The Causal Effect of Place:
Evidence from Japanese-American Internment

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Recent research has stressed the importance of long-run place effects on income and economic mobility, but the literature has struggled to isolate the causal impact of location. This paper provides new evidence on these effects using administrative data on over 100,000 Japanese-Americans who were interned during World War II. Internees were conditionally randomly assigned to camps in seven different states and held for several years. Restitution payments paid in the early 1990s to the universe of surviving internees allow us to measure their locations and outcomes nearly half a century after the camp assignments. Using this unique natural experiment we find, first, that camp assignment had a lasting effect on individuals' long-term locations. Next, using this variation, we find large place effects on individual economic outcomes like income, education, socioeconomic status, house prices, and housing quality. People assigned to richer locations do better on all measures. Random location assignment affected intergenerational economic outcomes as well, with families assigned to more socially mobile areas (as designated by Chetty et al., 2014) displaying lower cross-generational correlation in outcomes. Finally, we provide evidence that assignment to richer places impacted people's values and political views, a new and intriguing mechanism through which place effects operate. Together, this new *causal* evidence on location effects has broad implications for urban economics, as well as potential policy implications for policymakers struggling to resettle and integrate large refugee or immigrant populations.

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There are large, persistent differences in economic outcomes across all levels of geography within the United States, from broad regions to neighborhoods within cities. Research has found location to be an important determinant of employment, occupation choice, wages, education, intergenerational mobility, and crime (see, for example, Chetty et al. (2014a, 2014b, 2015a, 2015b), Cutler and Glaeser (1997), Boustan and Margo (2009), and Bertrand et al. (2000)). Understanding the *causal* impact of regional differences in productivity and social environment is therefore of first-order importance. This is especially true today, as policy makers are making difficult choices about where to resettle large refugee populations. Identifying and measuring the causal impact of place is difficult, however, because individual locations are not randomly assigned. Moreover, even when identifying variation is available, it is typically impossible to track individuals over a period sufficient to identify the long-run effects of the initial treatment.

In this paper, we provide new evidence on the causal impact of location assignment on economic outcomes using administrative data on Japanese-Americans relocated to internment camps during World War II. Between 1942 and 1946, roughly 100,000 Japanese-Americans, the majority United States citizens, were forcibly removed from their homes on the West Coast and detained in relocation centers in remote interior regions. Camp locations were geographically dispersed, ranging from California to Arkansas, and also had considerable variation in quality, as measured by metrics like the median income of the county in which the camp was located. Though initial camp assignment was primarily a function of place of residence at the time of relocation, conditional on prior location, we show that camp assignment was effectively random. This random variation allows us to identify the underlying causal effect of place. Moreover, effectively random mid-war reassignments or transfers allow us to distinguish between the treatment effect of the camps themselves and long run place effects.

Under the Civil Liberties Act of 1988, the United States government recognized the great injustice that was done to the Japanese-American community, issuing a formal apology and awarding reparation payments of $20,000 to the universe of surviving internees. Using data collected by the Department of Justice in the early 1990s to locate individuals eligible for compensation, we are able to identify economic outcomes for 60,000 former internees. Although the Japanese internment was unquestionably a tragic episode in American history, the scale of

the geographic randomization it entailed, along with nearly full follow up with survivors half a century after the initial treatment presents a rich source of data for economic analysis.

We find, first, that initial camp assignment has a large and lasting effect on the location of internees. Internees are more likely to live in the state where they were interred, and they are more likely to live closer to their last camp of internment than any other camp. The differences we identify are sizable, with the odds of living in state fifty years after internment increasing by 16 to 19 percentage points. Using data on people who were transferred between camps, we can distinguish between the treatment effect of the camp itself and the causal impact of an internee's eventual location. [1] We show that not only does camp assignment affect an internee's long-run geographic location, but it also affects the quality of their long-run location as well. Internees assigned to camps in higher income regions wind up living in 'better neighborhoods', measured along several dimensions, many years down the road.

With this conditionally random variation in hand, we next turn to identifying the *causal* impact of location on individual outcomes. We do this in several ways. First, we use data from the redress records to match individuals to house prices and housing quality information. We show, using both regressions and a regression discontinuity approach, that the income levels near the assigned camp had a large causal effect on these prices. Assignment to a 10 percent wealthier region is associated with 1.5 to 2 percent increase in house prices, which holds for various regional income measures and with a battery of controls.

Next, to assess the impact of location on income, education, and other variables, we turn to micro data from the 1980 Census. The data collected by the War Relocation Authority (WRA) and Department of Justice under the Japanese-American Redress Verification System (JARVIS) are sufficiently detailed that we can probabilistically match internment information to Census responses. Using this approach, we find significant effects of camp assignment on individual

[1] For example, the prominent actor and former-internee George Takei, was transferred from a camp in Arkansas to a camp in California, from which he was ultimately released. It is interesting to speculate how his career might have been impacted by this transfer.

income, education, and socioeconomic status. Internees assigned to wealthier locations have higher incomes, and are more likely to complete college, and work in higher-status occupations.

Moreover, given the richness of our data, we are able to explore the effect of location on intergenerational income patterns. We find that internee families assigned to places marked as more mobile by Chetty et al. (2014) indeed have a lower intergenerational correlation in incomes than their peers who were randomly assigned elsewhere.

Finally, we use data from the large Japanese American Research Project (JARP) survey to replicate our findings on income, education, and mobility using entirely independent, directly measured data on a subset of internees. This survey, with over 4,000 respondents across three generations and over 1,000 total questions, allows us to similarly compare outcomes for a subset of families assigned to different camps while conditioning on their initial location. We find nearly identical effects using this approach, which provides strong confirmation of our Census-based findings. Internees experience very similar income and educational gains in this data, and assignment to more mobile locations has the same impact on intergenerational mobility. This dataset also lets us dig deeper into some of the suggestive mechanisms underlying our findings. In particular, while internees in richer locations do not report experiencing less discrimination, they do show meaningful changes in their own economic views and values.

This new and powerful evidence of the causal effect of place contributes to an important literature exploring location and neighborhood effects. Seminal studies include Kling, Katz, and Liebman (2007) and Chetty, Hendren, and Katz (2015), which look at the impact of better neighborhoods as part of the Moving to Opportunity Experiment, and Oreopolous (2003), which looks at public housing in Toronto. Chyn (2016) explores the impact of exogenous changes in neighborhoods associated with housing project demolitions in Chicago. These papers used random assignment to different housing environments within a labor market to look at outcomes, and in contrast to us, found small effects for adults. A number of papers have similarly used random assignment to look at the impact of location and in-nationality networks for political refugees (see Edin and Fredricksson (2003), Aslund et al. (2011), Damm (2009, 2014), Beaman (2012), and Gould et al. (2004)). Our work complements these papers by looking at place effects

more generally (as opposed to nationality networks), in the US, over a considerably longer time period, a much broader set of outcomes, and with a large sample.

Our findings also contribute to the literature on intergenerational mobility. Recent influential work by Chetty, Hendren, Kline, and Saez (2014) documents the stark differences that exist across regions of the United States in the degree of intergenerational income mobility. They create measures of intergenerational mobility by location using administrative earnings records over several decades. They find that a one standard deviation increase in their metric of "absolute upward mobility" across commuting zones is associated with a 0.2 standard deviation increase in the income rank of children at 25 percentile of the income distribution. Subsequent work by Chetty and Hendren (2015) and Chetty, Hendren, and Katz (2015) seeks to determine whether these differences across location are causal, exploiting variation in the timing of parental moves and experimental variation in locations within a labor market from the MTO respectively.

Utilizing both the random variation of camp assignment in our data, and the absolute upward mobility metric created by Chetty et al., we confirm that the intergenerational correlation in economic outcomes between parents and children is substantially weakened for those families assigned to more mobile locations. Moreover, similarly to the findings reported in Gould et al. (2011) that Yemenite immigrants placed into better regions of Israel become more assimilated into Israeli society, we find evidence in the JARP survey that Japanese-Americans interred in more mobile regions are also more likely to report values suggestive of a greater sense of agency, optimism, and risk tolerance. These results add to the literature on intergenerational mobility in the United States by suggesting one possible mechanism though which location may influence the differences in economic outcomes we observe across camps.

Previous economic research using data from Japanese-American internment has focused on the direct effects of internment on detainees' labor market outcomes (Chin, 2005), educational attainment (Saavedra, 2013b), and mortality (Saavedra, 2013a). All three papers employ an empirical strategy using Japanese-Hawaiians, the majority of whom were not interred, or other Asian-American populations, as control groups in a difference-in-differences methodology. With this identification strategy, Chin finds that internment reduced the annual earnings of male

internees, and Saavedra (2013b) finds that attending school in the internment camps reduced the probability of receiving college and post-graduate degrees, as well as the returns to schooling in general. Relative to these studies, we focus on identifying the differential effects of regional assignment among internees, rather than the effect of internment in general.

The remainder of this paper proceeds as follows. Section II provides historical background; Section III discusses the datasets used and provides summary statistics; Section IV demonstrates the exogeneity of camp assignments; Section V shows that internment camp location affected post-internment location choice; Section VI presents evidence for the impact of camp assignment on long-run individual economic outcomes; Section VII discusses intergenerational mobility; Section VIII analyzes attitudes toward work and entrepreneurship, and Section IX concludes.

II. Historical Background

Following the Japanese attack on Pearl Harbor and the United States' declaration of war on Japan, the U.S. military became increasingly suspicious of the sizeable community of Japanese-Americans living on the West Coast. Officials doubted the loyalty of this population to the U.S. and feared that individuals of Japanese descent would spy for the Japanese navy or engage in acts of sabotage on military and civilian infrastructure. Though no concrete evidence supported the claim that Japanese-Americans were disloyal, Lieutenant General John DeWitt advised the Naval Department to lobby the President for the power to exclude Japanese-Americans from the West Coast. On February 19, 1942, President Roosevelt signed Executive Order 9066, which gave the military authorization to exclude any individuals from areas deemed to be strategic military zones (U.S. Army, 1943). In practice, this power was applied almost exclusively to enforce the mass relocation of individuals of Japanese ancestry from Arizona, California, Oregon, and Washington, two-thirds of whom were citizens of the United States.

The U.S. Army Western Defense Command planned and enforced the evacuation of Japanese-Americans, first to temporary Assembly Centers, then to permanent Relocation Centers that were being constructed throughout the Western United States. A civilian agency, the War Relocation

Authority (WRA), was created to oversee operation of these camps in the post-relocation phase of internment. Over 100,000 people were ultimately held in ten internment camps across seven states: Arizona, Arkansas, California, Colorado, Idaho, Utah, and Wyoming. These sites were selected by the WRA subject to military approval. It was determined that the camps should be located on public land which had the potential to support a population of approximately 10,000 detainees per camp (U.S. Army, 1943). However, combined with the military's concerns about keeping the camps at a distance from critical infrastructure, the locations selected tended to be in remote interior regions, far from the internees' homes.

The first group to be held by the WRA arrived at Manzanar Relocation Center on March 29, 1942. By September of that year, nearly the entire Japanese population of the West Coast had been transferred to an internment camp. Although a small number of internees were permitted to petition for release to join the military fighting in Europe, attend college in the East, or work in areas outside the West Coast, the majority remained interred though at least 1945, or over three years on average. The last internees were not released until March 1946, well after Japan's surrender in August 1945. Camp assignment, however, was not permanent, with approximately a quarter of internees being transferred to a different location at some point before their release. The most significant of these transfers took place in the fall of 1943, when the government initiated a policy to segregate over 10,000 individuals deemed to be disloyal at the Tule Lake camp in northern California (U.S. DOI & WRA, 1946a).

Japanese exclusion was revoked in January 1945, and as internees were released throughout the year, many chose not to return to their original homes on the West Coast due to housing shortages and racial hostility, but rather settled in new areas throughout the United States. Although the WRA provided some assistance in the form of rail passes and temporary grants to offset the costs of resettling, most internees were released from locations that were both geographically distant and economically dissimilar to where they had been living prior to relocation (U.S. DOI & WRA, 1946b). Given the spatial distribution of the camps and the economic heterogeneity of the surrounding regions, it is not unreasonable to suspect that the location where individuals were interred and subsequently released may have had causal effects on their long-run life outcomes well beyond the direct effects of internment.

Despite the harm that internment had caused, it would be decades before the United States government recognized the great injustice done to the Japanese-American community. In addition to the obvious loss of liberty and violation of civil rights, internees incurred significant economic losses of income and property while they were detained, for which they were not fully compensated. Moreover, the 1983 Congressional Commission on the Wartime Relocation and Internment of Civilians concluded that not a single act of espionage or disloyalty was ever verified that could have justified internment on the grounds of national security. Rather, internment was recognized as the culmination of decades of racial hostility to Japanese and Asian immigrants on the West Coast (U.S. CWRIC, 1983). A growing redress movement among former internees led to the passage of the Civil Liberties Act of 1988, under which the government issued a formal apology and agreed to award each surviving internee $20,000 in compensation.

