediately received a medical screening from a licensed professional who uncovered his hypothyroidism. Within two weeks of his diagnoses and subsequent treatment, Frank's condition improved and over the course of the next year he grew over 5 inches. By the end of Frank's time in the program he was not only ready for school, but his life chances and even his chance at life had been dramatically changed by one short medical examination provided through Head Start.[7]

While widely perceived as a schooling program for poor children, Head Start acts as a provider of, and gateway to, an entire suite of services aimed at serving and supporting the “whole child" (Gibbs, Ludwig, & Miller, 2013). In addition to providing educational services that focus on

[7]This story was obtained from the Early Childhood Learning and Knowledge Center at https://eclkc.ohs.acf.hhs.gov/about-us/article/head-start-timeline as well as from the short film “Pancho" which can be found at https://www.youtube.com/watch?v=OUSSV2k0PmA.

cognitive and social development, Head Start invests heavily in the health of its enrollees by administering health screenings, providing nutritious meals and snacks, and connecting families with medical, dental, and mental health services to ensure children are receiving the care and attention they need (Office of Head Start, 2022). Further, Head Start is committed to fostering parental involvement and positive parent-child interactions, works to ensure that families have access to stable, safe housing arrangements, and acts as a provider of high-quality child care for poor families in search of formal care arrangements, all of which have positive impacts on child safety (Datta et al., 2016; Jacob, Ludwig, & Miller, 2013; Li-Grining & Coley, 2006). Thus, the Head Start program acts as a *de facto* investment into the health and safety of poor children.

In this chapter, I investigate the contemporaneous impacts of Head Start on the most severe measure of child health and well-being: mortality. To my knowledge, there exists only one previous study that has provided insight in this general direction (Ludwig & Miller, 2007). This research focused on the downstream mortality benefits of Head Start among 5- to 9-year-olds over the period 1973 to 1983. In my analysis, I extend this research by investigating contemporaneous effects among age-eligible 3- and 4-year-olds over the more recent period 1983 to 2007. Despite the establishment of Head Start programming in 1994, traditional Head Start programs that serve age-eligible 3- and 4-year-olds account for the overwhelming majority of Head Start enrollments—see Appendix B Figures B.1. Further, in 1990, following the passing of the Head Start Expansion and Quality Improvement Act, Head Start funding and enrollments began to rise. Additional expansions throughout the 1990s led to continued increases up until 2002, when program funding and enrollments stabilized—see Figure 2.1. This led to substantial variation in funding across localities, even among similarly poor labor market areas—see Appendix B Figures B.2, B.3, B.4. The spatially varied expansion and age requirements of the program, together, provide the means to identify the potential contemporaneous mortality benefits of Head Start.

As a first step, I use state-level enrollment and funding data over the period 1988 to 2007 and show that Head Start funding is positively linked to enrollments for 3- to 4-year-olds, but only marginally so for children less than 3 years of age. I also show that, for the 3- to 4-year-old age group, Head Start funds are more effective at generating enrollments in states with larger shares of children in poverty. These results are important for three reasons. First, they supply confidence in the use of Head Start funding as a proxy for enrollments.[8] Second, they show that Head Start funds are almost entirely directed toward programming for 3- and 4-year-old children. Third, they provide intuition in subsequent analyses when testing for heterogeneous mortality benefits in relatively low and high poverty-eligible communities.

Next, in my primary analysis, I estimate the impact of Head Start funding on population-level mortality of age-ineligible and age-eligible children. I define age-ineligible children to be the 1- to 2-year-old age group and age-eligible children to be the 3- to 4-year-old age group. The outcome variable of interest in this analysis is the all-cause mortality rate of age-ineligible and age-eligible children. The explanatory variable of interest is real Head Start funding per age-eligible child (AEC)—i.e, real Head Start funding per 3- and 4-year-old. To combat the issue of zero-inflated death counts, I employ a sample of 50 large commuting zones (CZ) that account for over half of all 1- to 4-year-old persons and deaths in the United States over the sample period 1983 to 2007. Estimating log-log and log-linear fixed effect mortality regressions, I find that, relative to age-ineligible children, a $500 increase in Head Start funding per AEC is associated with a 3.1 to 4.8 percent reduction in the mortality of 3- to 4-year-olds. These results are robust to accounting for contemporaneous establishment and potential expansion of state-run preschool programs, the inclusion of 0-year-olds into the age-ineligible group, the inclusion of 5-year-olds into the age-eligible group, and the partitioning of all-cause mortality into non-external and external causes of death.

Lastly, I investigate heterogeneous mortality benefits in relatively low and high poverty-eligible communities. First, I divide the CZs in my sample into relatively low- and high-poverty subsamples based on the the share of 0- to 4-year-olds in poverty. I find that the estimated mortality benefits of Head Start on 3- to 4-year-old mortality are pronounced in more poverty-eligible

[8] Head Start enrollments are only available at the state level. Total Head Start payments to local grantees, however, are available at the county level.

CZs, as one would expect. Second, I divide the CZs in my sample into two groups based on the relative representation of Black children in the population. Black children are disproportionately represented among those in poverty and, therefore, make up a disproportionate share of Head Start enrollments (Early Childhood Learning and Knowledge Center, 2004; Nolan, Garfinkel, Kaushal, Nam, & Waldfogel, 2016). Further, even when holding poverty constant, Black children face a higher risk of death relative to their White counterparts (Schwandt et al., 2021). I find that the estimated mortality benefits of Head Start on 3- to 4-year-old mortality are also pronounced in disproportionately Black CZs.

The rest of the chapter is organized as follows. Section (2.2) provides a brief history of Head Start and the services that the program provides, as well as a discussion of the known benefits of the Head Start program. Section (2.3) describes the data and empirical model employed to investigate the impact of the Head Start program on child mortality. Section (2.4) presents and discusses the empirical results. Section (2.5) concludes.

2.2 Head Start and Its Potential Impacts on Child Mortality

As part of President Lyndon B. Johnson's War on Poverty, Head Start was established by the Office of Economic Opportunity (OEO) in 1965 as an eight-week summer program for pre-primary aged children in relatively poor counties (Ludwig & Miller, 2007). Given the immediate success of the program, however, Congress authorized Head Start as a primarily part day, nine-month program in 1966 and by 1982 summer-only programs had been phased out entirely (Early Childhood Learning and Knowledge Center, 2022c). Now administered by the Administration for Children and Families in the Department of Health and Human Services, the goal of the program is to promote school readiness by enhancing the social and cognitive development of children through the provision of educational, health, nutritional, social, and other services (Office of Head Start, 2022). Currently, Head Start programs serve nearly 1 million children each year and, since its inception, the program has served roughly 40 million children nationwide (Early Childhood Learning and Knowledge Center, 2019).

Children must be age- and poverty-eligible to be enrolled in Head Start. According the Head Start Program Performance Standards, age-eligibility is determined by the date used to determine eligibility for public school in the community in which the Head Start program is located. Children must be 3 years old, or have turned 3 years old, by this date and be no older than the age required to attend school (Early Childhood Learning and Knowledge Center, 2022b). Early Head Start programming, which was established as part of the Head Start Act Amendments of 1994, serves similarly poverty-eligible children that fall below the traditional Head Start program age cutoff. Despite the establishment of programming for this younger age group, however, Head Start has always, and continues to, primarily serve poverty-eligible 3- and 4-year-old children—see Figure B.1. A child is determined to be poverty-eligible for Head Start if (1) the family's income is equal to or below the poverty line, (2) the family is eligible for or, in the absence of child care, would be potentially eligible for public assistance such as Temporary Assistance for Needy Families (TANF) or Supplemental Security Income (SSI), (3) the child is homeless, or (4) the child is in foster care (Early Childhood Learning and Knowledge Center, 2022b).

Head Start is typically thought of as a schooling program for poor children. As noted by Ludwig and Miller (2007), however, education only accounts for roughly 40 percent of the programs budget (Currie & Neidell, 2007; Richmond, Stipek, & Zigler, 1979). Instead, Head Start acts as a provider of, and gateway to, an entire suite of services aimed at serving and supporting the "whole child" (Gibbs et al., 2013). Head Start providers administer health screenings and ensure that children are up to date on immunizations. Head Start Program Performance Standards also require each enrolled child to have access to health insurance as well as a health care provider and, if a child is lacking either, programs must assist in securing benefits and care as quickly as possible (Early Childhood Learning and Knowledge Center, 2022a). Access to public health insurance has been shown to reduce the risk of mortality among children (Aizer, 2007; Goodman-Bacon, 2018). Programs also ensure that their enrollees are recent on their state's Early and Periodic Screening, Diagnosis, and Treatment (EPSDT) schedule.[9] Additionally, Head Start programs provide nutritious meals and snacks, which alleviates food insecurity and has immense benefits for the health and development of poor children (Gundersen, Kreider, & Pepper, 2011). According to current Head Start Program Performance Standards, programs operating for fewer than six hours per day must provide meals and snacks that provide one third to one half of a child's daily nutritional needs (Early Childhood Learning and Knowledge Center, 2022b). The requirement is one half to two thirds of a child's daily nutritional needs for programs operating more than six hours per day.

The Head Start program also requires local providers to meet staffing requirements, both in quantity and quality, as well as adhere to program safety standards (Early Childhood Learning and Knowledge Center, 2022b). Such regulations have been shown reduce the incidence of unintentional injury among children in day care, but they also tend to increase operating costs and crowd some children out of care (Currie & Hotz, 2004). Thus, local Head Start programs act as a provider of high-quality child care for poor families that are priced out of the market and, instead, often rely on informal care arrangements for their children (Datta et al., 2016). This alleviates the dilemma faced by low income families between (1) providing quality care to their children at the expense labor market opportunities or (2) allocating time towards labor market opportunities at the expense of their child's care. In addition to this, Head Start programs are committed to fostering positive parent-child interactions, parental involvement, and work to ensure that families have access to stable, safe housing arrangements, all of which has a positive impact on the safety of children (Jacob et al., 2013; Li-Grining & Coley, 2006). For instance, in 2019, approximately 59,000 families served by Head Start experienced homelessness. Of those families, 27 percent were assisted in finding housing during the program year and roughly 69,000 Head Start families received housing assistance, such as subsidies, utilities, and repairs (Early Childhood Learning and Knowledge Center, 2019).

[9]The Early and Periodic Screening, Diagnostic and Treatment (EPSDT) benefit provides comprehensive and preventive health care services for children under age 21 who are enrolled in Medicaid. Such continuous care is known to reduce the risk of mortality (Gray, Sidaway-Lee, White, Thorne, & Evans, 2018).

The comprehensive services provided by Head Start programs suggest a wide range of potential benefits. Prior work on Head Start shows that these benefits include improved test scores, decreased likelihood of grade repetition, reductions in obesity among adolescents, as well as increases in adult human capital and economic self-sufficiency.[10] There is one one study in particular that provides prior evidence on the impact of Head Start on child mortality, which is the aim of this investigation. Ludwig and Miller (2007) explore a discontinuity in Head Start funding across US counties at the time the program was launched in 1965. Specifically, the OEO targeted the poorest 300 counties and provided assistance in the application process for local organizations. Ludwig and Miller (2007) show that this led to large reductions in population-level mortality among 5- to 9-year-olds in these counties over the period 1973 to 1983. Since this age group is comprised of children that had previously passed through the Head Start program, this study documents the downstream impact of Head Start of child mortality. To my knowledge, however, there has yet to be an investigation into the potential contemporaneous impact of Head Start on the child mortality—e.g., among 3- and 4-year-olds who make up the majority of Head Start enrollments.

In 1990, following the passing of the Head Start Expansion and Quality Improvement Act, Head Start funding and enrollments began to rise. Additional expansions in 1992, 1994, and 1998 led to continued increases up until 2002, when program funding and enrollments stabilized (see Figure 2.1). Rather than being funneled through state governments, regional Head Start offices provide funds directly to local entities, referred to as "grantees", that are both public and private agencies, with most being non-profit organizations, community action agencies, and school districts. Thus, Head Start funding and program expansions throughout the 1990s varied significantly across local labor markets within and across states—see Appendix B Figures B.2 and B.3. Further, by 2002, Head Start funding varied substantially among similarly poor localities—see Appendix B Figure B.4. The spatially heterogeneous expansion of Head Start

[10] See, for instance, Bryant, Burchinal, Lau, and Sparling (1994); Garces, Thomas, and Currie (2002); Currie and Neidell (2007); Gibbs, Ludwig, and Miller (2011); Carneiro and Ginja (2014); Barr and Gibbs (2017); Johnson and Jackson (2019); Bailey, Sun, and Timpe (2021).

throughout the 1990s and the directing of these additional funds towards the enrollment of 3- and 4-year-old children provide the means to identify the contemporaneous mortality benefits of the Head Start program. In the following section, I describe the data and empirical strategy employed to investigate the potential contemporaneous mortality benefits of the Head Start program.

2.3 Methods

2.3.1 Data and Measurement

The analysis that follows relies on two main data sources. The first is the annual Consolidated Federal Funds Reports (CFFR) from 1983 to 2007 (U.S. Census Bureau, 2011). These reports provide detailed information on the geographic distribution of federally funded items, including total Head Start payments to local grantees by county.[11] From these data I generate the explanatory variable of interest in my analysis: real Head Start funding per 3- and 4-year-old.[12] I convert the nominal Head Start funding data provided in these reports to real 2020 dollars using the U.S. Bureau of Labor Statistics (BLS) Consumer Price Index. I then aggregate the county-level funding data to the 1990 ERS Commuting Zone (CZ) level using the crosswalks provided by the Penn State Commuting Zones/Labor Markets data repository.[13] Population counts for 3- and 4-year-olds come from the county-level estimates produced by the Surveillance, Epidemiology, and End Results (SEER) Program (National Cancer Institute, 2021).

Figure 2.1 plots the CFFR Head Start funding data against published Head Start funding and enrollment data pulled from the Early Childhood Learning and Knowledge Center (2008). The Head Start funding data obtained from the CFFR closely track those published by the Early Childhood Learning and Knowledge Center (ECLKC) in all years except 2000 and 2006,

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[11] In the CFFR records, Head Start funds are recorded under the code 13.600 prior to 1991 and 93.600 from 1991 on.

[12] Children < 3 and ≥ 5 years old account for a small portion of Head Start enrollments and, therefore, funding. The 3- to 4-year-old age group, however, has always been the main focus of the program—see Appendix B Figure B.1.

[13] Crosswalk files were obtained at `https://sites.psu.edu/psucz/data/`.

when Congress included "advance" funds within Head Start's annual appropriations.[14] The vertical dashed line in Figure 2.1 indicates the timing of the Head Start Expansion and Quality Improvement Act, which began the expansion of Head Start funding and enrollments over the period 1990 to 2002. Specifically, real Head Start funding rose from $350 per 3- and 4-year-old child in 1989 to $1,235 in 2002 while, over the same period, enrollments doubled. While this provides some confidence that increases in Head Start funding are positively linked to enrollments, I use state-level data to investigate this more thoroughly in section 2.4.1.

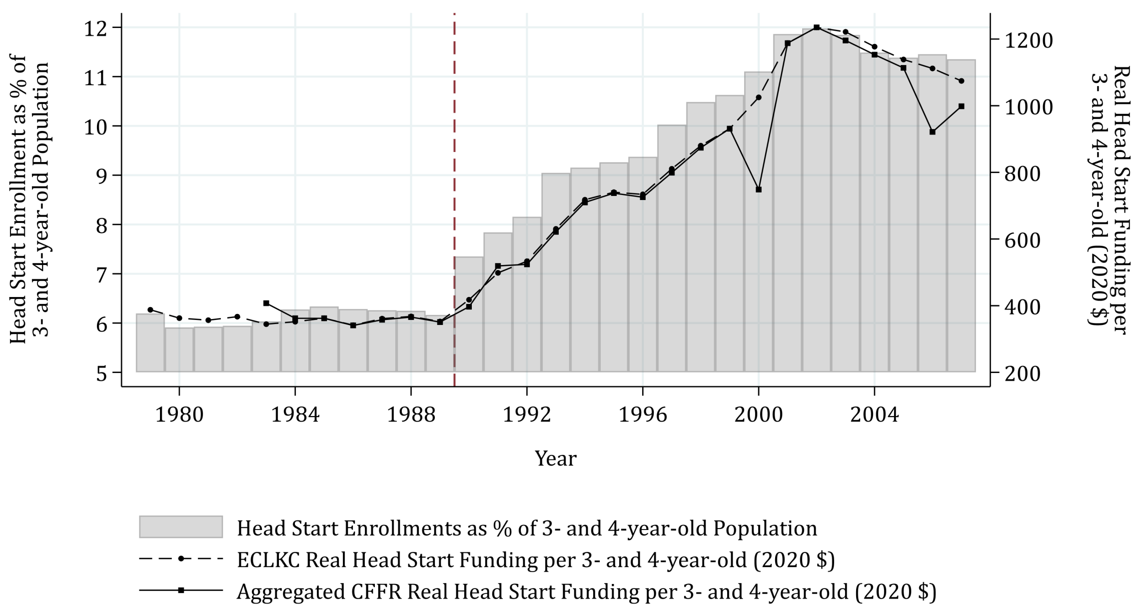

Figure 2.1: Head Start Funding and Enrollments, 1979-2007

The second main source of data are the NCHS Vital Statistics Multiple Cause of Death Files (National Vital Statistics System, 2021). For the years 1983 to 1988, these records are publicly available with county identifiers via the National Bureau of Economic Research Public Use Data

[14]For instance, as a result of the Consolidated Appropriations Act of 2000, Congress began including "advance" funds within Head Starts annual appropriations. Thus, of the $5.267 billion appropriated, $1.4 billion was reserved for FY2001 making only $3.867 billion in appropriated funds available in FY2000. A similar circumstance applies for Head Start funds in FY2006. All of the results presented in this study are robust to (1) the exclusion of 2000 and 2006 data in the analysis and (2) CZ-level interpolation of Head Start funding in 2000 and 2006.

Archive.[15] The mortality records for the years 1989 to 2007, however, were obtained upon request from the NCHS National Vital Statistics System. These detailed data contain all records of death over the period 1983 to 2007. In these mortality data, there are two age-related issues to discuss. First, since an individuals date of birth is not provided in the mortality records, it is impossible to identify whether a child was *approximately* age-eligible for Head Start on the date of their death.[16] Only an individuals age in years at the time of death is provided. For 1- to 2-year-olds and 4-year-olds in the mortality records, it is certain that former are age-ineligible and the latter are age-eligible for Head Start. It is the 3-year-old and 5-year-old groups in the mortality data that are likely to contain both age-ineligible and age-eligible instances of death. The second age-related issue is that, relative to other age groups, child mortality is rare—which is actually a very good thing, to be sure. This results in overwhelming number of zero death counts among single-year age groups and at low levels of geography—e.g. the county level.

These two age-related issues motivate the following. First, despite the fact that a non-trivial portion 3-year-olds in the mortality data are likely age-ineligible for Head Start, I aggregate the mortality data into a 1- to 2-year-old age-ineligible group and a 3- to 4-year-old age-eligible group. This helps to limit the presence of zero death counts in the data at the expense of introducing age-ineligible instances of death into the age-eligible group. Second, I aggregate the mortality data up from the county level to the 1990 ERS CZ level, as was alluded to previously. Similar to the age aggregation, this helps combat zero death count issues, but also allows for a more precise measure of treatment since federal Head Start funds flow directly to local grantees and, therefore, vary substantially across localities within states—see Appendix B Figure B.2. Lastly, I subset my sample of analysis to those CZs that, at the beginning of the sample period in 1983, are the 50 largest in terms of age-eligible population—i.e., population of 3- to 4-year-olds. While this seems like a substantial reduction of the data, these CZs—listed in Appendix B Figure B.1—account for

[15] Access to this data is provided at `https://www.nber.org/research/data/mortality-data-vital-statistics-nchs-multiple-cause-death-data`.

[16] I use the term approximately here because, in addition to a child's date of birth, to precisely observe age-eligibility one would also need to know the age requirements for public school in the community and year in which the child is located.

55.7 percent of the 1- to 4-year-old population and 51.9 percent of 1- to 4-year-old deaths in the United States over the sample period 1983 to 2007. Further, these CZs account for just under half (45.8 percent) of appropriated Head Start funds over the same period.

These decisions have a number of implications for the analysis and results that follow. First, since zero death counts are now absent, I can combine these death data with local-level, age-specific population counts from the SEER Program (National Cancer Institute, 2021) and estimate log-log and semi-log mortality regressions, as is common in the health economics literature. Second, if there are any contemporaneous mortality benefits of Head Start that are detectable at the population level, the presence of age-ineligible instances of death in the age-eligible 3- to 4-year-old group suggests that these estimates may actually be a lower bound. Finally, the truncation of the sample to the large labor market areas discussed above may limit the extrapolation of these estimates to the country more broadly, since these CZs are large metropolitan areas that are not generally representative of all localities across the United States.

The data sources described above are used to generate the explanatory variable of interest—i.e., Head Start funding per 3- and 4-year-old—and the outcome variable of interest—i.e., age-specific all-cause mortality rates. In addition to these, three other data sources were used to construct additional variables used in the analysis. County-level employment characteristics were obtained from the Bureau of Labor Statistics Local Area Unemployment Statistics database (U.S. Bureau of Labor Statistics, 2021) and total personal income data, converted to real 2020 dollars using the U.S. Bureau of Labor Statistics (BLS) Consumer Price Index, were collected from the Bureau of Economic Analysis Economic Profile by County series (Bureau of Economic Analysis, 2021). Additionally, data on the population share of children living in poverty was obtained from the U.S. Census Bureau's Small Area Income and Poverty Estimates (SAIPE) (U.S. Census Bureau, 2021).[17] Table 2.1 reports the sample summary statistics for the variables

[17] These poverty data were only available for 1989, 1993, 1995, and 1997-2007. Further, the estimated share of 0- to 4-year-olds and 0- to 17-year-olds in poverty are both available at the state level, but only the latter is available at the county-level. I utilize the state-by-year relationship between 0- to 4-year-old and 0- to 17-year-old poverty estimates to impute the share of 0- to 4-year-olds in poverty at the county level.

Table 2.1: Means and Standard Deviations for Major Variables

	Largest 50 CZs (N = 1,250)		Largest 40 CZs (N = 1,000)		Largest 30 CZs (N = 750)	
Population						
1- to 2-year-olds	85,015	(81,102)	98,256	(85,667)	115,528	(92,592)
3- to 4-year-olds	84,691	(80,286)	97,848	(84,771)	115,016	(91,565)
Deaths						
1- to 2-year-olds	41.59	(42.43)	47.72	(45.32)	55.63	(49.68)
3- to 4-year-olds	22.19	(21.98)	25.37	(23.45)	29.48	(25.66)
Mortality						
Crude Mortality Rate: 1- to 2-year-olds	50.62	(17.48)	49.78	(16.85)	48.89	(16.33)
Crude Mortality Rate: 3- to 4-year-olds	27.27	(10.33)	26.62	(9.98)	26.01	(9.78)
Head Start Funding						
Real Head Start Funds per AEC	611.84	(369.49)	592.56	(358.91)	584.86	(353.61)
Additional Explanatory Variables						
Real Personal Income per Capita (Thousands)	43.47	(8.69)	44.99	(8.33)	46.70	(8.40)
Unemployment Rate	5.76	(2.44)	5.59	(2.02)	5.51	(1.74)
Percent Female	48.87	(0.20)	48.85	(0.18)	48.87	(0.16)
Percent Black	17.21	(10.95)	16.29	(9.62)	17.75	(10.29)
Percent Non-White, Non-Black	5.20	(4.56)	5.77	(4.65)	6.21	(5.00)
Percent of 0- to -4-year-olds in Poverty*	20.20	(7.41)	19.27	(6.00)	18.50	(5.70)

Sources: CFFR, 1983-2007; NCHS NVSS, 1983-2007; SEER, 1983-2007; BEA CAINC30, 1983-2007; BLS LAUS, 1983-2007; SAIPE, 1989-2007.
Notes: *Poverty data are only available for years 1989, 1993, 1995, and 1997-2007. Crude mortality rates are calculated as deaths per 100,000. See Appendix B Table B.1 for the full list of sample CZs. Standard deviations in parentheses.

employed in the analysis that follows. It should be noted that, compared to those excluded, the more populous sample CZs have (1) higher personal income per capita, (2) lower unemployment rates, (3) a larger population share of non-white children, (4) a smaller percentage of young children living in poverty, and (5) lower child mortality rates.

2.3.2 Empirical Model

Despite the establishment of Early Head Start programming in 1994, traditional Head Start programming that serves age-eligible 3- and 4-year-olds was the primary thrust of the program over the period 1983 to 2007—see Appendix B Figure B.1. Further, the expansion of the Head Start program that began in 1990 led to considerable variation in funding across local labor market areas over period 1990 to 2002—see Appendix 2 Figures B.2, B.3, and B.4. These two facts, taken together, supply the means to identify the potential impact of Head Start on child mortality. Specifically, I estimate

$$\begin{aligned} \ln M_{ait} = \beta_1 \textit{HS funding per AEC}_{it} \times (\textit{3- and 4-year-olds})_a + \\ \beta_2 \textit{HS funding per AEC}_{it} + \beta_3 (\textit{3- and 4-year-olds})_a + \\ X'_{ait}\Gamma_1 + X'_{it}\Gamma_2 + \alpha_i + \delta_t + \varepsilon_{ait} \end{aligned} \tag{2.1}$$

where he outcome variable, $\ln M_{ait}$, is the natural log of the all cause mortality rate for age group a in CZ i and year t. The coefficient of interest, β_1, captures the effect of Head Start funding per age-eligible child on the mortality of 3- and 4-year-olds, relative to 1- and 2-year-olds. The fixed effects estimator—i.e., the inclusion of α_i—ensures the comparison of children in the same labor market area. Thus, β_2, which captures the effect of Head Start funding per age-eligible child on the mortality of 1- and 2-year-olds, absorbs any change in CZ-level Head Start funding that is correlated with local trends in child mortality. Differences in mortality risk across age groups that are common across labor market areas, over time are captured by β_3 while national trends in both child mortality and Head Start funding are controlled for through year fixed effects (δ_t). Additionally, I include time-varying age- and CZ-specific controls that attempt to capture other potential changes in observable that codetermine child mortality. The age-specific demographic controls include the percent of the population that are female, Black, or Non-White, Non-Black while the CZ-specific economic indicators are real personal income per capita and the unemployment rate. I account for potential groupwise heteroskedasticity and contemporaneously correlated disturbances through thus use of panel corrected standard errors (PCSE) techniques, where panels are defined at either the CZ or age-CZ level.

There are two potential sources of bias which need to be addressed. The first source of potential bias is changes in other welfare programs that (1) are plausibly spatially and contemporaneously correlated with expansions in Head Start funding and (2) have similar potential to impact child mortality (Almond, Hoynes, & Schanzenbach, 2011; Grogger, 2004; Guldi, Hawkins, Hemmeter, & Schmidt, 2016; Spencer et al., 2021). However, in so far as changes in these programs, unlike Head Start, impact 1- to 2-year-olds and 3- to 4-year-olds similarly, their impact should be absorbed by the main effect of Head Start funding in the model (β_2). Second, over the sample period 1983 to 2007, a number of states established, or had previously established, state-run preschool programs for at-risk youth similarly targeted by Head Start (Barnett, Robin, Hustedt, & Schulman, 2003)—see Appendix B Table B.3. In so far as (1) the establishment and expansion of these state-run programs was coincident with expansions in Head Start funding across local labor market areas and (2) these programs provide similarly protective services to poor families and children, then the estimated effect of Head Start on child mortality is likely to be biased downward. To address this, I undertake a series of restrictions that subset the sample to include only those CZs in states where Head Start was the primary provider of preschool programming by 2003. I also introduce state by year fixed effects to account for state-level changes in both welfare policy and preschool programming over the sample period.

