Early Childhood Conditions and Mortality: Evidence from Japanese American Internment*

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Abstract: Using War Relocation Authority records linked to the Social Security Death Index, I investigate how internment during early childhood affected the life expectancy of male internees. Using un-interned Japanese Hawaiians as a control group, difference-in-differences estimates suggest that incarceration within the first four years of life decreased life expectancy by approximately two years. Furthermore, the internees from low socioeconomic status families drive almost the entire effect, decreasing their life expectancies by three years. The results are robust to controlling for endogenous fertility decisions, and data on Chinese Americans suggest that the identifying assumption is satisfied.

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1 Introduction

Over the last decade, economists have become increasingly interested in whether early childhood conditions can explain adult health and economic disparities. Answering this question requires finding a cohort of children who faced large and sudden changes during their early childhoods. Health economists have often turned to unique historical events, such as the 1918 influenza pandemic (Almond, 2006), hookworm eradication (Bleakley, 2007) and hospital desegregation in the American South (Chay et al., 2009). This study examines whether early childhood environments can explain variations in life expectancy by exploring a tragic event in American history: the internment of over 100,000 Japanese Americans during World War II.

Following the bombing of Pearl Harbor by the Empire of Japan, President Roosevelt signed Executive Order 9066, which allowed the War Department to declare any area a "military zone" from which it could exclude any person it deemed a threat (Ng, 2002). President Roosevelt signed the Order in February of 1942, and by March of that year, the United States Army had forced the evacuation of all Japanese Americans (even children and US citizens) from the West Coast. The government created the War Relocation Authority (WRA) to detain all Japanese Americans residing on the West Coast, built internment camps composed of military-style barracks, and surrounded them with barbed wire and armed guards, who on more than one occasion shot internees suspected of attempting escape (Ng, 2002). The internees shared small rooms that ranged from 320 to 480 square feet with three to seven people and had to wait in lines to use communal showers, bathrooms and dining halls (Jensen, 1999). The internment camps were small communities consisting of hospitals, schools and housing facilities, all erected in a matter of months.

In this paper, I ask whether the internment of the Japanese Americans disproportionately affected the life expectancies of the youngest children within the camps. This question is relevant for two reasons. Firstly, the World War II internment of Japanese Americans is an important part of American history that affected tens of thousands of children, and the long-term health effects of internment on the children are still unknown. Secondly, the internment of the Japanese Americans gives us an opportunity to study how large changes in early childhood environments can affect longevity. The health conditions within the camps were harsh, especially for young children. Nutrition was poor, dust storms caused high rates of asthma among children, and poorly insulated nurseries reached 104 degrees during the summer months (Jensen, 1999). These poor health conditions could have negatively affected the development of the youngest children within the camps and may have affected their adult health outcomes decades later.

I start by linking records from the War Relocation Authority to death records from the Social Security Death Index. To my knowledge, this study is the first to track the longevity of internees who have survived infancy. Then I construct a control group of death records with Japanese surnames who were likely not interned. A difference-in-differences methodology suggests that incarceration within the first four years of life decreased life expectancy by 1.6 years or 3 percent. Furthermore, the internees from low socioeconomic status families drive almost the entire effect. Internment during early childhood decreased life expectancy by almost 3 years or 5 percent for the internees from low socioeconomic status families and had no effect on the internees from high socioeconomic status families. This finding suggests that human capital investments might mitigate the effects of poor early childhood health.

These findings contribute to the growing literature on how early childhood shapes adult economic and health outcomes (see Almond and Currie (2011) for a detailed survey). Douglas

Almond (2006) presents evidence suggesting that the 1918 influenza pandemic lowered the incomes and educational attainment of the cohort that was in utero during the pandemic. Anne Case and Christina Paxson (2009) argue that the cohort of children who were two years old during the pandemic had worse cognition at elderly ages. Hoyt Bleakley (2007) finds that hookworm infection during childhood decreased the human capital development of children from the American South. Kenneth Chay, Jonathan Guryan, and Bhashkar Mazumder (2009) argue that hospital desegregation in the American South decreased the black-white test score gap 17 years later because black children had access to better health care during childhood following desegregation.

Section 2 examines the historical background of Japanese American internment and previous internment research. Section 3.1 discusses the WRA records sample, Section 3.2 discusses the SSDI sample, and Section 3.3 covers the record linkage process and investigates how it might affect the results. Section 4 outlines the econometric model, and Section 5 analyzes the results. Section 6 concludes.

2 Historical Background

After Pearl Harbor, Lieutenant General John DeWitt recommended to President Roosevelt that the military remove all Japanese Americans from the West Coast (Ng, 2002). Officially, the War Department was worried about espionage. In the event that the Empire of Japan invaded the West Coast of the United States, the War Department did not trust Japanese Americans to remain loyal. However, these concerns were more rooted in racism than reality. Neither Germans nor Italians were subjected to the same degree of scrutiny. One month before Pearl Harbor, Curtis Munson submitted a report to the President and the Secretary of War indicating that there was no reason to question the loyalty of Japanese Americans to the United States. In 1988, the United States paid $20,000 in redress to every surviving internee, and in 1990, President George Bush signed an official letter of apology.

Although a small number of non-West Coast Japanese Americans—mostly with connections to the Japanese government—were incarcerated as well, Japanese Hawaiians were not subject to mass relocation despite the strategic importance of Hawaii for two reasons. Firstly, more than 100,000 Japanese Americans lived in Hawaii, and relocating them across the Pacific to remote camps in the continental United States would have been highly costly. Secondly, Japanese Americans comprised over one-third of the Hawaiian population, but only 2 percent of the West Coast population, and removing a third of the labor force in Hawaii would have had devastating economic consequences.

The US Army evacuated the first group of Japanese Americans on March 22, 1942. Five days later, the government illegalized voluntary evacuation from the West Coast. While the internment camps were under construction, the WRA housed the internees at the assembly centers, which consisted of fairgrounds and racetracks. The internees slept in stables that reeked of horse manure (Fiset 1999). The assembly centers were never designed to house humans. The internees stayed at the assembly centers for several months, and by October of 1942, the WRA had moved the internees to the permanent relocation centers.