The internment of Japanese-Americans was unquestionably a tragic episode of American history. However, several features make it a unique and valuable source of data to analyze the economic effect of place. First, because individuals were forcibly, and, as we argue, randomly, assigned to disparate locations, the causal effect of place can be cleanly identified in the absence of self-selection. Second, internment affected the entire Japanese population of the West Coast states, ensuring both a large sample size and that individuals of all ages, educational levels, and socio-economic groups are represented. Third, the Department of Justice was tasked with identifying former internees eligible for compensation under the Civil Liberties Act, ensuring that nearly every surviving internee was located fifty years after the Japanese exclusion. Long-run economic outcomes can therefore be identified over a time period that would be impossible to replicate in a controlled experiment.

III. Data and Summary Statistics

Our analysis employs data from multiple sets of records collected on Japanese-American internees between 1942 and the early 1990's. The first, which we refer to as the WRA dataset, contains background information on 110,000 of the 120,000 individuals that were held in WRA

custody between 1942 and 1946. The dataset is publically available through the National Archives and Records Administration (NARA), and includes name, age, family, prior place of residence, educational attainment, occupations, and initial camp placement for each internee.

To identify which internees were among the roughly 25% who were transferred between camps, we link the WRA data to a set of rosters recording entry and exit dates for all individuals who were ever held at each camp. Although the original documents are available in microfilm through NARA, the main fields of interest have been coded to text by Ancestry.com and are searchable in the collection "U.S. Final Accountability Rosters of Evacuees at Relocation Centers, 1942-1946." Unfortunately, the individual identifier in the WRA dataset was not coded by Ancestry, so we are unable to match the two datasets directly on a single field. However, by linking records based on names and birthdates, we are able to locate 90% of the WRA sample in the Final Accountability Rosters.

The third dataset we incorporate was collected by the Department of Justice, the agency responsible for identifying and locating former internees who were eligible for reparation payments under the Civil Liberties Act of 1988. The records of the Japanese-American Redress Verification System (JARVIS) are held in a restricted-access collection by NARA, and include address information for all surviving internees that the Justice Department was able to locate between 1990 and 1999. As part of the verification of internment status, the agency also recorded the original WRA identification numbers assigned to potential recipients, allowing us to directly link over 60,000 former internees to their camp assignment history and pre-internment characteristics.

For our analysis, we drop observations where prior place of residence or initial camp assignment are missing in the WRA data. We also restrict the sample to internees who were residing in the continental United States prior to internment. Although a small number of Japanese-Americans from Alaska and Hawaii were interred, these cases were handled differently from the mass-relocation policy on the West Coast. For example, only 2,000 Japanese-Hawaiians were selected for internment, and this small group is unlikely to be representative of the broader population of 158,000 people of Japanese ancestry who were living in Hawaii in 1942 (U.S. CWRIC, 1983).

After imposing these restrictions, we are left with a sample of 61,427 internees who appear in the linked WRA-JARVIS dataset.

Summary statistics for this linked sample are presented by initial camp assignment in Table 1. The first feature to note is that with nearly 50 years elapsing between internment and the passage of the Civil Liberties Act, internees appearing in our sample were relatively young when they were relocated. The mean age is between 17 and 19, although with approximately half of sample over the age 18, we still observe a sufficiently large number of working-age individuals to ask how location assignment may have differentially affected internees with different occupations or levels of education. Moreover, since multiple generations were affected by internment, the data also allow us to access how camp assignment influenced intergenerational mobility among interred families.

Two-thirds of the interred Japanese-Americans were born in the United States, and significant pre-internment demographic differences existed between the American-born Issei generation, and the Nisei, who emigrated to the U.S. from Japan prior to the restriction of immigration from Asia in the 1920s. The Nisei, being older, were more likely to be married and have completed their education than the younger Issei, while the Issei tended to be more integrated into American society and more likely to read and speak English. We also know from WRA documents that the Issei were more likely to be granted early release from the internment camps upon petition and loyalty review than the Nisei, and consequently we include controls for whether an individual was born in the United States or Japan in our empirical analysis (U.S. DOI & WRA, 1946b).

While the WRA-JARVIS dataset provides a rich set of pre-internment covariates, we have also obtained the recently released non-anonymized 1940 Census micro data. We are able to match these data to JARVIS records at the individual level for most of our sample, giving us a plethora of pre-treatment variables to use as controls and to test for the exogeneity of camp assignment.

Our main treatment variable is the log of median income in the county where each camp was located, as measured in 2010[2]. We take this to be a measure of the quality of the location to which internees were assigned. Summary statistics on regional income are included in Section E of Table 1. In the county surrounding every camp, median income is lower than the average median income in counties from which detainees were relocated (also measured in 2010), reflecting the remote locations where the camps were constructed. In all but one case, this difference is greater than one negative standard deviation, and is greater than three negative standard deviations for four camps. Thus, most internees experienced a substantial negative shock in regional income. While all camps were on average poorer than the areas in which internees were previously living, there is also considerable variation in the treatment effect, with the richest region around Gila River in Pinal County, AZ having a median income 170% that of the poorest region around Rohwer in Desha County, AR.

We obtain one set of our outcome variables using the addresses recorded in the JARVIS data. The vast majority of surviving internees were sill living in the United States, with only 1,000 émigrés appearing in the sample, nearly all of whom relocated to Japan. For internees remaining in the United States, changes in location can be measured directly by calculating the distance between camps and the population-weighted centroid of 1990s zip code. Data on house prices is obtained by searching for each address on Zillow.com. Despite the fact that many elderly internees lived in institutions such as nursing and retirement homes, as well typographical errors in addresses, we are able match 73% of our sample to a house price, ignoring homes constructed after the year the address was recorded by the DOJ. We also identify median income of each internee's census tract in 2010 by geocoding the addresses.

The JARVIS data alone is insufficient to match internees to outcomes on income and education. We must therefore rely on a different approach to collect data on these variables. We begin with samples drawn from the 5% 1980 Census dataset from IPUMS (Ruggles, 2015), restricting attention to individuals listing both their race and primary ancestry as Japanese. As we do not observe whether an individual was interred, let alone camp assignment, in the IPUMS data, we

[2] In three cases where camps were located on the border of two counties or in a county that split after internment, we use income in the county with the larger population.

must use a probabilistic matching technique utilizing the observable variables of birth year, birth quarter, gender, birthplace, and public use micro data area (PUMA), as well as house prices and characteristics to construct a set of potential matches. To identify individual outcomes from this dataset, we employ a bootstrapping methodology that is detailed in Section VI.

Finally, we have obtained survey data on more than four thousand Japanese respondents from the Japanese American Research Project (JARP), a three-generation sociological survey directed by UCLA sociologist Gene Levine (1997). The survey, sponsored by the Japanese American Citizens League, the National Institute of Mental Health, and the Carnegie Corporation asked over 1,000 questions on a wide range of economic and social questions throughout the 1960s. The survey also includes information about individual families before, during, and after internment, including residence and income. While we cannot link these data directly to the WRA-JARVIS files, this information makes it possible to replicate our experimental design within the JARP data alone. The richness of the survey makes it possible to both confirm our findings in an entirely separate data source and to explore the impact of internment location on a very large number of outcomes.

IV. Exogeneity of Camp Assignment

For Japanese-Americans living on the West Coast, internment camp assignment was primarily a function of residence at the time relocation was enforced. A map of internment camp locations is shown in Figure 1. As shown in Figure 2, the U.S. Army divided the West Coast Exclusion Area in 108 Exclusion Zones, each containing approximately 1,000 individuals to be relocated. The boundaries of these areas were informed by data collected in the 1940 Census and drawn according to existing physical or legal borders. Consideration was also given to preserving communities and socio-economic groups within these areas, and therefore with few exceptions, individuals residing in the same Exclusion Zone were ultimately transferred to the same internment camp (U.S. Army, 1943).

The mapping from Exclusion Zones to camps was influenced by factors that included a desire to evacuate areas of the highest military priority first. Since relocation began before construction of

several internment sites was complete, residents of the first areas to be relocated were transferred to the first operational camps. After military concerns, a secondary objective was to minimize the distance that communities were moved from their homes and to assign them to an area with a similar climate. However, by partitioning the Exclusion Zones based on population, cities with large Japanese-American populations such as Los Angeles and Sacramento saw communities sent to multiple Relocation Centers with considerable geographic dispersion. On the other hand, more sparsely populated areas were clustered together for camp assignment. In addition, the Army ensured that each camp received both urban and rural populations in order to provide balance to communities that were intended to be largely self-sustaining (U.S. Army, 1943).

Given this plan for the relocation of Japanese-Americans, we argue that conditional on county of residence and generation, the quality of the region surrounding the camp internees were assigned to is uncorrelated with personal characteristics that may affect our outcomes of interest. We test this identification assumption by regressing the log of median income in the camp county on personal characteristics and a set of fixed effects for county of origin cross generation for both first and final camp assignments. Including county fixed effects controls for the selection of individuals into different areas prior to internment while still maintaining variation in camp assignment. Interacting the county fixed effects with generation controls for differences between the Issei and Nisei generations that may have led to the groups sorting into different initial locations.

Although the WRA dataset includes variables of interest such as education, it does not contain other possible predictors of economic outcomes that could be correlated with camp assignment such as income, occupation, and housing. To show that these variables are also uncorrelated with our treatment effect we utilize a subset of the full unanonymized 1940 Census dataset provided by IPUMS that includes all individuals in the continental United States listing their race as Japanese (Ruggles, 2015). In theory, every internee in the WRA data who was born before 1940 should appear in the Census, but in practice the lack of individual identifiers makes it difficult to link observations between the datasets. To maximize the number of potential matches, we exploit information on family structures to link records, which allows us to find 32,560 individuals, or 57%, of our sample in the Census.

The estimates for the exogeneity tests are presented in Panel A of Table 2, with each coefficient obtained from a separate regression. No variable is significant at the 10% level, indicating that once initial location and generation are controlled for, regional incomes in both the internee's first and last camp assignment are effectively random. Given the age-based attrition that is necessarily present between the 1942 and the 1990s, another concern may be that attrition is non-random, or that the quality of location assignment is correlated with mortality. To test for non-random attrition, we use the full WRA sample and regress a dummy for appearing in the JARVIS data on the log of income at initial and final camp assignment, again including fixed effects for county of origin cross generation. The result of this regression is presented in Panel B of Table 2. Neither first nor last camp income predicts attrition with statistical significance. Likewise, camp income does not predict whether an internee was among the 1000 people that were residing outside the United States in 1990.

We conduct similar tests in the JARP data, though there we have information only on a respondent's final camp of internment. We see no statistically or economically significant relationship between assignments to better camps and a respondent's pre-internment income or family social status, conditional on initial location. In unreported tests, we also find no correlation between assignment to richer camps and year of arrival in the United States, years of schooling, gender, and other covariates.

Given our Census matching procedure, another concern may be that the probability of finding a link between the WRA and Census is itself correlated with the treatment effect. We test for this possibility in Section B, where the independent variable is an indicator for an observation being linked to the 1940 Census. We restrict the sample to the set of potential matches by excluding individuals in the WRA data born or immigrating to the United States in 1940 or later. The coefficient is insignificant with a t-statistic of 0.326 and 1.504 for first and last camps respectively, alleviating concern that our record-linkage procedure matches a subset of individuals who were assigned to locations unrepresentative of the full sample. At this point, we also test whether matching the JARVIS data to the 1980 Census dataset is non-random, and again find no correlation between camp income and the probability of finding a match. As an

additional check, we test whether the treatment effect could be correlated with the *number* of potential matches we locate in the 1980 data, and find that this too is not predicted by camp income.

V. Effect of Camp Assignment on Long-Run Location

We use multiple approaches to demonstrate the relationship between a person's conditionally randomly assigned camp and their location of residence later in life, particularly at the time of redress circa 1992.

State of Residence

Our first approach, which we implement at the state level, creates a vector of all individual × state of residence pairs. We then code a dummy variable equal to one if the individual does indeed live in that state. We regress this dummy variable against measures reflecting internment in that state and fixed effects for initial state × destination state × generation pairs, as well adding individual trait × destination × generation controls in some specifications. The final regression assumes the form:

$$live\_in\_state\_s_{i,s} = \alpha_{o\times s\times gen} + \beta\times interred\ measure_{is} + \gamma\times X_{is} + \varepsilon_{is} \qquad (1)$$

where $\alpha_{o\times s\times gen}$ represent the fixed effects, X_{is} the controls, and β the coefficient of interest. To deal with the obvious intra-person correlation, as well as possible correlation across people from the same location, we cluster the standard errors by county of origin.