The empirical strategy outlined above makes use of population-level data identify the potential mortality reducing impact of Head Start programs. Ideally, however, one would like to subset instances of child mortality and the population into four groups: (1) age-ineligible and poverty-ineligible, (2) age-ineligible and poverty-eligible, (3) age-eligible and poverty-ineligible, and (4) age-eligible and poverty-eligible. The empirical hypothesis being that the estimated impact of Head Start on child mortality in group (4) would negatively deviate from that of groups (1)-(3), all else equal. The age definitions and lack of information on family income in the mortality data, as well as the lack of age-specific estimates of populations in poverty over the full sample period, make it impossible to perform this analysis. The 1- to 2-year-old and 3- to 4-year-old age groupings described above provide a rough approximation of age-eligibility, but the mortality and population data are comprised of of both poverty-ineligible and poverty-eligible children.

Instead of foregoing the issue of poverty eligibility entirely, I perform the following analysis as a final test. First, I subset the CZs in my sample into relatively low- and high-poverty groups based on the the average share of 0- to 4-year-olds in poverty over the 1989-2007 period. I then estimate the Eq. 2.1 model separately for these two groups and compare the estimated coefficient of interest (β_1). In other words, I test if the estimated impact of Head Start funding on the mortality of 3- and 4-year-olds is more pronounced in CZs with a higher level of poverty eligibility, all else equal. Second, I subset the CZs in my sample into two groups based on the relative representation of Black children in the population. Black children are disproportionately represented among those in poverty and, therefore, make up a disproportionate share of Head Start enrollments (Early Childhood Learning and Knowledge Center, 2004; Nolan et al., 2016). Further, even when holding poverty constant, Black children face a higher risk of death relative to their White counterparts (Schwandt et al., 2021). I similarly estimate the Eq. 2.1 model among CZs with relatively low and high representations of Black children in the population, separately, and compare the estimated impact of Head Start funding on the mortality of 3- and 4-year-olds across these groups.

2.4 Results

2.4.1 Impact of Head Start Funding on Enrollments

Given the coincident rise of Head Start funding and enrollments shown Figure 2.1, it has been assumed, thus far, that additional Head Start funding increases enrollment. While existing research supports this assumption (Herbst & Kose, 2021), this can be tested directly using state-level, age-specific enrollments from the Kids Count Data Center (2021) and CFFR Head Start funding data aggregated to the state-level over the period 1988 to 2007.[18] Specifically, I estimate regressions of the form

$$\textit{HS Enrollment}^{j}_{it} = \beta^{j}\,\textit{HS funding per AEC}_{it} + \alpha^{j}_{i} + \delta^{j}_{t} + \varepsilon^{j}_{it} \tag{2.2}$$

where $\textit{HS Enrollment}^{j}_{it}$, the percentage of children in age group j that are enrolled in Head Start in state i and year t, is specified to be a function of real Head Start funding per AEC. Time-invariant characteristics that correlate with enrollments and funding across states are captured through state fixed effects (α_i) while national trends in both enrollments and funding are captured through year fixed effects (δ_t). The superscript j throughout Eq. 2.2 indicates that separate models are estimated for each age group. I use the age definitions provided by the Kids Count Data Center in this analysis: < 3-year-olds and 3- to 4-year-olds.

The results of this exercise are presented in Table 2.2. Real Head Start funding per AEC is in units of $500 so that the estimates reflect the percentage point increase in enrollments in response to a $500 increase per-capita funding. The results presented in column (1) of panel A approximately match the relationship implied by Figure 2.1. Specifically, in the aggregated data, real Head Start spending rose from $350 per AEC in 1989 to $1,235 in 2002 while, over the same period, the percentage of 3- and 4-year-olds enrolled in Head Start rose from 6.2 to 12 percent. The implied estimate corresponding to those produced in Table 2.2 is 3.28, a bit smaller than the coefficient estimate of 4.014.

After incorporating year and state fixed effects, column (3) indicates that, all else equal, a $500 increase in real Head Start funding per AEC increases the percentage of < 3-year-olds by 0.33 percentage points (Panel A) while the same increase in per capita funding increases the enrollment of 3- to 4-year-olds by 1.98 percentage points (Panel B). Thus, despite the establishment

[18] Unfortunately, county-level Head Start enrollment data is unavailable, otherwise it would be employed as a more precise measure of treatment across local labor market areas.

Table 2.2: State-level Impact of Head Start Funding on Age-Specific Head Start Enrollments

	(1)	(2)	(3)	(4)
Panel A: Estimates for < 3-year-olds				
HS funding per AEC	0.342***	0.238***	0.334***	0.280***
	(0.038)	(0.021)	(0.052)	(0.042)
HS funding per AEC × (Medium Poverty)				0.222***
				(0.040)
HS funding per AEC × (High Poverty)				-0.067***
				(0.023)
Panel B: Estimates for 3- to 4-year-olds				
HS funding per AEC	4.014***	4.481***	1.979***	1.492***
	(0.183)	(0.178)	(0.223)	(0.177)
HS funding per AEC × (Medium Poverty)				0.523***
				(0.074)
HS funding per AEC × (High Poverty)				0.610***
				(0.153)
States	51	51	51	51
N	1,020	1,020	1,020	1,020
Year FEs		X	X	X
State FEs			X	X

Sources: KCDC, 1988-2007; CFFR, 1983-2007; SAIPE, 1989-2007.
Notes: Estimates are generated by PCSE estimation, accounting for heteroskedastic and contemporaneously correlated disturbances across panels (states). Standard errors in parentheses. District of Columbia is also included as a "state". Levels of statistical significance are indicated by * $p < 0.1$, ** $p < 0.05$, and *** $p < 0.01$.

Early Head Start programming in 1994, funds were largely directed toward the recruitment and service of 3- and 4-year-old children in Head Start programs. The 1.98 percentage point increase in Head Start enrollments among 3- and 4-year-olds represents a 19.4 percent increase above the sample mean of 10.2.

In column (4), I allow for heterogeneous impacts of Head Start funding across poverty terciles. The expectation being: since Head Start eligibility is poverty-based, increases in funding in high-poverty states will be more effective in generating enrollments, all else equal. To generate these poverty terciles, I employ the state-averaged share of 0- to 4-year-olds in poverty over the period 1989 to 2007. The estimates behave according to expectation among 3- to 4-year-old age group. That is, increases in Head Start funding per AEC generate larger increases in enrollments in states with more young children in poverty. The results presented here (1) supply confidence in the use of Head Start funding per AEC as a proxy for 3- and 4-year-old enrollments and (2) suggest that the model estimated impact of Head Start on child mortality may be more detectable in high-poverty communities, given the stronger relationship between funding and enrollments.

2.4.2 Impact of Head Start on Child Mortality

Tables 2.3 and 2.4 present the main results of estimating Eq. 2.1 under a log-log and log-linear specification, respectively.[19] Reported is the estimated coefficient of interest ($\hat{\beta}_1$), which captures the effect of Head Start funding per AEC child on the mortality of 3- and 4-year-olds, relative to 1- and 2-year-olds, as well as the estimated main effect of Head Start funding per AEC ($\hat{\beta}_2$). Column (1) presents the results when no additional explanatory variables are included in the model, while columns (2) and (3) introduce economic and demographic controls. Columns (4) and (5) allow correction for groupwise heteroskedasticity at the CZ and age-CZ level, respectively, with the latter also correcting for potential contemporaneous correlation of disturbances across panels. Column (6) employs Prais-Winsten estimation, correcting for first-order autocorrelation in the disturbances through a panel-specific specific AR(1) process, where panels are defined at the age-CZ level. Panels A, B, and C report the results for the largest 50, 40, and 30 CZs, respectively, as defined by the size of the 3- to 4-year-old population in 1983, the beginning of the sample period.

Beginning with Table 2.3, in column (1) of panel A I find that, relative to the mortality of 1- to 2-year-olds, a 1 percent increase in Head Start funding per AEC is associated with a 0.04 percent reduction in 3- to 4-year-old mortality. This is the most conservative estimate produced in Table 2.3. To put this estimate into context, a $500 increase in Head Start Funding per AEC

[19] These specifications are log-log and log-linear with respect to the outcome variable and explanatory variable of interest. All other explanatory variables are non-transformed and included as defined in Table 2.1.

represents a 81.7 percent increase above the sample mean.[20] The estimate then suggests that this \$500 increase is associated with a 3.3 percent reduction in 3- to 4-year-old mortality. The largest estimate comes from column (6) of panel C, where the coefficient estimate of -0.056 suggests that, a \$500 increase in Head Start Funding per AEC is associated with a 4.8 percent reduction in the mortality of the 3- to 4-year-old age group, all else equal. Under the assumption that the state-level relationship between funding and enrollments can be extended to the CZs in my sample, these estimates suggest a mortality elasticity with respect to enrollment of -0.17 to -0.25.[21] Further, a \$500 increase in Head Start funding per AEC across the CZs in the sample reflects a total increase in Head Start funding of \$2.1 billion (2020 \$) and total decrease of 38 3- to 4-year-old deaths.[22] The implied morality benefits of such an increase in Head Start funding should be considered along side the wide range of benefits that the Head Start program has on long-term outcomes for children. The behavior of the difference estimate ($\hat{\beta}_1$) is consistent across the columns of Table 2.3. The standard errors are *marginally* increased at times in columns (4) through (6) after making corrections for potential groupwise heteroskedasticity, contemporaneous correlation across groups, and panel-specific first-order autocorrelation in the disturbances.

Table 2.4 reports the results of models identical to those reported in Table 2.3, but under a log-linear specification where real Head Start funding per AEC is defined in units of \$500. The difference estimates ($\hat{\beta}_1$) produced here suggest mortality benefits of Head Start that are similar to those under the log-log specification. The main difference between the two, however, is the behavior of the estimated main effect. In column (1) of Panel A I find that, relative to

[20] For reference, a movement from the 10th to 90th percentile of sample Head Start funding per AEC is an \$865 increase and the average increase in per capita funding over the 1990-2002 period was \$760.

[21] In response to a \$500 increase in Head Start funding per AEC, the enrollment of 3- and 4-year-olds increased by 1.98 percentage points from a sample mean of 10.2—a 19.4 percent increase. Alternatively, a \$500 increase in Head Start funding per AEC is associated with a 3.3 to 4.8 percent reduction in 3- to 4-year-old mortality. Thus, -0.033/0.194 = -0.17 and -0.048/0.194 = -0.25

[22] This is calculated by using the sample averages for the 50 large CZs in Table 2.1 and the estimate implied by column (5) of Panel A in Table 2.3. Specifically, \$500 × 84,691 × 50 = 2,117,275,000 and -0.033 × [27.27 × (84,691/100,000)] × 50 = -38.

Table 2.3: Impact of Head Start Funding on All-Cause Child Mortality Under Log-Log Specification

	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: 50 Largest CZs						
Ln(HS funding per AEC)	-0.040**	-0.040**	-0.040**	-0.040**	-0.040**	-0.045***
× (3- and 4-year-olds)	(0.016)	(0.016)	(0.016)	(0.017)	(0.017)	(0.017)
Ln(HS funding per AEC)	0.04	0.032	0.035	0.035	0.035	0.036
	(0.025)	(0.026)	(0.025)	(0.027)	(0.026)	(0.025)
N	2,500	2,500	2,500	2,500	2,50	2,500
Panel B: 40 Largest CZs						
Ln(HS funding per AEC)	-0.053***	-0.053***	-0.055***	-0.055***	-0.055***	-0.053***
× (3- and 4-year-olds)	(0.016)	(0.016)	(0.016)	(0.019)	(0.016)	(0.016)
Ln(HS funding per AEC)	0.071***	0.061**	0.062**	0.062**	0.062**	0.058**
	(0.026)	(0.026)	(0.026)	(0.026)	(0.027)	(0.026)
N	2,000	2,000	2,000	2,000	2,000	2,000
Panel C: 30 Largest CZs						
Ln(HS funding per AEC)	-0.051***	-0.051***	-0.055***	-0.055**	-0.055***	-0.056***
× (3- and 4-year-olds)	(0.018)	(0.018)	(0.018)	(0.022)	(0.019)	(0.020)
Ln(HS funding per AEC)	0.089***	0.076**	0.067**	0.067*	0.067**	0.073**
	(0.029)	(0.030)	(0.030)	(0.036)	(0.031)	(0.030)
N	1,500	1,500	1,500	1,500	1,500	1,500
CZ FEs	X	X	X	X	X	X
Year FEs	X	X	X	X	X	X
Economic Controls		X	X	X	X	X
Demographic Controls			X	X	X	X
CZ Clustered SE				X		
Age-CZ PCSE					X	X
Age-CZ-specific AR(1)						X

Sources: CFFR, 1983-2007; NCHS NVSS, 1983-2007; SEER, 1983-2007; BEA CAINC30, 1983-2007, BLS LAUS, 1983-2007.
Notes: Panel corrected standard errors (PCSE) adjust for heteroskedastic and contemporaneously correlated disturbances across panels (age-CZ). Estimated models that allow for age-CZ-specific first-order autocorrelation in the disturbances are achieved via Prais–Winsten regression. Standard errors in parentheses. Levels of statistical significance given by * $p < 0.1$, ** $p < 0.05$, and *** $p < 0.01$.

the mortality of 1- to 2-year-olds, a \$500 increase in Head Start funding per AEC is associated with 3.1 percent reduction in 3- to 4-year-old mortality.[23] Again, this is the most conservative

[23]The marginal effects of non-transformed variables in semi-log models are obtained through the following calculation: $[\exp(\hat{\beta}) - 1] \times 100$ (Halvorsen & Palmquist, 1980).

estimate produced in Table 2.4. The largest estimate comes from column (5) of Panel C, where the coefficient estimate of -0.048 suggests that, a $500 increase in Head Start Funding per AEC is associated with a 4.7 percent reduction in the mortality of the 3- to 4-year-old age group, all else equal.

Table 2.4: Impact of Head Start Funding on All-Cause Child Mortality

	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: 50 Largest CZs						
HS funding per AEC	-0.032**	-0.032**	-0.032**	-0.032**	-0.032**	-0.035**
× (3- and 4-year-olds)	(0.014)	(0.013)	(0.013)	(0.014)	(0.015)	(0.015)
HS funding per AEC	0.034**	0.028*	0.023	0.023	0.023	0.028
	(0.017)	(0.017)	(0.017)	(0.016)	(0.018)	(0.018)
N	2,500	2,500	2,500	2,500	2,500	2,500
Panel B: 40 Largest CZs						
HS funding per AEC	-0.044***	-0.044***	-0.044***	-0.044***	-0.044***	-0.042***
× (3- and 4-year-olds)	(0.014)	(0.014)	(0.014)	(0.015)	(0.014)	(0.014)
HS funding per AEC	0.035**	0.027	0.021	0.021	0.021	0.027
	(0.017)	(0.017)	(0.017)	(0.018)	(0.018)	(0.018)
N	2,000	2,000	2,000	2,000	2,000	2,000
Panel C: 30 Largest CZs						
HS funding per AEC	-0.046***	-0.046***	-0.048***	-0.048***	-0.048***	-0.047***
× (3- and 4-year-olds)	(0.015)	(0.015)	(0.015)	(0.017)	(0.017)	(0.017)
HS funding per AEC	0.042**	0.034*	0.021	0.021	0.021	0.03
	(0.018)	(0.018)	(0.018)	(0.021)	(0.019)	(0.019)
N	1,500	1,500	1,500	1,500	1,500	1,500
CZ FEs	X	X	X	X	X	X
Year FEs	X	X	X	X	X	X
Economic Controls		X	X	X	X	X
Demographic Controls			X	X	X	X
CZ Clustered SE				X		
Age-CZ PCSE					X	X
Age-CZ-specific AR(1)						X

Sources: CFFR, 1983-2007; NCHS NVSS, 1983-2007; SEER, 1983-2007; BEA CAINC30, 1983-2007, BLS LAUS, 1983-2007.
Notes: Panel corrected standard errors (PCSE) adjust for heteroskedastic and contemporaneously correlated disturbances across panels (age-CZ). Estimated models that allow for age-CZ-specific first-order autocorrelation in the disturbances are achieved via Prais–Winsten regression. Standard errors in parentheses. Levels of statistical significance given by * $p < 0.1$, ** $p < 0.05$, and *** $p < 0.01$

In all the additional analysis that follows, I make use of the model specification given by column (5) of Tables 2.3 and 2.4. Appendix B Table B.2 reports the full model results of this specification, showing the coefficient estimates for all explanatory variables included in the model. As expected, the main effect for the 3- to 4-year-old age group is negative and large in absolute value. This is consistent with the fact that the risk of mortality falls as children age. Interestingly, real personal income per capita (thousands of 2020 $) is estimated to be positively associated with 0- to 4-year-old mortality. In the most significant instances, the estimates suggest that a $10,000 increase in personal income per capita is associated with 6 percent increase in 0- to 4-year-old mortality, all else equal. However, the exclusion of the unemployment rate from the model inflates the estimated effect of the per capita income variable. Similarly, the exclusion of income per capita inflates the estimated effect of the unemployment rate, which then becomes a statistically significant predictor of child mortality. Thus, it is likely that both income per capita and the unemployment rate are capturing the impact of macroeconomic fluctuations on child mortality, as discussed in the previous chapter of this dissertation.

Though the risk of mortality is known to be higher for male children (Costa, da Silva, & Victora, 2017), the percent of children that are female has no impact on child mortality at the population level. This is unsurprising since there is little to no variation in the sex-composition of the population across CZs, over time. Alternatively, the representations of non-white children in the population are significant predictors of child mortality. First, a larger representation of Black children in the population is associated with increased mortality rates for 1- to 4-year-olds, all else equal. This is consistent with previous studies as well was the fact that Black children are disproportionately represented among those in poverty, which increases the risk of child mortality (Nolan et al., 2016; Schwandt et al., 2021). Specifically, a 1 percentage point increase in the percent of Black children is associated with an approximate 1 percent increase in the mortality of 1- to 4-year-olds. Second, a larger representation of non-White, non-Black children in the population is associated with a decrease in mortality for 1- to 4-year-olds, all else equal. Given that the relative mortality of Native American children behaves more similarly to that of

Black children, this is likely to be driven by Hispanic and Asian/Pacific Islander children who make up the majority of the non-White, non-Black group and are known to have a decreased risk of mortality, even compared to White children.[24] A 1 percentage point increase in the percent of non-White, non-Black children is associated with an approximate 3 percent decrease in the mortality of 0- to 4-year-olds.

In Appendix B Figure B.5, I plot the Eq. 2.1 difference estimate ($\hat{\beta}_1$) as I sequentially subset the sample to increasing large CZs. Similar to the estimates presented in the tables above, the estimated mortality benefits appear to increase as I subset to increasingly large CZs. All estimates are statistically different from zero at the 95% confidence level. I should also note that the qualitative nature of the results presented above obtain when expanding the sample to include the largest 75 or 100 CZs, as previously defined. To estimate the Eq. 2.1 models for this group of CZs, however, requires an adjustment of death counts by adding a small constant to extinguish the presence of zeros, which is likely to be problematic. Further, the magnitude and precision of the estimated model coefficients are sensitive to the size of the of adjustment—e.g., 0.01 versus 0.1.

2.4.3 Sensitivity Tests and Robustness

Next, I verify that that the estimated effects of Head Start funding on child mortality are robust to accounting for concurrent establishment and expansions in state-run preschool programs, the addition of 0-year-olds to the age-ineligible group, the addition of 5-year-olds to the age-eligible group, as well as testing for cause-specific effects of Head Start on mortality. As shown in Appendix B Table B.3, many of the states represented in my sample had established state-run preschool programs or funding initiatives by 2003, with 6 states having these initiatives in place prior to 1983, the beginning of the sample period (Barnett et al., 2003).[25] Despite this, Head Start

[24]The fact that Hispanic children have a decreased risk of mortality compared to White children despite their lower socioeconomic status is well-known. This phenomenon also obtains across the age distribution and has been termed the Hispanic, or Latino, paradox (Markides & Coreil, 1986; Markides & Eschbach, 2005).

[25]Nearly all of these programs, like Head Start, target poor, at-risk children.

continued to be the primary provider of public preschool programming for 3-year-olds. This is because the majority of state-run programs target 4-year-olds, as is evidence by the enrollment figures presented in Appendix B Table B.3. This creates a potential omitted variable bias if the establishment and expansion of these programs were coincident with the expansion of Head Start programming over the same period. Unfortunately, the state-run preschool enrollment data presented in Appendix B Table B.3 are only available beginning in 2003, otherwise I could include them time-varying state-level control in my model.

Table 2.5: Robustness of Mortality Effects when Accounting for State Preschool Programs

	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Log-Log Specification						
Ln(HS funding per AEC)	-0.041**	-0.041**	-0.052**	-0.052**	-0.061**	-0.063**
× (3- and 4-year-olds)	(0.017)	(0.017)	(0.022)	(0.022)	(0.027)	(0.028)
Ln(HS funding per AEC)	0.016	-0.057	0.036	-0.008	0.091*	-0.024
	(0.031)	(0.046)	(0.040)	(0.049)	(0.047)	(0.077)
Panel B: Log-Linear Specification						
HS funding per AEC	-0.033**	-0.033**	-0.033*	-0.033*	-0.039	-0.041*
× (3- and 4-year-olds)	(0.014)	(0.015)	(0.018)	(0.018)	(0.024)	(0.025)
HS funding per AEC	0.018	0.014	0.013	0.030	0.046	0.064
	(0.020)	(0.028)	(0.023)	(0.027)	(0.029)	(0.065)
CZs	45	45	29	29	23	23
N	2,250	2,250	1,450	1,450	1,150	1,150
CZ FEs	X	X	X	X	X	X
Year FEs	X		X		X	
State-Year FEs		X		X		X
Controls	X	X	X	X	X	X
Age-CZ PCSE	X	X	X	X	X	X
Table B.3 Column (1) Restriction	X	X				
Table B.3 Column (2) Restriction			X	X	X	X
Drop California CZs					X	X

Source: CFFR, 1983-2007; NCHS NVSS, 1983-2007; SEER, 1983-2007; BEA CAINC30, 1983-2007, BLS LAUS, 1983-2007.
Notes: Panel corrected standard errors (PCSE) adjust for heteroskedastic and contemporaneously correlated disturbances across panels (age-CZ). Standard errors in parentheses. Levels of statistical significance given by * $p < 0.1$, ** $p < 0.05$, and *** $p < 0.01$.

Given that Head Start programs were the primary public preschool provider for the 3-year-old age group, even by 2003, I am less concerned with this impact of state-run initiatives on this age group. Instead, it is the 4-year-old age group that presents potential issues. However, recall the previously discussed age-related issues in the mortality data. It is likely that, given the definition of age in these data, there are a non-trivial number of age-ineligible deaths among the 4-year-old age group. Not age-ineligible with respect to Head Start, but age-ineligible with respect to the overwhelming majority of state-run preschool programs over the period since many of these programs, like Head Start, utilize the age-requirements defined local public schools. Thus, only a fraction of the 3- to 4-year-old age group in the mortality data were likely to be age-eligible for state-run preschool initiatives over the sample period.

Regardless, I take two measures to test the sensitivity of my results to accounting for state-run preschool initiatives. First, I perform a series of sample restrictions that remove CZs located in states with significant state-run programs.[26] The states included in these sample restrictions are shown in columns (1) and (2) of Appendix B Table B.3. Note that in the most restrictive subsample of states, given by column (2), Head Start continued to be the primary provider of public preschool by 2003. I also exclude all California CZs as an additional test. Second, and in addition to these sample restrictions, I test the inclusion of state by year fixed effects in place general year fixed effects, in an attempt to capture the potential effects state-level program expansions over the sample period.

The results of the exercise described above is presented in Table 2.5. In general, the estimated mortality benefits of Head Start are robust to the state-run preschool program restrictions as well as the inclusion of state by year fixed effects. In fact, the difference estimate appears to increase as the subsample is increasingly reduced. The standard errors of the estimates in both

[26] I remove a CZ from the sample only if the largest city in labor market area is also in the state of interest. The exception being CZ 11304 which lists Arlington, VA as the largest city while the labor market area is inclusive of Washington D.C., which has had significant preschool programming for 3- and 4-year-olds since the 1960s. This CZ is excluded in all sample restrictions.

the log-log and log-linear specification, however, become increasingly inflated, as is expected given the daringly small sample size in columns (5) and (6).

Next, I test the addition of 0-year-olds into the group age-ineligible children. These results are presented in Table 2.6. Relative to the mortality of 0- to 2-year-olds, the estimated mortality benefits are quite large. In fact, relative to the estimates presented in Tables 2.3 and 2.4, they are double in size. The 0-year-old age group is a slightly less plausible group for comparison, however, since this age group includes very young infants whose characteristics and risk of mortality are more different from 1- to 2-year-olds than 1- to 2-year-olds are from 3- to 4-year-olds. In any case, the results are robust to, if not strengthened, upon the inclusion of this age group.

At the other end of the age spectrum, I include 5-year-olds into the age-eligible group of 3- and 4-year-olds. Similar to 3-year-olds, there are likely to be a nontrivial number age-ineligible 5-year-old deaths in the mortality data—i.e., children that are enrolled in kindergarten. Further, many in this age group were likely age-eligible for state-run preschool programs targeted at 4-year-olds. Another potential issue is the fact that, over the same period, there was a reversal in the share of kindergarteners enrolled in half-day versus full-day kindergarten.[27] Despite these issues, which motivated the exclusion of the age group in the main analyses, I re-estimate the Eq. 2.1 models with 5-year-olds included in the age-eligible group of children.

The results are presented in Table 2.7 where, similar to the main analysis, 0-year-olds have once again been excluded from the age-ineligible reference group. The estimated mortality benefits of Head Start are slightly smaller compared to the main results for 3- to 4-year-olds presented in Tables 2.3 and 2.4. The attenuation of the estimated effects is unsurprising considering that, compared to 3- and 4-year-olds, this group is the most likely to be age-treated by both state-run preschool programs and public schooling and, therefore, less affected by expansions in Head Start funding. Though not reported, I also perform the state-level restrictions and include state

[27]See data aggregated from the Child Trends Data Bank at `https://www.newamerica.org/education-policy/edcentral/fulldayk/`.

Table 2.6: Robustness of Mortality Effects to the Inclusion of 0-year-olds Among Age-Ineligible

	(1)	(2)	(3)
Panel A: Log-Log Specification			
Ln(HS funding per AEC)	-0.083***	-0.091***	-0.124***
× (3- and 4-year-olds)	(0.023)	(0.022)	(0.023)
Ln(HS funding per AEC)	0.070***	0.066**	0.095***
	(0.025)	(0.027)	(0.030)
Panel B: Log-Linear Specification			
HS funding per AEC	-0.063***	-0.075***	-0.103***
× (3- and 4-year-olds)	(0.019)	(0.018)	(0.018)
HS funding per AEC	0.041**	0.037**	0.051***
	(0.016)	(0.016)	(0.019)
CZs	50	40	30
N	2,250	2,250	1,450
CZ FEs	X	X	X
Year FEs	X	X	X
Economic Controls	X	X	X
Demographic Controls	X	X	X
Age-CZ PCSE	X	X	X

Sources: CFFR, 1983-2007; NCHS NVSS, 1983-2007; SEER, 1983-2007; BEA CAINC30, 1983-2007, BLS LAUS, 1983-2007.
Notes: Panel corrected standard errors (PCSE) adjust for heteroskedastic and contemporaneously correlated disturbances across panels (age-CZ). Standard errors in parentheses. Levels of statistical significance given by * $p < 0.1$, ** $p < 0.05$, and *** $p < 0.01$.

by year fixed effects as described above, but under the 1- to 2-year-old and 3- to 5-year-old age grouping. I find that the results are robust to these tests. Lastly, while the estimated effects under this age grouping are slightly smaller in percentage terms, it is important to note that they are associated with a 50 percent larger population and, therefore, are more economically meaningful when tabulating the implied mortality benefits.