Gwenn Jensen (1999) documents the harsh health environments that the internees faced. The Manzanar, Poston and Gila River camps were located in the deserts of Arizona and eastern California, and the internees frequently battled the heat. The barracks had poor insulation and cooling systems, which caused temperatures at times to exceed 104 degrees in the nurseries. Dehydration and overheating—preventable conditions—caused most of the infant deaths within the camps. The internees could only bring one suitcase of clothes per person. The internees in the northwestern camps did not bring adequate clothing and frequently complained of the cold. The water pipes in the camps were recycled from oil and gas lines, which led to frequent water contamination. The WRA rationed less than 40 cents a day for food per internee and fed the internees an imbalanced diet with little Japanese food. There were also problems with dust storms. Because dust would get inside the barracks through cracks, the internees were exposed to dust inhalation even when indoors, which led to higher rates of asthma among children (Jensen 1999).

Aimee Chin (2005) published the only economics paper on Japanese American internment. Her study looks at the long-run effects of labor market withdrawal on working-age male internees. She does not examine females because many females did not participate in the labor force during the internment period (and consequently did not withdraw from it). Working-age males presumably suffered economic losses related to internment for two reasons. Firstly, these internees had to sell, store or abandon their property before relocating, and because these sales had to happen quickly, they often received below-market prices. Secondly, they had to leave their careers and could not continue to build human capital through work experience. Although the internees were encouraged to work in the camps, the career possibilities were limited, and many internees did not gain as many technical skills as they would have outside of the camps. Chin uses the fact that the internees who were children and adolescents during the time of internment did not suffer an interruption in their labor market experience and employs a difference-in-differences approach similar to the one used in this study. Chin estimates that internment decreased the 1970 earnings of adult male internees by 9 to 13 percent.

3 Data

The strategy of this paper is to take records from the War Relocation Authority and link them to the death records that appear in the Social Security Death Index. Subsections 3.1 to 3.3 describe this process in detail.

3.1 The WRA Records Sample

To manage the internees, the War Relocation Authority collected the personal information of every internee who entered each of the ten WRA camps. They also collected information for all of the births within the internment camps that occurred in 1942. The Bancroft Library of the University of California at Berkeley converted these files into electronic form in the 1960s, and now most of the variables in the files are publicly available through the National Archives. The data include the following information concerning the internees: first, middle and surnames; gender; assigned assembly and relocation centers; previous town, county and state; birth year; parents' birth countries; and father's occupation. The data also include information that in most cases is not relevant for interned children, such as occupation, marital status, and academic degrees.

I restrict the sample to male internees because females were more likely to change their surnames and are therefore less likely to be linked to their death records. I also drop a small number of WRA records with missing names. The remaining WRA records contain 8,651 male internees born between 1932 and 1942, with a mean birth year of 1937.2. Of these internees, 85.80 percent are from California, 9.33 percent are from Washington, 2.73 percent are from Oregon, and 1.73 percent are from Hawaii. Each internment camp housed between 585 (Minidoka) and 1,519 (Colorado River) children in the sample. The internees came from a variety of population densities. For example, 20.29 percent previously resided in towns with a population of less than 2,500, whereas 22.48 percent came from a city with a population of one million or more (almost all from Los Angeles). Approximately half of the observations had a father who worked in farming (33 percent as a manager/operator and 16 percent as laborers), 20 percent of the observations had a father working as a non-farming manager or official, 10.5 percent had a father who was a skilled craftsmen or semi-professional. No other category comprises more than 10 percent of the sample.

3.2 The Social Security Death Index Sample

The Social Security Death Index (SSDI) contains information on all deaths reported to the Social Security Administration since 1962. Although the Death Master File is not publicly available for download, the SSDI can be searched from several genealogy websites. Each death record includes an individual's name, age at death, birth date, death date, state in which his or her Social Security card was issued, address in which the last social security benefit was received (if the individual received Social Security benefits) and Social Security Number. The Death Master File does not contain gender or race. Mark Hill and Ira Rosenwaike (2001) find that between 93 and 96 percent of all deaths in the United States of individuals who were 65 or over are in the SSDI.

I attempt to construct a sample of death records that contains any Japanese American internees who have already died and Japanese Americans who were not interned to act as a control group. Ideally, I would have searched for the death records of all Japanese Americans. However, because race is not available in the death records, I instead exploited the fact that Japanese Americans have surnames that are unique relative to those of other Americans. I searched the SSDI (accessed through www.rootsweb.ancestry.com in August 2011) for all individuals born between 1932 and 1942 with a surname appearing in the WRA records sample, as described in the previous subsection. In other words, I construct of sample of death records with Japanese American surnames. I exclude a small number of names that appear in the WRA records (such as Torres and Williams) because they are most likely from multiracial families. For each of the excluded names, I search for the individual by using the individual's first name, surname and birth year. If there is a perfect match, then that record is added to the constructed death records sample. Although almost all older deaths appear in the database, many younger deaths appear as well. This sample gives us 12,952 death records. Diane Lauderdale and Bert Kestenbaum (2000) construct a list of the 50 most common Japanese American surnames, of which 48 are represented in the constructed SSDI sample.

Because the SSDI does not have gender, I infer the gender of each observation in the SSDI based on the first name. I label an observation as male if at least 90 percent of the observations in the WRA records with that first name are male. I apply to same rationale to identify female observations. I code gender as missing if no one in the WRA records has the same first name or if between 10 and 90 percent of the people with that first name are male.

I then restrict the SSDI sample to (inferred) males resulting in 6,161 death records with a mean life span of 64.22 years. Because the SSDI includes date of birth and date of death, I calculate life span as a fraction of a year. For example, I code an observation who was born on January 1, 1937 and who died on March 16, 2001 as having a life span of 64.2026 years ($64.2026 = 64 + 74/365.25$).

3.3 Record Linkage

We would like to link records from the WRA to death records in the SSDI. Unfortunately, unique identifiers, such as Social Security Numbers, do not appear in the publicly available WRA records. We can link records by using first name, middle initial, surname and birth year.