The result of these regressions is presented in the first panel of Table 3. In our first specification, we measure internment using a dummy for state of first camp assignment. We find that being initially assigned to live in a state causes a roughly 16 percentage point increase in the likelihood of living there in 1992, conditional on one's initial location. This effect is statistically significant at the 1% level.

As discussed above, roughly a quarter of the internees were transferred between camps at some point during the war. When we regress our state of residence measure on a dummy for final state assignment, the coefficient grows to 19.3 percentage points, and when we include dummies for both initial and final internment assignment, the former loses significance while the latter remains at roughly 20 percentage points. This is significant, because it helps us distinguish between long term location effects and differences due to location effects within the camps themselves. This, in addition to further evidence we present later, suggests the former matters more than the later. Finally, in column (4), we control for possible trends by demographic groups to live in different destinations. Specifically, we control for high school graduate-destination-generation, college graduate-destination-generation, age-destination-generation, and married-destination-generation fixed effects. We find that, despite nearly a thousand additional controls, the impact of internment remains largely the same.[3]

We repeat this approach in Table 4 using the data from the Japanese American Research Project. While the geographic data are not precise enough to use our continuous distance measure, we can replicate specification (1). We report the effect of internment on the probability of living in state for first-generation Japanese immigrants immediately after the war, and then for 1st, 2nd, and 3rd generation immigrants at the time of the survey. The coefficients reported in columns (1), (3), (5), and (7) are very similar to the results from the WRA data and show no evidence that the "stickiness" of place declines over time. There are multiple ways of handling the standard errors in this data. Here, we report standard errors clustered both by family and by pre-internment location, and the results remain significant under either alternative. In columns (2), (4), (6), and (8), we add origin-destination fixed effects. Unfortunately, here the geographies consist of states and major sub-state regions rather than counties. As a result, these fixed effects now soak up considerably more of the variation.[4] Nevertheless, we detect significant, though smaller, impacts of internment on subsequent location.

[3] We also repeat this exercise dropping individuals interred in each state, one at a time. The range of coefficients from this exercise is: 0.072-0.24 and remains statistically significant at the 1% level in all of these regressions.

[4] To improve the precision of these results, we divide California in a northern and southern region (based on proximity to LA and SF). This division does not qualitatively affect our results, which remain statistically and economically significant either way.

A second way to approach the impact on state of residence is to evaluate the impact state-by-state. We do this with a series of logistic regressions, assessing whether residence in a particular state is associated with having been assigned there as the first-state of internment.[5] The number of observations now corresponds to the number of individuals, and once again, we control for initial state-generation fixed effects. The results are reported in Table 5. We find that, for all states save Idaho[6] (and possibly Wyoming), internment is associated with an increased likelihood of living in that state. The positive odds ratios range from 1.11 in Wyoming to 3.0 in Colorado and are generally statistically significant, despite the loss in power from analyzing each state alone.

Continuous Distance

Another approach to analyzing this impact is to use a continuous distance measure in place of our binary live-in-state outcome. This allows us to capture the possibility that internment in the country's interior is associated with decreased likelihood of returning to the West Coast, or that being interned in Minidoka, Idaho is associated with an increased likelihood of moving to nearby Salt Lake City, Utah.

To implement this approach, we once again create a vector for each individual, this time recording individual-camp pairs. For each pair, we calculate the distance between the camps' location and the individual's residence in 1992. We then regress this continuous distance on measures of place of internment, initial-location × camp × generation fixed effects, and controls for demographic characteristics-camp pairs. Again, the specification takes the form:

$$distance\_to\_camp\_c_{ic} = \alpha_{o\times c\times gen} + \beta\times interred\ measure_{ic} + \gamma\times X_{ic} + \varepsilon_{ic} \quad (2)$$

where again $\alpha_{o\times s\times gen}$ represent the fixed effects, X_{is} the controls, and β the coefficient of interest. The results are reported in Table 3 columns (5) through (8), where again we cluster the

[5] We obtain very similar results when using last assigned state, as expected.
[6] Which makes sense if you've ever been to Idaho.

standard errors by initial county location. We find that internment in a camp greatly reduces the distance between an individual's ultimate residence and their camp assignment, with people living roughly 10-20% closer to their last assigned camp. These results are statistically significant at the 1% level in all specifications.

These results are comparable to those found in studies reporting relocation rates among immigrants who were initially assigned to settle in randomized locations in Sweden (Aslund, 2005) and Denmark (Damm, 2009). These papers report that 62 (48) percent of immigrants remained in their assigned municipality after 4 (7) years. Relative to these studies, we are able to demonstrate that persistence in location remains surprisingly strong many decades after the initial randomization. We also find that, rather than decaying over time, the magnitude of the effect is roughly constant over the post-internment period.

Heterogeneity and Timing

It is interesting to consider what segments of the population are most impacted by this forced randomization in terms of their post-internment locations. Due to the detailed records kept by the WRA, we can explore the impact of camp assignment by gender, education, occupation categories, and age. Appendix Table 1 reports the results of running our lived-in-state specification on subsamples of the population.

A number of interesting differences emerge from the table. First, the location of higher-educated individuals, professionals, and younger people are less influenced by the randomization than their peers. This may be because these people have more opportunities to move, or because they have higher incentives to move as their outcomes may be more place-dependent. In contrast, people who were already married, people who had spent time in Japan, and people working in agriculture were more affected by the randomization. Again, this may be because the gains to moving, or the costs to remaining in place, are smaller (i.e. they already have a spouse, are more accustomed to rural life, etc.). The differences are large and statistically significant. They are also interesting in light of the findings in Chetty (2015a), which find larger place effects for the young.

Of course, our main measure of internees' final or permanent location comes from the addresses supplied as part of the 1992 redress program. Although we cannot track interim locations for most internees, there is one source of data that is available on post-internment location for a subset of internees – the Social Security Death Index (SSDI). Using names and birthdates, we are able to match internment records to the death index for men who expired in a given year. We do not attempt to match women, since the SSDI does not consistently record maiden names, making it difficult to locate women who married after 1942. Using this dataset, we reran our continuous distance specifications on subsamples of men who passed away in a given year. The coefficients are reported in Figure 3.

We find that impact on distance occurs almost immediately and stays roughly fixed through the 45 year period. This effect is almost remarkably similar to the effect recovered (in Table 3) from using addresses from the restitution file. Thus, it appears that the random assignment of location attributable to interment occurred early on, and that the effect did not weaken or grow significantly over time.

VI. Effect of Camp Assignment on Individual Economic Outcomes

Having established an impact of internment on internees' permanent locations, we now examine whether or not assignment to a higher income area is associated with better individual outcomes. We tackle this question using two primary approaches. First, we consider the effect of internment on house prices, housing quality and neighborhood income, as these are the variables that we observe for the largest number of internees by matching the addresses supplied by the universe of redress recipients to housing data from Zillow. This allows us to provide initial indirect evidence of increased income and wellbeing. We show that internment in richer areas is associated with higher housing prices and quality using both standard regressions models and a regression discontinuity approach across assignment zone boundaries in Los Angeles.

We then turn to directly estimating the effect on income, education, and socioeconomic status using data from the 1980 Census. The publically available micro-data contain a 5% sample of the

U.S. and report Japanese ancestry. Limiting to this population, we probabilistically match individuals in the internment data to the Census micro-data subsample using birthplace, year, quarter, puma of residence, gender, and house prices and characteristics. The process is described in more detail below. Pooling our estimates over one hundred bootstrapped samples, we find significant effects on income, occupational status, and education. We present evidence that these effects persist within occupation and location, shedding new light on the mechanism behind place effects.

House Prices, House Quality, and Neighborhood Income

As discussed above, internee addresses at the time of redress were matched to house prices using Zillow. We first test the impact of being assigned to a higher income camp using the specification:

$$\ln House\ Price_i = \alpha_{origin \times gen} + \beta \times Ln\ Avg\ Income\ Near\ \ Camp_i + \gamma \times X_i + \varepsilon_i \qquad (3)$$

The results are reported in Table 6 for internment income measured in the county where the camp was located and within a 50-mile radius. We find, robustly, an elasticity between 0.15-0.20 that is statistically significant at the 1% level when clustering the standard errors by pre-internment county. As can be seen in column (3), this result is robust to the inclusion of individual level controls, such as age, gender, marital status, and education in 1942. One potential concern is that these differences may relate to differences in the camps themselves, rather than the surrounding area. To address this concern, in columns (2), (6), and (10) we again control for both first and last camp. We see that only the last camp, which we have shown affected long-term location, impacts later life outcomes. This is more consistent with long term treatment effects than within camp effects.

We next explore the impact of location assignment on housing quality independent of price. To construct our measure of housing quality, we take the universe of Zillow prices and house characteristics matching internee addresses and then run a regression of

$$\ln House\ Price_i = \alpha_{zip} + \beta_1 \times Bed\ Rms_i + \beta_2 \times Bath\ Rms_i + \beta_3 \times Sqft_i + \beta_4 \times Age_i + \gamma_{type} + \varepsilon_i \qquad (4)$$

where α_{type} are dummies for structure type (e.g. apartment, condo, townhouse). We use the coefficients β, γ from the above regression[7], along with each house's individual characteristics, to construct a quality measure for each internee that does not depend on the local price level. This measure can be read in dollar equivalents. We then use this quality measure as the dependent variable in Columns (5) - (7) in specification 3 reported above. We find that, like house prices, housing quality is significantly increased by random assignment to a wealthier location. The magnitude is smaller than the effect on prices though, at roughly $1/16^{th}$ of a standard deviation, as compared to the $1/4^{th}$ of a standard deviation on prices.

Finally, we explore the impact of camp assignment on neighborhood quality, as proxied by census-tract median income. We find that, robustly, people assigned to camps in higher income regions live in higher income neighborhoods 50 years later. The magnitude (roughly $1/10^{th}$ of a standard deviation) falls somewhere between the effect on prices and quality described above.

Regression Discontinuity

Though we have established that camp assignments were not correlated with other determinants of later life outcomes conditional on generation and county of origin, it is also possible to explore the impact of assignment with a regression discontinuity approach. Though this approach sacrifices power, we feel that as a supplement to our main analysis it helps clarify the identification assumptions.

To implement a regression discontinuity, we limit ourselves to individuals we could match to the 1940 Census micro-data. These data include rough addresses for internees only two years before internment. We geocode these addresses, when possible, to locations within the known evacuation zone. For feasibility, we limit ourselves to individuals assigned to either Gila River, AZ or to Rohwer and Jerome, AR in Los Angeles County (see Figure 4). We are able to locate between 50-75% of this sample at an appropriate location, depending on the tolerance we allow for imperfect address matches. We then calculate, for each of the two to three thousand individuals, the distance between their address and the border to the next zone.

[7] Reported in the appendix.

Using distance to the boundary as the run variable, we calculate the average house price for evenly spaced bins, both in 1940 before internment and in 1992 fifty years after. The result for each bin, along with best fit lines, is reported in Figure 4. As the figure shows, living just across the border in the richer Gila River evacuation zone is associated with a $200K increase in housing prices, though no difference existed prior to internment. This result is statistically significant when running

$$\ln House\ Price_i = Distance_i + \beta_1 \times Distance_i \times 1(Distance > 0)_i + \beta_3 \times 1(Distance > 0)_i + \varepsilon_i \quad (5)$$

on intervals various of various sizes around the cutoff (see Table 7).

This framework also presents a natural exogeneity test using price data from the 1940 Census itself. A sizeable majority of the matched internees were renters in 1940, and therefore we use the rents reported in the Census. We show in Figure 4 that there is no significant discontinuity at the assignment border prior to internment. The estimated discrepancy in 1940 is insignificant and negative. In Appendix Table 2, we show that over various intervals around the cutoff the estimated discontinuity in 1940 alternates between positive and negative and are generally not statistically significant. We think this provides strong evidence that internment indeed had a causal effect on later life outcomes.

Census Match

We are, of course, interested in non-housing outcomes as well. To obtain data on these outcomes, we first turn to the 1980 Census Public Use Microdata. As discussed above, these data allow us to identify individuals of Japanese ancestry, along with their birth date, PUMA of residence, gender, house price, and housing characteristics. Often, this information is sufficient to uniquely identify a single individual in the JARVIS redress files. In many cases, though, the match is not one-to-one, and we need to create bootstrap samples.

We create these bootstrap samples by randomly matching individuals in the Census and JARVIS datasets that match on the characteristics above. To prevent double counting, this matching is done without replacement, leading to different sample sizes across bootstrapped samples. We

construct one hundred bootstrap samples using independent random matching across samples, conditional on the traits.