The last test performed in this section is the disaggregation of all-cause mortality into non-external and external causes of death. External causes of death include intentional and unintentional injury, poisoning, and complication of medical or surgical care. Over the period 1983 to 2007, non-external causes account for roughly 63 percent of all instances of death among 1- to 4-year-olds, while external causes account for 37 percent. Further, external causes of death

Table 2.7: Robustness of Mortality Effects to the Inclusion of 5-year-olds Among Age-Eligible

	(1)	(2)	(3)
Panel A: Log-Log Specification			
Ln(HS funding per AEC)	-0.035**	-0.048***	-0.044***
× (3- to 5-year-olds)	(0.016)	(0.013)	(0.016)
Ln(HS funding per AEC)	0.029	0.058**	0.065**
	(0.024)	(0.023)	(0.027)
Panel B: Log-Linear Specification			
HS funding per AEC	-0.027**	-0.036***	-0.035**
× (3- to 5-year-olds)	(0.013)	(0.012)	(0.014)
HS funding per AEC	0.019	0.02	0.022
	(0.016)	(0.016)	(0.016)
CZs	50	40	30
N	2,250	2,250	1,450
CZ FEs	X	X	X
Year FEs	X	X	X
Economic Controls	X	X	X
Demographic Controls	X	X	X
Age-CZ PCSE	X	X	X

Sources: CFFR, 1983-2007; NCHS NVSS, 1983-2007; SEER, 1983-2007; BEA CAINC30, 1983-2007, BLS LAUS, 1983-2007.
Notes: Panel corrected standard errors (PCSE) adjust for heteroskedastic and contemporaneously correlated disturbances across panels (age-CZ). Standard errors in parentheses. Levels of statistical significance given by * $p < 0.1$, ** $p < 0.05$, and *** $p < 0.01$.

account for a larger share of all-cause mortality among 3- to 4-year olds—42 percent—compared to the 1- to 2-year-old age group—32 percent. Head Start programming has the potential to impact both broad categories of death. Through its provision of health screenings, nutritious meals and snacks, and the linking of families to health benefits and health care providers, Head Start reduces the risk from non-external causes of death. Further, the Head Start program is committed to fostering parental involvement, positive parent-child interactions, ensures that families have access to stable, safe housing arrangements, and acts as a provider of high-quality child care for poor families that often rely on informal care arrangements. This enhances child safety and, therefore, has the potential to reduce the risk of death from external causes, such as unintentional injuries, which make up the majority of external deaths among the 1- to 4-year-olds.

Table 2.8: Impact of Head Start Funding on Cause-Specific Child Mortality

	Dependent Variable is *Ln* of Non-External Cause Mortality Rate			Dependent Variable is *Ln* of External Cause Mortality Rate		
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Log-Log Specification						
Ln(HS funding per AEC)	-0.028	-0.041**	-0.052***	-0.081**	-0.063*	-0.045
× (3- and 4-year-olds)	(0.020)	(0.020)	(0.020)	(0.041)	(0.036)	(0.044)
Ln(HS funding per AEC)	0.047*	0.024	0.027	0.009	-0.007	-0.005
	(0.025)	(0.021)	(0.022)	(0.053)	(0.054)	(0.067)
Panel B: Log-Linear Specification						
HS funding per AEC	-0.029	-0.043*	-0.045*	-0.096**	-0.071*	-0.059
× (3- and 4-year-olds)	(0.023)	(0.023)	(0.023)	(0.048)	(0.038)	(0.045)
HS funding per AEC	0.043	0.048*	0.066*	-0.045	-0.013	-0.013
	(0.035)	(0.029)	(0.035)	(0.075)	(0.069)	(0.090)
CZs	50	40	30	50	40	30
N	2,500	2,000	1,500	2,500	2,000	1,500
Zero Death Counts	2	0	0	20	9	5
CZ FEs	X	X	X	X	X	X
Year FEs	X	X	X	X	X	X
Economic Controls	X	X	X	X	X	X
Demographic Controls	X	X	X	X	X	X
Age-CZ PCSE	X	X	X	X	X	X

Source: CFFR, 1983-2007; NCHS NVSS, 1983-2007; SEER, 1983-2007; BEA CAINC30, 1983-2007, BLS LAUS, 1983-2007.
Notes: Panel corrected standard errors (PCSE) adjust for heteroskedastic and contemporaneously correlated disturbances across panels (age-CZ). Standard errors in parentheses. Levels of statistical significance given by * $p < 0.1$, ** $p < 0.05$, and *** $p < 0.01$.

Now, Head Start programs have no control over instances of natural disaster or the misadministration of medical services. Further, there are likely to be a number of other causes of death that, with respect to Head Start services, are causally distant. Over the sample period, however, there was a transition from the International Classification of Disease (ICD) 9th Revision to the ICD 10th Revision in 1999, which makes it difficult to locate similar specific causes of death in the pre-1999 and post-1999 period. Thus, I use the broad categories of all non-external and external causes of death.[28] Even under this broad categorization, however, there appears to be a small "classification-shift" among these cause-specific measures of mortality in 1999. With this in mind, I include an age-specific classification indicator in my models to account for the potential age-specific impact of the classification shift on the model estimates. The disaggregation of the mortality data into non-external and external causes also reintroduces the problem of zero death counts—2 zeros among non-external cause death counts and 20 zeros among external cause death counts. To maintain the use of the Eq. 2.1 model that has been employed thus far, I adjust all death counts by a small constant—e.g., 0.01—that allows for the log transformation of all mortality rates and,, therefore, the use of log-log and log-linear mortality regressions.

The results, which are presented in Table 2.8, should be interpreted with caution. Despite the inclusion of age-specific indicators for the pre- and post-ICD transition, the slight inconsistency of the non-external and external cause of death classification across the two periods is likely to impact the model estimates. Further, the magnitude and precision of the estimates are sensitive to the size of the constant used in the adjustment of death counts—e.g., 0.01 versus 0.1. In any case, the qualitative nature of the results suggest that, relative to the 1- to 2-year-old age group, increases in Head Start funding per AEC are associated with reductions in 3- to 4-year-old mortality among both non-external and external causes of death, as hypothesized. One potential explanation for the diminishing impact of Head Start on external causes of death in increasingly populous CZs is the role of motor vehicle accidents and deaths. Motor vehicle related deaths make up a substantial portion of external death among children. This is particularly true among children in less population dense areas, who more likely to spend time in vehicles and, therefore, are more likely to die in motor vehicle crashes (Kmet & Macarthur, 2006; West, Rudd, Sauber-Schatz, & Ballesteros, 2021). Head Start, acting as a provider of formal child care for families that often rely on informal care arrangements, potentially reduces the time that children spend

[28] External causes of death are those identified by codes E800-E999 in the ICD 9th Revision and codes V01–Y89 in the ICD 10th Revision.

commuting in vehicles throughout the day. In so far as this obtains, the effects should be largest in less populous CZs where children are more likely to spend time in vehicles—e.g., column (4)—and smaller in more populous areas where children spend less time in vehicles—e.g., column (6).

2.4.4 Heterogeneous Impacts in Poverty-Eligible, At-Risk Communities

In this final section, I test for heterogeneous impacts of Head Start on child mortality across relatively poverty-eligible and at-risk communities. This analysis is motivated by two facts concerning Head Start enrollments. First, children must be poverty-eligible to be enrolled in Head Start programs. Thus, the ability to detect the potential contemporaneous mortality benefits of Head Start at the population level should increase when focusing on communities with a larger share of poverty-eligible children—see Table 2.2. Second, given their overrepresentation among the poor (Nolan et al., 2016), Black children are disproportionately represented in Head Start enrollments (Early Childhood Learning and Knowledge Center, 2004). Beyond these facts of enrollment, it is also the case that poor children face an increased risk of mortality (Aber, Bennett, Conley, & Li, 1997) and, even when holding poverty constant, Black children face a higher risk of death relative to their White counterparts (Schwandt et al., 2021).

First, I subset the CZs in my sample into relatively low- and high-poverty groups based on the the average share of 0- to 4-year-olds in poverty over the 1989-2007 period. I then estimate the Eq. 2.1 model separately for these two groups and compare the estimated coefficient of interest (β_1). Second, I subset the CZs in my sample into two groups based on the relative representation of Black children in the population. I similarly estimate the Eq. 2.1 model among CZs with relatively low and high representations of Black children in the population and compare the estimated impact of Head Start funding on the mortality of 3- and 4-year-olds across these groups.[29]

[29] Another way to get at heterogeneous impacts in communities of color would be to partition the mortality data by race. However, the limited representation of Black persons in the total population and, therefore, mortality data, results in death counts that are overinflated with zeros, even in the largest CZs.

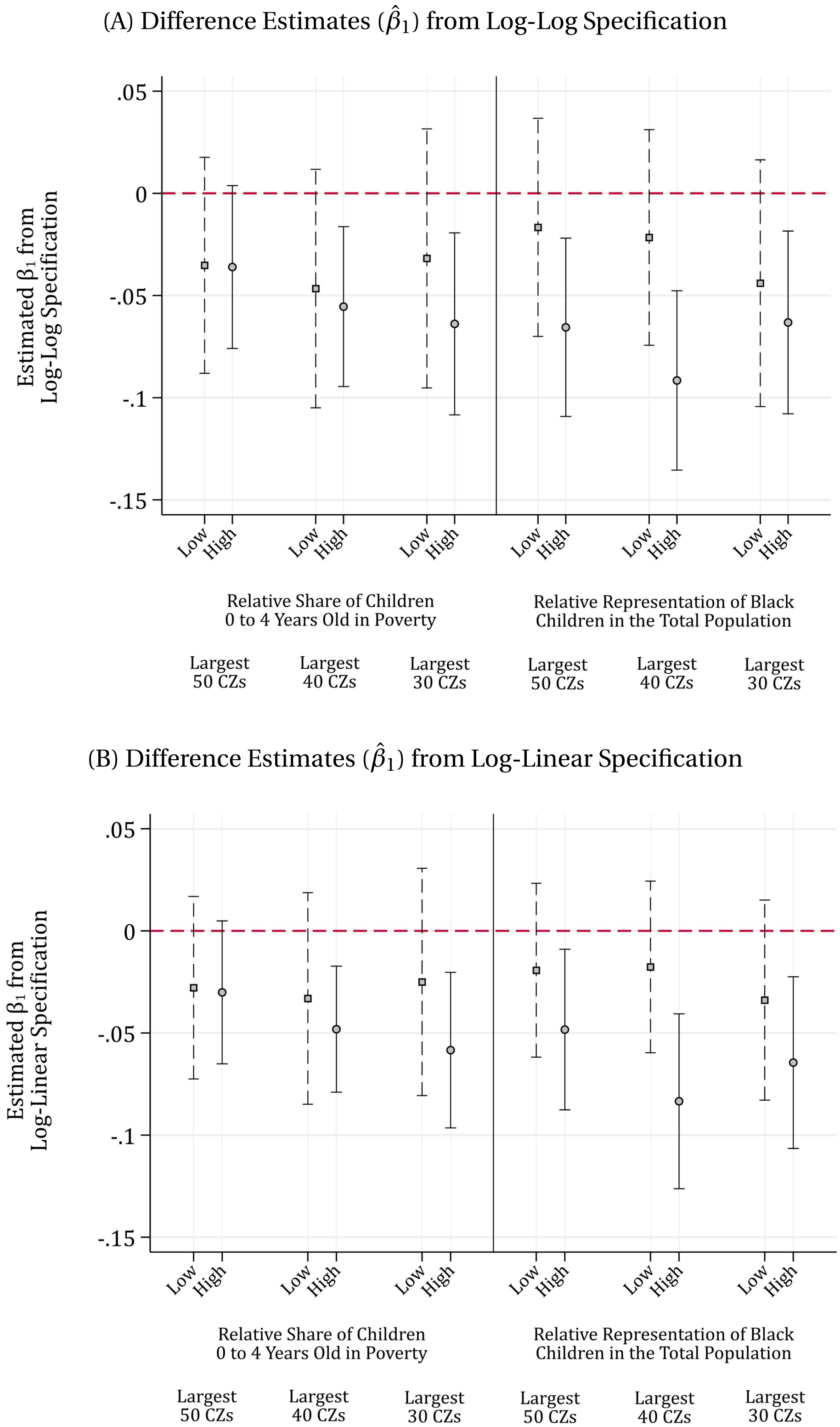

Figure 2.2: Heterogeneous Mortality Effects in Relatively Poverty-Eligible and At-Risk CZs

The results of this exercise are presented graphically in Figure 2.2. Plotted is the Eq. 2.1 estimated coefficient of interest ($\hat{\beta}_1$), which is the estimated impact of real Head Start funding per AEC on 3- to 4-year-old mortality, relative to the 1- to 2-year-old age group. Panels A and B report the results under the log-log and log-linear specification, respectively. As hypothesized, the general finding is that the estimated impact of Head Start on the mortality of age-eligible children is more pronounced in high-poverty and disproportionately Black communities. The difference estimate is negative in all tests. However, in all but two instances, the estimated impact of Head Start funding on 3- to 4-year-old mortality increases in absolute value and is more precisely estimated—i.e., statistically different from zero at the 95% confidence level—in CZs with (1) a larger share of children in poverty and (2) relatively large populations of Black children. The intuitive behavior of the estimates in these tests supply increased confidence in all of the results previously discussed.

2.5 Discussion and Conclusion

In this chapter, I utilize variation in program expansion across local labor market areas and age-eligibility requirements to investigate the potential contemporaneous mortality benefits of Head Start among age-eligible children. I employ a sample of 50 large CZs that account for roughly over half of the 0- to 4-year-old population in the U.S. over the 1983-2007 sample period. These CZs also account for roughly half of Head Start appropriations over the same period. As a first step, I verify the use of Head Start funding as a proxy for enrollments by showing that, at the state level, funding and enrollments are positively linked, all else equal. The funding-enrollment relationship is particularly strong in high-poverty states, which is reassuring since children must be poverty-eligible to participate in Head Start programs. Next, I estimate the impact Head Start funding per age-eligible child (AEC) on child mortality and find that, relative to 1- and 2-year-olds, a $500 increase in per capita Head Start funding is associated with a 3.3 to 4.8 percent reduction in the mortality of 3- and 4-year-olds. This result is robust to accounting for the contemporaneous establishment and potential expansion of state-run preschool programs, the inclusion of 0-year-olds and 5-year-olds in the definition of age-ineligible and age-eligible children, and the use of broadly defined cause-specific measures of mortality. Finally, I show that estimated mortality benefits of Head Start are intuitively pronounced in program-targeted and high-mortality risk communities—e.g., relatively poor and disproportionately Black labor market areas.

The results of this study have important implications. First, Head Start not only provides a wide range of long-term benefits for children later in life, but it provides immediate protection from the most severe outcome a child can face: death. This finding updates and extends of Ludwig and Miller (2007) who found that Head Start programming has downstream mortality-reducing effects among 5- to 9-year-olds. Second, given that Head Start acts as a *de facto* investment into the health and safety of disadvantaged children, these results suggest that disparities in child mortality across socioeconomic groups may be attenuated by increasing public investment into families and children. Moreover, and in agreement with the findings of the previous chapter, a lack of public investment into the care of pre-primary aged children may be a potential source of relatively high child mortality rates in the U.S. relative to other similarly developed economies. Finally, nested in the ambitious Build Back Better Act put forward by the current administration is a dramatic overhaul of child care in the United States. The legislation outlines the rollout of universal preschool for 3- and 4-year-olds, the largest expansion of universal and free education since the establishment of public high school 100 years ago. The current administration estimates that the universal preschool program will expand access to free preschool for more than 6 million children per year. While it is difficult, and potentially problematic, to extrapolate the results of this paper to the U.S. more generally, the qualitative nature of the results in this paper suggest such a policy has the potential to reduce the risk of mortality among age-targeted children.

Chapter 3

Revisiting the Wages of Virtue and the Relative Pay of Care Work

3.1 Introduction

In this chapter, I undertake an investigation into relative pay of care work. That is, holding all else constant, do workers receive a wage penalty for working in care-intense occupations? Care work, or caring labor, is an umbrella term that includes both *nurturant care* and *reproductive care* work—also commonly referred to as *direct care* and *indirect care*, respectively.[30] Nurturant care occupations have been described as jobs in which workers "provide a face-to-face service that develops the human capabilities of the recipient" (England, Budig, & Folbre, 2002, p. 455). Here, human capabilities, or what some term "human capacities", are broadly defined to include physical and mental health, physical and cognitive skills, as well as non-cognitive and emotional skills such as self-confidence, self-discipline, empathy, and care (Braunstein, Van Staveren, & Tavani, 2011; England et al., 2002). In some sense, all work can be considered nurturant care, at least indirectly, through its promotion of human welfare. However, the key distinction is that nurturant care work involves close personal or emotional interaction between the care worker and those being cared for (Braunstein et al., 2011; Folbre, 2001, 2006). Generally, nurturant care occupations—which include child care providers, nursing assistants, classroom teachers, midwives, family physicians, psychiatrists, and so on—have high levels professionalization and human capital. Research in feminist and labor economics provides several theoretical rationale as to why workers in nurturant care occupations might receive lower wages—i.e., a *care penalty*.

[30] Duffy (2005) uses the term *reproductive labor* to encapsulate all forms of care work, and then distinguishes between *nurturant reproductive labor* and *nonnurturant reproductive labor*. Instead, I employ the the terms *nurturant care* and *reproductive care*, similar to Budig, Hodges, and England (2019).

The notion of reproductive care, which has roots in feminist conceptions of domestic labor and social reproduction, includes labor activities that have been traditionally performed in the household, but have been increasingly marketized over the last decade (Duffy, 2005, 2007). Duffy (2005) defines reproductive labor as “work that is necessary to ensure the daily maintenance and ongoing reproduction of the labor force" (p. 70). Reproductive care occupations— which include cooks, food preparation workers, waiters and waitresses, barbers and hairdressers, and launderers—have lower levels of professionalization and human capital relative to nurturant care occupations, generally (Budig et al., 2019; Duffy, 2005). Historically, workers in reproductive care occupations are disproportionately Black and Hispanic women (Duffy, 2007; Glenn, 1992). Relative to nurturant care work, reproductive care occupations require less direct, intimate interaction between the care worker and care recipient. Despite the differences between the two conceptualizations of care work, however, both provide “essential services for human health, development, and maintenance" (Budig et al., 2019, p. 295).

The question of whether workers nurturant and reproductive care occupations experience a wage penalty is important for a number of reasons. First, women are disproportionately represented in caring occupations (Budig et al., 2019; England et al., 2002). Thus, the existence of wage penalties for caring labor has implications for gender equity in labor market outcomes (F. Blau & Kahn, 2017). Second, while care work has been historically performed (unpaid) by women within the household, the role of paid care work has increased in recent decades with the expansion of women’s labor force participation and the accompanying need for child care, a shift in population demographics that has increased demand for elderly care, and a general increase in the use of markets to provide goods and services (Duffy, 2007; Folbre & Nelson, 2000; Goldin, 2006; Mather, Jacobsen, & Pollard, 2015). Such a shift, accompanied by wage penalties in caring occupations, has the potential to contribute to increasing polarization in the U.S. labor market (David & Dorn, 2013; David, Katz, & Kearney, 2006; Dwyer, 2013). Third, the existence of a care penalty has implications for the quality of market care substitutes. In so far as (1) low wages induce higher rates of job turnover, all else equal (Dale-Olsen, 2006), and (2) relatively low paying jobs attract relatively less productive workers with low earning potential, then wage penalties for caring labor may have deleterious impact on the quality of market care substitutes and, consequently, long-term economic growth via changes in human capacities investments (Braunstein, Bouhia, & Seguino, 2020; Braunstein, Seguino, & Altringer, 2021; Braunstein et al., 2011; Folbre & Nelson, 2000). With these considerations in mind, the Build Back Better legislation (H.R. 5376) currently being debated in Congress includes initiatives to provide competitive wages and benefits for direct care workers.

This study is not the first to investigate the impact of care work on wages. England et al. (2002) and Budig et al. (2019) use the National Longitudinal Survey of Youth 1979, and a categorical coding of occupations into care and non-care work, to estimate the care penalty via standard fixed-effects semi-log earnings functions. Hirsch and Manzella (2015) use cross-sectional and longitudinal data from the Current Population Survey, along with a continuous measure of care work derived from the 2007 Occupational Information Network (O*NET), to investigate the care penalty. In this chapter, I seek to build upon and update the work of England et al. (2002), Hirsch and Manzella (2015), and Budig et al. (2019) by using the National Longitudinal Survey of Youth 1997 (NLSY97, Waves: 1997-2017) to estimate the impact of care work on wages. I employ both a categorical definition of care work, similar to those of England et al. (2002) and Budig et al. (2019), and a continuous measure of care work derived from the 2019 O*NET 25.1 Database, similar to the approach of Hirsch and Manzella (2015). Using these measures, I show the continued existence of wage penalties among nurturant care occupations, while there appears to be no wage penalty for workers in reproductive care occupations, all else equal. Testing for heterogeneous care penalties across the occupational skill distribution, I find that the wage penalty for nurturant care work increases in relatively high-skill occupations among men. Alternatively, the wage penalty for nurturant care work is null, if not a slight wage premium, in relatively high-skill occupations among women. I explore potential explanations for the inconsistent behavior of these estimated care penalties across gender, such as occupational crowding and selection via occupational segregation, or sorting.

The rest of the chapter is organized as follows. Section (3.2) provides a discussion of theoretical rationale for the presence of a care penalty. Section (3.3) provides an overview studies that have previously investigated the pay penalty of care work. Section (3.4) describes the data and empirical model employed to investigate the relative pay of care work, while Section (3.5) presents and discusses the empirical results. Section (3.6) concludes.

3.2 Why a Care Penalty?

Research in feminist and labor economics provides several theoretical rationale as to why workers in caring occupations might receive lower wages—i.e., a *care penalty*. These theories can be placed within three broad frameworks—*compensating differentials*, *discrimination*, and *market frictions and failures*. Most of the theoretical rationale presented here pertain to nurturant care work, though some may also apply to reproductive care. At the end of this section I provide a discussion of reproductive care and how these theoretical frameworks may or may not apply.

Beginning with the standard neoclassical framework, the labor theory of *compensating differentials*—which has a deep history going back to Adam Smith's foundational *An Inquiry into the Nature and Causes of the Wealth of Nations*—emphasizes the role that job amenities and disamenities have on wage determination. Since markets tend toward equilibrium and labor is compensated their marginal product, the only way for relative wage differences to arise among similarly productive workers is if there some form of non-monetary remuneration that compensates for this relative difference. Thus the term, compensating differentials. In this framework, relative wage differences arise from the exogenous individual preferences of the marginal worker that either (1) is willing to accept a lower wage for the utility-enhancing properties of a job or (2) requires a higher wage for the utility-demoting properties of a job. Holding worker productivity constant, jobs that provide amenities, such as the deeply human satisfaction derived from assisting and caring for others, will have relatively low levels of monetary compensation. Thus, the care worker that is willing to accept a lower wage for the intrinsic properties of caring labor is monetarily penalized for their altruism. However, England et al. (2002) argue that every job self-selects workers who find the work performed to be enjoyable, fulfilling, or, at least, less onerous. In other words, every job provides some sort of compensating differential. Further, the “catch-all" theory of compensating differentials, which emphasizes the potential altruistic motivations of care workers, ignores several other characteristics of care work that are likely sources of relatively low wages.

Within the *discrimination* framework, the *devaluation hypothesis* posits that cultural ideas deprecate women and, since women are disproportionately represented in caring occupations, care work is devalued by cognitive association (England, 2005; Levanon, England, & Allison, 2009). However, England et al. (2002) add that, even after accounting for the sex composition of occupations, care penalties may persist since caring labor requires nurturant skills—e.g., patience, empathy, and concern—that might be considered as more “natural" rather than arduously acquired skills and, therefore, these skill may go unnoticed or uncompensated. Further, it may be that the nurturant and accommodating skills of care workers do not go unnoticed, but instead they are taken advantage of to suppress wages—an intentional as opposed to passive form of discrimination. Another hypothesis within the *discrimination* framework stems from dichotomy produced by Western thought that one either works for love *or* money, but not both, since the commodification of care crowds out genuine passion and makes the sacred profane (England & Folbre, 1999; Folbre & Nelson, 2000; Nelson, 1999). This requires that care workers prove their proper motivation by accepting a wage penalty. The irony, as England et al. (2002) note, is that society bestows merit on caring labor while, at the same time, withholding very means through which merit is typically rewarded.[31].

Another theory that can be grouped into the discrimination framework is that of *occupational crowding*. Originally developed by Bergmann (1971) as pertaining to the discrimination of Black individuals in employment, the theory quickly applies to the crowding of women into

[31] On might argue that this is a special case of compensating differentials. As Adam Smith notes in his foundational *An Inquiry into the Nature and Causes of the Wealth of Nations*, “Honour makes a great part of the reward of all honourable professions. In point of pecuniary gain, all things considered, they are generally under-recompensed..." (Book 1, Ch. 10). The question then becomes, is this outcome efficient, equitable, and, therefore, desirable?

feminine occupations (Bergmann, 1974; Levanon et al., 2009). In the context of care, the theory of occupational crowding posits that women are crowded into occupations associated with women's traditional roles—i.e., care work. This crowding is driven both on the supply side through gendered preferences and the balancing of marriage-labor market costs, as well as on the demand side via employer and/or customer preferences—i.e., discrimination (Lee Badgett & Folbre, 2003). The result is an oversupply of labor , particularly women's labor, into care occupations which has consequences for wages.

Within the *market frictions and failures* framework, the nature of care work and its product, as well as particularities of market transactions for care work, introduce frictions that cause care work to be under-compensated relative to similar, but less care-intense, forms of employment. First, some scholars propose that care work has more indirect social benefits than other types of work (England, 2005; Folbre, 1994). In economics speak, caring labor creates important externalities that cannot always be captured by those who created them in individual transactions (Folbre & Nelson, 2000).[32] For instance, care investments in children not only benefit children themselves, but also the society at large. In standard economic theory, goods and services that provide profuse social benefits are undervalued and, consequently, so is their labor. Further, the full extent of the individual-level product of caring labor is often unrealized in the present. For instance, parents do not recommend a daycare center or a particular primary school teacher to their associates based on the unrealized life outcomes of their children. This has the potential generate *market for lemons* dynamics where quality care is not entirely eliminated from the market, but the willingness of persons to pay for care is limited by imperfect information regarding the unrealized, future product caring labor (G. Akerlof, 1970). In other market care sectors, such as healthcare, the full extent of the marginal product of caring labor is more tangible in the present.

[32] In some sense, all forms of labor produce externalities that are difficult to capture in individual transactions. The argument that caring labor produces relatively high amounts of public goods stems from the notion that care work involves a high level of investment in human capabilities relative to other types of work in which products are more readily consumed (England, 2005).

Another hypothesis within the *market frictions and failures* framework emphasizes the economic dependence of care recipients. Children, person with disabilities, the elderly, and those suffering from physical and mental illness often need care when they are most unable to work to pay for it (Danziger & Seefeldt, 2003; Li & Dalaker, 2019; Stuart, 2006). Thus, care recipients are often income constrained and rely on payments via third parties—e.g., family members or the public. As noted by England et al. (2002), the compensation of care workers will then rely on the willingness and ability of these third parties to pay for the care required—i.e., the altruism and affluence of family members and/or society. Moreover, the willingness of family members and/or the society may be not only be influenced altruism but also by the difficulties associated with measuring the full product of caring labor, as discussed above.

The *prisoner of love* hypothesis is a notion that straddles the *compensating differentials* and *market frictions and failures* frameworks (England, 2005; Folbre, 2001). As stated previously, the *compensating differentials* framework posits that the care worker is willing to accept a lower wage to enjoy the intrinsic rewards that come from caring labor. In other words, they are a *prisoner of love*. However, Folbre (2001) adds a *market frictions and failures* wrinkle to the *prisoner of love* story by emphasizing the role that the nature of care work has on cultivating altruistic preferences. In other words, the altruistic preferences often associated with caring labor are endogenous to the work (England, 2005). Essentially, paid care workers become attached to care recipients after they start the job—e.g., child care workers who become attached to the children they care for, healthcare workers that empathize with their patients, counselors who worry about their clients, and human resource workers that develop concern for their co-workers. These emotional connections put care workers in a vulnerable position that may discourage them from (1) withholding their services to demand higher wages or (2) acting on more remunerative job opportunities (England, 2005).