Small variations in spelling and typographical errors might prevent two records that are true matches from matching exactly with respect to all four variables. For example, if a Billy T. Ogawa born in 1941 appears in the WRA records, we can reasonably link this record to a Bill T. Ogawa born in 1941 in the SSDI. To link records that we can be reasonably sure are true matches, I use the following 6-step deterministic record linkage algorithm:

1. Link any two records that match perfectly with respect to first name, middle name, surname and birth year. Set these links aside and continue to step 2.
2. Link any two records that match perfectly with respect to first name, surname and birth year so long as the middle initial does not conflict. This step applies if the middle initial is missing in one of the data sets. Set these links aside and continue to step 3.
3. Phonetically code first names using the Soundex algorithm.[1] Link records that exactly match with respect to middle initial, surname, birth year and phonetically coded first name. Set these links aside and continue to step 4.
4. Link records that match exactly with respect to first name, middle initial and surname so long as the birth year differs by at most one year. Set these links aside and continue to step 5.
5. Recode short first names into a common long name ("Bill" or "Billy" to "William"). Link any two records that match exactly with respect to middle initial, surname, birth year and recoded first name. Set these links aside and continue to step 6.
6. Repeat step 5 while allowing for non-conflicting middle initials (this step is binding if middle initial is missing in either the SSDI or WRA sample).

[1] See Herzog, Scheuren and Winkler (2007) for details on the Soundex algorithm.

This algorithm results in 1,066 links, which comprise the "Treatment Group." Historical studies that use deterministic record linkage algorithms and have similar linkage rates include Ferrie (1996) and Long (2005).

We might wonder whether the record linkage process introduces bias. To test for such bias, Table (1) compares the linked and unlinked WRA sample for three variables: (1) the proportion with a high socioeconomic status (HSES) father (determined by the father's occupation), (2) the proportion from a city with a population of at least 25,000 and (3) the proportion born in California. The differences in the sample proportions between the linked and unlinked sample are never statistically significant. For example, 36.5 percent of the linked sample have a HSES father, whereas 36.4 percent of the unlinked sample have a HSES father. Testing the null hypothesis that the proportions are equal yields a *p*-value of 0.884. The differences never have a *p*-value smaller than 0.3. Because this paper compares the internees who entered the camps in their early childhoods with those who entered as older children, we must also rule out that the record linkage process introduces bias for only either the older internees or the younger internees. Table (1) also tests whether there are differences between the linked and unlinked WRA records for the older (born between January of 1932 and December of 1937) and younger (born between January of 1938 and December of 1942) cohorts. Again, the differences are never significant, and the *p*-values are large.

After linking the internees to the death records, we must also construct a "Control Group." We could use all of the unlinked death records. However, that sample would likely contain internees who were not linked because of variations in names and non-Japanese Americans who happened to have Japanese surnames. Furthermore, we know that most of the un-interned Japanese Americans were in Hawaii and that few Japanese Hawaiians were incarcerated (Chin, 2005). For these reasons, I restrict the control group to the unlinked Japanese Americans with Social Security Cards issued in Hawaii. This decision raises the question of whether Japanese Hawaiians are a good control group for West Coast Japanese Americans. I address this question using data from Chinese Americans in Section 5.1.

4 The Econometric Model

The ideal experiment to find the long-run effects of early childhood internment on mortality would be if only a subset of Japanese Americans were interned, and if internment were randomly assigned. Without such an experiment, we could compare the average age at death of the youngest cohort of internees with that of the non-internees from the same cohort. This estimate might be biased because most internees were from the West Coast and the longevity of West Coast Japanese Americans might have differed from that of non-West Coast Japanese Americans even in the absence of internment. Because West Coast Japanese Americans normally migrated from Hawaii into to the more racially hostile West Coast to pursue greater economic opportunities, we might expect systematic differences between those who chose to migrate and those who did not. Alternatively, we could compare the distribution of life spans for those who were interned in early childhood with the distribution for those who were interned at later ages, but this estimate might be biased if life expectancy trends over time. To overcome these challenges, I follow a difference-in-differences strategy that will allow us to subtract out a fixed level difference between the treatment and control groups and a secular cohort trend. Difference in differences across cohorts (instead of across time) appears in Esther Duflo (2001) to analyze the effects of a school construction program in Indonesia and in Aimee Chin (2005) to analyze the long-run effects of withdrawing from the labor market during Japanese American internment.

A difference-in-differences strategy suggests estimating the equation:

$$y_{ics} = \alpha + \beta_c + \gamma_s + \delta L_i + \theta L_i \times \mathbf{1}[c \geq 1938] + \varepsilon_i \quad (1)$$

where y_{ics} is the life span of individual i born in cohort c and state $s \in \{\text{California, Hawaii, Oregon, Washington, Other}\}$. The parameters β_c and γ_s are birth cohort and state fixed effects, respectively. The regressor $L_i = 1$ if the death record of individual i is linked to a WRA record. Otherwise, $L_i = 0$. By construction of the sample, if $L_i = 0$, then individual i's death record was unlinked to the WRA records and had a Social Security card that was issued in Hawaii. If the interaction term $L_i \times \mathbf{1}[c \geq 1938] = 1$, then individual i was incarcerated during early childhood. The coefficient θ is the difference-in-differences parameter of interest.

We would also like to know to what extent high socioeconomic status (HSES) may have mitigated the long-run effects of early childhood internment on mortality. To test this hypothesis, I estimate the following variation of equation (1):

$$y_{icsj} = \alpha + \beta_c + \gamma_s + \delta_j L_i + \theta_L L_i \times \mathbf{1}[c \geq 1938] \times \mathbf{1}[\text{LSES}] + \quad (2)$$

$$\theta_H L_i \times \mathbf{1}[c \geq 1938] \times \mathbf{1}[\text{HSES}] + \varepsilon_i$$

where j indexes the occupation of individual i's father. The coefficient on L_i is δ_j and varies with the father's occupation. The indicator variables $\mathbf{1}[\text{LSES}]$ and $\mathbf{1}[\text{HSES}]$ equal one if the individual's father has a LSES or HSES occupation, respectively. HSES jobs include professionals, semi-professionals, skilled craftsmen, managers (except farm mangers) and official occupations. LSES occupations include farm workers, fishermen, service workers, unskilled laborers, salesmen and clerical occupations. The treatment effect is θ_L for LSES internees and θ_H for HSES internees.