Income, Education and Socioeconomic Effects

We use these bootstrap samples in two ways. First, we run the following regression in each sample, storing the coefficient of interest β.

$$\ln Income_i = \alpha_{origin \times gen} + \beta \times Ln\ Avg\ Income\ Near\ Internmet\ Camp_i + \gamma \times X_i + \varepsilon_i \quad (6)$$

We report summary statistics on the distribution of these estimated coefficients in Panel C of Table 8. Though the coefficients vary depending on the sample, they by and large point to large income effects, with an elasticity of individual income of roughly 0.2. The 5th percentile of the coefficient ranges always lies far above zero.

In addition to running the regressions separately sample by sample, we also use the bootstrap sample to create weights for Census-JARVIS matches. The samples allow us to assess how frequently a JARVIS person is paired with a Census respondent (note for unique matches this is 100%). We can use these frequencies as weights in the universe of possible matches (i.e. Census and JARVIS individuals that match birth places, dates, etc.) to estimate the impact on income, education, and other Census variables.

Specifically, we run a weighted regression of income/education on the log of income near the assignment camp, where the observations are possible Census-JARVIS pairs.

$$\ln Income_i = \alpha_{origin \times gen} + \beta \times ln\ Avg\ Income\ Near\ Internmet\ Camp_i + \gamma \times X_i + \varepsilon_i \quad (7)$$

The regressions, again, control for fixed effects for county of origin times generation. The regressions are weighted by, alternately, the frequency with which a pair shows up in the universe of bootstrap samples or such that each Census respondent's matches sum to one.[8] We

[8] It does not make sense to have Jarvis respondents' weights sum to one, since a JARVIS respondent might not be in the Census 5% sample. However, since a large fraction of people with Japanese descent in the mainland USA were interred, the reverse might be justifiable.

report the result of these regressions in Table 8, where standard errors are clustered by individual to address clear intra-personal correlation in the error terms.

Before turning to our pooled sample income and education results, we note that the data themselves provide a sanity check on this matching procedure. We have two sources of house price data—the known JARVIS prices from Zillow and the reported home prices from the Census pairing. Our matching procedure should force them to be similar, and we therefore should expect them to have similar slopes with respect to the income of camp assignment. Indeed, columns (1) and (2) show that we do recover very similar effects across the measures from the two different datasets. That suggests the probabilistically matched Census data is, at least in part, reproducing the known outcomes.

Turning to Columns (3) an (4), we see that camp income has a large and significant effect on personal and household income. The estimated impact, unsurprisingly, is close to the average bootstrap sample estimate and is relatively insensitive to the choice of weights. We find an elasticity of individual income with respect to internment site income of roughly 0.22 and for household income of roughly 0.13-0.15. This result holds, again, when we control for demographics such as pre-internment education, age, and marital status, as can be seen in columns (5) - (8).[9]

Of course, income itself is noisy and sensitive to decision about labor supply. As an alternative test for location's causal effect on labor market outcomes, then, we also look at the impact on education and occupational socio-economic or prestige scores. Table 9 shows the estimated impact on three common occupation-based measures: the Duncan Socio-Economic Index, the

[9] As it's impossible to isolate exactly what feature of assignment locations are impacting outcomes, we've thus far demonstrated the causal impact of place on long-run outcomes using a reduced form approach. Still, calculating IV elasticity is useful to compare these impacts with those found in Chetty et al. (2015) and elsewhere. The Chetty et al. paper reports that children under 13 at the time of randomization lived in zip codes whose mean incomes were $1346 higher in adulthood and had household incomes that were $2231 higher (a ratio of 1.7 and an elasticity of 3.3). Our IV elasticity estimates (Appendix Table 3) are smaller, ranging from 1.1-1.8. When performing our regression in levels, however, we also get large and statistically-significant coefficients in the range of 0.7-0.9.

Hauser-Warren Socio-Economic Index, and the Nakao-Treas Occupational Prestige Score. It also shows the impact of internment location income on having completed four or more years of college. We find that all three scores were higher for those randomly assigned to higher income internment camps[10].

To understand whether or not coefficients of the size we recover are common given this data setup, we conduct placebo experiments on our pooled bootstrap sample. Specifically, while maintaining the JARVIS-Census matches weights, we randomly assign camp incomes across actual camps without replacement. Then we rerun the above regression separately for each bootstrap sample where the only change is the value of log average income near the camp, and store the distribution of coefficients. We show the distribution of placebo coefficients, along with the average value from the true pooled sample, in Figure 5. As can be seen in the figure, the placebo distribution is centered on zero and rarely produces coefficients as large as the one recovered from the true data. The true coefficient is larger than all or virtually all of the placebo coefficients.

These findings raise several interesting questions about the underlying mechanism. The first is the extent to which these effects reflect changes in `real income' and the extent to which they reflect differences in local price levels. We seek to answer this question in two ways in Table 10. First, we use data from the Census to subtract out the average PUMA housing costs (following Ganong and Shoag (2015), we use 12 times monthly rent or 5% of house prices) from internee household incomes. We then regress this measure of `real income' on log income of one's internment camp using our bootstrap approach. In column (1) of Table 10 find that impact on incomes net of housing cost remain large, statistically significant, and comparable in magnitude. A second approach for gauging the impact above and beyond the effect of price levels involves regressing outcomes (individual income, house price, house quality, and tract income) while controlling for county of residence fixed effects. In other words, we can test whether individuals interred in richer camps have better outcomes than those interred in poorer

[10] The magnitude indicates that a one-unit increase in log camp income is associated with a 0.1-0.15 standard deviation increase in SEI. This is equivalent to the difference between a maid and a laborer in the Duncan scale.

camps, even conditional on winding up in the same county. In Columns (2)-(5) of table 10, we report these effects. The coefficients again are generally smaller, reflecting some impact price level impact, but remain large and significant. Thus, while there appears to be some price level effects, there are substantial real effects as well.

The within-county effects imply that internment in a poorer location is associated with negative outcome above and beyond the impact on ones' location itself. This scarring effect indicates that the mechanism does not entirely stem from one's immediate labor market opportunities. Further evidence to that effect can be found by, alternatively, including occupation fixed effects in these regressions. In columns (6) and (7) of Table 10, we show that the negative effects persist even within occupations. While fully characterizing the mechanism behind these long run location effects lies beyond the scope of this paper, the within location and occupation results are intriguing and suggestive about its proper interpretation.

Next, we turn again to the JARP data to confirm these income results using direct survey evidence. In Table 11 we regress income for 2nd and 3rd generation Japanese immigrants on the log income of the county where their family was interred. Most 1st generation immigrants are no longer working at the time of the survey (the median age is 73), and so we have current income data. For the Nisei and Sansei respondents for whom reliable data is available[11], in columns (1) – (3) we find an elasticity of 0.22 for family income with respect to the income in county on internment. This is *virtually identical* to the estimates produced in our Census matching approach. Again, there are many ways to handle the standard errors in these regressions. We report standard errors clustered by family, by pre-internment location, and simple Huber-White heteroskedasticity robust standard errors, and the results remain highly significant across approaches. We also include specifications that, like before, control for the pre-internment location of the family, respondent sex, and age. We find these controls have little effect on the estimated elasticity.

[11] We limit the sample to those between 22 and 50 to avoid low income reports by students or retirees. Roughly 80% of the Nisei and Sansei respondents fall into this range.

In columns (4) – (6) of Table 11, we explore the impact of assignment to a richer location on college attendance in the JARP data. The educational attainment of the Issei and the Nissei generations was not affected by camp assignment, and the share of these generations attending college was substantially lower than the Sansei generation. Among the younger Sansei respondents, however, the internment assignment of their family had large and significant effects. Those assigned to a 1 standard deviation richer camp (.13 log points in this sample) had a roughly 4 percentage point increase in the likelihood of having attended college. This effect is statistically significant and robust to controls. It is larger in magnitude than found using the Census matching approach, though this makes sense given the fact that those data did not distinguish between the generations.

VII. Effect of Camp Assignment on Intergenerational Economic Outcomes

Our data also provide a unique opportunity to test for the causal impact of location on inter-generational mobility using random variation in location assignment across the country and over time horizons similar to recent work by Chetty et al. (2014a, 2015a). To test for this impact, we first collected the Chetty et al. (2014a) measure of absolute upward mobility by county of internment. We then marry pre-internment parental outcome measures from the 1940 Census with children's outcomes from the Jarvis-WRA data. Using this data, we run regressions of the form:

$$\begin{aligned}\log/\mathrm{rank}(Child\ Outcome_i) &= \alpha_{prior\ loc\times gen} + \beta_1 \times \log/\mathrm{rank}(Child\ Outcome_i) + \beta_2 \times Mobility\ Measure \\ &+ \beta_3 \times \log/\mathrm{rank}(Child\ Outcome_i) \times Mobility\ Measure + \varepsilon_i \end{aligned} \quad (8)$$

We interpret the coefficient β_3 as measuring the extent to which living in a higher mobility place moderates the intergenerational transmission of outcomes. The parental outcomes available to us in the 1940 Census are annual rent and occupational prestige score of the household head. Again, the child outcomes in the JARVIS-WRA data are rent, house quality, and median tract income. The results of these regressions are reported in Table 12. The first thing to note is that the log based measures produce an intergenerational elasticity measure similar in spirit to those found in Solon (2004). However, as Chetty et al. and others note, these specifications can do a poor job in

the presence of non-linearity. We therefore also replicate the rank-rank regressions used by Chetty et al. and Dahl and DeLeire (2008). Ranks are calculated in sample.

We find that the descriptive measures created by Chetty et al. indeed have a causal effect. The slope of child outcomes with respect to parental outcomes, across measures, is weakened for those randomly assigned to more mobile places. The magnitude is meaningful – a one standard deviation change in the Chetty et al. mobility measure is associated with a 10% smaller slope on average – and is statistically significant for all but a handful of observations. We believe that this provides powerful, well-identified evidence in support of the conjectured causal channel.

Of course, we would like to measure the impact of assignment to more mobile regions on the intergenerational transmission of income, not just covariates of it. While it is not possible to recover a large enough sample by matching the 1940 Census micro data probabilistically to the 1980 Census, we can investigate this question in the JARP, which asks Issei respondents about their highest annual income bracket prior to internment. We can therefore explore how this income report correlates with their descendants' incomes based on the location of internment.

The results, which use the same specification as above with income as the outcome variable, are reported in Table 13. We find that, as before, parental income has less of an effect on children's incomes in high mobility places. The magnitude is very similar to what we found in Table 12. In the JARP data, assignment to a one standard deviation more mobile location reduces the slope by roughly 10%.

VIII. Effect of Camp Assignment on Discrimination and Values

We have shown that an internee's randomly assigned location causally affected their economic outcomes and the economic outcomes of their descendants. In this section we discuss whether or not these results might be informative about outcomes for other groups. We also provide suggestive evidence of a novel channel through which these location impacts might operate.

Japanese-American internees were clearly not a random sample of the U.S. population. Still, it is not clear that these internees and their descendents are differentially affected by the average income of their surroundings relative to the broader population. While we cannot test this directly, we provide suggestive evidence that these place impacts are similar for Japanese and non-Japanese Americans in Figure 6. There we show that the slope of average incomes for Japanese and non-Japanese Americans in the 1980 Census with respect to state average incomes are statistically indistinguishable. This holds true when instrumenting for one's actual location (which may be endogenous) with the income in one's state of birth as well. This is suggestive evidence that place impacts might have similar impacts

One potential reason for Japanese Americans to have a differential impact may be that they face different degrees of discrimination across places, and that this discrimination correlates with income. We are able to test this hypothesis directly, thanks to the richness of the JARP data.

In Table 14, we regress answers by second and third generation JARP respondents to questions on discrimination on log income of the internment camp. Unfortunately, these questions were not asked of first generation respondents. Between 8-18% of respondents report experiencing discrimination, yet as shown in Columns (1) – (3), there is no significant correlation between having experienced this type of discrimination and camp incomes. The magnitudes are quite small, and if anything are weakly positive. We similarly find no effect of camp income on whether or not the JARP respondents have a non-Japanese romantic partner or non-Japanese best friend. Together, these results suggest that the place effects here are not operating via differences in discrimination.

While JARP respondents do not report different degrees of discrimination in richer and poorer places, they do report significant differences in their answer to various questions on their values. The JARP survey asked second and third generation respondents multiple yes-or-no questions about their worldview, such as whether or not they agree that "A man's success is driven by luck," or that "Americans stress occupational success too much".

Overall, we detected three main themes in these questions; how much the respondents valued money or were materialistic, how much the respondents felt they had agency over their lives, and how optimistic the respondents were about events in general. To make the analysis tractable, we divided questions based on these themes and created indices for each category by summing question responses. We then standardized these indices to have a mean zero and a standard deviation of one.