All of these theoretical rationale apply to nurturant care. For example, the *market frictions and failures* and *prisoner of love* frameworks were conceptualized with nurturant care work specifically in mind (Duffy, 2005; Folbre, 2001; Folbre & Nelson, 2000). However, to what extent to these frameworks apply to reproductive care work? First, with respect to *compensating differentials*, it unclear how reproductive care workers—particularly those that provide less intimate, interactive care such as cleaners, food preparation workers, launderers, etc.—would derive any greater satisfaction from their jobs than non-care workers would. Thus, selection into care work via the intrinsic properties caring labor may less relevant within the reproductive care framework. The *devaluation hypothesis* and theory *occupational crowding*, however, are particularly relevant for reproductive care work. Like nurturant care, reproductive care work is historically linked to the gender division of labor within households. While reproductive care occupations are somewhat less feminized relative to nurturant care occupations, women are still disproportionately represented in reproductive care compared to non-care occupations (Budig et al., 2019). Further, Black and Hispanic women tend to be overrepresented in reproductive care jobs, which adds an intersectional layer to the *devaluation hypothesis* (Duffy, 2005, 2007). Lastly, since reproductive care occupations have lower levels of professionalization and human capital relative to nurturant care occupations, the level of occupational closure is less substantial. Thus, *occupational crowding* effects may be stronger in reproductive care occupations and a potential source of depressed wages in these jobs, all else equal.

3.3 Review of Prior Evidence

As I mentioned in the introduction, three prior studies have set out to identify the existence of a care penalty and expose potential mechanisms. The first to provide insight in this direction were England et al. (2002), who used the 1982-1993 waves of the National Longitudinal Survey of Youth 1979 (NLSY79) and a binary classification of occupations into care and non-care work to estimate the causal impact of care work on wages. The occupations coded as care work—i.e., those that met the operational definition of providing a face-to-face service that develops the human capabilities of the recipient—generally included those occupations within the healthcare and education industries, as well as child care workers, social workers, and clergy/religious workers. The working definition of care work employed in this study focused on nurturant care occupations.[33] To mitigate omitted variable bias, England et al. (2002) exploited the longitudinal nature of the NLSY79, estimating fixed-effects semi-log earnings functions—for women and men separately—that controlled a variety of wage-relevant, individual and job-specific characteristics, including the sex composition of an individual's occupation-industry cell. Average care penalties were found on the order 5–6 percent with substantial heterogeneity found within caring occupations. For instance, wage premiums were found to exist for a number of medical-related jobs including nurses and other non-doctor healthcare occupations.

One limitation of the England et al. (2002) study was the relatively young age of persons in the sample—the eldest were 35 in 1993, the final year of the sample. Recently, Budig et al. (2019) updated and extended the work of England et al. (2002) using the 1979-2012 waves of the NLSY79, where the eldest respondents were 54 in the final year of the sample (2012). Budig et al. (2019) employ a similar definition of care work used in this study, classifying occupations as either nurturant care work or reproductive care work, and estimate fixed-effects semi-log earnings functions similar to those employed by England et al. (2002). The baseline estimated wage penalties among nurturant care occupations in this extended sample of the NLSY79 were 14 percent among women and 11 percent among men—roughly 2–3 times higher than those found by England et al. (2002). Budig et al. (2019) show that the returns to labor market experience are significantly lower for college graduates in nurturant occupations relative to non-nurturant care occupations, which explains why the care penalties found by England et al. (2002), who employed a younger sample of the NLSY79, are much smaller than those found by Budig et al. (2019). The estimated wage penalties among reproductive care occupations, all else equal, were significantly smaller—1.4 and 5.5 percent for women and men, respectively. Additionally, Budig et al. (2019) test whether or not occupational closures—i.e., high education and high licensing

[33] England et al. (2002) also employed a broader classification of care work that included interactive service work such as retail sales and receptionist occupations. The care penalties estimated under this broader coding scheme were similar in size to the more restrictive definition of care work.

requirements—attenuate the wage penalty received among nurturant care workers.[34] Their estimates indicate that care penalties are reversed, reflecting wage premiums, in caring occupations with relatively high educational and licensing requirements. However, the estimated wage penalties in nurturant care occupations with the least amount of closure—i.e., low education and low licensing—were not as large as hypothesized.

Hirsch and Manzella (2015) were the first to employ a continuous measure of caring labor to estimate the impact of care work on wages. Specifically, the authors employ cross-sectional and longitudinal data from the Current Population Survey (CPS), along with continuous measures of care work derived from the 2007 Occupational Information Network (O*NET). The 2007 O*NET indicators used to measure the level of care across occupations were the "Assisting and Caring for Others" and "Concern for Others" indices, as well as a *caring index* that was integrative of the two. Pooling employed persons between the ages of 18–65 years across the 2006–2008 CPS samples, and estimating standard Mincerian semi-log earnings functions via OLS, Hirsch and Manzella (2015) find that a one standard deviation in the *caring index* is associated with a net 4 percent decrease in wages among women and 9 percent decrease among men. Then, employing longitudinal data for job switchers over the 2003/2004–2008/2009 period and estimating semi-log wage change equations, Hirsch and Manzella (2015) find much smaller wage penalties where a one standard deviation increase in the *caring index* is estimated to reduce wages by roughly 1.4 percent for women and 1.8 for men, all else constant. Since Hirsch and Manzella (2015) do not employ the binary classifications of care work used by England et al. (2002) and Budig et al. (2019), it is difficult to make comparisons regarding the magnitude of the care penalties estimated across the NLSY79 and CPS samples. In the analysis that follows, I show that the continuous measure of care derived by Hirsch and Manzella (2015) is more representative of the nurturant care framework and produces care penalty estimates that behave similarly to the categorical measure of nurturant care employed by England et al. (2002) and Budig et al. (2019).

[34]The authors also explore the role of other wage-equalizing institutions—e.g., public employment and unionization—in mediating the care penalty. The results are highly variable across women, men, and different types of caring labor.

3.4 Methods

3.4.1 Data and Measurement

To investigate the relative pay of care work, I employ data from the 1997-2017 waves of the National Longitudinal Survey of Youth 1997 (NLSY97). The NLSY97 is a nationally representative sample of 8,984 men and women born during the years 1980 through 1984 so that respondents were ages 13 to 17 years during the initial round of interviews (1997) and 33 to 37 years of age in the most recently observed round (2017).[35] Unlike England et al. (2002) and Budig et al. (2019), who derive person-year panels of employment data from the NLSY79, I retain all instances of employment for each individual using unique job identifiers in the NLSY97. This allows for multiple employment observations within years. After deletions for rows with missing data and reducing the sample to all individuals with two or more observations—to accommodate the fixed-effects methods described below—the final sample of analysis is composed 162,620 person-job-year observations spread across 8,708—4,250 women and 4,458 men—of the original 8,984 respondents. The average number of job-year observations per individual is approximately 19 while the average number of unique jobs held per individual is 9.

There are both advantages and disadvantages to these data. First, the NLSY97 provides an advantage over other longitudinal samples—such as the Current Population Survey Outgoing Rotation Group (CPS-ORG) monthly earning files employed by Hirsch and Manzella (2015)—since one can precisely observe the labor supply and employment history of individuals over the life course. Another advantage is the ability to observe all instances and histories of employment through unique job identifiers. A disadvantage of the sample is truncation with respect to age, similar to the limitations faced by England et al. (2002). As noted previously, since all individuals in the sample were born during the years 1980 through 1984, the eldest of respondents were 37 years of age in 2017. Evidence suggests that, all else equal, wage growth among nurturant care workers is lower than that for non-care workers (Budig et al., 2019). Thus, the relative wage

[35]Respondents of the NLSY97 were interviewed annually from 1997 to 2011 and biannually thereafter—e.g., 2013, 2015, and 2017.

differences identified in what follows, while accurate for workers under the age of 38, are likely understated for older workers.

The outcome variables of interest in all analyses are the natural log of real hourly wages and the natural log of real hourly compensation.[36] The latter is inclusive of overtime, tips, bonuses, and the like. Compared to previous studies, mine is the first to investigate care penalties using this more inclusive measure of compensation. This is likely to be important since there may be differences in pay across care and non-care occupations that are not captured exclusively by wage rates.[37] While all non-zero wage observations are included initially, I drop observations for which real hourly wages are reported to be below \$5 or above \$75 to test the sensitivity of the estimated care penalties to outliers. Further, the models that make use of hourly wage rates exclude bartenders and waiters/waitresses since the hourly wage rates in these occupations deviate substantially from their hourly compensation.[38]

The explanatory variable of interest is the care intensity of occupations. I begin my analysis similar to England et al. (2002) and Budig et al. (2019), using a categorical (dummy) measure where an occupation is assigned a value of 1 if it satisfies the operational definition of nurturant care—i.e., it requires giving a face-to-face service to a client or customer of the organization in which one is employed which increases the capabilities of this recipient—and 0 otherwise. Appendix C Table C.1 provides the complete list of occupations in the NLSY97 (2002 Census codes) that are coded as nurturant care work. Generally, these include: counselors, social, and religious workers; teachers and other education workers; healthcare professionals and support workers; child care workers; and personal/home care aides. Similarly, I employ a categorical (dummy) measure of reproductive care. My coding of occupations as reproductive care work—

[36] Reported hourly pay rates were converted to 2020 U.S. \$ using the Consumer Price Index. Yearly adjustment factors were obtained via the `cpigen` command in Stata.

[37] For instance, reproductive care occupations include Bartenders, Waiters and Waitresses, Barbers, Hairdressers, Hairstylists, and Cosmetologists. Workers in these occupations often receive "voluntary gifts", or tips, for their service (Lynn, 2016, 2021). Further, structural differences between care and non-care occupations may cause workers in care-intense occupations to receive little, to no pay bonuses.

[38] Hourly compensation in these occupations are more reflective of the hourly wage rate since a substantial portion of hourly earnings are derived from tips.

shown in Appendix C Table C.2—is similar to that of Budig et al. (2019) and includes bartenders, waiters and waitresses, food preparation workers, janitors and maids, personal appearance workers, as well as launderers, generally.[39] The categorical classification of occupations into nurturant and reproductive care work are exclusive, meaning there is no overlap between the two.

Next, I employ Occupational Information Network (O*NET Database 25.1) job descriptor data to derive a continuous measure of *care work* across occupations.[40] The O*NET data provide a plethora of ratings pertaining to the skills and tasks required within occupations as well as the environment of a worker within an occupation. The O*NET-derived *care index* employed in my analysis is integrative of two measures—similar to Hirsch and Manzella (2015). From the O*NET *Work Activities* file I collect ratings for the importance of the activity titled *Assisting and Caring for Others* (ElementID = 4.A.4.a.5). Additionally, the importance of *Concern for Others* (ElementID = 1.C.3.b) was drawn from the O*NET *Work Styles* file. These two measures are highly correlated across the 873 occupations in the O*NET database ($r = 0.775$, $p < 0.000$). Given their high correlation, I construct a *care index* that is integrative of both measures using Principle Component Analysis (Hotelling, 1933; **?**). The primary principle component accounts for approximately 89 percent of the overall variation between the two indicators, with each indicator being assigned equal weight in the generation of the *care index*. Using the minimum value and the range of the generated *care index*, I scale the index to range from zero to one to aid in the interpretation of my empirical estimates.

Now, the O*NET data correspond to 2018 SOC occupations and codes while the occupation data provided in the NLSY97 are coded according the the 2002 Census classification. I employ the crosswalk provided by the Integrated Public Use Microdata Series (IPUMS) to merge the

[39] The classification of occupations as reproductive care work employed by Budig et al. (2019) is largely derivative of that previously employed by Duffy (2005).

[40] The O*NET Database 25.1 was accessed at `https://www.onetcenter.org/database.html#individual-files`.

O*NET-derived *care index* to the NLSY97 sample (Ruggles et al., 2020).[41] While the majority of matching was straight forward, some occupations in the NLSY97 required inspection. For example, the 2002 Census occupation “Emergency Medical Technicians and Paramedics" (3400) should be matched to the 2018 SOC occupations "Emergency Medical Technicians" (292042) and "Paramedics" (292043). However, these 2018 SOC occupations are absent in the O*NET data. Instead the 2002 Census occupation “Emergency Medical Technicians and Paramedics" (3400) is matched to the 2018 SOC occupation "Firefighters" (332011). Similar discrepancies exist for 33 other occupations in the NLSY97 and a complete list of “hand-matched" occupations are presented in Appendix C Table C.3.

Further, some NLSY97 occupations map to multiple occupations in the O*NET data, and vice versa. The 2002 Census occupation “Physicians and Surgeons" (3060) provides a good example. In the O*NET data, Neurologists, Obstetricians, Pediatricians, Hospitalists, Urologists, and so on, are all provided as separate occupations. In this case, each detailed occupation in the 2018 SOC O*NET data is assigned to the single “Physicians and Surgeons" 2002 Census occupation. Several other instances like the one described above occur. Once the crosswalk is used to match 2002 Census occupations to 2018 SOC O*NET occupations and data, the set is collapsed on 2002 Census occupations using mean assignment. For example, “Physicians and Surgeons" are assigned the mean of O*NET indicators across Neurologists, Obstetricians, Pediatricians, Hospitalists, Urologists, and so on.

The density plot presented in Figure 3.1 shows the distribution of occupations in the NLSY97 sample—488 unique occupations—across the domain of the O*NET-derived *care index*. The height of the distribution is situated just to the left of center with some lumping at the higher end of the *care index* domain. The relationship between the previously discussed categorical care work indicators and the O*NET-derived *care index* is shown in the density plots presented in Figure 3.2. The sample-weighted distribution of non-care work occupations across to domain of the *care index* is presented by the dashed line (light grey), while the sample-weighted distribution

[41] The crosswalk can be accessed at `https://usa.ipums.org/usa/volii/occtooccsoc18.shtml`.

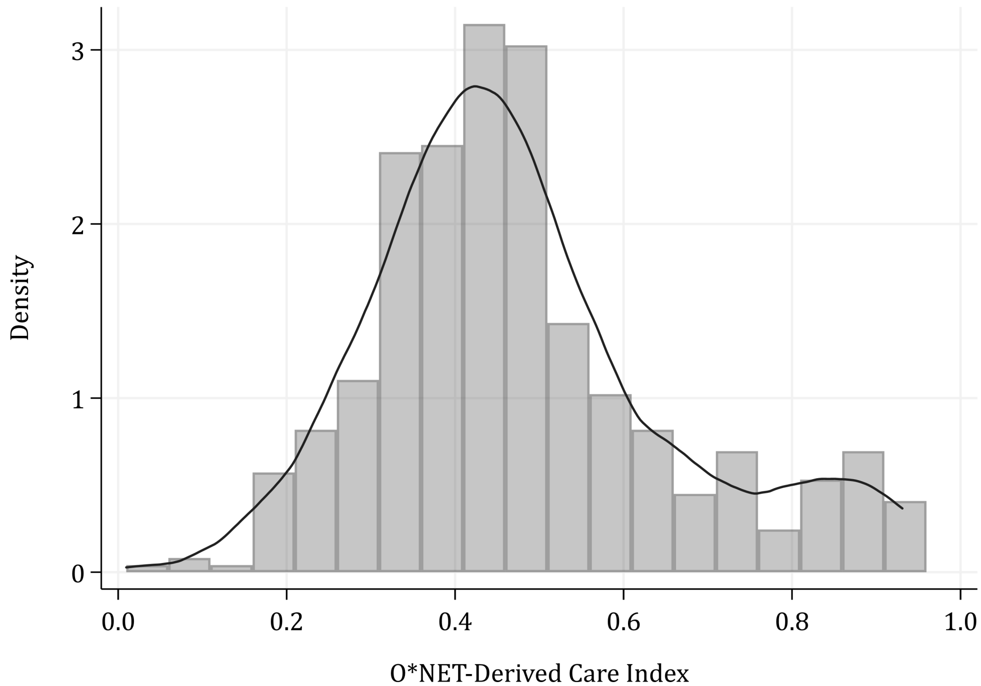

Figure 3.1: Distribution of NLSY97 Occupations Across the O*NET-Derived Care Index

of reproductive and nurturant care work occupations across the domain of the *care index* are presented by the dotted (medium grey) and solid (dark grey) lines, respectively. Reassuringly, the categorical classification of occupations into non-care and care work—originally derived by England et al. (2002) and updated by Budig et al. (2019)—behaves as expected. The density of reproductive and nurturant care occupations are situated to the right of non-care occupations, with nurturant care occupations making up the majority of those at the tail of the *care index* distribution. There is a considerable degree of overlap between non-care and reproductive care occupations relative to non-care and nurturant care occupations, suggesting that the O*NET-derived *care index* is a better measure of nurturant care intensity across occupations. The intuitive ordering of non-care and care occupations, as well as the varying degrees of overlap between, motivates the use of the continuous *care index* as an additional measure of care intensity across occupations.

In addition to the *care index*, I derive two additional control variables from the O*NET data to control for other occupation-specific determinants of hourly earnings. The first is a

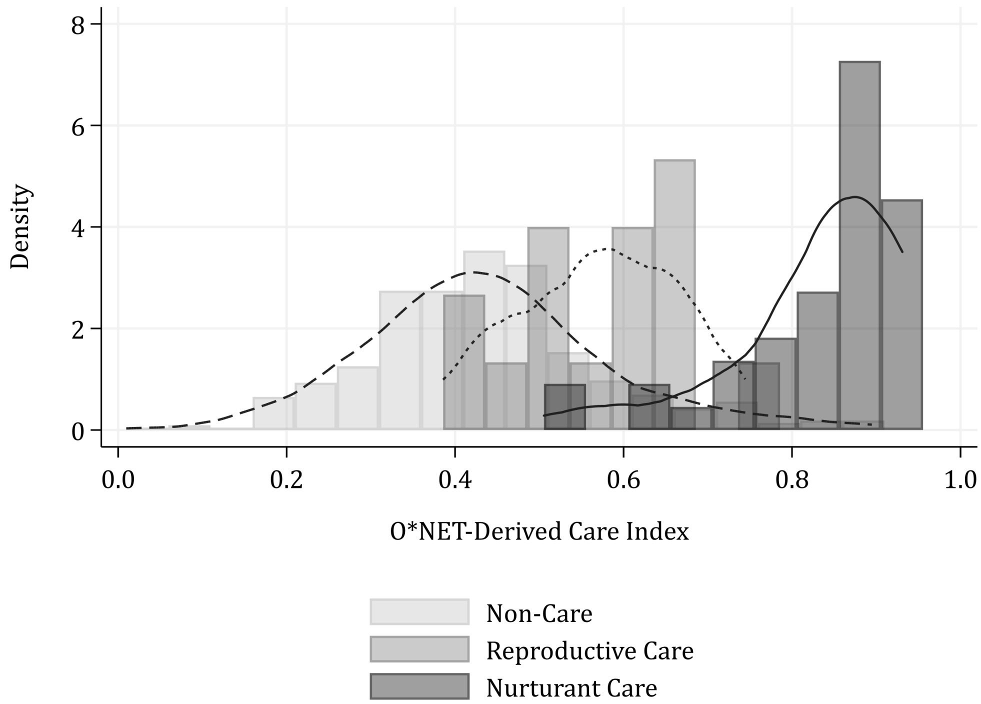

Figure 3.2: Relationship Between the Care Work Categorical Classification and the O*NET-Derived Care Index

PCA-derived *hazard index* which loads several indicators that capture the risk of bodily harm: *Exposed to Contaminants* (4.C.2.b.1.d), *Exposed to Hazardous Conditions* (4.C.2.c.1.d), *Exposed to Hazardous Equipment* (4.C.2.c.1.e), *Exposed to High Places* (4.C.2.c.1.c), *Exposed to Minor Burns, Cuts, Bites, or Stings* (4.C.2.c.1.f), and *Exposed to Whole Body Vibration* (4.C.2.b.1.f). The second is a PCA-derived *skills index* which loads several indicators that capture the basic skill competencies required across occupations: *Reading Comprehension* (2.A.1.a), *Active Listening* (2.A.1.b), *Writing* (2.A.1.c), *Speaking* (2.A.1.d), *Mathematics* (2.A.1.e), and *Science* (2.A.1.f). The *care index* is marginally negatively correlated with the *hazard index* ($r = -0.132$, $p < 0.000$) and marginally positively correlated with the *skills index* ($r = 0.091$, $p = 0.007$) across occupations in the O*NET database.

I employed one additional data set to construct variables employed in my empirical analysis. As the *devaluation hypothesis* suggests, the care penalty may simply arise from the fact that women are overrepresented in caring occupations and, therefore, since society tends to devalue women relative to men, the valuation of work in these occupations will be lower than it would be otherwise due to their association with the feminine (England, 2005; England et al., 2002). In an attempt to account for this phenomenon, I employ data on the sex composition of occupations derived from 2001-2005 American Community Survey (ACS) Census samples. These data include the harmonized occupations of employed persons under the Census Bureau's 2010 ACS occupation classification scheme (Ruggles et al., 2020). Using these data I calculate the percent female within each 2010 Census occupation for each sample. Then, using the occupation crosswalk provided by IPUMS, I merge the ACS-derived measures to the 2002 Census occupations in the NLSY97 sample.[42]

The means and standard deviations for the major variables of the NLSY97 sample are presented in Table 3.1. While not the focus of this research, the raw gender wage gap is 0.114 log points—approximately 12 percent—with women's mean real hourly wage being $2.68 less than that for men. The raw gender hourly compensation gap roughly 0.094 log points—roughly 10 percent.[43] Roughly 21 percent of person-job-year observations for women are within nurturant care occupations while only 5 percent of employment observations are within nurturant care occupations for men. The disproportionate representation of women in reproductive care work is less drastic, with 14 percent of person-job-year observations being contained within reproductive care occupations for women versus 10 percent for men. Additionally, the observed average *care index* among women is higher than that observed for men by nearly one standard deviation.[44]

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[42]The crosswalk employed to map 2010 ACS Census occupations to the 2002 Census occupations in the NLSY97 was accessed at `https://usa.ipums.org/usa/volii/occ_acs.shtml`. Similar to the merging of the O*NET and NLSY97 data, there were 11 occupations in the NLSY97 data that were matched to the 2010 ACS Census occupations upon inspection—see Appendix C Table C.4—as well as instances of multiple matching.

[43]The raw gender pay gaps in the NLSY97, both using sample means and medians, are smaller than those produced by larger, more age-representative samples—e.g., the median pay gap among fill-time workers was roughly 18 percent in 2015 (Gould, Scheider, & Geier, 2016). This is likely due to the fact that the gender pay gap grows with age (Gould et al., 2016).

[44]Average measures of care work vary somewhat across race/ethnicity, within gender—see Appendix C Table C.5.

Table 3.1: Means and Standard Deviations for Major Variables

	Women $N = 81{,}175$		*Men* $N = 81{,}445$	
Wages and Compensation				
Real Hourly Wage ($)	14.94	(37.99)	17.62	(46.72)
Ln Real Hourly Wage	2.481	(0.61)	2.595	(0.66)
Real Hourly Compensation ($)	16.99	(45.81)	20.72	(201.11)
Ln Real Hourly Compensation	2.589	(0.60)	2.683	(0.66)
Care Measures				
Nurturant Care (dummy)	0.207	(0.41)	0.053	(0.22)
Reproductive Care (dummy)	0.142	(0.35)	0.102	(0.30)
Care Index*	0.583	(0.16)	0.467	(0.14)
Human Capital and Labor Supply				
Enrolled (dummy)	0.587	(0.89)	0.450	(0.79)
Highest Grade Completed	13.061	(2.70)	12.393	(2.59)
Degree				
None (dummy)	0.193	(0.39)	0.247	(0.43)
GED (dummy)	0.069	(0.25)	0.099	(0.30)
HS Diploma (dummy)	0.498	(0.50)	0.488	(0.50)
Associates Degree (dummy)	0.052	(0.22)	0.040	(0.20)
Bachelors Degree (dummy)	0.151	(0.36)	0.105	(0.31)
Masters Degree (dummy)	0.030	(0.17)	0.017	(0.13)
Doctoral Degree (dummy)	0.002	(0.04)	0.001	(0.03)
Professional Degree (DDS/JD/MD) (dummy)	0.005	(0.07)	0.003	(0.06)
Years of Employment Experience	5.670	(4.46)	5.841	(4.62)
Years Spent in Unemployemnt	0.397	(0.72)	0.469	(0.78)
Number of 6+ Week NILF Spells	3.940	(4.20)	3.651	(4.03)
Other Job Characteristics				
Union (dummy)	0.059	(0.24)	0.077	(0.27)
Self Employed (dummy)	0.050	(0.22)	0.062	(0.24)
Part-time (dummy)	0.516	(0.50)	0.392	(0.49)
Usual Hours per Week	29.549	(13.90)	33.387	(14.41)
Job Tenure (in years)	1.713	(2.26)	1.811	(2.42)
Hazard Index*	0.146	(0.11)	0.303	(0.21)
Skills Index*	0.373	(0.14)	0.325	(0.15)
Majority Female Occupation (dummy)**	0.803	(0.40)	0.331	(0.47)

Source: NLSY97, Waves 1997-2017.
Notes: *Indicates O*NET-derived indices. **Makes use of Census-derived sex composition of occupation. Standard deviations in parentheses.

Other differences in observables across gender are evident in Table 1.1. For instance, women are more likely to be enrolled in schooling and, therefore, outperform men in educational attainment. Women have slightly less employment experience, lower time spent in unemployment, and slightly more NILF spells relative to men. Women are less likely to be self-employed, work in jobs with lower rates of unionization, and are more likely to be working part-time despite higher levels of occupational skill requirements. Lastly, and as expected, women tend to work in female-dominated occupations while men tend to work in male-dominated occupations.

3.4.2 Empirical Model

To investigate the impact of care work on wages, I employ the NLSY97 sample described above and estimate fixed-effects semi-log earnings functions of the form

$$\ln W_{ijt} = \beta CARE_{ijt} + X'_{ijt}\gamma + \alpha_i + \delta_t + \varepsilon_{ijt}. \tag{3.1}$$

Here, $\ln W_{ijt}$ is the natural log of real hourly earnings for indvidual i in job j at time (year) t; $CARE_{ijt}$ is the independent variable of interest—initially defined categorically (dummy) and then replaced with the O*NET-derived *care index*—which measures the extent to which an individual's occupation is associated with care work; X_{ijt} is a vector time- and job-varying controls which attempt to capture other potential changes in observables hypothesized to codetermine hourly earnings. These controls include measures of *human capital and labor supply*—such as enrollment status, highest grade completed, indicator variables for highest degree received (7 dummies), cumulative years of employment experience, cumulative years spent in unemployment, and cumulative number of 6+ week not-in-the-labor-force (NILF) spells—as well as *other job characteristics*—including union status (dummy), self-employment status (dummy), part-time status (<35 hours per week) (dummy), usual hours worked per week, years of job-specific tenure, O*NET-derived occupation-specific hazard and skill indices, and an ACS-derived indicator for female dominated occupations (> 50% female). In addition, each model estimated below includes several indicators for geography.[45]

The α_i are individual fixed-effects that are meant to capture unobserved, time-invariant personal characteristics that have potential additive effects on wage earnings and, therefore,

[45]These include urbanicity (urban, rural, or unkown), MSA status (not in MSA, in MSA but not in central city, in MSA and in central city, or in MSA with central city location unknown), and broad Census region (Northeast, North Central, South, and West).

are likely be correlated with model covariates. The inclusion of these effects are principal to the identification β, as well as the estimated coefficients on other model covariates (Angrist & Pischke, 2008). For instance, consider the evidence that workers in caring occupations have relatively low earnings relative to other occupations, all else constant. Workers with low (high) productivity and earning potential—an unobserved, individual-level characteristic—may then select into (out of) care occupations. In the absence of individual effects, the estimated coefficient of interest (β) may then be biased downward due to this potential selection on unobservables. Another example is the notion that care occupations provide intrinsic rewards—the so-called "warm glow" of caring labor—so that they attract workers who are more motivated by non-monetary compensation and, therefore, are less likely to bargain for higher wages. In so far as these unobserved personal motivations are time-invariant, the individual fixed-effects should net the estimated effect of care work on wages (β) of this additional source of selection. Year fixed-effects, the δ_t, on the other hand, are meant to control for time-varying shocks that are common across all individuals—such as potential changes in the federal minimum wage or wage shocks resulting from large-scale economic downturns (Clemens & Wither, 2019).