It is tempting to equate the terms "internment effect" and "treatment effect" in this context, but to do so is incorrect. We are comparing the differences between the internees who were younger children and the internees who were older children during their incarceration (the first difference) and subtracting out a secular cohort trend identified by the difference in life expectancy between the younger and older cohorts of Japanese Hawaiians (the second difference). Therefore, the "treatment effect" is not the effect of internment on life expectancy but rather the effect of early childhood internment on life expectancy relative to the effect of internment at an older age. We can think of "age during internment" as a measure of treatment intensity. The youngest cohorts are at a plausibly greater risk of suffering health shocks and are exposed consequently to high intensity of treatment. Health shocks may have affected the older cohorts at a lower intensity. In this paper, the identified "treatment effect" is the difference between the high intensity treatment and the low intensity treatment.

The identifying assumption is that in the absence of internment, the cross-cohort life expectancy of the internees would have been parallel to the cross-cohort life expectancy of the un-interned Japanese Hawaiians. Of course, we cannot test this assumption empirically because all internees were by definition interned. However, we can test whether the assumption is plausible by testing whether the cross-cohort life expectancy trend of Chinese Americans in California, Oregon and Washington is parallel to the cross-cohort life expectancy of Chinese Hawaiians. I perform this test in the Results section.

5 Results

5.1 Nonparametric and Graphical Evidence

Figure (1) nonparametrically graphs the relationship between age at death and birth year for both the internees linked to the Social Security Death Index and the un-interned Japanese Hawaiians. Each nonparametric regression line in Figure (1) is a locally linear smooth with a bandwidth of 1.5. Alternative bandwidths (within reason) do not qualitatively change Figure (1). The nonparametric regression lines in Figure (1) are downward sloping only because the death records were collected at the same time. The youngest cohort was only 69 years old when the SSDI death records were extracted. Consequently, we do not observe any deaths that occurred after that age. In contrast, because the 1932 cohort was 79 years old, we observe the deaths of septuagenarians.

This figure provides the first graphical evidence suggesting that internment during early childhood decreased life expectancy relative to the life expectancy of the older internees. For the 1932 to 1936 cohorts, the internees' mean age at death closely tracks that of the Japanese Hawaiians across cohorts. The internees from these cohorts were between the ages of 6 and 10 when they entered the internment camps. For the cohorts born after 1936, the regression line for the internees dips below the regression line for the un-interned Japanese Hawaiians. This gap reaches approximately two years for the 1938 cohort (who were four years old when they entered the camps) and increases for the cohorts who entered the internment camps at younger ages.

Figure (1) raises the question of whether Japanese Hawaiians are a good control group for Japanese internees who were almost exclusively from the West Coast. The identifying assumption is that in the absence of internment, the cross-cohort trend of life expectancy for West Coast Japanese Americans would be parallel to that of Japanese Hawaiians. Because this assumption is counterfactual, we cannot test it directly. Instead, I test whether the cross-cohort life expectancy trend of West Coast Chinese Americans is parallel to the cross-cohort life expectancy trend of Chinese Hawaiians.

Because the SSDI does not have race, I identify Chinese death records by using surnames. Lauderdale and Kestenbaum (2000) list the 50 most common Chinese surnames. I extracted the death records from the SSDI with Social Security cards issued in California, Hawaii, Oregon, and Washington, and with one of the 50 Chinese surnames listed in Lauderdale and Kestenbaum (2000). Figure (2) uses these data and graphs nonparametric regression lines for Chinese Americans that are analogous to the regression lines in Figure (1). Two features of Figure (2) are notable. Firstly, the lines appear to be parallel, suggesting that the difference-in-differences assumption is satisfied. Secondly, Figure (2) indicates that West Coast Chinese Americans lived longer than Chinese Hawaiians, whereas Figure (1) suggests that West Coast Japanese Americans died at younger ages than Japanese Hawaiians. The latter feature of Figure (2) suggests that, if anything, using Japanese Hawaiians as a control underestimates the effect of internment on mortality in adulthood.

Because the data were collected at the same time and the cross-cohort trends are downward sloping, the older cohorts are not entirely comparable with the younger cohorts. We might wonder whether this issue introduces any type of bias. Figures (3)-(5) address this concern. Figure (3) displays the same local linear smooth line depicted in Figure (1) but with only the deaths that occurred by age 69 to make all cohorts comparable. Again, the nonparametric regression lines for the internees and un-interned Japanese Hawaiians from the 1932 to 1936 cohorts match closely with one another. After these cohorts, however, the nonparametric regression line of the internees dips below that of the Japanese Hawaiians by approximately 2 years, with the gap widening for the youngest cohorts. Figure (4) is analogous to Figure (3) but uses Chinese death records. The same partners that appear in Figure (2) show up in Figure (4). The cross-cohort trends are parallel, and West Coast Chinese Americans lived longer than Chinese Hawaiians.

Whereas Figures (1) and (3) only examine the local means, Figure (5) provides evidence of how early childhood conditions affected the entire distribution of age at death. Figure (5) graphs kernel density estimates of the age at death for the internees and Japanese Hawaiians from the 1932 to 1937 birth cohort and the 1938 to 1942 birth cohort. Because the youngest birth cohort was 69 years old when the death records were collected, I only graph the density for the deaths that occurred between the ages of 30 and 69 (I do not graph the densities below 30 only because they are trivially close to zero). Again, the densities for both internees and un-interned Japanese Hawaiians match closely with one another for the older birth cohort. We can formally test the null hypothesis that the two samples were drawn from the same distribution by using the Kolmogorov-Smirnov test. This test gives us a *p*-value of 0.980. Consequently, we cannot reject the null hypothesis. For the younger cohort, however, the probability of death occurring between the ages of 30 and 55 is uniformly higher for the internees than for Japanese Hawaiians. Because the Kolmogorov-Smirnov test for the younger cohort yields a p-value of 0.002, we can reject the null hypothesis that the samples were drawn from the same distribution for all conventional significance levels.