Columns (4) – (6) of Table 14 show the results of regressing these indices on the log income of the family's internment county. We find that there are significant differences in the values espoused by ex ante identical internees assigned to different locations. Internees in richer areas are significantly more likely to answer yes to questions evoking materialism, agency, and optimism. The magnitudes indicate that a doubling in camp county income is associated with 0.4-0.5 standard deviation increase in these indices, a modest but meaningful effect. These results suggest an intriguing mechanism that might underlie some of causal impact of place, namely that people assigned to different locations also develop different economic attitudes and values. These findings contribute to an interesting and exciting new area of cultural economics.

IV. Conclusion

Our analysis provides new evidence of causal long run place effects using the strong persistence in families' locations fifty years after internment ended and Japanese-Americans were allowed to return to civilian life. We have shown that an internee's place of final release from detention strongly influenced where they were still living half a century later. This effect is surprisingly constant, even over decades. Rather than seeing the effect decay over time, we observe that the relative distance that the average former internee lives from their camp assignment in 2010 is essentially the same as in 1965. Moreover, using internees who transferred between camps, we were able to distinguish between this inertia effect and the treatment impact of the camp itself. This persistence in geographic location allows us to estimate causal impacts without confounding variation due to self-selection into these locations.

We find that the quality of location seems to matter a great deal for individual economic outcomes. While there is little doubt that internment was a harmful experience for all internees, we find that those assigned to the poorest regions fare worse later in life relative to their peers who were assigned to wealthier regions of the country by many different metrics. Quality of camp location has been shown to be strongly predictive of, among other outcomes, later-life income, neighborhood quality, and occupational status. Again, using information on transfers, we can distinguish these place effects from the potential treatment effect from the camps themselves.

Importantly, we find that not only are internees' own outcomes affected, but those families assigned to an area with a greater degree of social mobility see weaker intergenerational correlation in incomes. This is a strong confirmation of the cross-sectional patterns observed in Chetty et al. (2014). Moreover, we replicate these individual and intergenerational effects in two data sources: stochastically matched Census micro data and the Japanese American Research Project's direct surveys. Both data sets show similar, large effects. The JARP data also show the intriguing result that assignment to richer locations has a causal impact on the values or worldview of internees and their descendants. People assigned to richer locations are more likely to give answers consistent with materialism, agency, and optimism than ex ante identical people assigned elsewhere. This suggestive evidence suggests yet another channel by which causal location effects might operate.

By identifying the underlying causal impact of place in a unique natural experiment, our findings are relevant to the understanding of the large and persistent differences in economic opportunities across the United States. Moreover, given recent developments with the inflow of refugees from the Middle East and North Africa to Europe, our results also shed light on possible long-run consequences of government policy toward the resettlement of economic and political refugees that goes beyond existing literature, which has tended to focus on nationality networks and ethnic enclaves rather than the effect of location quality itself. This evidence highlights the importance of location assignment not just for the refugees themselves, but also for the outcomes of their descendants for decades to come.

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Table 1

Mean for Surviving Japanese-American Internees by Initial Place of Internment

	Initial Internment Camp									
	Central Utah	Colorado River	Gila River	Granada	Heart Mountain	Jerome	Manzanar	Minidoka	Rohwer	Tule Lake
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
A. Personal Characteristics (%)										
Female	53.4	52.3	51.9	53.7	54.3	52.4	50.9	53.8	51.8	52.3
Married	22.5	18.5	21.1	21.5	22.1	19.4	22.7	19.5	21.0	19.1
Born in U.S.	66.2	73.3	67.8	72.9	69.1	74.0	61.4	65.4	70.1	69.1
Age	19.2	17.6	18.2	17.5	18.4	17.5	18.5	18.6	17.8	18.1
B. Education (%)										
High School	48.8	44.4	46.7	41.3	46.0	46.0	47.6	51.3	49.2	45.6
College	11.9	8.7	8.7	9.3	9.1	7.0	7.9	8.9	6.2	8.5
C. Occupations (%)										
Professional	9.9	6.4	8.8	6.6	9.4	5.9	8.4	8.7	6.6	7.6
Clerical	12.3	17.9	12.3	15.5	18.5	17.2	16.3	17.4	16.5	13.9
Service	6.8	9.0	4.7	7.3	10.4	11.6	14.3	9.6	8.8	5.7
Agricultural	21.6	14.7	16.0	21.3	25.7	10.3	11.0	14.6	14.0	14.5
Skilled	3.0	4.6	3.5	5.0	5.8	3.5	3.8	5.5	3.5	3.8
Semi-Skilled	3.4	12.5	7.8	10.2	14.1	10.4	8.8	6.0	5.8	6.9
Unskilled	0.8	2.1	0.9	0.5	3.1	6.0	0.7	0.9	0.5	1.0
D. Regional Income										
Income in County of Origin	70,649	54,463	56,898	54,079	62,201	51,032	54,794	62,331	54,221	58,215
Income in County of Camp	44,852	38,251	49,088	33,646	47,264	31,676	44,090	39,636	28,734	39,218
E.Number of Internees	4,552	10,660	7,608	3,883	6,013	4,493	5,782	4,929	4,729	8,778

Notes: Table displays summary statistics by initial camp assignment for internees linked between the WRA and JARVIS datasets. English speaking and English literacy variables were recorded by the WRA only for adults and children over the age of nine. Occupations are those recorded by the WRA as any occupation for which an individual was qualified, not necessarily the individual's current occupation. Regional income variables are measured in 2010.

Table 2

Exogeneity Tests for Location Assignment and Attrition

	Dependent Variable Log Income in First Camp County			Dependent Variable Log Income in Last Camp County		
Panel A: Exogeneity Tests	Coef	T-Stat	Obs	Coef	T-Stat	Obs
WRA Variables						
Female	-0.0011	-0.705	61,427	-0.0008	-1.113	61,427
Age in 1942	0.0001	0.981	61,427	-0.0000	-0.631	61,427
High School	-0.0042	-1.537	32,366	-0.0034	-1.175	32,366
College	0.0025	1.013	20,759	-0.0002	-0.044	20,759
1940 Census Variables						
Log of Rent	-0.0018	-0.815	23,748	-0.0007	-0.398	23,748
Log of 1940 House Value	0.0057	1.286	7,270	0.0012	0.295	7,270
Urban	0.0023	0.186	32,560	-0.0033	-0.238	32,560
Farm	-0.0075	-0.601	32,560	-0.0028	-0.211	32,560
Labor Force Participation	0.0016	1.132	19,548	0.0008	0.553	19,548
Earns Over $50 Non-Wage Income	-0.0006	-0.294	19,357	-0.0018	-0.777	19,357
Occupation Score	0.0003	0.972	7,231	0.0002	0.802	7,231
Working	-0.0088	-1.157	7,598	-0.0012	-0.187	7,598
Unemployed	0.0066	1.298	7,598	-0.0032	-0.737	7,598
JARP Variables						
Pre-Internment Income	-	-	-	-0.0088	-0.054	259
Father is a Farmer	-	-	-	-0.0100	-1.08	783

	Independent Variable Log of Income in First Camp Coun			Independent Variable Log of Income in Last Camp County		
Panel B: Attrition and Record Linkage	Coef	T-Stat	Obs	Coef	T-Stat	Obs
Dependent Variable						
Living at Time of Redress	0.0017	0.188	106,987	0.0233	0.615	106,987
Emigrated from United States	0.0041	1.368	106,987	-0.0041	-0.577	61,427
Matches 1940 Census	0.0084	0.326	57,573	0.0456	1.504	57,573
Matches 1980 IPUMS	0.0027	0.469	61,427	0.0043	0.465	61,427
IPUMS Matches per JARVIS Observation	0.4223	0.766	7,374	0.5849	1.523	7,374
JARVIS Matches per IPUMS Observation	-0.0052	-0.104	7,374	0.0051	0.080	7,374

Notes: Each coefficient in the table depicts the result of a separate regression. In Panel A the log of income in first and last camp counties are regressed on individual characteristics. In Panel B the log of camp income is the independent variable. All regressions include fixed effects for county of origin cross generation and have standard errors clustered by county of origin. The sample in the high school regression is restricted to adults at least 18 years old. The sample in the college regression is restricted to adults at least 22 years old. Non-wage income is defined by the 1940 Census as income from sources other than wages and salaries, including business income, interest and dividends, government transfers, and consumption of own farm produce. Number of IPUMS and JARVIS matches are conditional on at least one match being found. Occupational income score measures the median total income of all persons sharing the individual's reported occupation.

Table 3

Estimates of Effect of Internment on 1990 Location

	Dependent Variable Indicator for 1990's State of Residence				Dependent Variable Log Distance from Internment Camp			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
First Place of Internment	0.1608***		-0.0128		-0.2114***		-0.0517	
	(0.0392)		(0.0767)		(0.0560)		(0.0628)	
Last Place of Internment		0.1930***	0.2039***	0.1930***		-0.2480***	-0.2068***	-0.1075***
		(0.0374)	(0.0710)	(0.0373)		(0.0449)	(0.0328)	(0.0310)
Additional Fixed Effects								
College × Destination				X				X
Married × Destination				X				X
Age Dummies × Destination				X				X
Internees	60,307	60,307	60,307	60,305	59,477	59,477	59,477	59,449
Observations	3,075,657	3,075,657	3,075,657	3,075,555	594,770	594,770	594,770	594,490
R^2 (adjusted)	0.0258	0.0372	0.0372	0.0452	0.0238	0.0271	0.0273	0.0288

Notes: Columns (1) to (4) of table depict the increase in probability that an internee lives in a state when addresses are observed in the 1990's, conditional on having been interred in that state. Columns (5) to (8) depict the the log of distance between an internee's place of residence in the 1990's and their place of internment, relative to other internment camps. Standard errors are clustered by county of origin in all specifications. State regressions include fixed effects effects for origin x destination x generation. Distance regressions include fixed effects county of origin x camp. In columns 4 and 8, age is measured in decades for the fixed effect to make the estimation feasible.

Table 4

Estimates of Effect of Internment on Location in Japanese American Research Project Survey

	Indicator for State of Residence after Internment		Indicator for State of Residence at Survey		Indicator for State of Residence at Survey		Indicator for State of Residence at Survey	
VARIABLES	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Place of Internment	0.145	0.081	0.129	0.099	0.300	0.069	.242	.066
SE Clustered by Family	(.014)***	(.010)***	(.013)***	(.014)***	(.029)***	(.012)***	(.044)***	(.009)***
SE Clustered by Pre-Internment Loc.	(.023)***	(.007)***	(.040)***	(.007)***	(0.068)***	(.001)***	(0.046)***	(.015)***
Generation of Respondent	1st Generation: Issei		1st Generation: Issei		2nd Generation: Nisei		3rd Generation: Sansei	
Destination-Origin FE		X		X		X		X
Number of Respondents	827	827	1,043	1043	1867	1679	641	567
Observations	24,810	24528	22,946	25358	63478	55,407	20512	18810
R-squared	.019	.362	.016	.171	.04	.297	.027	.309

Notes: Notes: Columns (1) and (2) of table depict the increase in probability that an internee lived in a state when addresses were observed in the Japanese American Research Project Survey in the early-to-mid 1960's, conditional on having been interred in that state. The data are for first generation Japanese immigrants, most of whom were interned during WW2. Columns (3) and (4) of table depict the increase in probability that an internee lived in a state immediately after internment, conditional on having been interred in that state. This question is asked of the same sample as in (1) and (2). Differences in sample size come from non-response. Columns (5)-(6) depict the increase in probability that an internee lived in a state when addresses were observed in the Japanese American Research Project Survey for third-generation immigrants, conditional on their grandparents having been interred in that state. We split northern and southern California into two entities when using origin-destination fixed effects to improve precision and drop California residents that cannot be assigned. We report two sets of standard errors, one where we cluster by respondent and one where we cluster by the origin of the internee. The standard errors are reported in parentheses.

Table 5

Logistic Regressions for Probability of Remaining in State of Internment (Odds Ratio)

	Dependent Variable						
	Indicator for State of Residence in 1990's						
	AR	AZ	CA	CO	ID	UT	WY
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
First State of Internment	1.6498	1.6984***	1.1249***	3.0223***	0.6568**	2.3904***	1.1121
	(0.7127)	(0.2280)	(0.0290)	(0.3214)	(0.1072)	(0.3963)	(0.8292)
Observations	58,406	61,377	61,395	61,228	61,270	61,377	60,643
Last State of Internment	1.9820	1.6855***	1.2137***	3.5700***	0.7702	2.4315***	0.9501
	(0.9054)	(0.2278)	(0.0307)	(0.3398)	(0.1236)	(0.3774)	(0.7078)
Observations	58,406	61,377	61,395	61,228	61,270	61,377	60,643

Notes: The dependant variable is a dummy for living in the state listed above. Regressions are run separately for each state. Regressions control for state of origin-destination fixed effects.