3.5 Results

3.5.1 The Pay Penalty for Care Work

I begin by estimating Eq. 3.1 using the natural log of real hourly wages as the outcome variable of interest. The results are presented in Table 1.2. Each cell reports the estimated coefficient of interest ($\hat{\beta}$) from a separately estimated Eq. 3.1 model. In column (1), the only controls included, besides person and year fixed effects, are geographic indicators. Column (2) introduces measures of human capital and historical labor supply, while column (3) introduces non-care-related job characteristics. In column (4), I introduce an indicator variable for female dominated occupations to control for the previously discussed devaluation hypothesis. Lastly, I remove individual fixed effects in column (6) and cluster standard errors at the individual level. This tests the sensitivity of the estimates to potetnial selection on wage-relevant unobservables.

Nurturant care occupations tend to be relatively high-skill jobs with above average human capital requirements—see Figure 3.4. Thus, the emergence of a care penalty only appears in column (3) after controlling for differences human capital and non-care-related job characteristics. This is consistent with the fact that the wage penalty for nurturant care work—unlike the gender pay gap, for instance—is unobservable in unadjusted data. The coefficient estimate of -0.068 in column (3) of Panel A suggests that, all else equal, women in nurturant care occupations experience a wage penalty of 6.6 percent. The implied wage penalty among men is 5.8 percent. Controlling for the fact that nurturant care occupations are disproportionately female in column (4), the estimated care penalty is marginally reduced for women. The estimated care penalty is more significantly reduced for men, suggesting that a portion of estimated wage penalty may be related to the cost of taking up gender non-conforming jobs (G. A. Akerlof & Kranton, 2000; Badgett & Folbre, 1999). The estimated wage penalty for nurturant care workers increases for both women and men when moving from a fixed effects specification to the pooled OLS model in column (5), particularly for men, suggesting that selectivity of those with low earnings potential or proclivity into caring labor—relative to others of their sex—may be more pronounced for man than woman.

Compared to nurturant care, the coefficient estimates for reproductive care behave oppositely. That is, there appears to be a wage penalty for reproductive care in column (1), but this disappears by column (3) after including controls for differences in human capital and non-care-related job characteristics. For women, there appears to be a wage premium, all else equal. The coefficient estimate of 0.094 in column (3) of Panel A suggests that women in reproductive occupations experience a wage premium of 9.9 percent relative to women in non-reproductive care occupations, all else equal. Similar to women, the observed wage penalty for reproductive care work among men is completely absorbed by non-care-related job characteristics. However, since the wage penalty is much larger than that for women in column (1), the result is that men in reproductive care occupations experience neither a wage penalty nor wage premium. When controlling for the feminization of occupations in column (4), the coefficients are slightly inflated

Table 3.2: Estimated Impact of Care Work on *Ln* Real Hourly Wages

	(1)	(2)	(3)	(4)	(5)
Panel A: Women					
Nurturant Care (Dummy)	0.018**	-0.006	-0.068***	-0.065***	-0.083***
	(0.009)	(0.009)	(0.009)	(0.009)	(0.009)
Reproductive Care (Dummy)	-0.017*	-0.002	0.094***	0.100***	0.101***
	(0.009)	(0.009)	(0.010)	(0.010)	(0.010)
Care Index	-0.069***	-0.080***	-0.136***	-0.128***	-0.161***
	(0.022)	(0.021)	(0.020)	(0.021)	(0.022)
N	76428	76428	76428	76428	76428
Individuals	4,249	4,249	4,249	4,249	4,249
Panel B: Men					
Nurturant Care (Dummy)	0.029	-0.009	-0.060***	-0.047***	-0.105***
	(0.018)	(0.017)	(0.016)	(0.016)	(0.017)
Reproductive Care (Dummy)	-0.120***	-0.112***	0.004	0.018*	0.016
	(0.009)	(0.009)	(0.010)	(0.010)	(0.011)
Care Index	-0.180***	-0.195***	-0.211***	-0.193***	-0.276***
	(0.025)	(0.024)	(0.023)	(0.025)	(0.028)
N	79761	79761	79761	79761	79761
Individuals	4,458	4,458	4,458	4,458	4,458
FE Model	X	X	X	X	
Human Capital & Labor Supply Controls		X	X	X	X
Job Characteristics Controls			X	X	X
Majority Female Occupation Control				X	X
Pooled OLS Model					X

Source: NLSY97, Waves 1997-2017.
Notes: Each cell reports the estimated coefficient of interest ($\hat{\beta}$) from a separately estimated Eq. 3.1 model. Models include the controls specified in Table 3.1 unless otherwise indicated, as well as controls for urban/rural residence (dummies), MSA residence (dummies), Census region (dummies). Standard errors in parentheses. Levels of statistical significance given by * $p < 0.1$, ** $p < 0.05$, and *** $p < 0.01$.

similar to the behavior of the estimated coefficients for nurturant care—again, the effect appears to be slightly larger for men. Lastly, and unlike the results for nurturant care, the pooled OLS model produces estimates that are nearly identical to those produced by the fixed effect model.

Moving on to the *care index*, Table 3.2 shows the existence of a wage penalty throughout. As discussed previously, the *care index* is a better measure of nurturant care intensity across occupations—see Figure 3.2. However, unlike the estimates produced under the categorical

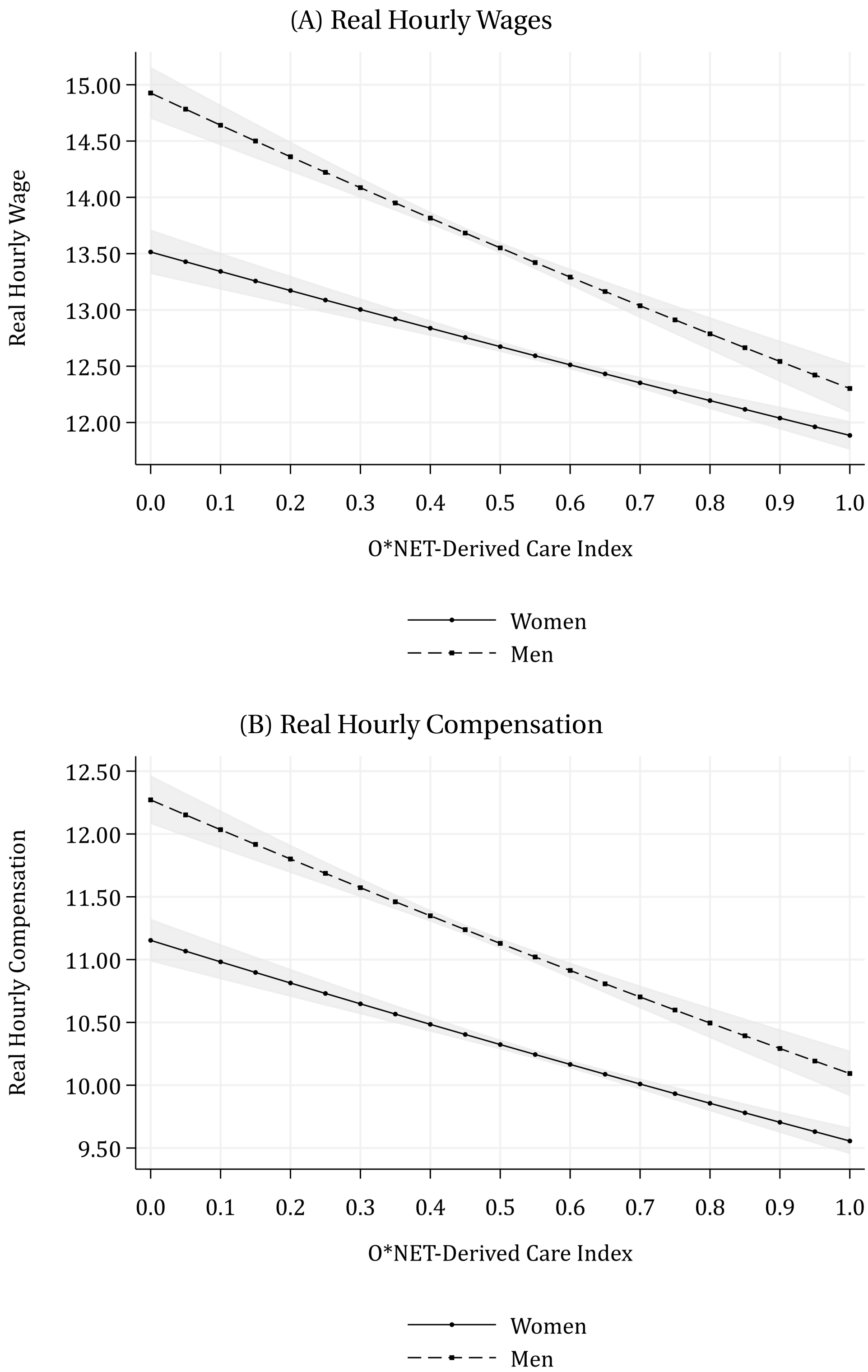

Figure 3.3: Estimated Care-Wage and Care-Compensation Gradients

definition of nurturant care, the estimated wage penalty via the continuous *care index* are larger for men than women. Since the *care index* takes on values in the range 0 to 1, the estimated coefficient reflects the impact on wages when moving from the least care-intense to most care-intense occupation. However, I interpret these estimates as a movement across the interdecile range of the *care index*, which is a movement from 0.28 to 0.75. For instance, in column (4) of panel A, after for controlling human capital, non-care-related job characteristics, and the feminization of occupations, the coefficient estimate of -0.128 suggests that a movement across the interdecile range of the *care index* is associated with a 5.8 percent reduction in hourly wages among women, all else equal. The implied wage penalty among men is 8.7 percent. The estimated care-wage gradients implied by column (4) of Table 3.2 are shown graphically in panel A of Figure 3.3. One should note that these hourly wage predictions for women and men are produced by separately estimated Eq. 3.1 models. All else equal, model predicted hourly wages are higher for men than women across the *care index* continuum. Given the relatively large estimated wage penalty among men, predicted hourly wages for women and men appear to converge in increasingly care-intense occupations and suggest that the gender pay gap may be smaller in care-intense occupations, all else equal.

Next, I perform an identical analysis to that presented in Table 3.2 but use the natural log of real hourly compensation as the outcome variable of interest. As stated previously, this measure is inclusive of overtime, tips, bonuses, and the like, in addition to an individual's hourly pay rate. The use of this alternative measure of earnings is likely to be important since there may be differences in pay across care and non-care occupations that are not captured exclusively by wage rates. For instance, reproductive care occupations include Bartenders, Waiters and Waitresses, Barbers, Hairdressers, Hairstylists, and Cosmetologists. Workers in these occupations often receive "voluntary gifts", or tips, for their service (Lynn, 2016, 2021).

Two things stand out from the results presented in Table 3.3. First, the estimated wage penalties produced by the categorical classification of nurturant care and the continuous *care index* are inflated when employing real hourly compensation as the outcome variable of interest.

Table 3.3: Estimated Impact of Care Work on *Ln* Real Hourly Compensation

	(1)	(2)	(3)	(4)	(5)
Panel A: Women					
Nurturant Care (Dummy)	-0.022**	-0.046***	-0.096***	-0.094***	-0.125***
	(0.009)	(0.009)	(0.009)	(0.009)	(0.009)
Reproductive Care (Dummy)	0.095***	0.112***	0.210***	0.221***	0.260***
	(0.008)	(0.008)	(0.009)	(0.009)	(0.010)
O*NET-Derived Care Index	-0.121***	-0.132***	-0.162***	-0.155***	-0.200***
	(0.022)	(0.021)	(0.020)	(0.021)	(0.022)
N	81175	81175	81175	81175	81175
Individuals	4,250	4,250	4,250	4,250	4,250
Panel B: Men					
Nurturant Care (Dummy)	-0.012	-0.049***	-0.105***	-0.091***	-0.161***
	(0.018)	(0.017)	(0.016)	(0.017)	(0.017)
Reproductive Care (Dummy)	-0.065***	-0.054***	0.057***	0.081***	0.101***
	(0.009)	(0.008)	(0.009)	(0.010)	(0.011)
O*NET-Derived Care Index	-0.195***	-0.207***	-0.222***	-0.195***	-0.264***
	(0.025)	(0.024)	(0.024)	(0.025)	(0.028)
N	81445	81445	81445	81445	81445
Individuals	4,458	4,458	4,458	4,458	4,458
FE Model	X	X	X	X	
Human Capital & Labor Supply Controls		X	X	X	X
Job Characteristics Controls			X	X	X
Majority Female Occupation Control				X	X
Pooled OLS Model					X

Source: NLSY97, Waves 1997-2017.
Notes: Each cell reports the estimated coefficient of interest ($\hat{\beta}$) from a separately estimated Eq. 3.1 model. Models include the controls specified in Table 1.1 unless otherwise indicated, as well as controls for urban/rural residence (dummies), MSA residence (dummies), Census region (dummies). Standard errors in parentheses. Levels of statistical significance given by * $p < 0.1$, ** $p < 0.05$, and *** $p < 0.01$.

Second, reproductive care work appears to be more generously compensated relative to non-reproductive care occupations, all else equal. This now obtains for both men and women in Table 3.3, with the estimated wage premium being significantly larger for women compared to men. These results have important implication for future work on the relatve pay of care work. Specifically, measures of earnings that exclude wage augmenting payments, such as overtime, tips, and bonuses, are likely to understate the wage penalty faced by nurturant care workers and (2) overstate the wage penalty faced by reproductive care workers. For example, Budig et al. (2019) find small wage penalties for reproductive care workers using hourly wage rates in the NLSY79. The results produced here suggest that these wage penalties may be extinguished, or even reversed, when accounting for other additional forms of compensation that are relevant to reproductive care occupations.

3.5.2 Sensitivity Tests and Robustness

In this section I test the sensitivity of the previously discussed results to two sample restrictions. First, and as mentioned previously, the NLSY97 is a relatively young sample—33 to 37 years of age in the most recently observed year (2017). Therefore, many of the person-year-job observations employed the analysis thus far are for workers who are relatively young, still enrolled in schooling, and, therefore, part-time. In an attempt to identify person-job-year observations that reflect more permanent employment endeavors, I subset the NLSY97 to full-time employment observations. I define full-time employment to be those person-year-job observations in which an individual works at least 30 hours in a usual week. In addition to this, I limit the observations in my sample to those with real hourly wages and real hourly compensation that are ≥ \$5 and ≤ \$75. This tests the sensitivity of the results to the presence uncharacteristically low and high wage observations. The estimates produced by these tests are likely to more comparable to those produced by previous studies that employ similar restrictions (Budig et al., 2019; England et al., 2002; Hirsch & Manzella, 2015).

I begin as before, employing real hourly wages as the outcome variable of interest. The results are presented in Table 3.4. For reference, column (1) reports identical estimates to those present in column (4) of Table 3.2. In column (2) I limit the sample to only full-time observations—i.e., employment observations in which an individual works at least 30 hours in a usual week—and in column (3) I limit the sample to wage observations that are ≥ \$5 and ≤ \$75. In column (4), where I will focus my discussion, I apply both sample restrictions together.

Table 3.4: Sensitivity of the Estimated Impacts of Care Work on *Ln* Real Hourly Wages

	(1)	(2)	(3)	(4)
Panel A: Women				
Nurturant Care	-0.065***	-0.055***	-0.033***	-0.039***
	(0.005)	(0.007)	(0.004)	(0.005)
Reproductive Care	0.100***	0.027**	0.084***	0.015**
	(0.007)	(0.011)	(0.005)	(0.008)
Care Index	-0.128***	-0.112***	-0.088***	-0.085***
	(0.013)	(0.016)	(0.009)	(0.011)
N	76,428	38,108	74,977	37,502
Individuals	4,249	4,086	4,248	4,076
Panel B: Men				
Nurturant Care	-0.047***	-0.087***	-0.032***	-0.067***
	(0.016)	(0.021)	(0.012)	(0.015)
Reproductive Care	0.018*	-0.004	0.030***	0.019*
	(0.010)	(0.015)	(0.007)	(0.010)
Care Index	-0.193***	-0.159***	-0.178***	-0.143***
	(0.025)	(0.027)	(0.018)	(0.020)
N	79,761	49,005	77,864	48,240
Individuals	4,458	4,334	4,455	4,324
FE Model	X	X	X	X
Controls	X	X	X	X
Full-Time Restriction		X		X
Wage Restriction			X	X

Source: NLSY97, Waves 1997-2017.
Notes: Each cell reports the estimated coefficient of interest ($\hat{\beta}$) from a separately estimated Eq. 3.1 model. Models include the controls specified in Table 1.1 unless otherwise indicated, as well as controls for urban/rural residence (dummies), MSA residence (dummies), Census region (dummies). Standard errors in parentheses. Levels of statistical significance given by * $p < 0.1$, ** $p < 0.05$, and *** $p < 0.01$,.

Beginning with the categorical measure of nurturant care, the sample restrictions result in an estimated wage penalty that is reduced among women, but inflated among men. Specifically, the estimated wage penalty among women falls from 6.3 percent in column (1) to 3.8 percent in column (4). Among men, the estimated wage penalty increases from 4.6 percent in column (1) to 6.5 percent in column (4). Now, moving to the *care index*, I find that the estimated wage penalties are reduced among women and men after applying the sample restrictions. The coefficient estimate of -0.128 in column (1) of panel A suggests that a movement across the interdecile range of the *care index* is associated with a 5.8 percent decrease in hourly wages, all else equal, while the coefficient estimate of -0.085 in column (4) of panel A suggests a 3.9 percent reduction in wages in response to the same movement across the *care index*. The implied wage penalties among men in columns (1) and (4) of panel B are 8.7 percent and 6.5 percent, respectively. Lastly, the estimated positive impact of reproductive care work on the hourly wages of women is significantly reduced after reducing the sample to full-time employment observations in column (2). In column (4), the coefficient estimate of 0.015 implies that, all else equal, the wages of women in reproductive care occupations are more or less comparable to those for women in non-reproductive care occupations—a marginal 1.5 percent wage premium, to be precise. The estimate produced among men in column (4) of panel B is similar, suggesting a marginal 1.9 percent wage premium for men in reproductive care occupations relative to men in non-reproductive care work, all else equal.

Table 3.5 performs identical tests to those presented in Table 3.4, but with the outcome variable of interest being real hourly compensation. Focusing again on column (4), the estimated wage penalties using the categorical measure of nurturant care work are 6.1 percent 10.3 percent among women and men, respectively. Using the continuous *care index*, the implied wage penalties are 5.8 percent and 7.8 percent for women and men, respectively, in response to a movement across the interdecile range of the *care index*. As expected, the estimated wage premia for women and men in reproductive care occupations are inflated when using real hourly compensation in place of real hourly wages. This can be seen by comparing the column (4) coefficient estimate for reproductive care across Tables 3.4 and 3.5. Lastly, and with regard to the full-time and wage restricted sample, Appendix C Tables C.7 and C.8 report the model estimated coefficients for a number of other covariates including highest grade completed, highest degree received, union status, job tenure, the O*NET-derived occupational hazard and skill indices, and the majority female occupation indicator.

Here I will take a moment to summarize the results presented thus far. First, and similar to previous studies that have employed alternative samples of data, I have found the presence

Table 3.5: Sensitivity of the Estimated Impacts of Care Work on *Ln* Real Hourly Compensation

	(1)	(2)	(3)	(4)
Panel A: Women				
Nurturant Care	-0.094***	-0.081***	-0.061***	-0.063***
	(0.009)	(0.012)	(0.007)	(0.009)
Reproductive Care	0.221***	0.120***	0.208***	0.104***
	(0.009)	(0.014)	(0.008)	(0.011)
Care Index	-0.155***	-0.160***	-0.108***	-0.128***
	(0.021)	(0.026)	(0.016)	(0.018)
N	81,175	39,325	78,952	38,475
Individuals	4,250	4,093	4,249	4,083
Panel B: Men				
Nurturant Care	-0.091***	-0.128***	-0.075***	-0.109***
	(0.017)	(0.020)	(0.013)	(0.016)
Reproductive Care	0.081***	0.032**	0.088***	0.048***
	(0.010)	(0.014)	(0.008)	(0.011)
Care Index	-0.195***	-0.190***	-0.183***	-0.173***
	(0.025)	(0.028)	(0.019)	(0.022)
N	81,445	49,552	79,181	48,627
Individuals	4,458	4,336	4,455	4,326
FE Model	X	X	X	X
Controls	X	X	X	X
Full-Time Restriction		X		X
Wage Restriction			X	X

Source: NLSY97, Waves 1997-2017.
Notes: Each cell reports the estimated coefficient of interest ($\hat{\beta}$) from a separately estimated Eq. 3.1 model. Models include the controls specified in Table 1.1 unless otherwise indicated, as well as controls for urban/rural residence (dummies), MSA residence (dummies), Census region (dummies). Standard errors in parentheses. Levels of statistical significance given by * $p < 0.1$, ** $p < 0.05$, and *** $p < 0.01$.

of wage penalties for care work among women and men in the NLSY97. These care penalties, however, only obtain when employing the categorical measure of nurturant care and the continuously measured *care index*. Among women, care penalties exist on the order of 3.8 to 6.1 percent, depending on which measure of care is employed—e.g., the *nurturant care* indicator or the *care index*—and the measure of hourly earnings—e.g., hourly wages or hourly compensation. Among men, the estimated care penalties are on the order of 6.5 to 10.3 percent. The care penalties found above are similar to those found by England et al. (2002), who employ a similarly young

NLSY79 sample. Compared to the care penalties found by Budig et al. (2019), who employ a much more age-representative NLSY79 sample, the estimated wage penalties found here are smaller. This is somewhat expected since, all else equal, wage growth among care workers is lower than that for non-care workers over the life course (Budig et al., 2019). The continued presence of wage penalties in nurturant care occupations speaks to the importance of policies that seek to provide competitive wages for “direct care" workers, such as those proposed in the current administrations Build Back Better agenda.

Second, I have found that workers in reproductive care occupations do not face wage penalties similar to those in nurturant care occupations. This is similar to Budig et al. (2019), who find only marginal wage penalties for reproductive care workers in their more age-representative NLSY79 sample. In fact, I find that the hourly wages of women and men in reproductive care occupations are comparable to those in non-reproductive care jobs, all else equal—a 1.5 and 1.9 percent wage premium for women and men, respectively. The slight wage premia found in my analysis grow when using hourly compensation in place of hourly wages. This is not to say that the wages of those in reproductive care occupations are not low, generally. Further, reproductive care jobs are not likely to provide benefits such as healthcare, paid leave, retirement plans, and job security that are likely to be provided in the generally more professionalized nurturant care occupations. The results presented above simply state that, holding human capital and non-care-related job characteristics constant, the hourly earnings of workers in reproductive care occupations are comparable, if not slightly higher, relative to workers in non-reproductive care occupations. The slight wage premia found in my analysis, however, are likely to disappear with age since wage growth in reproductive care occupations is likely to limited.

From the results presented above, there are two implications regarding the measurement of care work and the measurement of earnings. First, the O*NET-derived *care index*, which was originally employed by Hirsch and Manzella (2015), appears to provide a continuous measure of care work that captures the essence of nurturant care. This is evidenced by the distribution of non-care and nurturant care occupations across the *care index* continuum in Figure 3.2, as well as the similar behavior of the nurturant care indicator and the *care index* in the results presented above. The two measures behave similarly in the results of the following section as well. The continuously measured *care index*, however, does not properly capture the essence of reproductive care as researchers have previously defined it.[46] Second, it is important for future research to consider non-wage monetary compensation in their measure of hourly earnings. The estimated wage penalties produced by the nurturant care indicator and the *care index* increased when using hourly compensation in place of real hourly wages. Thus, structural differences across occupations may limit care workers—nurturant care workers, specifically—from receiving tips and pay bonuses that augment an individuals wage. The use of hourly compensation in place of hourly wages had an opposite effect on the estimates for reproductive care, which is likely due to the fact that workers in reproductive care occupations—such as bartenders, waiters and waitresses, barbers, and personal appearance workers—often receive “voluntary gifts", or tips, that augment their wage rate.

3.5.3 Heterogeneous Impacts Among Low- and High-Skilled Occupations

In this section I investigate whether the previously estimated wage penalties obtain across the occupational skill distribution. The measure of care employed in this analysis is the O*NET-derived *care index*. All of the results that follow obtain, at least qualitatively, when employing the categorical measure of nurturant care in place of the *care index*—see Appendix C Table C.9. Figure 3.4 plots the 488 unique occupations represented in the NLSY97 along two dimensions. On the horizontal axis is the O*NET-derived *care index* and on the vertical axis is the O*NET-derived *skill index*. The dashed lines in the figure represent the median values of the respective indices.[47] The figure shows that nurturant care occupations are largely located in the upper right-hand

[46]This is somewhat expected since the O*NET job descriptors that make up the *care index*—e.g., “Assisting and Caring for Others" and “Concern for Others"—are more in-line with the interactive, intimate, and relational conception of nurturant care work (Duffy, 2005).

[47]Not the median values in relation to the 488 unique occupations in the NLSY97, but the median values for the larger universe of occupations from which these measures were generated the O*NET Database 25.1.

quadrant, meaning they are, on average, relatively high-care, high-skill occupations.[48] However, there are several nurturant care occupations that fall into the lower right-hand side quadrant. Reproductive care occupations fall almost entirely into lower right-hand side quadrant, meaning they are relatively high-care, low-skill.

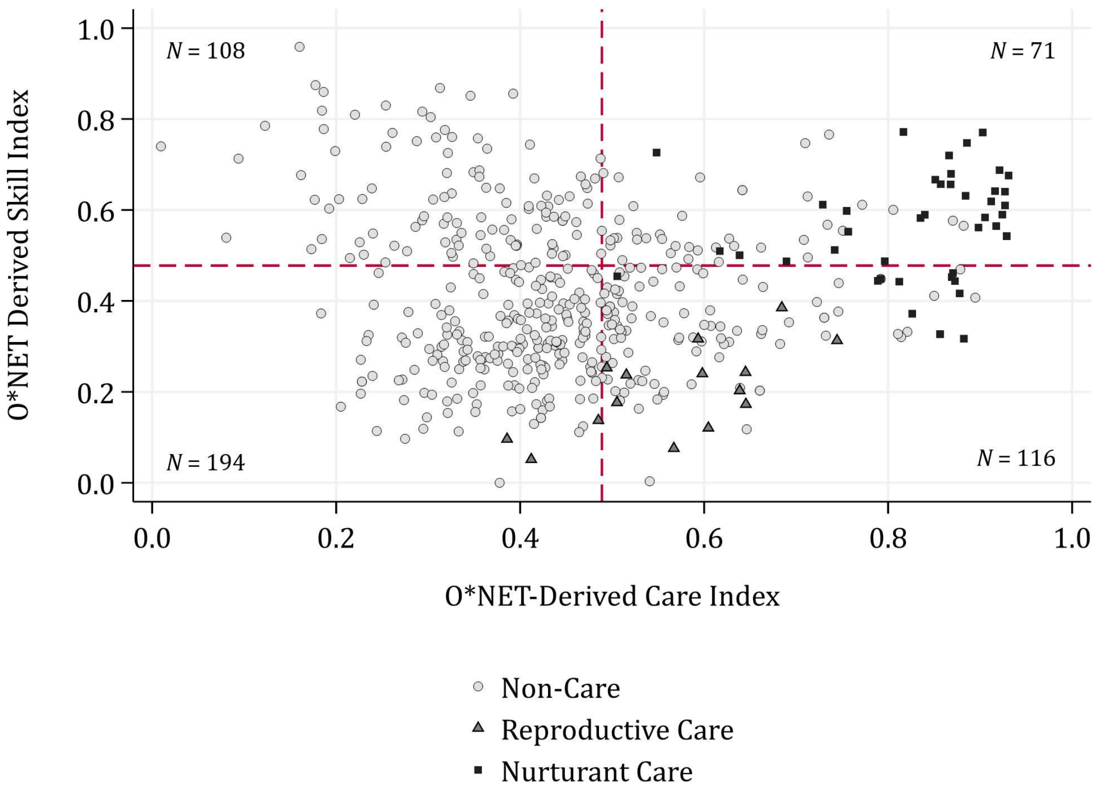

Figure 3.4: Distribution of NLSY97 Occupations Across the O*NET-derived Care and Skill Indices

Table 3.6 presents the representation of NLSY97 sample observations with respect to the quadrants shown in Figure 3.4.[49] Among women, the majority of employment observations are located in relatively high-care, low-skill occupations. The majority of employment observations for men are located in low-care, low-skill occupations. Relative to men, women are more likely to

[48]I use the terms "low-skill" and "high-skill" technically, not normatively. Occupations are "low skill" or "high skill" in reference to the O*NET-derived *skill index*.