5.2 Difference in Differences Results

A simple 2×2 table demonstrates the concept of difference in differences in this setting. Table (2) displays the mean age at death for the internees (the treatment group) and un-interned Japanese Hawaiians (the control group) for the younger and older cohorts. The older cohort in the control group lived an average of 65.89 years, whereas the younger cohort in the control group lived an average of 60.97 years. Because neither of these groups was incarcerated, we interpret the difference of 4.92 years in these means as the secular cohort trend. The older cohort in the treatment group lived an average of 65.8 years, whereas the younger cohort in the treatment group lived an average of 59.05 years—a difference of 6.75 years. Subtracting out the fixed cohort trend, we obtain our difference-in-differences estimate of -1.83 years. Reproducing Table (2) with only the deaths that occurred before the age of 69 decreases the secular cohort trend (as expected), but the difference-in-differences estimate is hardly changed. For the remainder of the analysis, I leave the deaths that occurred after the age of 69 in the sample, because they increase the sample size and allow us to estimate the difference between the older internees and the older Japanese Hawaiians more precisely.

We can add controls, cohort fixed years, and state fixed effects in a regression framework by estimating equations (1) and (2). The estimates from these regressions appear in Table (3). Each column is a separate regression with state and birth cohort fixed effects. Column (1) of Table (3) presents the difference-in-differences estimate of the early childhood internment effect. I estimate that internment during early childhood decreased life expectancy by -1.63 years. The estimate is significant at the five percent level and standard errors that are robust to heteroskedasticity are in parentheses. Standard errors clustered at the state/year of birth level are in brackets, but because clustering shrinks the standard errors, I take the more conservative approach of not clustering when calculating significance. Column (3) presents the same regression depicted in Column (1) but with the natural log of age at death as the dependent variable. The results suggest that internment during early childhood decreased life expectancy by 3 percent. The estimate is significant at the 5 percent level.

Columns (2) and (4) of Table (3) present estimates of equation (2), which allows for different treatment effects for the internees from high socioeconomic status families and low socioeconomic status families. The estimates suggest that early childhood internment decreased life expectancy by 2.6 years and 4.6 percent for the internees from LSES families and that the children from HSES families were unaffected. The coefficient on the LSES treatment effect is significant at the 5 percent level, whereas the coefficient on the HSES treatment effect is statistically equal to zero. Although the coefficient is only significant for the LSES internees, this finding does not mean that only a small group was affected—over 60 percent of the internees had a father with a LSES occupation. Several plausible reasons can explain why internment may have had a greater effect on the children from LSES families. Internees could make purchases within the camps through mail order catalogs. Many internees who were accustomed to the southern California climate were unprepared for the northwestern United States winter, and internees frequently complained about the cold. It is possible that the HSES families could more easily purchase winter clothing and blankets. Similarly, the HSES families may have purchased additional fans to stay cool during the summer months. Another possible explanation is that after the camps closed, the HSES families may have been able to recover from the financial burdens of internment more quickly. Lastly, the HSES families may have increased their human capital investments in children who spent their early childhoods within the camps and thereby offset any negative early childhood effects.

Table (4) addresses how early childhood incarceration affected the probability of early death. Table (4) presents the marginal effects of probit regressions for an indicator variable equal to one if the observation died by a given age on the same set of regressors presented in equation (2). Each column is a separate probit regression, and includes state and birth year dummies. The results suggest that early childhood internment increased the probability of dying by the ages of 50, 55 and 60 by 6, 10 and 9 percentage points for the LSES internees, respectively. The coefficients for the HSES internees are never statistically different from zero. The fact that the coefficient jumps from 4 to 10 between 45 and 55 suggests that many of these premature deaths occurred during their late 40s and early 50s.

5.3 Selection and Endogenous Fertility

Before January of 1942, West Coast Japanese Americans had no reason to believe that they would be relocated to internment camps while other Japanese Americans would be allowed to continue living their lives. However, internees may have changed their fertility decisions in response to learning about their future incarceration. If true, this response could explain a negative estimated treatment effect even in the absence of a true treatment effect. For example, suppose that high socioeconomic status parents postponed having children because they did not want them to grow up in a harsh environment. Then the children born in the camps during 1942 may have had lower life expectancies even in the absence of an early childhood internment effect because they came from lower socioeconomic status families.

One could argue that selection on fertility may have occurred earlier, perhaps after Pearl Harbor. However, for selection following Pearl Harbor to explain the results, it must have affected West Coast Japanese Americans *more* than Japanese Hawaiians. Given the location of Pearl Harbor on the Hawaiian Islands and the strategic geographic importance of Hawaii to the War, this explanation seems highly unlikely.

To test whether selection on fertility can explain the results, I re-estimate equations (1) and (2) by dropping the observations born in 1942. This restriction implies that every individual in the sample was conceived within the first quarter of 1941 and eliminates the possibility that parents made their fertility decisions in response to the news of internment or Pearl Harbor. These estimates appear in Table (5). The estimates are similar to the estimates in Table (3). The estimates suggest that internment during early childhood decreases life expectancy by 1.9 years and 3.6 percent on average, both of which are significant at the five percent level. Furthermore, early childhood internment affects the internees from low socioeconomic status families more, decreasing their life expectancies by 2.7 years and 5.2 percent.

6 Discussion and Conclusion

This paper provides evidence that early childhood conditions may have profound effects on life expectancy. I estimate that the male internees who entered the camps within the first four years of life died approximately two years earlier. Furthermore, the effect is almost entirely due to the internees from low socioeconomic status families. This finding suggests that internment during early childhood decreased life expectancy by almost three years for LSES children. These results suggest that either financial means helped mitigate the hardships of internment or investments after relocation offset the negative effects of internment.