Table 6

Baseline Estimates of the Effect of Internment Location on House Prices, House Quality, and Median Zip Code Income

	Dependent Variable Log of House Price			Dependent Variable Log House Quality			Dependent Variable Log of Tract Code Median Income		
A. Log of Income in Camp County	(1)	(2)	(3)	(5)	(6)	(7)	(9)	(10)	(11)
Last Camp	0.198***	0.195**	0.202***	0.204***	0.193**	0.207***	0.0476***	0.0699**	0.0450***
	(0.0425)	(0.0873)	(0.042)	(0.0360)	(0.0775)	(0.0357)	(0.0121)	(0.0345)	(0.0114)
First Camp		0.00303			0.0145			-0.0278	
		(0.0724)			(0.0675)			(0.0321)	
Observations	44,747	44,747	44,709	40,747	40,747	40,714	58,325	58,325	58,265
R^2 (adjusted)	0.098	0.099	0.106	0.105	0.105	0.114	0.065	0.066	0.094
Controls									
Demographic Controls			X			X			X
Origin-Generation Fixed Effects	X	X	X	X	X	X	X	X	X
B. Log of Income in 50-Mile Radius									
Last Camp	0.156***	0.135*	0.160***	0.167***	0.143**	0.170***	0.0394**	0.0509*	0.0373**
	(0.0348)	(0.0688)	(0.034)	(0.0279)	(0.0584)	(0.0273)	(0.0161)	(0.0281)	(0.0159)
First Camp		0.0258			0.0285			-0.0141	
		(0.0532)			(0.0492)			(0.0254)	
Observations	44,747	44,747	44,709	40,657	40,657	40,625	58,325	58,325	58,265
R^2 (adjusted)	0.098	0.099	0.106	0.105	0.105	0.114	0.065	0.066	0.094
Controls									
Demographic			X			X			X
Origin-Generation Fixed Effects	X	X	X	X	X	X	X	X	X

Notes: This table regresses characteristics of internee addresses from the 1992 JARVIS redress files on fixed effects for county of origin-generation pairs and a measure of regional income around the camp of internment. The construction of the house quality measure is described in the text. The standard errors are clustered by county of origin.

Table 7

Regression Discontinuity Approach in Los Angeles County

	Dependent Variable: Zillow House Price				
	(1)	(2)	(3)	(4)	(5)
Interned in Gila River (Wealthy Location)	0.247***	0.291***	0.262**	0.168	0.397**
	(0.0925)	(0.107)	(0.133)	(0.168)	(0.196)
Distance to Assignment Boundary	-0.00481	0.0230	-0.0964	-0.0750	-0.132
	(0.0254)	(0.0347)	(0.0599)	(0.159)	(0.398)
Distance to Boundary * Interned in Gila River	-0.0577	-0.157***	0.111	0.322	-0.995
	(0.0428)	(0.0604)	(0.110)	(0.265)	(0.777)
Constant	13.14***	13.18***	13.08***	13.09***	13.08***
	(0.0543)	(0.0609)	(0.0709)	(0.0907)	(0.105)
Interval Around Boundary	4 miles	3 miles	2 miles	1 mile	1/2 mile
Observations	743	589	364	162	79
R-squared	0.015	0.027	0.030	0.059	0.063

Note: During WWII, Japanese residents of Los Angeles county were assigned to internment camps based on complex assignment zones (see Figure 4). By matching to the 1940 Census, we recovered addresses for internees in LA County who were assigned to either Rohwer/Jerome, AR (poor regions) or Gila River, AZ (a wealthy region). We then geocoded the distance from each of these addresses to the assignment zone boundaries. In this table, we regress the log of internee housing prices against distance to the boundary, interacted distance, and a dummy to capture a discrete causal effect due to assignment. Distance is controlled for linearly, and columns 1-5 vary the interval being studied.

Table 8

IPUMS Income Regressions

Panel A: Bootstrap Weights	Log House Price (Zillow) (1)	Log House Price (IPUMS) (2)	Log Individual Income (3)	Log Household Income (4)	Log House Price (Zillow) (5)	Log House Price (IPUMS) (6)	Log Individual Income (7)	Log Household Income (8)
Log Income in Last Camp County	0.1861***	0.1552**	0.2223*	0.1258**	0.1680**	0.1407**	0.2327**	0.1228**
	(0.0720)	(0.0608)	(0.1258)	(0.0633)	(0.0717)	(0.0610)	(0.1043)	(0.0612)
Demographic Controls					X	X	X	X
Observations	7,333	6,776	6,634	7,345	7,329	6,773	6,631	7,341
R^2 (adjusted)	0.1195	0.1080	0.0362	0.0301	0.1349	0.1214	0.3033	0.0785
Panel B: Census Weights								
Log Income in Last Camp County	0.2331***	0.2170***	0.2358*	0.1578**	0.2148***	0.2027***	0.1995*	0.1463**
	(0.0706)	(0.0630)	(0.1296)	(0.0662)	(0.0701)	(0.0632)	(0.1076)	(0.0636)
Demographic Controls					X	X	X	X
Observations	7,361	6,803	6,660	7,373	7,357	6,800	6,657	7,369
R^2 (adjusted)	0.1308	0.1194	0.0419	0.0293	0.1476	0.1316	0.3055	0.0772
Panel C: Distribution of Bootstrap Coefficients								
Log Income in Last Camp County (Mean Across Bootstrap Samples)	0.1879***	0.1578***	0.2246**	0.1265**	0.1870***	0.1569***	0.2261**	0.1266**
5th/9th Percentile	[.1001,.2885]	[.0647,.2333]	[.0755,.4153]	[.0084,.2257]	[.0993,.2875]	[.06740,.2325]	[.07790,.4142]	[.0127,.2226]
Demographic Controls					X	X	X	X

Notes: Panel A and Panel B report the results of regressing house prices and income on the log of median income in the last county of internment in the full linked WRA-IPUMS sample. Each coefficient is produced from a separate regression. Standard errors are reported in parentheses and are clustered by census observation. Weights in Panel A are constructed as the ratio of the number of times a WRA-IPUMS match was linked when randomly drawing 100 bootstrap samples without replacement from the full sample. Weights in Panel B are constructed as one over the number of potential matches for each IPUMS observation, and sum to one for each IPUMS individual. Panel C reports the mean of the distribution of coefficients produced from running each regression specification separately in 100 bootstrap samples where WRA and IPUMS observations are uniquely matched. Standard errors in these regressions are clustered by county of origin. All regressions include county of origin-generation pairs.

Table 9
IPUMS Education and Occupational Standing Regressions

	Four Years College	Duncan Socio-Economic Index	HW Socio-Economic Index	NT Occupational Prestige Score	Siegel Occupational Prestige Score
	(1)	(2)	(3)	(4)	(5)
Log Income in Last Camp	0.0960**	2.7995*	1.8515*	3.2842**	1.2791
	(0.0375)	(1.5751)	(1.0297)	(1.2982)	(0.8016)
Observations	4,458	6,562	6,559	7,346	6,559
R^2 (adjusted)	0.0313	0.0315	0.0294	0.0357	0.0321

Notes: The table reports the results of regressing educational attainment and occupational standing on the log of median income in the last county of internment in the full linked WRA-IPUMS sample. Each coefficient is produced from a separate regression. Standard errors are reported in parentheses and are clustered by county of origin. Weights are constructed as the ratio of the number of times a WRA-IPUMS match was linked when randomly drawing 100 bootstrap samples without replacement from the full sample. Regressions for completing 4 years of college are limited to individuals younger than 18 in 1942.

Table 10

Outcome Regressions with Location and Occupation Controls

	Log Real Income	Log Individual Income	Log House Price (Zillow)	Zillow Quality	Tract Income	Log Individual Income	Log Household Income
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Log Income in Last Camp County	0.1443*	0.2458*	0.0873*	0.0848*	0.0704*	0.1579*	0.1182**
	(0.0740)	(0.1272)	(0.0479)	(0.0450)	(0.0420)	(0.0861)	(0.0534)
Controls		PUMA FE	County FE	County FE	County FE	Occupation FE	Occupation FE
Observations	7,307	6,634	6,452	6,396	6,462	6,385	6,558
R^2 (adjusted)	0.0821	0.0580	0.6093	0.6256	0.3218	0.4099	0.2538

Notes: Table reports the results of regressing house prices, house quality and income on the log of median income in the last county of internment in the full linked WRA-IPUMS sample with various controls. Each coefficient is produced from a separate regression. Standard errors are reported in parentheses and are clustered by census observation. Weights are constructed as the ratio of the number of times a WRA-IPUMS match was linked when randomly drawing 100 bootstrap samples without replacement from the full sample.

Table 11

Estimates of Effect of Internment on Income and Education in JARP Survey

	Log Family Income			College		
Variables	(1)	(2)	(3)	(4)	(5)	(6)
Log Income in Camp County (1st Gen)	0.222	0.222	0.203	0.295	0.292	0.304
SE Clustered by Family	(.116)*	(.117)*	(.112)*	(.142)**	(.138)**	(.144)**
SE Clustered by Pre-Internment Loc.	(.054)***	(.058)***	(.045)***	(.124)*	(.124)*	(.146)*
Huber-White SE	(.099)*	(.101)**	(.096)**	(.137)**	(.132)**	(.133)**
Generation of Respondent	Second and Third Generation, Ages 22 - 50			Third Generation, Ages 22+		
Pre-Internment Location FE		X	X		X	X
Demographic Controls			X			X
Observations	1,603	1,603	1,603	303	303	303
R-squared	0.003	0.007	0.077	0.012	0.018	0.053

Note: In columns (1)-(3), we match each second and third generation respondent to the camp assignment of their first generation relative. We then explore the effect of log income in the assignment camp county on reported family income in the JARP. A continuous measure of income was created using the mid-point of the JARP income bins. Column 1 includes no controls, column 2 includes fixed effects for the pre-Internment place of residence of the first generation relative, and column (3) adds a control for gender and a linear control for age. In columns (4)-(6), we match only and third generation respondents to the camp assignment of their first generation relative. This is because second generation respondents were generally too old to attend school after internment. We then regress log camp county income on a dummy variable for reporting having completed college in the JARP. We use the same controls as in (1)-(3) in columns (4)-(6) respectively. We report three standard errors for each specification: standard errors clustered by first generation internee (i.e. family), by pre-internment location of origin, and standard heteroskedasticity robust standard errors. Whereas in Table 8, there was intrinsic correlation across observations, that may not be the case here. Addtionally, in some regressions the number of clusters is small, making it helpful to check the non-clustered version as well.

Table 12

Intergenerational Mobility

	Dependent Variable					
	Rent (Zillow)		House Quality		Census Tract Income	
	Log	Rank	Log	Rank	Log	Rank
	(1)	(2)	(3)	(4)	(5)	(6)
Parents' 1940 Rent(Log/Rank)	0.1105*	0.2729**	0.1226*	0.2131**	0.1245**	0.2165**
	(0.0557)	(0.1108)	(0.0678)	(0.1043)	(0.0490)	(0.1040)
Parents' 1940 Rent (Log/Rank) x	-0.0023*	-0.0055**	-0.0023	-0.0039*	-0.0025**	-0.0040*
Absolute Upward Mobility	(0.0012)	(0.0025)	(0.0015)	(0.0023)	(0.0011)	(0.0023)
N	7,549	7,549	6,829	6,829	7,456	7,456
R^2	0.1125	0.1176	0.0880	0.0984	0.0417	0.0430
Parental (Head of Household) Occupational	0.2579**	0.2445*	0.3067**	0.191	0.1886***	0.2351***
Prestige Score (Log/Rank)	(-0.1077)	(-0.1271)	(-0.1474)	(-0.1276)	(-0.0696)	(-0.0738)
Parental (HH) Occupational Prestige x	-0.0056**	-0.0052*	-0.0067*	-0.0040	-0.0035**	-0.0045***
Absolute Upward Mobility	(-0.0024)	(-0.0028)	(-0.0034)	(-0.0029)	(-0.0016)	(-0.0017)
N	10,962	10,962	9,939	9,939	10,852	10,852
R^2	0.1099	0.1153	0.0913	0.1012	0.0451	0.0474

Notes: Table reports the results of regressing children's rent, house quality, and census tract income on parental rent and head of household occupational prestige score in 1940 (log on log and rank on rank) and an interaction term with absolute upward mobility score for county of internment (Chetty et al., 2014). Regressions are run for children under the age of 18 in 1942 whose parents are located in the 1940 census. All regressions include controls for absolute upward mobility and initial county x generation fixed effects. Standard errors are reported in parentheses and are clustered by county of origin.