[49]The representation of NLSY97 sample observations along these dimensions by race/ethnicity and gender are shown in Appendix C Table C.6. In agreement with the work of Duffy (2005, 2007), I find that Black and Hispanic workers, particularly women, are disproportionately located in high-care, low-skill occupations.

be in relatively high-skill occupations. This is consistent with the fact that women in the NLSY97 have higher educational attainment relative to men. There is little difference in the representation of women and men in low-care, high-skill occupations—e.g. 9.4 versus 10.6 percent of full-time employment observations. However, women are disproportionately employed in high-care, high-skill occupations relative to men—e.g. 20.5 versus 10.1 percent of full-time employment observations. It is also worth noting that, among both low- and high-skill occupations, there seems to be overcrowding in high-care occupations given the relative size of each occupational category indicated in Figure 3.4—though the NLSY97 is not the appropriate sample from which to draw such conclusions.

Table 3.6: Sample Representation in Care and Skill Occupations

	Women:		Men:		All:	
	All (*N=81,175*)	*Full-Time* (*N=39,325*)	*All* (*N=81,445*)	*Full-Time* (*N=49,552*)	*All* (*N=162,620*)	*Full-Time* (*N=88,877*)
Low Care, Low Skill	24.2%	21.9%	53.6%	54.4%	39.0%	40.0%
Low Care, High Skill	6.1%	9.4%	8.2%	10.6%	7.2%	10.0%
High Care, Low Skill	54.6%	48.3%	29.4%	25.0%	42.0%	35.3%
High Care, High Skill	15.1%	20.5%	8.8%	10.1%	11.9%	14.7%

Source: NLSY97, Waves 1997-2017.

To investigate whether the previously estimated wage penalties obtain across the skill distribution, I estimate Eq. 3.1 models that allow for an interaction between the O*NET-derived *care index* and a categorical measure of occupational skill. The categorical measure of skill is a binary indicator variable that identifies whether an occupation is above the O*NET-derived *skill index* median—see Figure 3.4. The results are presented in Table 3.7.[50] Reported are the main effect of the *care index* and the interaction effect between the *care index* and the high-skill occupation indicator.[51] The estimated main effect captures the impact of care work on earnings among

[50] An identical analysis using the categorical measure of *nurturant care* is reported in Appendix C Table C.9.

[51] In this case, the estimated main effect for the *High Skill* indicator has the following interpretation: it is the estimated difference in earnings between high- and low-skill occupations when the *care index* equals 0. A *care index* value of 0 is non-present in the data and, as mentioned previously, the *care index* interdecile range is between 0.28 and 0.75. Given the impractical interpretation of the coefficient estimate, I exclude it from the table.

low-skill occupations while the estimated interaction effect, combined with the estimated main effect, captures the impact of care work on earnings among high-skill occupations. I report the results for the full sample as well as the full-time, wage restricted sample.

Table 3.7: Estimated Impact of Care on Earnings in High vs. Low Skill Occupations

	Dependent variable is *Ln* real hourly wage		Dependent variable is *Ln* real hourly compensation	
	(1)	(2)	(3)	(4)
Panel A: Women				
Care Index	-0.166***	-0.129***	-0.208***	-0.170***
	(0.024)	(0.018)	(0.024)	(0.019)
Care Index × High Skill	0.236***	0.202***	0.229***	0.194***
	(0.042)	(0.033)	(0.043)	(0.034)
N	76428	37502	81175	38621
Individuals	4,249	4,076	4,250	4,084
Panel B: Men				
Care Index	-0.078***	-0.046**	-0.077***	-0.042**
	(0.024)	(0.020)	(0.025)	(0.021)
Care Index × High Skill	-0.218***	-0.160***	-0.308***	-0.233***
	(0.053)	(0.042)	(0.053)	(0.042)
N	79761	48240	81445	48591
Individuals	4,458	4,324	4,458	4,325
FE Model	X	X	X	X
Controls	X	X	X	X
Full-Time Restriction		X		X
Wage Restriction		X		X

Source: NLSY97, Waves 1997-2017.
Notes: Models include the controls specified in Table 3.1 unless otherwise indicated, as well as controls for urban/rural residence (dummies), MSA residence (dummies), Census region (dummies). Standard errors in parentheses. Levels of statistical significance given by * $p < 0.1$, ** $p < 0.05$, and *** $p < 0.01$.

I will focus my discussion on columns (2) and (4), which report the results for the previously discussed full-time, wage restricted sample. Column (2) of panel A indicates that, among women

Appendix C Table C.9 reports the estimated main effect for the *High Skill* indicator given its more straight-forward interpretation in those models.

in low-skill occupations, increasingly care intense jobs are associated with lower wages, all else equal. Specifically, the coefficient estimate of -0.129 suggests that a movement across the interdecile range of the *care index*—i.e., a movement from 0.28 to 0.75—is associated with 5.9 percent reduction in hourly wages. Among women in high-skill occupations, however, increasingly care intense jobs are associated with higher wages, all else equal. Combining the main and interaction effects in column (2) of Panel A suggests that a similar movement across the interdecile range of the *care index* is associated with 3.5 percent increase in hourly wages for women in high-skill occupations. These results are presented graphically in panel A of Figure 3.5. In column (4), which employs real hourly compensation as the outcome variable of interest, the estimated care penalty is inflated for women in low-skill occupations—a 7.7 percent decrease in hourly compensation in response to a movement across the interdecile range of the *care index*, all else equal. On the other hand, the estimated care penalty among women in high-skilled occupations becomes effectively null. As shown in panel A of Figure 3.6, predicted real hourly compensation among high-skilled occupations is relatively constant across the interdecile range of the *care index*.

The results among men are quite different compared to those for women. Beginning in column (2) of Table 3.7, the coefficient estimate of -0.046 suggests that, among men in relatively low-skilled occupations, a movement across the interdecile range of the *care index* is associated with a 2.1 percent decrease in hourly wages. Combining the main and interaction effects in column (2) of Panel B suggests that a similar movement across the interdecile range of the *care index* is associated with 9.2 percent decrease in hourly wages for men in high-skill occupations. Thus, the estimated care penalty is roughly 4 times larger for men in relatively high-skill occupations compare to men in low-skill occupations. These results are presented graphically panel B of Figure 3.5. The estimated care penalty in terms of real hourly compensation is nearly identical for men in relatively low-skill occupations, as shown in column (4) of panel B. The implied care penalty for men in relatively high-skill occupations, however, grows so that a movement

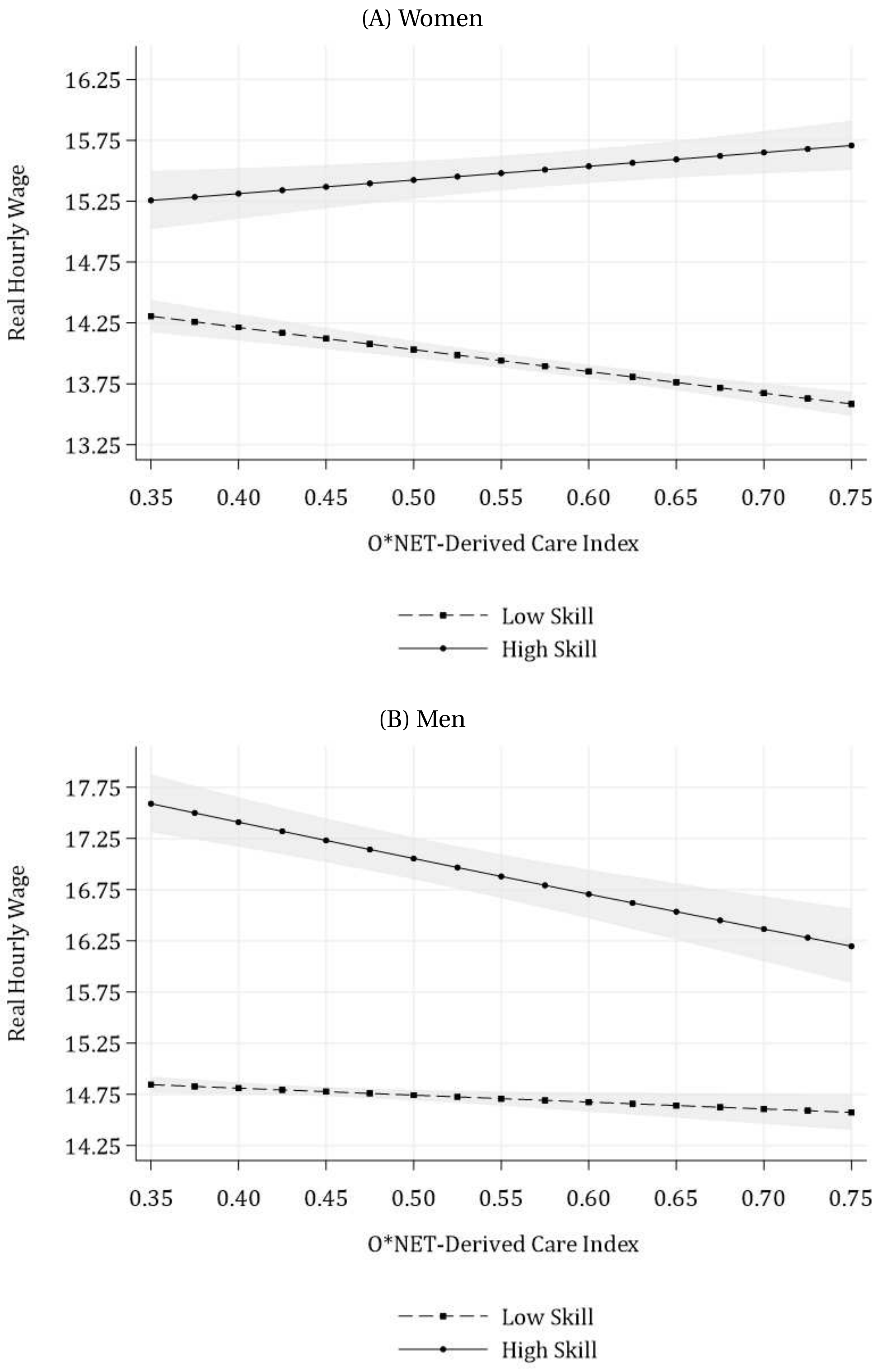

Figure 3.5: Estimated Care-Wage Gradients by Skill

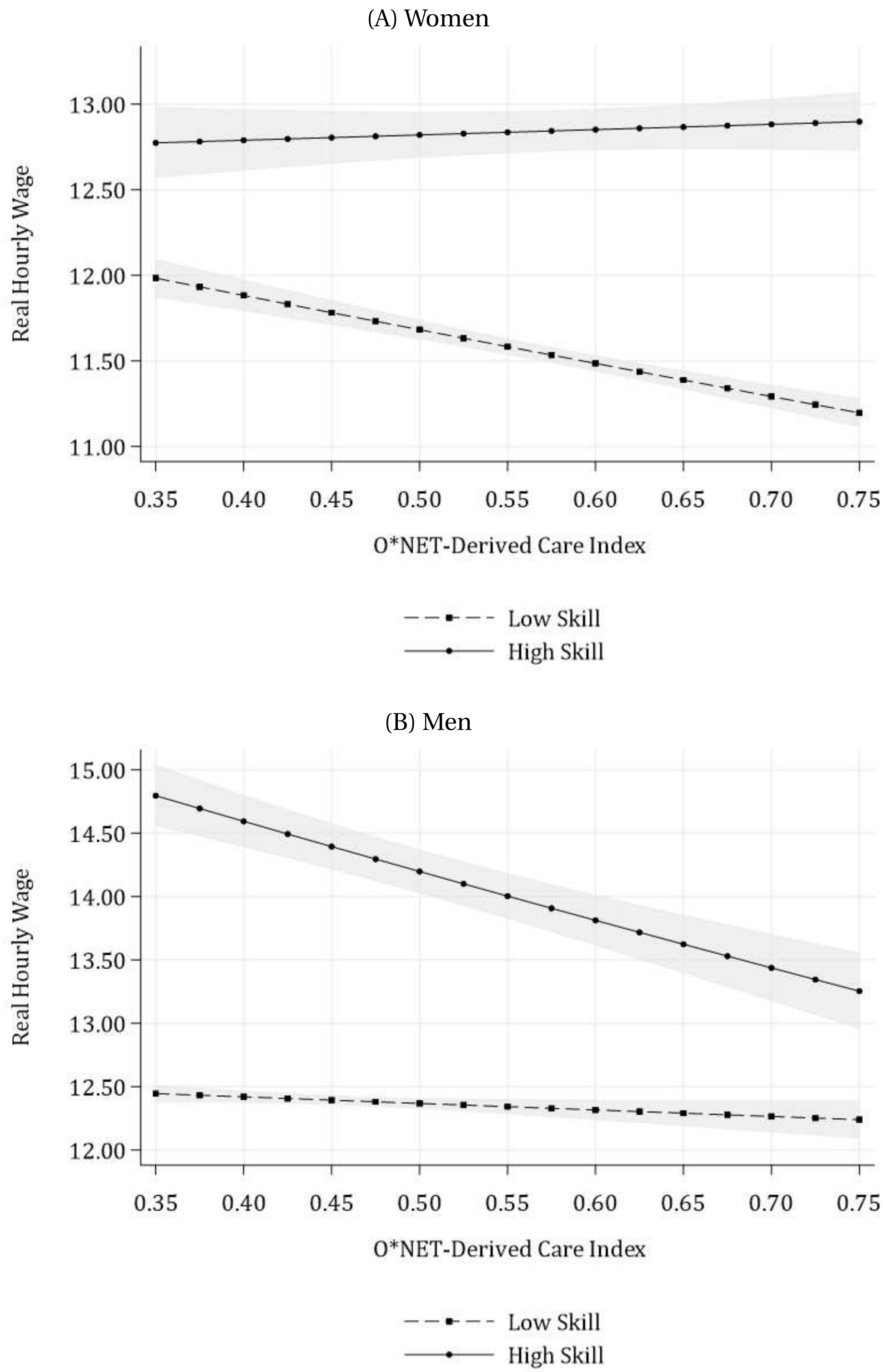

Figure 3.6: Estimated Care-Compensation Gradients by Skill

across the interdecile range of the *care index* is associated with a 12.1 percent reduction in hourly compensation, all else equal—see panel B of Figure 3.6.

Summarizing the results presented in this section, I find that estimated care penalties behave inconsistently across women and men in relatively low- and high-skilled occupations. Among low-skilled occupations, a movement across the interdecile range of the *care index* is associated with a 5.9 percent decrease in hourly wages and a 7.7 percent decrease in hourly compensation for women, all else equal. Among men in low-skilled occupations, however, this movement is associated with 2.1 percent decrease in real hourly wages and a 2 percent decrease in real hourly compensation, all else equal. Thus, estimated care penalties among workers in low-skill occupations appear to be larger for women than men. Alternatively, among high-skilled occupations, a movement across the interdecile range of the *care index* is associated with a 3.5 percent *increase* in hourly wages and a 1.1 percent *increase* in hourly compensation for women, all else equal. This stands in stark contrast to men in high-skilled occupations, where a similar movement in the *care index* is associated with 9.2 percent decrease in real hourly wages and a 12.1 percent decrease in real hourly compensation, all else equal. In other words, estimated care penalties among high-skill workers are much larger for men than women.

What explains the inconsistent behavior of the the estimated care penalties among men and women across relatively low- and high-skill occupations? First, why is the estimated care penalty larger for women in low-skill occupations relative to men? First, there could be differences in the particular jobs that women and men hold in the high-care, low-skill occupational space. For example, when I perform a similar analysis to the above, but use the nurturant care indicator in place of the *care index*, I find little difference in the estimated low-skill care penalty across men and women—see Appendix C Table C.9. The use of the nurturant care indicator ensures a comparison of similar care occupations across women and men. A second plausible explanation may be due to the fact that men are more likely to assume positions of authority and, therefore, realize higher earnings in female dominated occupations (Kraus & Yonay, 2000; Stojmenovska, Steinmetz, & Volker, 2021). Thus, a smaller care penalty. However, this is inconsistent with the behavior of the estimated care penalties in high-skill occupations, which are significantly larger for men.

Second, why is the estimated care penalty larger for men in high-skill occupations relative to women? Similar to the argument presented above, there could be differences in the particular jobs that women and men hold in the low-care, high-skill or high-care, high-skill occupational space. The results presented in Appendix C Table C.9 suggest that, if this is the case, it is more likely to be driven by differences in the low-care, high-skill occupational space. Another potential explanation of the care premium among women versus a care penalty among men in high-skill occupations is selection via occupational segregation, or sorting. Women in high-skill occupations tend to sort into high-care occupations, while men in high-skill occupations, at least in the NLSY97 sample, are equally dispersed into low and high care—see Table 3.6. Relatively skilled, productive women that would otherwise realize high earnings in low-care jobs may gravitate more towards high-care occupations relative to their male counterparts. Thus, differences in unobserved, wage-relevant characteristics across men and women may vary in low-care, high-skill versus high-care, high-skill occupations.

3.6 Discussion and Conclusion

In this chapter, I build upon and update the work of England et al. (2002), Hirsch and Manzella (2015), and Budig et al. (2019) by using the National Longitudinal Survey of Youth 1997 (NLSY97, Waves: 1997-2017) to estimate the impact of care work on wages. The empirical analysis undertaken shows that, whether measured categorically or continuously, nurturant care work is associated with a earnings penalty. Average wage penalties for nurturant care work are on the order of 3.8 to 6.1 percent among women and 6.5 to 10.3 percent among men. These estimated care penalties are net of several plausible confounding factors, such as the occupational skill requirements, the concentration of women in caring occupations, unobserved individual characteristics, as well as a number of other personal- and job-specific characteristics. I find no evidence of a wage penalty for reproductive care work, all else equal. I also find that average wage penalties for nurturant care work mask the presence of heterogeneous wage penalties at the intersections of gender and occupational skill. Specifically, estimated care penalties among workers in low-skill occupations appear to be larger for women than men, while estimated care penalties among high-skill workers are much larger for men than women.

The results of this paper generate a number of questions for future research. First, much of the empirical research on the relative pay of care work has relied on longitudinal data. This has benefits since one can employ fixed effects methods that account for unobserved, wage-relevant characteristics that may correlate with selection into caring labor. Longitudinal data also allow one to observe and control for labor supply and employment history. The downfall of these data, however, is that they are often relatively small samples that may not be occupationally representative of the broader economy. Additionally, these data limit ones ability to examine changes in the relative pay of care work over time. Previous research, as well as the results presented above, suggests that non-control for (1) labor supply and employment history and (2) unobserved, wage-relevant characteristics have some impact on estimated care penalties. With this in mind, an investigation into the relative pay of care work that makes use of larger, more historical data may provide insight on longer-term trends. For instance, how does the care penalty behave in time? And, with respect to trends over time, what is the relative contribution of changes in care versus non-care wages?

With respect to the heterogeneous wage penalties observed at the intersections of gender and occupational skill, the use of more age-representative and occupationally representative data may produce different results to those found in this study. The results presented in this study might also be driven by the particular measure of “skill" derived from the O*NET job descriptors. Another promising area for future research would be the investigation of these heterogeneous care penalties over time. For instance, among women, where do low- and high-skill nurturant care occupations tend to land in the wage distrubution? How does this compare to men? And how, if at all, has this changed in time? Work in this direction would also need to examine how women and men sort into care and non-care occupations, as well as the behavior of this sorting in time.

Lastly, this study shows the continued presence of wage penalties for care work, all else constant. As stated in the introduction to this chapter, the question of whether workers in caring labor receive a care penalty is important for a number of reasons including gender equity in labor market outcomes, increasing polarization in the U.S. labor market, and the quality of market care substitutes. Thus, the existence of care penalties in the labor market constitute a social problem (Braunstein et al., 2020; England et al., 2002). With this in mind, the Build Back Better legislation proposed by the current administration attempts to directly address the relative devaluation of care and care work. Policies outlined in this legislation would provide grants to states and other organizations for the purpose of promoting the recruitment, education and training, retention, and career advancements of direct care workers. These appropriated funds would be used to provide competitive wages, benefits, and other support services to the direct care workforce. The continued presence of wage penalties among nurturant care workers—i.e., direct care workers—provides evidence in support of such policy actions.

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Appendix A

Chapter 1: Supplementary Tables and Figures

Table A.1: The Distribution of Country-Year Observations Across Sample OECD Countries

Country	*N*	% of Sample
Australia	52	5.18
Austria	48	4.79
Belgium	56	5.58
Canada	56	5.58
Denmark	49	4.89
Finland	56	5.58
France	55	5.48
Greece	33	3.29
Iceland	23	2.29
Ireland	50	4.99
Italy	55	5.48
Japan	56	5.58
Netherlands	41	4.09
New Zealand	54	5.38
Norway	54	5.38
Portugal	43	4.29
Spain	46	4.59
Sweden	53	5.28
Switzerland	11	1.10
United Kingdom	56	5.58
United States	56	5.58
Total	1,003	100

Sources: HMD, 1960-2015; OECD ALFS 1960-2015.
Notes: Countries were included in the sample if mortality and unemployment rate data were observed at least 30 times over the 1960-2015 period.

Table A.2: Estimated Effects of the Employment-Population Ratio on *Ln* Mortality Rate

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A: Dependent variable is the natural log of the 0- to 4-year-old mortality rate								
Employment-Population Ratio (%)	0.015***	0.019***	0.033***	0.027***	0.033***	0.029***	0.016***	0.030***
	(0.003)	(0.001)	(0.004)	(0.003)	(0.005)	(0.005)	(0.003)	(0.002)
Panel B: Dependent variable is the natural log of the 5- to 9-year-old mortality rate								
Employment-Population Ratio (%)	0.001	0.003	0.004	0.004	0.005	-0.002	0.004	-0.007
	(0.002)	(0.002)	(0.003)	(0.004)	(0.005)	(0.005)	(0.006)	(0.005)
N	980	980	980	980	980	980	980	980
Countries	21	21	21	21	21	21	21	21
Country FEs	X	X	X	X	X	X	X	X
Year FEs	X	X	X	X	X	X	X	
Controls:								
Ln GDP per Capita		X	X	X	X	X	X	X
Female LFPR (%)			X	X	X	X	X	X
Govt. Consumption (% of GDP)				X	X	X	X	X
AR(1)					X			
Country-Specific AR(1)						X		
Country-Specific Time Trend							X	X

Sources: HMD, 1960-2015; OECD ALFS, 1960-2015; PWT, 1960-2015.
Notes: Panel corrected standard errors in parentheses. Levels of statistical significance are indicated by * $p < 0.1$, ** $p < 0.05$, and *** $p < 0.01$.

Table A.3: Formal early childhood care and education (ECCE) enrollment rates (%) in 2016, by country and age group

Country	0- to 2-year-olds	3-year-olds	4-year-olds	5-year-olds
Australia	39.0	64.3	90.8	100.0
Austria	20.2	75.9	92.5	96.9
Belgium	59.8	98.6	98.9	98.8
Denmark	61.8	96.6	97.7	97.5
Finland	30.5	73.0	79.3	84.5
France	56.7	99.4	100.0	100.5
Greece	11.5	27.1	65.4	93.7
Iceland	59.7	96.9	97.5	97.9
Ireland	16.6	48.5	89.9	92.2
Italy	35.5	92.4	95.9	96.4
Japan	22.5 (a)	84.0	94.7	96.0
Netherlands	55.9	88.3	96.0	99.2
New Zealand	42.7	88.7	92.6	98.0
Norway	55.3	95.8	97.0	97.3
Portugal	36.3	82.8	90.4	94.8
Spain	34.8	96.2	96.4	98.1
Sweden	46.5	94.6	93.7	94.6
Switzerland	38.0 (b)	2.5	48.1	98.5
United Kingdom	31.5	100.0	100.0	97.5
United States	28.0 (c)	38.4	66.7	91.2

Source: OECD Family Database "PF3.2 Enrolment in childcare and pre-school" (OECD, 2019b).
Notes: (a) Data from 2015, (b) data from 2014, and (c) data from 2011. There are no data on formal ECCE enrollments for Canada.

Table A.4: Relative Generosity of Formal ECCE Enrollments by Age Group

0- to 2-year-olds		3-year-olds	
"*Ungenerous*" 11 to 36.3%	"*Generous*" 37.9 to 61.8%	"*Ungenerous*" 2 to 88.2%	"*Generous*" 88.7 to 100%
Greece	Switzerland	Switzerland	New Zealand
Ireland	Australia	Greece	Italy
Austria	New Zealand	United States	Sweden
Japan	Sweden	Ireland	Norway
United States	Norway	Australia	Spain
Finland	Netherlands	Finland	Denmark
United Kingdom	France	Austria	Iceland
Spain	Iceland	Portugal	Belgium
Italy	Belgium	Japan	France
Portugal	Denmark	Netherlands	United Kingdom
4-year-olds		**5-year-olds**	
"*Ungenerous*" 48 to 93.7%	"*Generous*" 94 to 100%	"*Ungenerous*" 84 to 97.3%	"*Generous*" 97.4 to 100%
Switzerland	Japan	Finland	United Kingdom
Greece	Italy	United States	Denmark
United States	Netherlands	Ireland	Iceland
Finland	Spain	Greece	New Zealand
Ireland	Norway	Sweden	Spain
Portugal	Iceland	Portugal	Switzerland
Australia	Denmark	Japan	Belgium
Austria	Belgium	Italy	Netherlands
New Zealand	United Kingdom	Austria	Australia
Sweden	France	Norway	France

Notes: This table of country groupings is produced using the data provided in Table A.3. For each age group shown in Table A.3, countries that fall below the median value of formal ECCE enrollments are grouped into the relatively "Ungenerous" category, while countries above the median are grouped in to the relatively "Generous" category.

Table A.5: Relative Generosity of Paid Leave Policy, 1970-2015

Country	% of year observations where weeks of paid leave is above the sample median:
Austria	100.0
Sweden	100.0
Canada	97.9
Italy	97.8
Denmark	95.7
Finland	91.3
France	88.9
United Kingdom	87.0
Norway	86.3
Belgium	73.9
Iceland	73.6
Spain	58.7
Japan	50.0
Netherlands	34.1
Greece	30.3
Portugal	26.2
Ireland	4.4
Australia	2.1
New Zealand	0.0
Switzerland	0.0
United States	0.0

Notes: Data on weeks of mandated matertinty, paternity, and parental paid leave over the 1970-2015 period are drawn from the OECD Family Database (OECD, 2019b). In each year of the 1970-2015 period we calculate the median number of weeks with mandated paid maternity, paternity, and parental leave for the sample. Based on these yearly median values, we indicate whether a country is above or below "median generosity" for that year. For each country in the sample, we then calculate the share of observed years that their paid leave policy was above "median generosity". Countries that were over the yearly median for the majority of their observation years (≥ 50%) are labeled as relatively "Generous" while countries that were under the yearly median for the majority of their observation years are labeled relatively "Ungenerous". The cutoff is given by the dotted line in the table.