A back-of-the-envelope calculation can address whether internees received adequate compensation from the federal government. Kevin Murphy and Robert Topel (2006) estimate that the statistical value of a life-year is between $100,000 and $350,000. This estimate suggests that the internees who were incarcerated during early childhood should have been compensated at least $200,000 instead of the $20,000 dollars that they received in 1990.

Several limitations of these results are noteworthy. Most internees from the 1932 to 1942 birth cohort are still alive, and it is impossible to obtain a full picture of their life spans until this cohort passes away. This fact is the largest reason for the small linkage rate. As more internees die, it will be possible to increase the number of matches and to obtain a fuller picture of the effects of internment. Additionally, this study does not attempt to link the internees from older cohorts (born before or during 1931) to their death records. The data that can be used to do so are publicly available, and linking these databases may answer many interesting historical questions. Furthermore, this study does not address the cause of death. Early childhood conditions likely increased the probability of death from some causes (such as heart disease) but not all (such as traffic fatalities). Lastly, this paper does not examine labor market outcomes. Although this question cannot be answered using the same name-linking strategy, using Census data and an identification strategy similar to Chin (2005) may yield some insights. I am investigating this issue as part of an on-going project.

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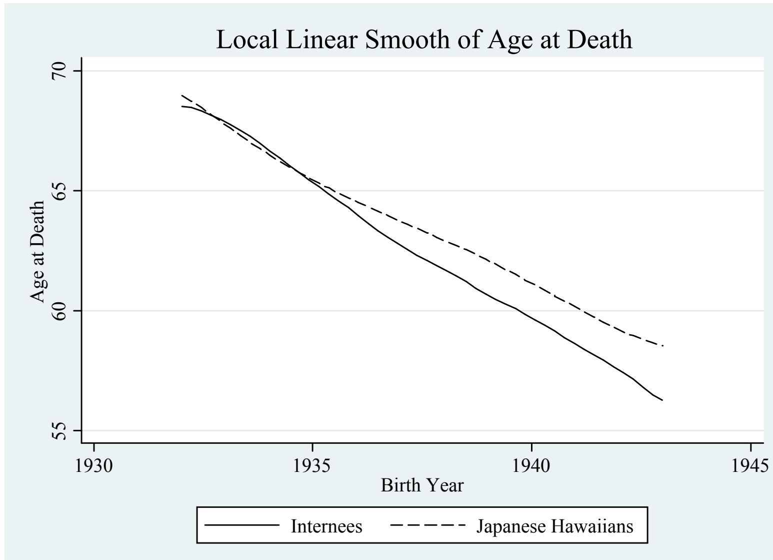

Figure 1: Internees' data are from the WRA records linked to the SSDI. Japanese Hawaiians are the unlinked death records with Japanese surnames and social security cards issued in Hawaii. The bandwidths are 1.5 for both lines. The kernel function is the Epanechnikov. The data are restricted to males who have died by August 2011.

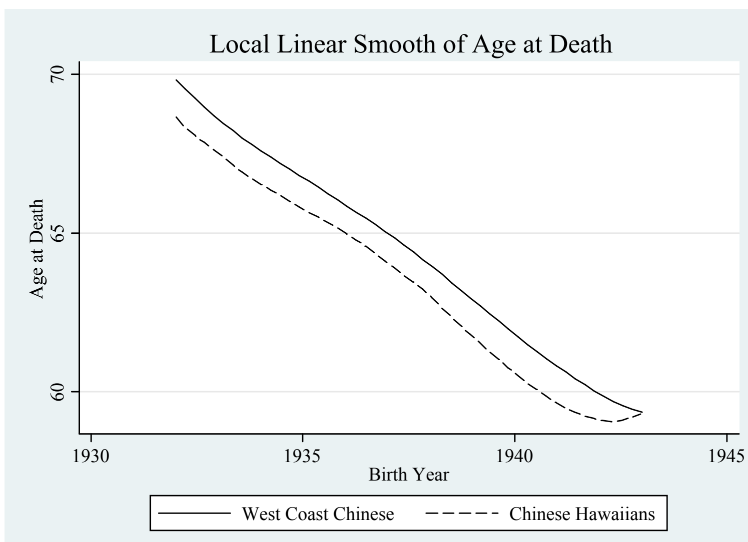

Figure 2: Data come from the SSDI death records with one of the 50 most common Chinese surnames according to Lauderdale and Kestenbaum (2000). The West Coast includes death records with social security cards issued in California, Oregon and Washington; Chinese Hawaiians are the death records with social security cards issued in Hawaii. The deaths may have occurred anywhere in the United States. The bandwidths are 1.5 for both lines. The kernel function is the Epanechnikov.

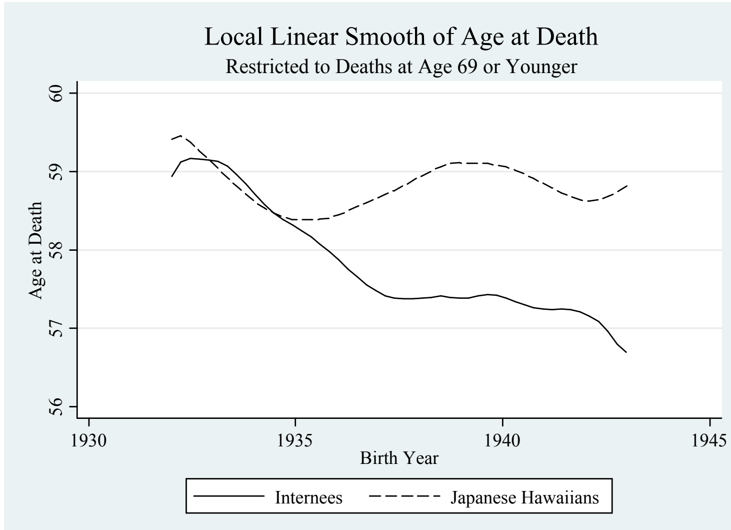

Figure 3: Internees' data are from the WRA records linked to the SSDI. Japanese Hawaiians are the unlinked death records with Japanese surnames and social security cards issued in Hawaii. The bandwidths are 1.5 for both lines. The kernel function is the Epanechnikov. The data are restricted to males who have died by August 2011.