Table 13

Intergenerational Mobility in the Japanese American Research Project Survey

	Dependent Variable			
	2nd and 3rd Generation Income			
	Log	Rank	Log	Rank
	(1)	(2)	(3)	(4)
1st Generation Income (Log/Rank)	0.460	0.565	0.461	0.566
SE Clustered by Family	(0.265)*	(0.323)*	(0.263)*	(0.326)*
SE Clustered by Pre-Internment Loc.	(0.160)**	(0.220)**	(0.145)**	(0.214)**
Huber-White SE	(0.228)**	(0.282)**	(0.233)**	(0.290)*
1st Generation Income (Log/Rank) x Mobility of Internment County	-0.009	-0.011	-0.009	-0.011
SE Clustered by Family	(.006)	(.007)	(.006)	(.007)
SE Clustered by Pre-Internment Loc.	(.004)**	(.005)*	(.003)**	(.005)*
Huber-White SE	(.005)*	(.006)*	(.005)*	(.006)*
Internment Camp FE			X	X
N	1,324	1,324	1,324	1,324
R^2	0.017	0.017	0.021	0.022

Notes: Table reports the results of regressing children and grandchildren family income on the first generation's income and an interaction term with absolute upward mobility score for county of internment (Chetty et al., 2014). All regressions include controls for absolute upward mobility and initial county x generation fixed effects. Standard errors are reported in parentheses and are clustered by first generation internee, pre-internment location, or unclustered Huber White standard errors.

Table 14

Estimates of Effect of Internment Location on Discrimination and Values in Japanese American Research Project Survey

	Experienced Discrimination in:			Values Response Index:		
VARIABLES	Housing (1)	School (2)	Job Market (3)	Materialism (4)	Agency (5)	Optimism / Risk Tolerance (6)
Log Income in Camp County (1st Generation)	0.0744	0.0153	-0.0706	0.558***	0.354*	0.407**
	(0.0747)	(0.0540)	(0.0518)	(0.186)	(0.201)	(0.197)
Pre-Internment Location FE	X	X	X	X	X	X
Demographic Controls	X	X	X	X	X	X
Observations	2,354	2,342	2,344	2,384	2,384	2,384
R-squared	0.008	0.004	0.008	0.025	0.010	0.031

Note: In this table, we match each second and third generation respondent to the camp assignment of their first generation relative. We then explore the effect of log income in the assignment camp county on survey responses in the JARP. All regressions use controls for sex, age, and pre-internment family location fixed effects. Standard errors are clustered by family. Columns 1-3 report use dummy variables for the survey questions about whether the respondent has been discriminated against in housing, school, or on the job respectively. There are no statistically significant differences. Columns 4-6 report the effect of random assignment to richer locations on indices of survey responses indicating a subject's materialism, self-precieved agency, and optimism or risk tolerance. A full list of questions is included in Appendix Table 4. All indices have been standardized to have a mean of zero and a standard deviation of 1.

Appendix Table 1A

Heterogeneity of Effect of Internment on 1990 Location

	Dependent Variable: Indicator for 1990's State of Residence					
	(1)	(2)	(3)	(4)	(5)	(6)
Last Place of Internment	0.199***	0.210***	0.211***	0.183***	0.177***	0.133***
	(0.0381)	(0.0369)	(0.0370)	(0.0355)	(0.0393)	(0.0300)
Last Place of Internment × Under 18 Years Old	-0.0120***					
	(0.00443)					
Last Place of Internment × Professional Occupation		-0.0439***				
		(0.00829)				
Last Place of Internment × College Education			-0.0753***			
			(0.0108)			
Last Place of Internment × Married				0.0246**		
				(0.0111)		
Last Place of Internment × Agricultural Worker					0.0837***	
					(0.0221)	
Last Place of Internment × Have Visited/Lived in Japan						0.101***
						(0.0167)
Sample	All	Age 25+	Age 25+	Age 25+	Age 25+	Age 25+
Observations	3,075,657	618,426	616,947	618,375	618,426	618,426
R^2 (adjusted)	0.037	0.041	0.041	0.041	0.042	0.043

Notes: Columns (1) to (6) of table depict the increase in probability that an internee lives in a state when addresses are observed in the 1990's, conditional on having been interred in that state. Standard errors are clustered by county of origin in all specifications. Regressions include fixed effects effects for origin x destination x generation.

Appendix Table 1B

Heterogeneity of Effect of Internment on 1990 House Prices

	Dependent Variable: Log of Zillow House Price at 1990 Location					
	(1)	(2)	(3)	(4)	(5)	(6)
Last Camp Income	0.298***	0.256***	0.266***	0.149	0.280***	0.194
	(0.0685)	(0.0647)	(0.0531)	(0.124)	(0.0612)	(0.123)
Last Camp Income × Under 18 Years Old	-0.188***					
	(0.0637)					
Last Camp Income × Professional Occupation		0.0672				
		(0.0943)				
Last Camp Income × College Education			0.0643			
			(0.105)			
Last Camp Income × Married				0.154		
				(0.182)		
Last Camp Income × Agricultural Worker					-0.0297	
					(0.0835)	
Last Camp Income × Have Visited/Lived in Japan						0.115
						(0.168)
Sample	All	Age 25+	Age 25+	Age 25+	Age 25+	Age 25+
Observations	44,747	8,320	8,303	8,320	8,320	8,320
R^2 (adjusted)	0.105	0.178	0.178	0.177	0.177	0.177

Notes: This table regresses the log of Zillow house price from internee addresses in the 1992 JARVIS redress files on individual characteristics and their interaction with income of last camp assignment and fixed effects for county of origin-generation pairs. The standard errors are clustered by county of origin.

Appendix Table 2

Regression Discontinuity Approach in Los Angeles County

	Dependent Variable: Log Rent in 1940			
	(1)	(2)	(3)	(4)
Interned in Gila River	0.213	-0.178	0.472**	-0.0641
	(0.164)	(0.180)	(0.230)	(0.179)
Distance to Assignment Boundary	0.0481	0.348***	-1.280***	0.204
	(0.0605)	(0.113)	(0.297)	(0.467)
Distance to Boundary * Interned in Gila River	-0.189**	-0.356**	1.343***	0.613
	(0.0923)	(0.147)	(0.374)	(0.635)
Constant	2.944***	3.254***	2.547***	2.982***
	(0.118)	(0.139)	(0.188)	(0.148)
Interval Around Boundary	3 miles	2 miles	1 mile	1/2 mile
Observations	538	331	144	73
R-squared	0.012	0.044	0.133	0.070

Note: During WWII, Japanese residents of Los Angeles county were assigned to internment camps based on complex assignment zones (see Figure 4). By matching to the 1940 Census, we recovered addresses for internees in LA County who were assigned to either Rohwer/Jerome, AR or Gila River, AZ. We then geocoded the distance from each of these addresses to the assignment zone boundaries. In this table, we regress the log of internee housing prices against distance to the boundary, interacted distance, and a dummy to capture a discrete causal effect due to assignment. Distance is controlled for linearly, and columns 1-4 vary the interval being studied.

Appendix Table 3

Instrumental Variables IPUMS Income Regressions

	Dependent Variable: Log Household Income							
	Bootstrap Weights				Census Weights			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Log Income in Restitution Zip (IV)	1.7786*	1.8010*	1.1479*	1.1121*	1.6453**	1.6342**	1.1679**	1.1146**
	(0.9183)	(0.9597)	(0.6213)	(0.6366)	(0.6656)	(0.6727)	(0.5056)	(0.5066)
First Stage F-Statistic	6.0	5.5	7.1	6.6	11.1	10.6	11.7	11.4
Demographic Controls		X		X		X		X
Age at Internment	All		Under 25		All		Under 25	
N	7,231	7,227	6,649	6,645	7,259	7,255	6,675	6,671

Notes: This panel regresses the log of median zip code income at the time of restitution on log household income reported in the IPUMS data, using the matching techniques described in section XXX of the paper. Log income of restitution zip code is instrumented using the log of median income in the last county of internment in the full linked WRA-IPUMS sample. First stage coefficients range from 0.09-0.13, and first stage F-statistics are reported in the table. All regressions control for pre-internment state-generation fixed effects. Demographics are married in 1942, sex, time in Japan pre-internment, pre-internment college education, pre-internment professional employment, and pre-internment agricultural employment dummies. Standard errors are reported in parentheses and are clustered by census observation. Weights in 1-4 are constructed as the ratio of the number of times a WRA-IPUMS match was linked when randomly drawing 100 bootstrap samples without replacement from the full sample. Weights in 5-8 are constructed as one over the number of potential matches for each IPUMS observation, and sum to one for each IPUMS individual. The magnitudes reported here are roughly comparable to the implied magnitudes in Chetty et al (2015). That paper reports estimates consistent with an elasticity of household income with respect to zip code income of 1.7.

Appendix Table 4

List of Questions in Values Indices

A. Materialism
Positive Responses
Success is the best way to judge a man
Next to health, money is the most important thing
Who has higher social value, people who make buy or sell things with practical use, or people like scholars and artists
Negative Responses
Spending time with family comes first
The way you make your money is more important than how much you make
Americans stress occupational success too much
B. Agency
Positive Responses
If you think a thing is right, do you think you should go ahead and do it even if it is contrary to usual custom?
If you try hard enough, you can get what you want
Most important qualities of a man are determinition and driving ambition
Most people in government are not really interested in the problems of the average man
People can't plan for the future, they need to just live for today
A man's success is just luck
I often worry about possible bad fortune
C. Optimism
Positive Responses
Things will improve
Negative Responses
I am often angry and irritated
Secret of happiness is not expecting too much out of life
I am often frighted or afraid of things
It's not fair to bring a child into this world

Note: Index is coded as a sum of the positive responses minus the sum of the negative responses. It is then standardized to make interpretation easier.

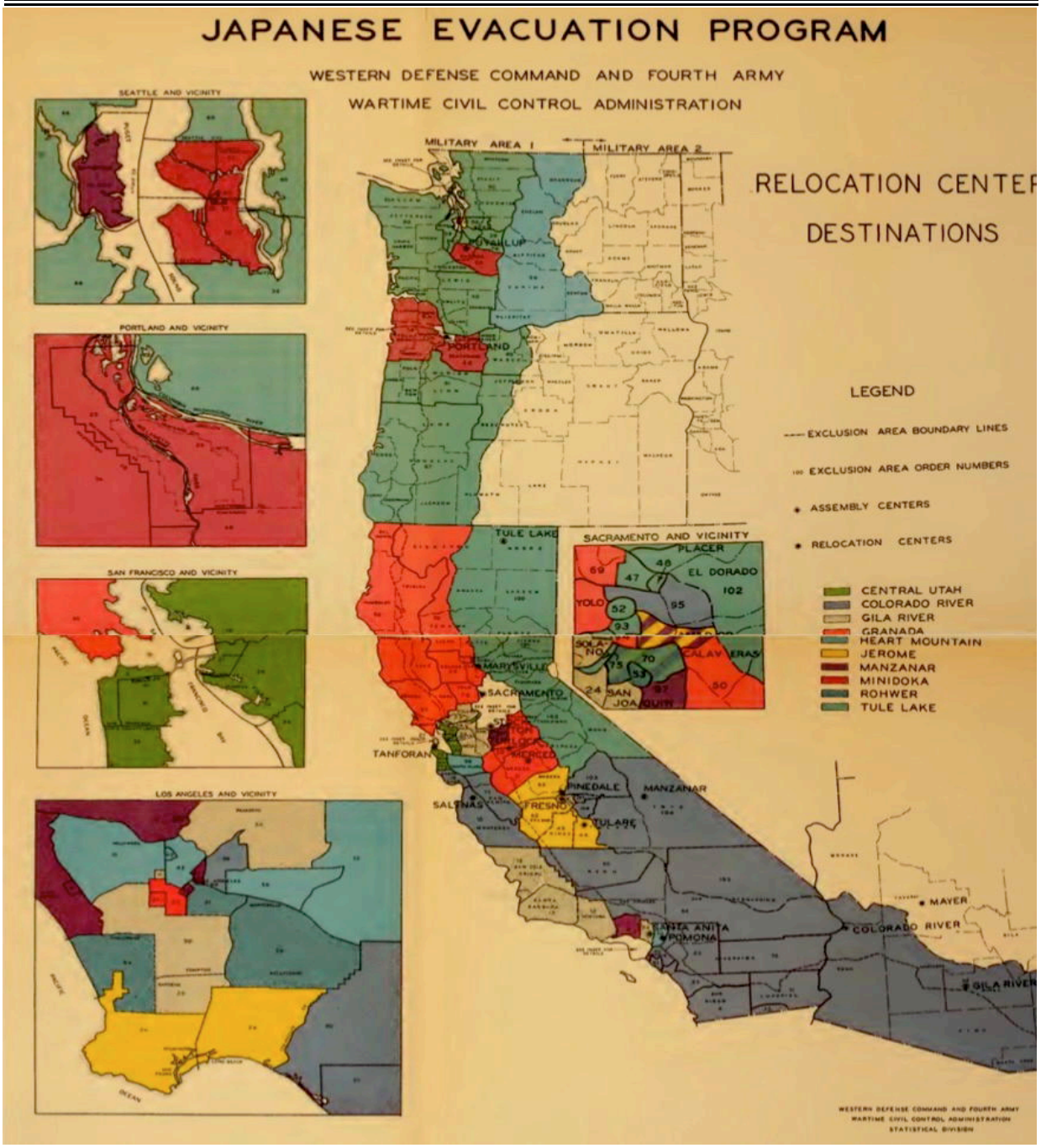
Figure 1

Map of Internment Camps

Source: Burton et al, 2002.