Table A.6: Relative generosity of paid leave policy, 1970-2015

Country	% of year observations where per capita public spending on the family is above the sample median:
Denmark	100.0
Finland	100.0
Norway	100.0
Sweden	100.0
Austria	96.4
Belgium	91.7
France	82.9
Iceland	82.2
United Kingdom	75.0
Australia	66.7
Ireland	40.0
New Zealand	38.2
Netherlands	27.8
Canada	0.0
Greece	0.0
Italy	0.0
Japan	0.0
Portugal	0.0
Spain	0.0
Switzerland	0.0
United States	0.0

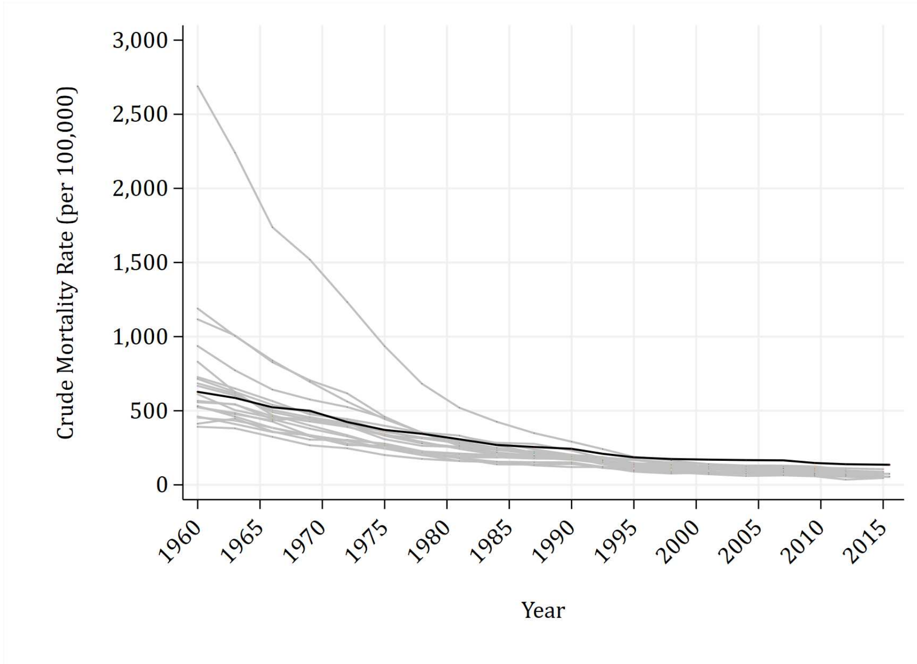
Notes: Per capita public expenditure on the family over the 1980-2015 period are drawn from the OECD Social Expenditure Database (OECD, 2019c). Similar to the ranking of paid leave generosity in Table A.5, in each year of the 1970-2015 period we calculate the median level of per capita public expenditure on the family for the sample. Again, based on these yearly median values, we indicate whether a country is above or below "median generosity" for that year. For each country in the sample, we then calculate the share of observed years that their public spending on families was above "median generosity". Countries that were over the yearly median for the majority of their observation years (≥50%) are labeled as relatively "Generous" while countries that were under the yearly median for the majority of their observation years are labeled relatively "Ungenerous". The cutoff is given by the dotted line in the table.

Table A.7: A Composite Measure of Care Policy Environment

Country	Formal ECCE enrollments (0- to 2-year-olds)	Formal ECCE enrollments (3- to 5-year-olds)	Paid leave policy (Table 12)	Per capita public spending on families (Table 14)	Care policy PCA score
Switzerland	38.0	49.8	0.0	0.0	-2.80
Greece	11.5	63.1	30.3	0.0	-2.72
United States	28.0	65.6	0.0	0.0	-2.49
Ireland	16.6	77.2	4.4	40.0	-1.87
Japan	22.5	91.8	50.0	0.0	-1.00
Portugal	36.3	89.5	26.2	0.0	-0.96
Australia	39.0	85.0	2.1	66.7	-0.60
New Zealand	42.7	93.2	0.0	38.2	-0.51
Spain	34.8	96.9	58.7	0.0	-0.31
Canada	(39.1)	(87.9)	97.9	0.0	-0.03
Italy	35.5	94.9	97.8	0.0	0.13
Netherlands	55.9	94.6	34.1	27.8	0.26
Finland	30.5	79.0	91.3	100.0	0.42
Austria	20.2	90.3	100.0	96.4	0.62
United Kingdom	31.5	100.0	87.0	75.0	0.93
Iceland	59.7	97.4	73.6	82.2	1.62
Sweden	46.5	95.9	100.0	100.0	1.69
Belgium	59.8	98.7	73.9	91.7	1.79
Norway	55.3	96.7	86.3	100.0	1.82
France	56.7	100.0	88.9	82.9	1.83
Denmark	61.8	97.6	95.7	100.0	2.18

Notes: Formal ECCE enrollment rates for Canada are imputed using the sample mean. Principal component analysis (PCA) was performed on the four measures shown in the table and the “Care policy PCA score” is generated using the only the first principle component, which accounts for 62% of the variation in the data. With respect to the ranking of countries in the sample, PCA performs nearly identically to a sum of z-scores analysis—the only change being that Greece moves above Switzerland as the least generous country.

(A) 0- to 4-year-olds



(B) 5- to 9-year-olds

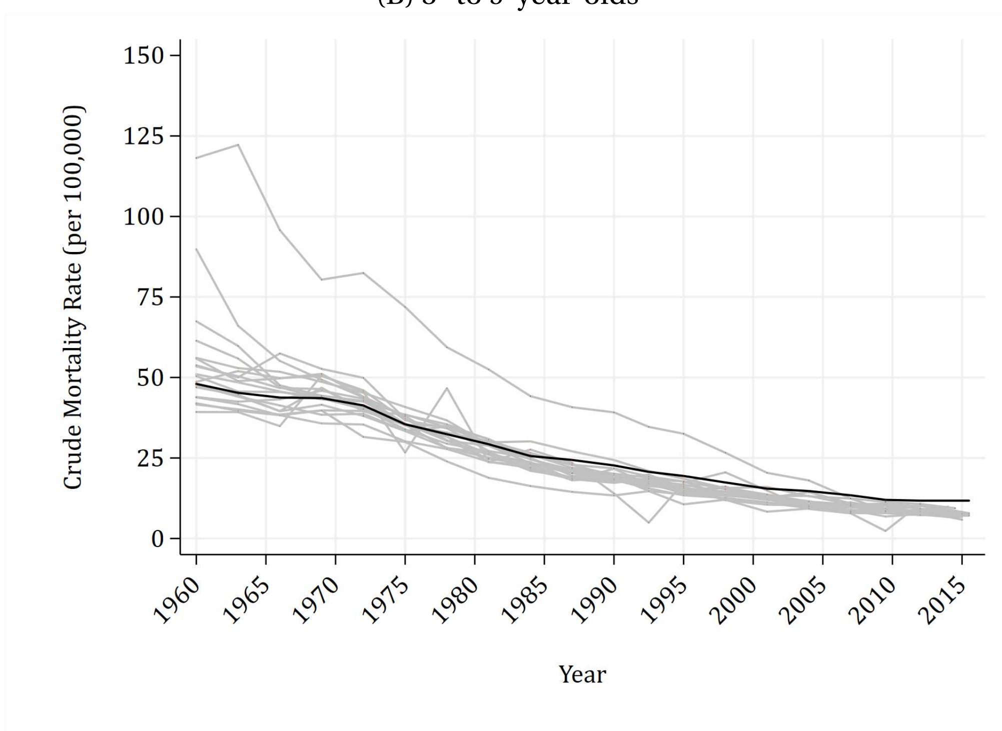


Figure A.1: Age-Specific Mortality Rates (per 100,000) Across OECD Sample

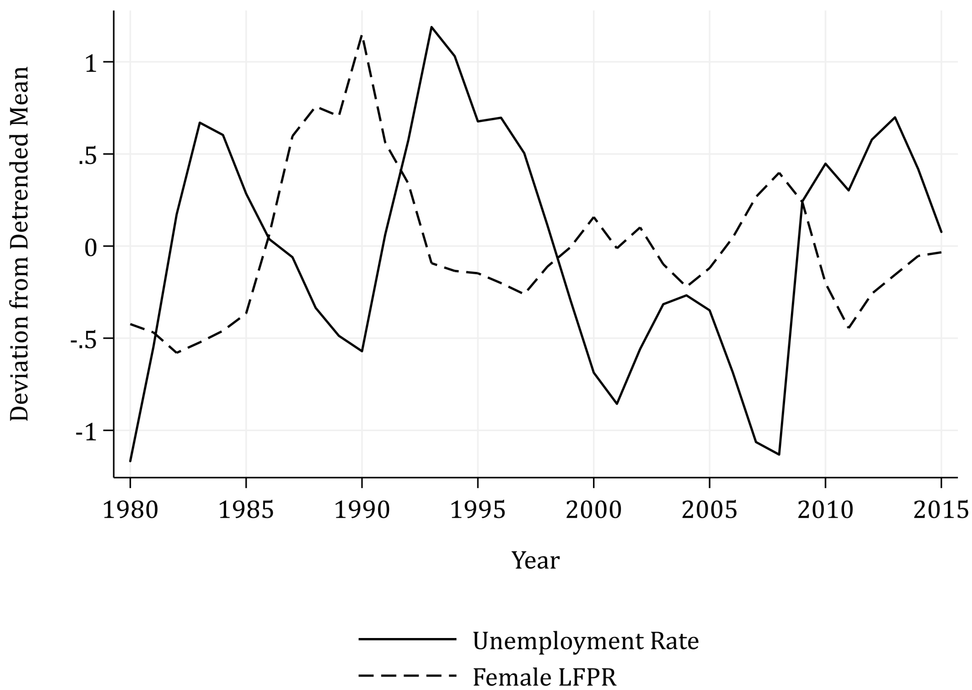


Figure A.2: Covariation Between Unemployment Rate and Female Labor Force Participation Rate

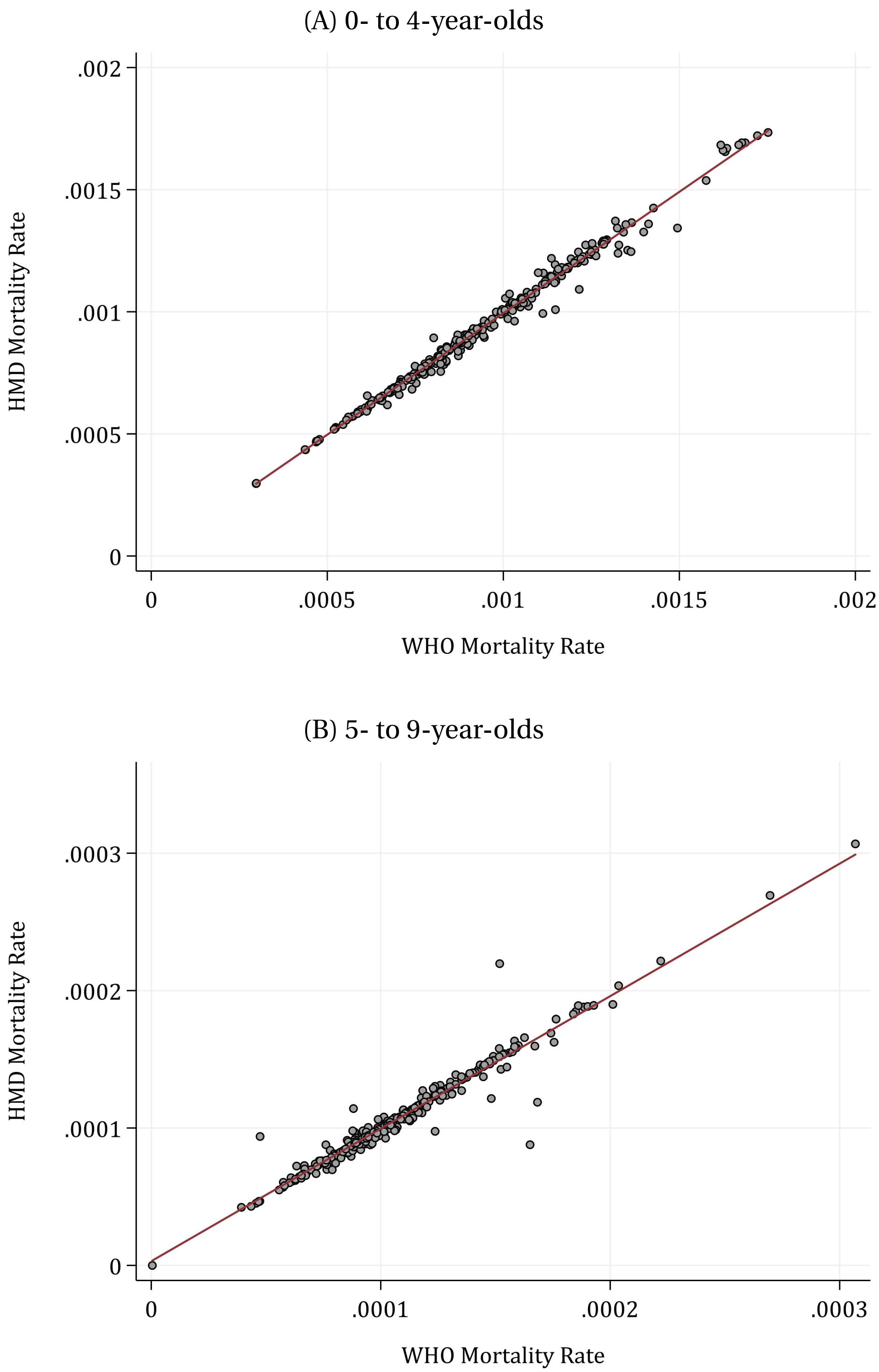


Figure A.3: Comparing 1994-2015 HMD and WHO Mortality Rates

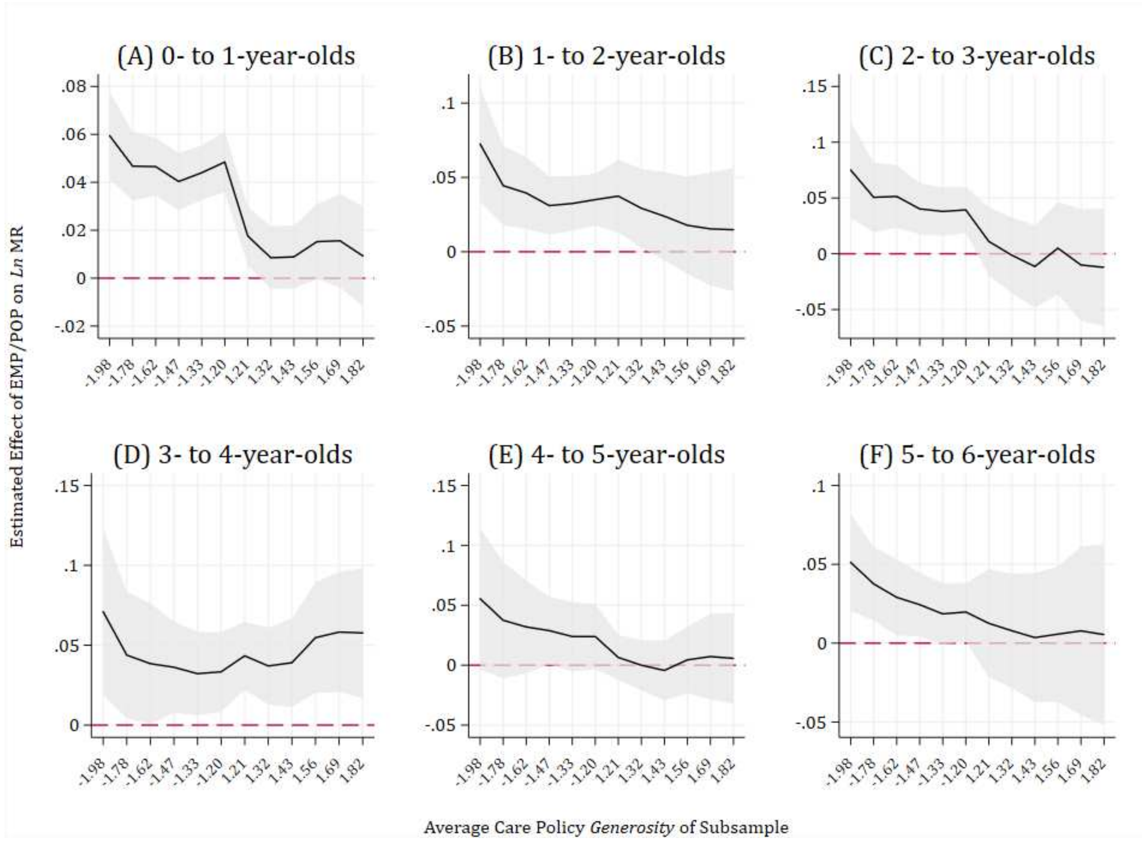


Figure A.4: Estimates of Procyclical Child Mortality Across Care Policy Environments via the Employment-Population Ratio

Appendix B

Chapter 2: Supplementary Tables and Figures

Table B.1: ERS 1990 Commuting Zones Included in Sample

ERS 1990 Commuting Zone	Largest City in Commuting Zone	ERS 1990 Commuting Zone	Largest City in Commuting Zone
900	*Charlotte, NC*	20500	*Boston, MA*
2000	*Virginia Beach, VA*	20901	*Bridgeport city, CT*
3300	*New Orleans, LA*	21501	*Minneapolis, MN*
5202	*Memphis, TN*	24100	*Milwaukee, WI*
6700	*Tampa, FL*	24300	*Chicago, IL*
7000	*Miami, FL*	24701	*St. Louis, MO*
9100	*Alanta, GA*	28900	*Denver, CO*
10700	*Birmingham, AL*	29502	*Kansas City, MO*
11302	*Baltimore, MD*	30402	*Tulsa, OK*
11304	*Arlington, VA**	31301	*San Antonio, TX*
11600	*Detroit, MI*	31600	*Brownsville, TX*
12200	*Grand Rapids, MI*	32000	*Houston, TX*
12501	*Dayton, OH*	33000	*Fort Worth, TX*
12701	*Cincinnati, OH*	33100	*Dallas, TX*
13101	*Louisville, KY*	33803	*Oklahoma City, OK*
14200	*Indianapolis, IN*	35001	*Phoenix, AZ*
15200	*Cleveland, OH*	36100	*Salt Lake City, UT*
15900	*Columbus, OH*	37200	*Fresno, CA*
16300	*Pittsburgh, PA*	37400	*Sacramento, CA*
17700	*Syracuse, NY*	37500	*San Jose, CA*
18000	*Buffalo, NY*	37800	*San Francisco, CA*
19400	*New York, NY*	38000	*San Diego, CA*
19600	*Newark, NJ*	38300	*Los Angeles, CA*
19700	*Philadelphia, PA*	38801	*Portland, OR*
20401	*Providence, RI*	39400	*Seattle, WA*

Source: USDA ERS 1990 Commuting Zones.
Notes: *Includes District of Columbia.

Table B.2: Full Model Results for Estimating the Impact of Head Start Funding on All-Cause Child Mortality

	(1)	(2)	(3)	(4)	(5)	(6)
Ln(HS funding per AEC)	-0.040**	-0.055***	-0.055***			
× (3- and 4-year-olds)	(0.017)	(0.016)	(0.019)			
Ln(HS funding per AEC)	0.035	0.062**	0.067**			
	(0.026)	(0.027)	(0.031)			
HS funding per AEC				-0.032**	-0.044***	-0.048***
× (3- and 4-year-olds)				(0.015)	(0.014)	(0.017)
HS funding per AEC				0.023	0.021	0.021
				(0.018)	(0.018)	(0.019)
3- and 4-year-olds	-0.597***	-0.592***	-0.593***	-0.387***	-0.305***	-0.306***
	(0.021)	(0.020)	(0.023)	(0.109)	(0.099)	(0.118)
Real Personal Income	0.006**	0.002	0.005	0.006**	0.003	0.005*
per Capita (10,000s)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)
Unemployment Rate (%)	-0.007	-0.008	-0.01	-0.007	-0.008	-0.009
	(0.005)	(0.006)	(0.007)	(0.005)	(0.006)	(0.007)
Share Female (%)	0.033	-0.001	-0.022	0.031	-0.005	-0.026
	(0.029)	(0.032)	(0.041)	(0.029)	(0.032)	(0.041)
Share Black (%)	0.008**	0.009***	0.010**	0.008**	0.009***	0.010**
	(0.003)	(0.003)	(0.004)	(0.003)	(0.003)	(0.004)
Share Non-White, Non-Black (%)	-0.031***	-0.028***	-0.033***	-0.031***	-0.029***	-0.032***
	(0.007)	(0.007)	(0.008)	(0.007)	(0.007)	(0.008)
CZs	50	40	30	50	40	30
N	2,500	2,000	1,500	2,500	2,000	1,500
CZ FEs	X	X	X	X	X	X
Year FEs	X	X	X	X	X	X
Economic Controls	X	X	X	X	X	X
Demographic Controls	X	X	X	X	X	X
Age-CZ PCSE	X	X	X	X	X	X

Source: CFFR, 1983-2007; NCHS NVSS, 1983-2007; SEER, 1983-2007; BEA CAINC30, 1983-2007, BLS LAUS, 1983-2007.
Notes: Panel corrected standard errors (PCSE) adjust for heteroskedastic and contemporaneously correlated disturbances across panels (age-CZ). Standard errors in parentheses. Levels of statistical significance given by * $p < 0.1$, ** $p < 0.05$, and *** $p < 0.01$.

Table B.3: State Preschool versus Head Start Enrollments Among Sample States as of 2003

State	Initial Year of State Program (Funding)	% of 3-year-olds enrolled in: State PreK	% of 3-year-olds enrolled in: Head Start	% of 4-year-olds enrolled in: State PreK	% of 4-year-olds enrolled in: Head Start	(1)	(2)
Wisconsin	1873	1	8	19	9		X
D.C.	1960	20	23	44	21	X	X
California	1965	5	6	11	14		
New York	1966	2	8	25	10		X
Maryland	1979	0	5	15	8		X
Oklahoma	1980	0	12	56	15		X
South Carolina	1984	1	11	29	10		X
Texas	1984	6	8	39	10		X
Illinois	1985	8	8	22	10	X	X
Massachusetts	1985	12	4	12	8	X	X
Michigan	1985	0	10	19	12		X
Washington	1985	2	4	7	8		
Oregon	1987	3	6	6	12		
Colorado	1988	1	6	14	9		X
Louisiana	1988	0	13	5	16		
Kentucky	1990	7	11	24	17	X	X
Ohio	1990	3	10	5	13		
Arizona	1991	0	5	6	11		
Georgia	1995	0	10	33	9		X
Virginia	1995	0	5	6	8		
Connecticut	1997	3	6	10	7		
Kansas	1998	0	8	6	9		
New Jersey	1998	15	5	24	7	X	X
Missouri	1998	3	8	5	11		
Tennessee	1998	1	7	2	13		
Alabama	2000	0	9	1	17		
North Carolina	2001	0	6	1	10		
Florida		0	6	0	10		
Indiana		0	5	0	8		
Minnesota*		1	6	2	9		
Mississippi		0	26	0	38		
Pennsylvania**		0	7	2	10		
Rhode Island		0	6	0	16		
Utah		0	3	0	9		
	Average:	2.76	8.26	13.24	11.88		
	Average excluding (1):	1.10	7.93	11.17	11.76		
	Average excluding (2):	1.05	7.60	3.75	12.45		

Source: State profiles provided by the National Institute for Early Education Research (NIEER) "State of Preschool Yearbook 2003". See `https://nieer.org/state-preschool-yearbooks/state-preschool-2003`.

Notes: Minnesota (*) and Pennsylvania (**) had no state-run program, but a limited number of local school districts offered some preschool programming.

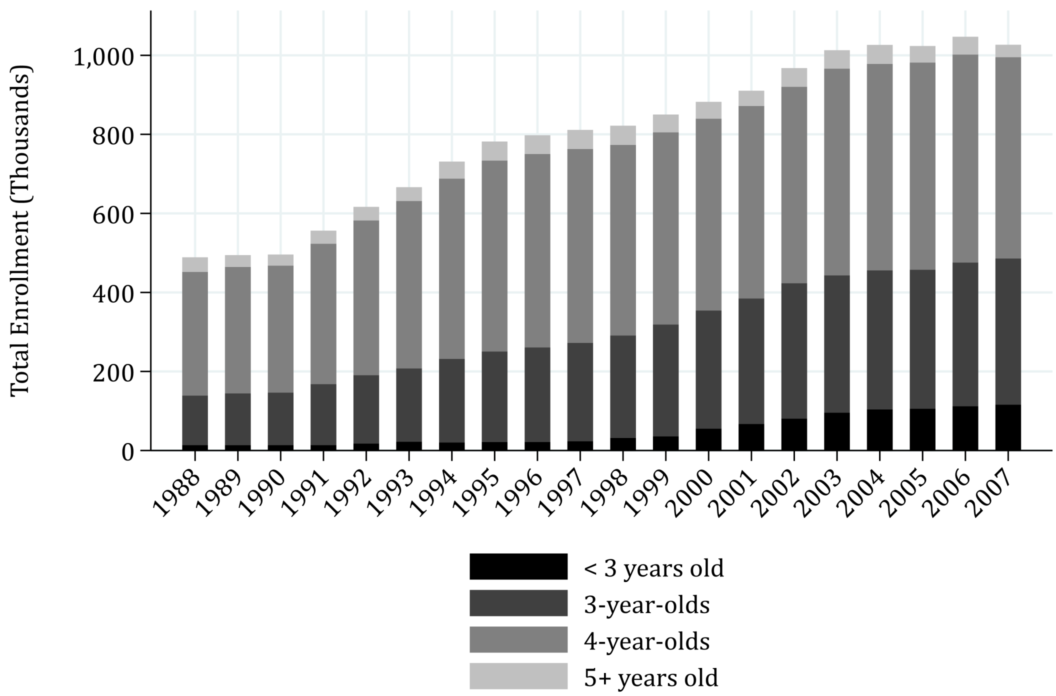


Figure B.1: Head Start Enrollments by Age Group, 1988-2007

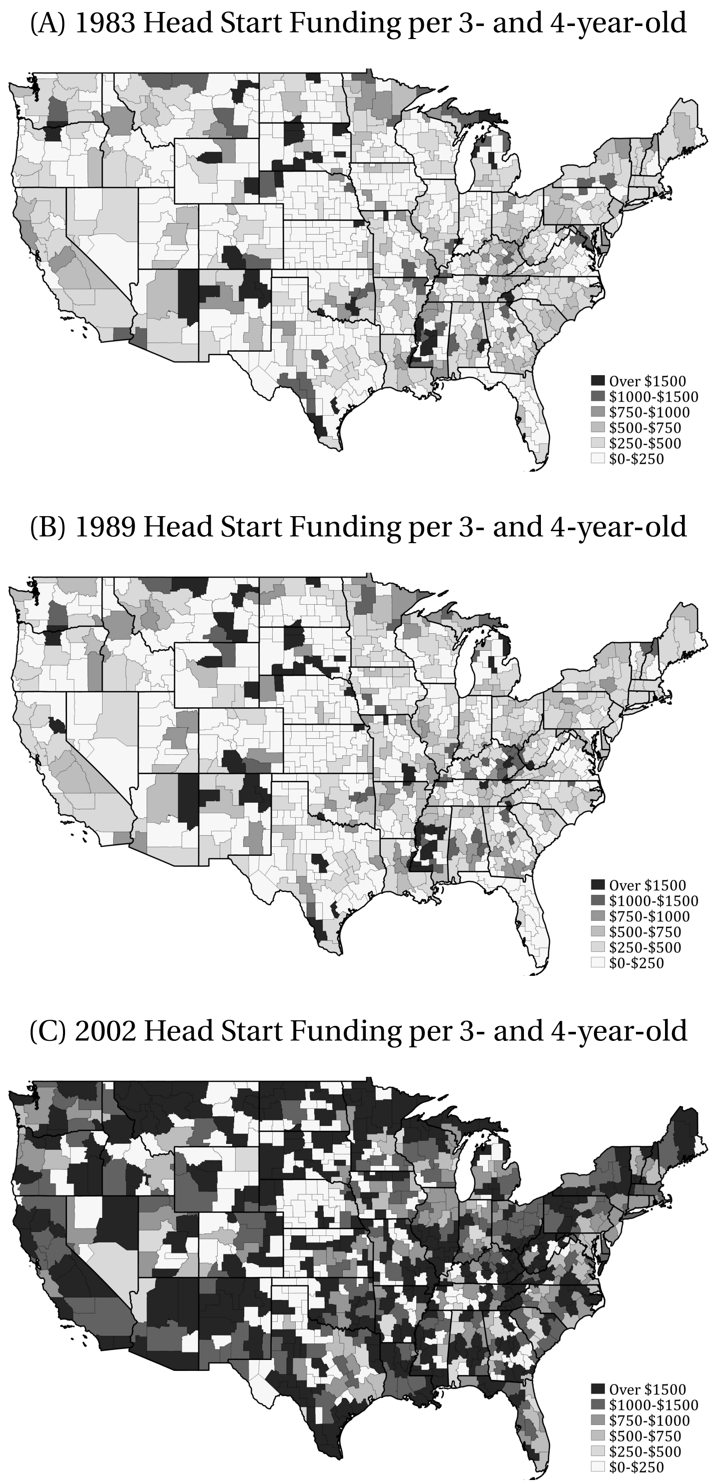


Figure B.2: Expansion of Head Start Funding Across All CZs in Contiguous U.S.