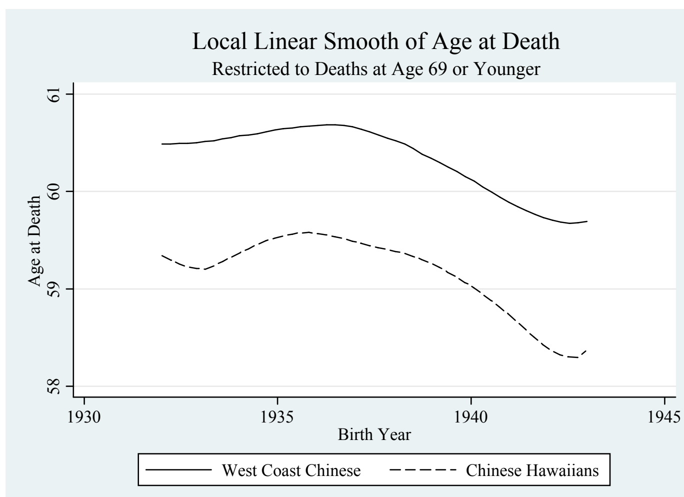

Figure 4: Data come from the SSDI death records with one of the 50 most common Chinese surnames according to Lauderdale and Kestenbaum (2000). The West Coast includes death records with social security cards issued in California, Oregon and Washington; Chinese Hawaiians are the death records with social security cards issued in Hawaii. The deaths may have occurred anywhere in the United States. The bandwidths are 1.5 for West Coast Chinese, and 2 for Chinese Hawaiians (because of a smaller sample size). The kernel function is the Epanechnikov.

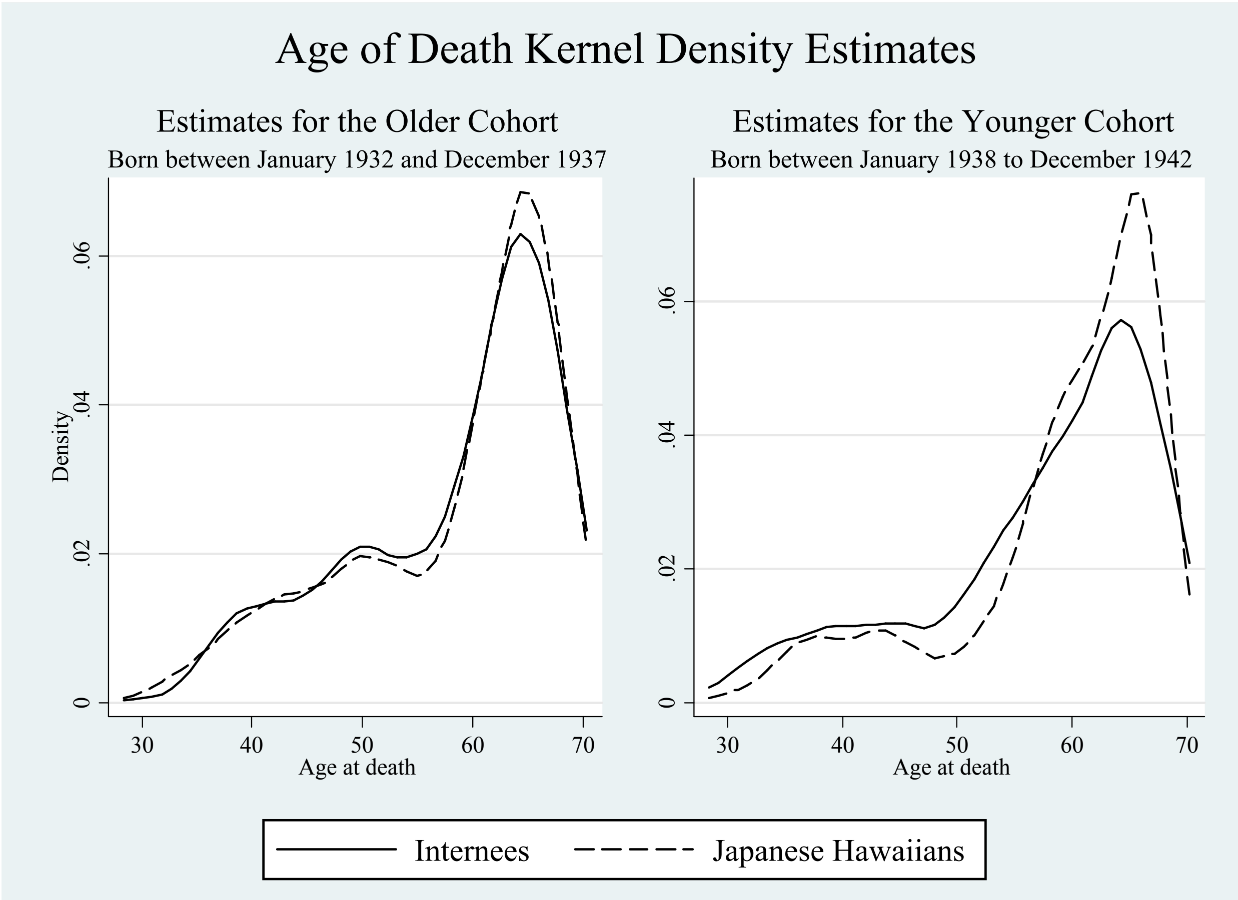

Figure 5: Internees' data are from the WRA records linked to the SSDI. Japanese Hawaiians are the unlinked death records with Japanese surnames and social security cards issued in Hawaii. The bandwidths are determined using Silverman's rule of thumb. The kernel function is the Epanechnikov. The data are restricted to male deaths that occurred between ages of 30 and 69.