Figure 2

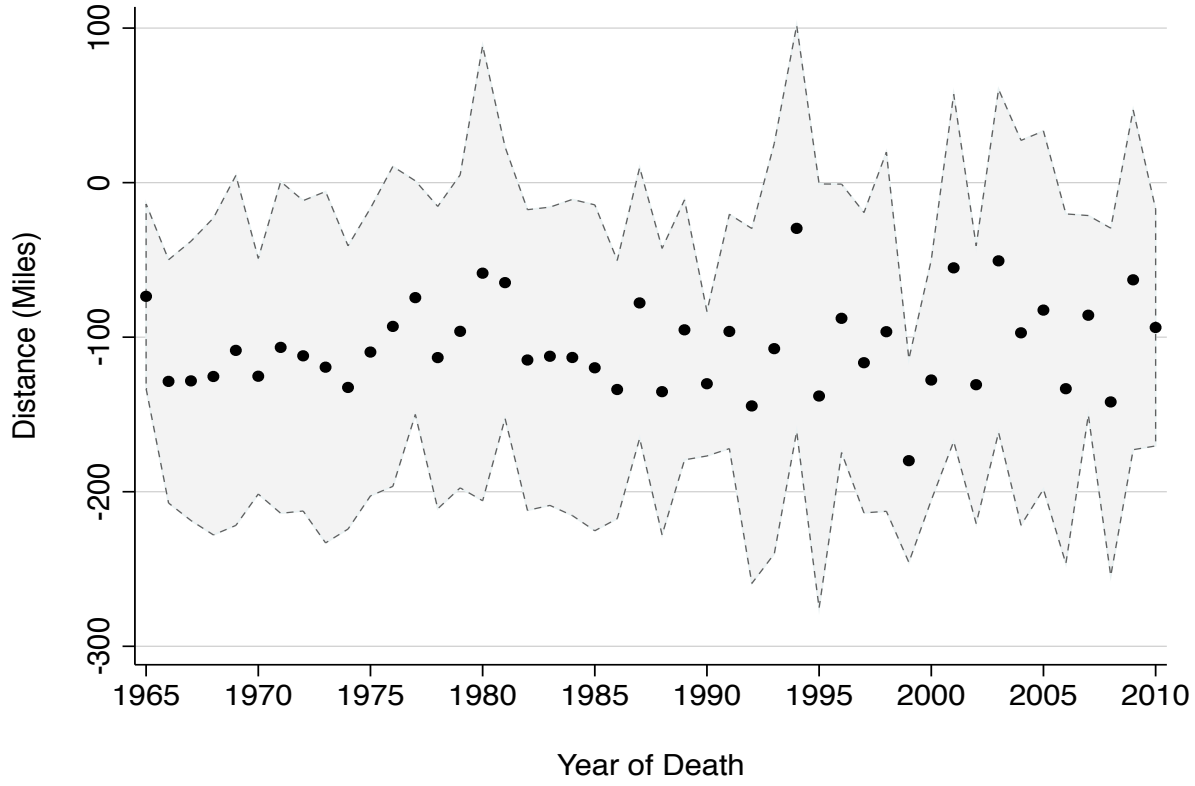
Camp Assignment by Exclusion Zone



Source: U.S. Army Western Defense Command and Fourth Army. 1943. "Final Report: Japanese Evacuation from the West Coast 1942." Washington, D.C.: United States Government Printing Office pg. 328-329.

Figure 3

Distance to First Camp Assignment at Time of Death Relative to Other Camps

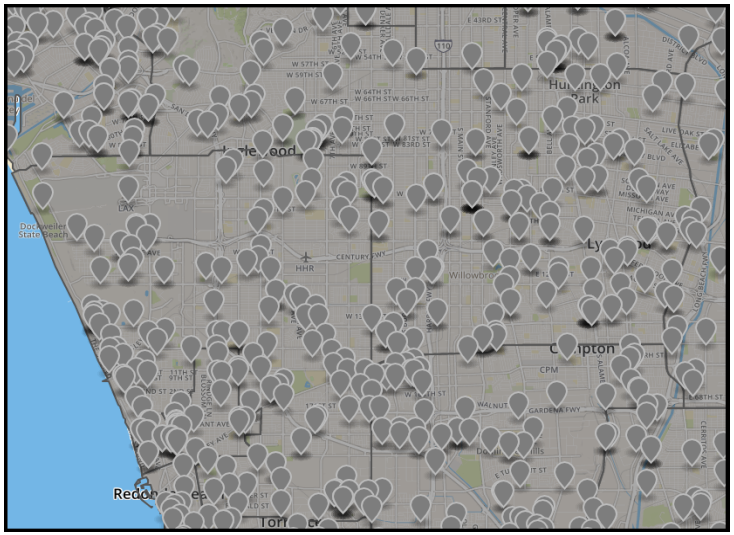


Notes: Figure depects the coefficeint β and 95% confidence interval estimated in the regression $(Distance)_i=\alpha+\beta(Camp)_{it*}T_i+w_i+\varepsilon_i$, where *Distance* measures the distance in miles between first camp assignment and zip code at time of death, *Camp* is a dummy variable for camp assignment, *T* is the indiviudal's year of death, and w is a fixed effect for county of origin cross generation cross camp assignment. Standard errors are clusterd by county of origin. The regression is run only for men who are matched to the Social Security Death Index due to the difficulty of matching women who changed their surnames upon marriage after internment. The coefficient β is intrepreted as measuring how many miles closer an individual lived to their first camp assignment relative to the locations of the other nine camps.

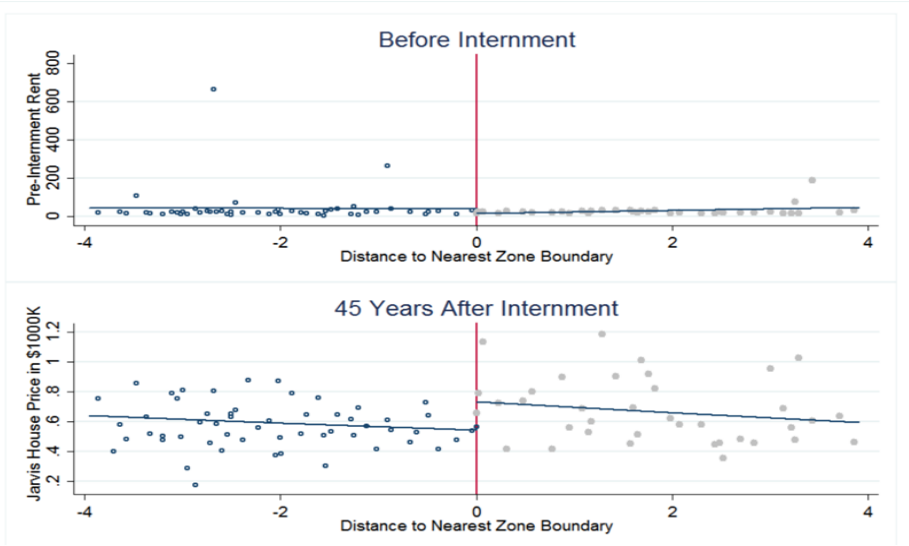
Figure 4

House Prices by Distance to Assignment Zone

Panel A



Panel B



Note: We match WRA-JARVIS data from 4 decades after internment to addresses in the recently declassified 1940 Census microdata. Panel A illustrates our geocoding procedure relative to internment camp exlcusions zones in Los Angeles. We measure the distance for internees sent to Gila River (a wealthy area) and to Arkansas (a poorer area), with distance measured as the distance to the nearest border of the alternate exclusion zone. We exclude addresses who could not be matched to an existing address with less than 20% difference in the string. Differences in rent before internment, in 1940, are graphed at the top of Panel B. The gap is not significant. Differences in the price of the internees house in 1992 is plotted in panel below. The gap at zero is roughly $183,000 and is statistically significant. We excluded houses priced more than $3m and rents above $1,500 in the dataset, as these appear to be significant outliers.

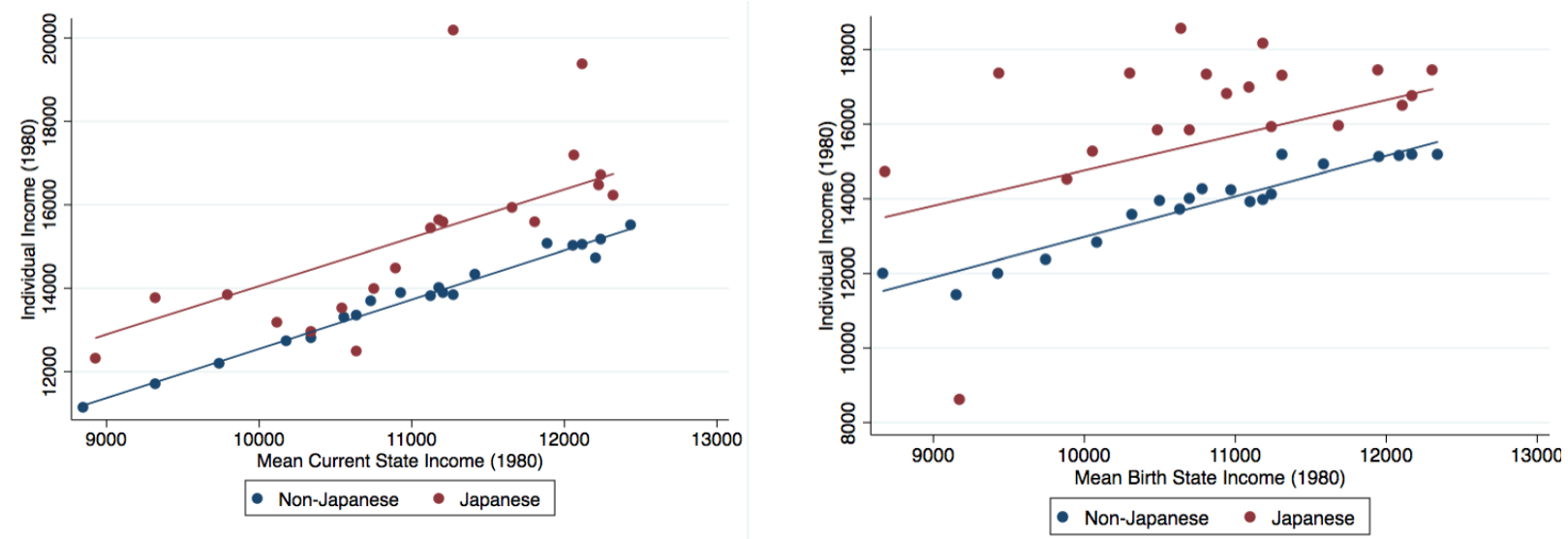
Figure 5

Estimated Bootstrap Coefficient Against Distribution of Placebo Coefficients

Notes: Figure displays the distribution of placebo coefficients estimated in the full set of WRA-IPUMS matches when income of the last camp region has been randomized. The vertical lines depict the estimated coefficient in the original pooled regressions. Weights in all regressions are constructed as the ratio of the number of times a WRA-IPUMS match was linked when randomly drawing 100 bootstrap samples without replacement from the full sample.

Figure 6

Gradient of IPUMS Income on Mean State Income for Japanese and Non-Japanese Populations



Notes: Figures plot a bin-scatter of 1980 IPUMS individual income against mean state income in 1980 state of residence and state of birth for Japanese and non-Japanese populations. The sample is restricted to adults between the ages of 25 and 65. Fitted regressions include controls for gender and age. In a regression of the form income=$a+b_1$*japanese+b_2*(state income)+b_3*Japanese*(state income)+c_i, where c_i is a fixed effect for gender x age and standard errors are clustered by state, the interaction term is estimated to be -0.016 for current state and -0.281 for birth state. Neither coefficient is significant.