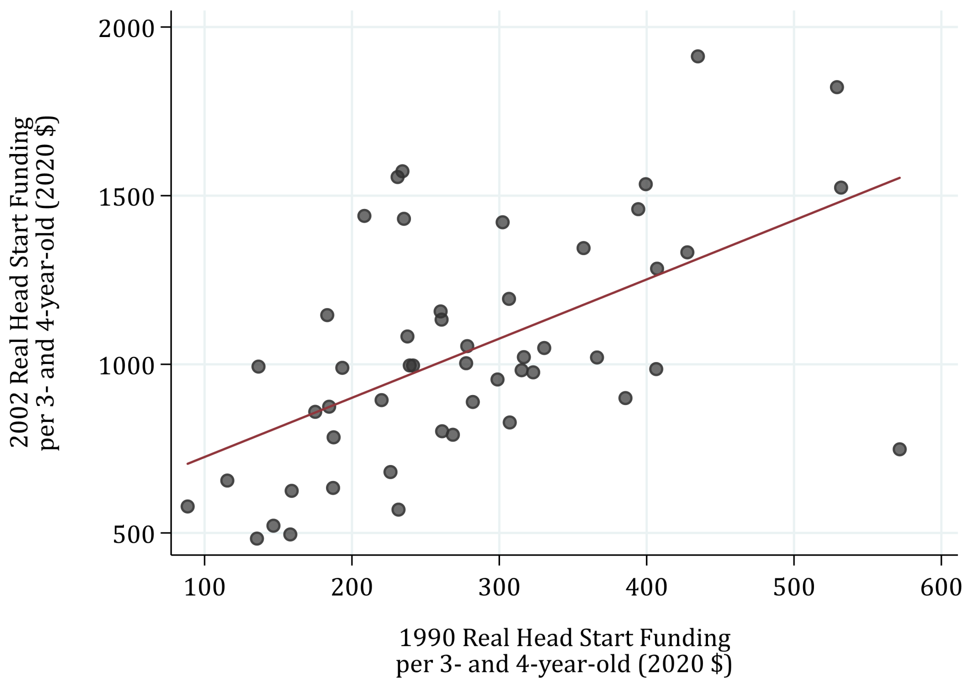


Figure B.3: Expansion of Head Start Funding Across Sample CZs from 1989 to 2002

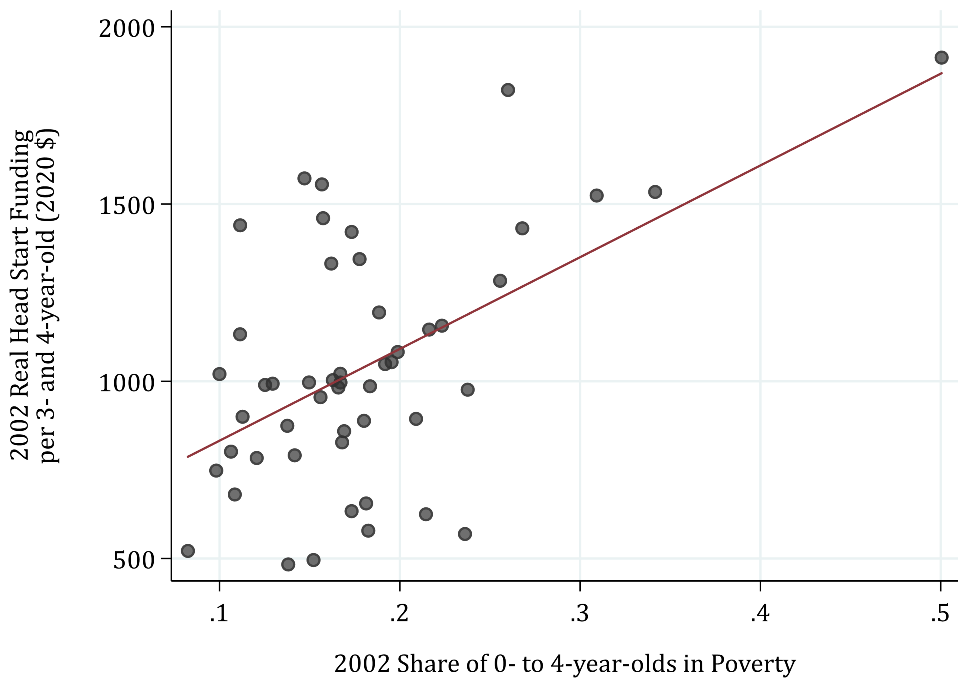
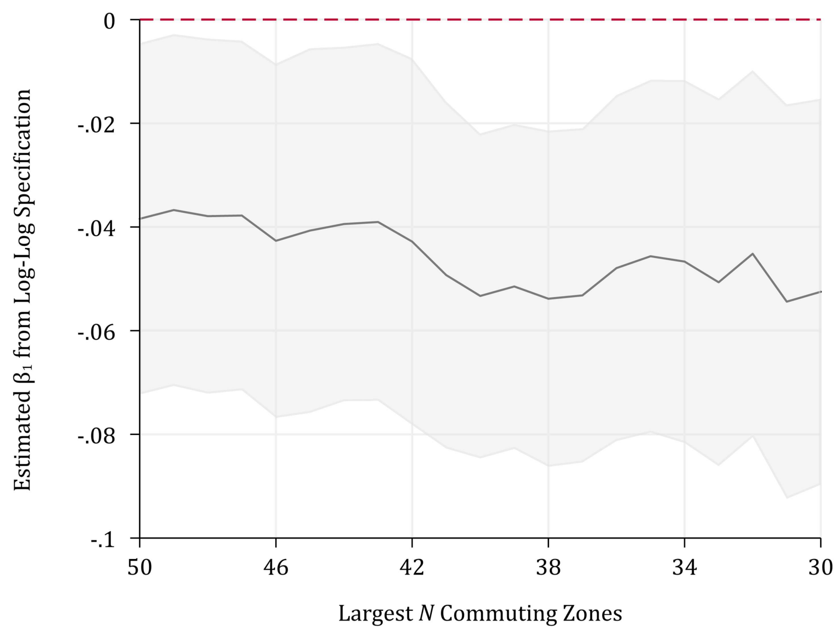


Figure B.4: Heterogeneous Head Start Funding Across Similarly Poor CZs by 2002

(A) Difference Estimate ($\hat{\beta}_1$) from Log-Log Specification



(B) Difference Estimate ($\hat{\beta}_1$) from Log-Linear Specification

Figure B.5: Impact of CZ Population Restrictions on Estimated Mortality Benefits of Head Start

Appendix C

Chapter 3: Supplementary Tables and Figures

Table C.1: List of NLSY97 Occupations (2002 Census Codes) Coded as Nurturant

NLSY97 Occupation	Code	NLSY97 Occupation	Code
Counselors, Social, and Religious Workers		Registered Nurses	3130
Counselors	2000	Audiologists	3140
Social Workers	2010	Occupational Therapists	3150
Miscellaneous Community and Social Service Specialists	2020	Physical Therapists	3160
Clergy	2040	Radiation Therapists	3200
Directors, Religious Activities and Education	2050	Recreational Therapists	3210
Religious Workers, nec	2060	Respiratory Therapists	3220
Teachers		Speech-Language Pathologists	3230
Postsecondary Teachers	2200	Therapists, All Other	3240
Preschool and Kindergarten Teachers	2300	Health Diagnosing and Treating Practitioners, All Other	3260
Elementary and Middle School Teachers	2310	***Health Care Technical and Support Occupations***	
Secondary School Teachers	2320	Dental Hygienists	3310
Special Education Teachers	2330	Emergency Medical Technicians and Paramedics	3400
Other Teachers and Instructors	2340	Health Diagnosing and Treating Practitioner Support Technicians	3410
Education, Training, and Library Workers		Licensed Practical and Licensed Vocational Nurses	3500
Librarians	2430	Opticians, Dispensing	3520
Teacher Assistants	2540	Healthcare Practitioners and Technical Occupations, nec	3540
Health Diagnosing and Treating Practitioners		Nursing, Psychiatric, and Home Health Aides	3600
Chiropractors	3000	Occupational Therapist Assistants and Aides	3610
Dentists	3010	Physical Therapist Assistants and Aides	3620
Dietitians and Nutritionists	3030	Massage Therapists	3630
Optometrists	3040	Dental Assistants	3640
Pharmacists	3050	Medical Assistants and Other Healthcare Support Occupations, nec	3650
Physicians and Surgeons	3060	***Personal Care and Service Workers***	
Physician Assistants	3110	Child Care Workers	4600
Podiatrists	3120	Personal and Home Care Aides	4610

Table C.2: List of NLSY97 Occupations (2002 Census Codes) Coded as Reproductive Care Work

NLSY97 Occupation	Code	NLSY97 Occupation	Code
Food Preparation and Serving Related Occupations		***Building and Grounds Cleaning and Maintenance***	
Food Preparation Workers	4030	First-Line Supervisors of Housekeeping and Janitorial Workers	4200
Bartenders	4040	Janitors and Building Cleaners	4220
Combined Food Preparation and Serving Workers, Including Fast Food	4050	Maids and Housekeeping Cleaners	4230
Counter Attendant, Cafeteria, Food Concession, and Coffee Shop	4060	***Personal Care and Service Workers***	
Waiters and Waitresses	4110	Barbers	4500
Food Servers, Nonrestaurant	4120	Hairdressers, Hairstylists, and Cosmetologists	4510
Food preparation and serving related workers, nec	4130	Miscellaneous Personal Appearance Workers	4520
Dishwashers	4140	***Production***	
		Laundry and Dry-Cleaning Workers	8300

Table C.3: NLSY97 Occupations (2002 Census Codes) That Were Hand-Matched to O*NET Occupations (2019 SOC Codes)

NLSY97 Occupation	Code	O*NET SOC Occupation	Code
Legislators	30	Chief Executives	11-1011.00
Public Relations Managers	60	Advertising and Promotions Managers Administrative Services Managers	11-2011.00 11-3012.00
Financial and Investment Analysts	840	Personal Financial Advisors	13-2052.00
Network Systems and Data Communications Analysts	1110	Computer Network Support Specialists	15-1231.00
Entertainers and Performers, Sports and Related Workers, All Other	2760	Musicians and Singers Dancers Athletes and Sports Competitors	27-2042.00 27-2031.00 27-2021.00
Media and Communication Equipment Workers, All Other	2960	Audio and Video Technicians Sound Engineering Technicians Camera Operators, Television, Video, and Film	27-4011.00 27-4014.00 27-4031.00
Emergency Medical Technicians and Paramedics	3400	Firefighters	33-2011.00
Medical Records and Health Information Technicians	3510	Medical Transcriptionists	31-9094.00
First-Line Supervisors of Protective Service Workers, All Other	3730	First-Line Supervisors of Correctional Officers First-Line Supervisors of Police and Detectives First-Line Supervisors of Firefighting and Prevention Workers	33-1011.00 33-1012.00 33-1021.00
Counter Attendant, Cafeteria, Food Concession, and Coffee Shop	4060	Fast Food and Counter Workers Baristas	35-3023.00 35-3023.01
Personal Care Aides	4610	Childcare Workers Nannies	39-9011.00 39-9011.01
Personal Care and Service Workers, All Other	4650	Childcare Workers Nannies Exercise Trainers and Group Fitness Instructors	39-9011.00 39-9011.01 39-9031.00
Sales and Related Workers, All Other	4960	Telemarketers Door-to-Door Sales Workers, News and Street Vendors, and Related Workers	41-9041.00 41-9091.00
Communications Equipment Operators, All Other	5030	Switchboard Operators, Including Answering Service Telephone Operators	43-2011.00 43-2021.00
Information and Record Clerks, All Other	5420	Receptionists and Information Clerks	43-4171.00

Stock Clerks and Order Fillers	5620	Shipping, Receiving, and Inventory Clerks	43-5071.00
Computer Operators	5800	Data Entry Keyers Word Processors and Typists Office Clerks, General Office Machine Operators, Except Computer	43-9021.00 43-9022.00 43-9061.00 43-9071.00
Office and Administrative Support Workers, All Other	5930	Office Clerks, General	43-9061.00
Computer Control Programmers and Operators	7900	Extruding and Drawing Machine Setters, Operators, and Tenders, Metal and Plastic Forging Machine Setters, Operators, and Tenders, Metal and Plastic Rolling Machine Setters, Operators, and Tenders, Metal and Plastic Cutting, Punching, and Press Machine Setters, Operators, and Tenders, Metal and Plastic Drilling and Boring Machine Tool Setters, Operators, and Tenders, Metal and Plastic	51-4021.00 51-4022.00 51-4023.00 51-4031.00 51-4032.00
Metal Workers and Plastic Workers, All Other	8220	Layout Workers, Metal and Plastic Plating Machine Setters, Operators, and Tenders, Metal and Plastic	51-4192.00 51-4193.00
Bookbinders and Bindery Workers	8230	Print Binding and Finishing Workers	51-5113.00
Job Printers	8240	Printing Press Operators	51-5112.00
Textile, Apparel, and Furnishings Workers, All Other	8460	Fabric and Apparel Patternmakers Upholsterers	51-6092.00 51-6093.00
Woodworkers, All Other	8550	Cabinetmakers and Bench Carpenters Furniture Finishers	51-7011.00 51-7021.00
Miscellaneous Plant and System Operators	8630	Water and Wastewater Treatment Plant and System Operators Chemical Plant and System Operators	51-8031.00 51-8091.00

Transportation Attendants	4550	Flight Attendants Passenger Attendants	53-2031.00 53-6061.00
Taxi Drivers and Chauffeurs	9140	Bus Drivers, Transit and Intercity	53-3052.00
Motor Vehicle Operators, All Other	9150	Heavy and Tractor-Trailer Truck Drivers Light Truck Drivers	53-3032.00 53-3033.00
Other transportation workers	9420	Traffic Technicians	53-6041.00
Material Moving Workers, All Other	9750	Laborers and Freight, Stock, and Material Movers, Hand	53-7062.00

Tank Car, Truck, and Ship Loaders	53-7121.00

Notes: The crosswalk used to link NLSY97 occupations (2002 Census Codes) and O*NET SOC occupation data can be accessed here: `https://usa.ipums.org/usa/volii/occtooccsoc18.shtml`.

Table C.4: NLSY97 Occupations (2002 Census Codes) That Were Hand-Matched to Census Occupations (2010 Census Codes)

NLSY97 Occupation	Code	2010 Census Occupation	Code
Lodging Managers	340	Miscellaneous Managers	430
Biomedical Engineers	1340	Miscellaneous Engineers	1530
Market and Survey Researchers	1810	Miscellaneous Social Scientists	1840
Miscellaneous Social Scientists and Related Workers	1860	Miscellaneous Social Scientists	1840
Nuclear Technicians	1940	Life, Physical, and Social Science Technicians	1960
Editors	2830	Editors, News Analysts, Reporters, and Other	2810
Miscellaneous Health Technologists and Technicians	3410	Health Technologists and Technicians	3530
Police and Sheriff's Patrol Officers	3850	Sheriffs, Bailiffs, Correctional Officers, and Other Police Officers and Detectives	3800 3820
Transit and Railroad Police	3860	Police Officers and Detectives	3820
Cooks	4020	Chefs and Cooks	4000
Motion Picture Projectionists	4410	Miscellaneous Entertainment Attendants and Related Workers	4430

Notes: The crosswalk used to link NLSY97 occupations (2002 Census Codes) and American Community Survey (ACS) Census occupation data (2010 Census codes) can be accessed here: `https://usa.ipums.org/usa/volii/occ_acs.shtml`.

Table C.5: Means and Standard Deviations of Care Measures by Race/Ethnicity and Gender

	Nurturant Care (dummy)		Reproductive Care (dummy)		Care Index	
Panel A: Women						
Non-Black/Non-Hispanic	0.207	(0.405)	0.155	(0.362)	0.580	(0.166)
Black	0.223	(0.416)	0.131	(0.337)	0.591	(0.167)
Hispanic	0.180	(0.385)	0.121	(0.326)	0.580	(0.158)
Panel A: Men						
Non-Black/Non-Hispanic	0.057	(0.232)	0.096	(0.295)	0.466	(0.139)
Black	0.048	(0.214)	0.125	(0.331)	0.466	(0.140)
Hispanic	0.048	(0.214)	0.091	(0.288)	0.471	(0.139)

Source: NLSY97, Waves 1997-2017.
Notes: Standard deviations in parentheses.

Table C.6: Sample Representation in Care and Skill Occupations by Race/Ethnicity and Gender

	Women:		**Men**:	
	All	*Full-Time*	*All*	*Full-Time*
Panel A: Non-Black/Non-Hispanic				
Low Care, Low Skill	23.6%	20.1%	51.4%	51.8%
Low Care, High Skill	7.3%	12.0%	9.8%	13.2%
High Care, Low Skill	53.2%	45.1%	28.8%	23.5%
High Care, High Skill	16.0%	22.8%	9.9%	11.5%
N	44,072	19,794	45,519	27,014
Panel B: Black				
Low Care, Low Skill	25.1%	24.4%	59.3%	60.2%
Low Care, High Skill	4.2%	5.9%	5.0%	6.1%
High Care, Low Skill	56.7%	51.3%	29.1%	25.9%
High Care, High Skill	14.0%	18.3%	6.6%	7.9%
N	21,024	11,067	18,431	11,159
Panel C: Hispanic				
Low Care, Low Skill	25.0%	22.7%	53.9%	55.4%
Low Care, High Skill	5.4%	7.6%	7.1%	8.3%
High Care, Low Skill	56.0%	52.3%	31.2%	27.5%
High Care, High Skill	13.5%	17.4%	7.8%	8.7%
N	15,338	8,122	16,757	10,994

Source: NLSY97, Waves 1997-2017.

Table C.7: Estimated Impact of Care Work on *Ln* Hourly Wages with Select Covariates Reported

	Women			***Men***		
	(1)	(2)	(3)	(4)	(5)	(6)
Nurturant Care	-0.039***			-0.067***		
	(0.008)			(0.015)		
Reproductive Care		0.015**			0.019*	
		(0.008)			(0.010)	
Care Index			-0.085***			-0.143***
			(0.017)			(0.020)
Highest Grade Completed	0.019***	0.018***	0.019***	0.022***	0.021***	0.021***
	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)
Highest Degree Received						
HS Diploma	0.025*	0.025*	0.025*	0.063***	0.064***	0.063***
	(0.014)	(0.014)	(0.014)	(0.012)	(0.012)	(0.012)
Associates Degree	0.091***	0.089***	0.090***	0.108***	0.108***	0.107***
	(0.021)	(0.021)	(0.021)	(0.023)	(0.023)	(0.023)
Bachelors Degree	0.196***	0.194***	0.193***	0.215***	0.214***	0.210***
	(0.021)	(0.021)	(0.021)	(0.023)	(0.023)	(0.023)
Masters Degree	0.310***	0.308***	0.306***	0.348***	0.346***	0.341***
	(0.028)	(0.028)	(0.028)	(0.034)	(0.034)	(0.034)
Doctoral Degree	0.485***	0.482***	0.478***	0.377***	0.377***	0.367***
	(0.062)	(0.061)	(0.061)	(0.101)	(0.101)	(0.099)
Professional Degree	0.468***	0.464***	0.471***	0.473***	0.464***	0.470***
	(0.062)	(0.062)	(0.062)	(0.080)	(0.081)	(0.080)
Union Status	0.105***	0.101***	0.102***	0.122***	0.120***	0.121***
	(0.008)	(0.008)	(0.008)	(0.007)	(0.007)	(0.007)
Job Tenure	0.009***	0.009***	0.009***	0.010***	0.010***	0.010***
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Hazard Index	0.161***	0.140***	0.144***	0.245***	0.246***	0.250***
	(0.027)	(0.027)	(0.027)	(0.015)	(0.015)	(0.015)
Skills Index	0.551***	0.529***	0.537***	0.428***	0.425***	0.442***
	(0.025)	(0.026)	(0.024)	(0.020)	(0.021)	(0.020)
Majority Female Occupation	-0.017**	-0.028***	-0.016**	-0.020***	-0.028***	-0.011*
	(0.007)	(0.007)	(0.007)	(0.006)	(0.006)	(0.006)
N	37,502	37,502	37,502	48,240	48,240	48,240
Individuals	4,076	4,076	4,076	4,324	4,324	4,324
FE Model	X	X	X	X	X	X
Controls	X	X	X	X	X	X
Full-Time Restriction	X	X	X	X	X	X
Wage Restriction	X	X	X	X	X	X

Source: NLSY97, Waves 1997-2017.
Notes: Estimated coefficients for highest degree received indicators are relative to "None" and the "GED" indicator is not reported in the table. Models include the controls specified in Table 3.1 unless otherwise indicated, as well as controls for urban/rural residence (dummies), MSA residence (dummies), Census region (dummies). Standard errors in parentheses. Levels of statistical significance given by * $p < 0.1$, ** $p < 0.05$, and *** $p < 0.01$.

Table C.8: Estimated Impact of Care Work on *Ln* Hourly Compensation with Select Covariates Reported

	Women			***Men***		
	(1)	(2)	(3)	(4)	(5)	(6)
Nurturant Care	-0.063***			-0.109***		
	(0.009)			(0.016)		
Reproductive Care		0.104***			0.048***	
		(0.011)			(0.011)	
Care Index			-0.128***			-0.173***
			(0.018)			(0.022)
Highest Grade Completed (years)	0.021***	0.020***	0.021***	0.026***	0.024***	0.024***
	(0.003)	(0.003)	(0.003)	(0.004)	(0.004)	(0.004)
Highest Degree Received						
HS Diploma	0.027	0.033**	0.026	0.059***	0.062***	0.059***
	(0.016)	(0.016)	(0.016)	(0.015)	(0.015)	(0.015)
Associates Degree	0.086***	0.087***	0.085***	0.096***	0.096***	0.094***
	(0.024)	(0.023)	(0.024)	(0.026)	(0.026)	(0.026)
Bachelors Degree	0.173***	0.173***	0.168***	0.186***	0.184***	0.179***
	(0.024)	(0.024)	(0.024)	(0.026)	(0.026)	(0.026)
Masters Degree	0.272***	0.270***	0.266***	0.299***	0.295***	0.289***
	(0.032)	(0.032)	(0.032)	(0.037)	(0.037)	(0.037)
Doctoral Degree	0.438***	0.430***	0.428***	0.318***	0.318***	0.307***
	(0.082)	(0.080)	(0.080)	(0.102)	(0.102)	(0.099)
Professional Degree	0.474***	0.463***	0.477***	0.373***	0.358***	0.367***
	(0.066)	(0.066)	(0.066)	(0.084)	(0.086)	(0.084)
Union	0.106***	0.100***	0.102***	0.133***	0.129***	0.131***
	(0.009)	(0.009)	(0.009)	(0.008)	(0.008)	(0.008)
Job Tenure	0.011***	0.011***	0.011***	0.011***	0.011***	0.012***
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Hazard Index	0.178***	0.144***	0.150***	0.199***	0.202***	0.203***
	(0.030)	(0.030)	(0.029)	(0.017)	(0.018)	(0.018)
Skills Index	0.486***	0.522***	0.459***	0.420***	0.427***	0.429***
	(0.027)	(0.027)	(0.026)	(0.022)	(0.024)	(0.023)
Majority Female Occupation	-0.011	-0.037***	-0.011	-0.018**	-0.035***	-0.009
	(0.007)	(0.007)	(0.007)	(0.007)	(0.007)	(0.007)
N	38,475	38,475	38,475	48,627	48,627	48,627
Individuals	4,083	4,083	4,083	4,326	4,326	4,326
FE Model	X	X	X	X	X	X
Controls	X	X	X	X	X	X
Full-Time Restriction	X	X	X	X	X	X
Wage Restriction	X	X	X	X	X	X

Source: NLSY97, Waves 1997-2017.
Notes: Estimated coefficients for highest degree received indicators are relative to "None" and the "GED" indicator is not reported in the table. Models include the controls specified in Table 3.1 unless otherwise indicated, as well as controls for urban/rural residence (dummies), MSA residence (dummies), Census region (dummies). Standard errors in parentheses. Levels of statistical significance given by * $p < 0.1$, ** $p < 0.05$, and *** $p < 0.01$.

Table C.9: Estimated Impact of Nurturant Care on Earnings in High vs. Low Skill Occupations

	Dependent variable is *Ln* real hourly wage		Dependent variable is *Ln* real hourly compensation	
	(1)	(2)	(3)	(4)
Panel A: Women				
High Skill	0.150***	0.101***	0.139***	0.105***
	(0.011)	(0.007)	(0.011)	(0.007)
Nurturant Care	-0.091***	-0.049***	-0.133***	-0.074***
	(0.011)	(0.009)	(0.011)	(0.009)
Nurturant Care × High Skill	0.090***	0.055***	0.100***	0.050***
	(0.017)	(0.014)	(0.017)	(0.014)
N	76428	37502	81175	38621
Individuals	4,249	4,076	4,250	4,084
Panel B: Men				
High Skill	0.200***	0.153***	0.200***	0.150***
	(0.011)	(0.008)	(0.011)	(0.008)
Nurturant Care	-0.026	-0.047***	-0.073***	-0.068***
	(0.022)	(0.018)	(0.024)	(0.019)
Nurturant Care × High Skill	-0.064**	-0.039	-0.069**	-0.065**
	(0.030)	(0.026)	(0.031)	(0.027)
N	79761	48240	81445	48591
Individuals	4,458	4,324	4,458	4,325
FE Model	X	X	X	X
Controls	X	X	X	X
Full-Time Restriction		X		X
Wage Restriction		X		X

Source: NLSY97, Waves 1997-2017.
Notes: Models include the controls specified in Table 3.1 unless otherwise indicated, as well as controls for urban/rural residence (dummies), MSA residence (dummies), Census region (dummies). Standard errors in parentheses. Levels of statistical significance given by * $p < 0.1$, ** $p < 0.05$, and *** $p < 0.01$.