WRA Record Summary Statistics for Linked and Unlinked Internees

Full Sample	Linked Mean	Unlinked Mean	Difference	P-value
HSES	0.365	0.364	0.002	0.915
	(0.015)	(0.006)	(0.016)	
City population at least 25,000	0.510	0.526	-0.017	0.318
	(0.016)	(0.006)	(0.017)	
Born in California	0.825	0.832	-0.007	0.557
	(0.012)	(0.004)	(0.012)	

Older Cohort	Linked Mean	Unlinked Mean	Difference	P-value
HSES	0.353	0.350	0.003	0.884
	(0.019)	(0.008)	(0.021)	
City population at least 25,000	0.501	0.509	-0.008	0.710
	(0.019)	(0.008)	(0.021)	
Born in California	0.833	0.842	-0.009	0.566
	(0.014)	(0.006)	(0.016)	

Younger Cohort	Linked Mean	Unlinked Mean	Difference	P-value
HSES	0.385	0.376	0.009	0.731
	(0.025)	(0.008)	(0.027)	
City population at least 25,000	0.525	0.545	-0.020	0.465
	(0.026)	(0.009)	(0.028)	
Born in California	0.810	0.822	-0.012	0.559
	(0.020)	(0.006)	(0.021)	

Table 1: The father's occupation determines socioeconomic status. HSES jobs include professionals, semi-professionals, skilled craftsmen, managers (except farm managers) and official occupations. LSES occupations include farm workers, fishermen, service workers, unskilled laborers, salesmen and clerical occupations. The older cohort includes internees born between January of 1932 and December of 1937, whereas the younger cohort includes internees born between January of 1938 and December of 1942. The sample is restricted to males.

Mean Age at Death			
	Japanese Hawaiians	Internees	Difference
Older Cohort	65.890	65.795	0.095
	(0.247), $N = 1753$	(0.402), $N = 671$	(0.470)
Younger Cohort	60.969	59.048	1.921
	(0.328), $N = 913$	(0.556), $N = 395$	(0.618)
Difference	4.921	6.747	-1.826
	(0.416)	(0.675)	(0.779)

Table 2: Internees are the WRA records linked to the SSDI. Japanese Hawaiians are the unlinked SSDI death records with Japanese surnames and Social Security Cards issued in Hawaii. The sample is restricted to males. The older cohort includes individuals born between January of 1932 and December of 1937, whereas the younger cohort includes individuals born between January of 1938 and December of 1942.

The Early Childhood Internment Effect on Life Expectancy

Dependent Variable	Age at Death		Log of Age at Death	
	(1)	(2)	(3)	(4)
Treatment Effect	-1.629**		-0.029**	
	(0.793)		(0.015)	
	[0.631]		[0.013]	
LSES Treatment Effect		-2.620**		-0.046**
		(1.027)		(0.019)
		[0.817]		[0.017]
HSES Treatment Effect		0.208		0.003
		(1.126)		(0.021)
		[1.083]		[0.021]
N	3732	3732	3730	3730
R^2	0.095	0.095	0.078	0.077

Table 3: Data are from the WRA records linked to the SSDI, and unlinked death records with Japanese surnames that were issued in Hawaii. All regressions include birth-year fixed effects, and state-of-birth dummies for California, Hawaii, Oregon and Washington. The father's occupation determines socioeconomic status. HSES jobs include professionals, semi-professionals, skilled craftsmen, managers (except farm managers) and official occupations. LSES occupations include farm workers, fishermen, service workers, unskilled laborers, salesmen and clerical occupations. The sample is restricted to males. Heteroskedasticity-robust errors are in parentheses, and standard errors clustered at the state/year level are in brackets. The stars correspond to the heteroskedasticity-robust standard errors.

* significant at 10%; ** significant at 5%; *** significant at 1%.

Probit Model Marginal Effects for Probability of Early Death

Dependent Variable: Indicator if Age at Death is	≤ 40	≤ 45	≤ 50	≤ 55	≤ 60	≤ 65
	(1)	(2)	(3)	(4)	(5)	(6)
LSES Treatment Effect	0.020	0.038*	0.064**	0.104***	0.089**	0.082*
	(0.016)	(0.023)	(0.029)	(0.033)	(0.039)	(0.047)
HSES Treatment Effect	0.011	0.002	0.003	-0.017	-0.023	0.018
	(0.024)	(0.031)	(0.037)	(0.044)	(0.049)	(0.058)
N	3675	3732	3732	3732	3732	3732
Pseudo R^2	0.044	0.037	0.020	0.020	0.035	0.055

Table 4: Data come from the WRA records linked to the SSDI, and the unlinked Japanese Hawaiians. Marginal effects $\partial y/\partial x$ are reported with delta-method standard errors in parentheses. All regressions include state and birth-year dummies and a dummy variable for each of the nine occupational categories for the internee's father.

* significant at 10%; ** significant at 5%; *** significant at 1%.

The Early Childhood Internment Effect on Life Expectancy
Excluding the 1942 Birth Cohort

Dependent Variable	Age at Death		Log of Age at Death	
	(1)	(2)	(3)	(4)
Treatment Effect	-1.850**		-0.036**	
	(0.837)		(0.016)	
	[0.644]		[0.013]	
LSES Treatment Effect		-2.719**		-0.052**
		(1.069)		(0.020)
		[0.799]		[0.015]
HSES Treatment Effect		-0.041		-0.002
		(1.208)		(0.023)
		[1.221]		[0.023]
N	3497	3497	3496	3496
R^2	0.083	0.081	0.068	0.067

Table 5: Data are from the WRA records linked to the SSDI and unlinked death records with Japanese surnames that were issued in Hawaii. All regressions include birth-year fixed effects and state-of-birth dummies for California, Hawaii, Oregon and Washington. The father's occupation determines socioeconomic status. HSES jobs include professionals, semi-professionals, skilled craftsmen, managers (except farm managers) and official occupations. LSES occupations include farm workers, fishermen, service workers, unskilled laborers, salesmen and clerical occupations. The sample is restricted to males. Heteroskedasticity-robust standard errors are in parentheses, and standard errors clustered at the state/year level are in brackets. The stars correspond to the heteroskedasticity-robust standard errors.

* significant at 10%; ** significant at 5%; *** significant at 1%.