The Political Consequences of Ethnically Targeted Incarceration: Evidence from Japanese-American Internment During WWII

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The Political Consequences of Ethnically Targeted Incarceration: Evidence from Japanese-American Internment During WWII*

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Abstract

What are the downstream political consequences of state activity explicitly targeting a minority group? This question is well studied in the comparative context, but less is known about the effects of explicitly racist state activity in liberal democracies such as the United States. We investigate this question by looking at an important event in American history—the mass internment of people of Japanese ancestry during World War II. We find that Japanese Americans who were interned or had family who were interned are significantly less politically engaged and that this demobilizing effect increases with internment length. We also find that camp experience matters: those who went to camps that witnessed violence or strikes experienced sharper declines. Taken together, our findings contribute to a growing literature documenting the demobilizing effects of ethnically targeted incarceration and expand our understanding of these forces within the U.S.

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1 Introduction

As immigrant populations have expanded across liberal democracies, there has been a concurrent rise in the use of punitive policies targeting immigrant and ethnic minority populations. Among various methods, scholars have identified putatively illiberal practices such as indefinite detention (Welch, 2002), corralling of unauthorized immigrants into crowded holding facilities (Bosworth and Turnbull, 2014), and reinforcement of border fencing and barriers that create hostile conditions for migrants (Roche, 2014). These contemporary policies highlight important tensions within liberal democracies over whether the rights and protections conferred to long-standing majority groups extend to immigrants and their progeny; these tensions have revealed themselves throughout history and continue to raise questions about the universal scope of democratic principles (Epstein, 2016).

The internment by the U.S. government of people of Japanese descent is a key historical case that frames the difference between American democratic principles and practice. In June of 1942, approximately 120,000 people of Japanese descent, the majority of whom were U.S. citizens living along the U.S. West Coast, were sent to internment camps throughout the American interior (Weglyn, 1976). Although this internment faced legal challenges as well as civil disobedience, it was not until the end of World War II that internment camps were shut down. By then, hundreds of thousands of people had been displaced and their lives severely disrupted by the state. This included not just adults, but also teenagers and children—many of whom spent formative years living in military camps.

Despite both the historical significance of this event and recent growing use of detention centers along the U.S.-Mexico border and elsewhere in the world (Sampson and Mitchell, 2013), relatively little research in political science has examined the downstream political consequences of such large-scale racial targeting within liberal democracies. Many studies in comparative politics have examined forced migration and the internment of racial or ethnic minorities, coming to the conclusion that such migrations serve to further inflame inter-ethnic conflict and reduce the trust that minority groups express toward not just majority groups, but also toward the state

(e.g., Lupu and Peisakhin, 2017; Zhukov and Talibova, 2018). In addition, a growing literature within American politics has examined the crippling impact of incarceration and other punitive encounters with governments on affected populations (e.g., Lerman and Weaver, 2014; White, 2019*b*). This literature—focused primarily on penal institutions (as opposed to military ones)—has found that state-sponsored incarceration depresses the political engagement not just of those incarcerated but also that of their extended families (White, 2019*a*).

The context of Japanese internment provides an important instance within American history to explore the effects of a systematic and ethnically targeted policy on subsequent political attitudes and behavior. First, the internment of Japanese Americans during WWII was a large-scale government activity, putting it on par with ethnically driven state activity in a comparative context. In addition, such episodes are rarer in liberal democracies, thereby affording an opportunity to assess the impact of explicit ethnic targeting outside the more well-studied context of authoritarian or weak states. Moreover, the legacy of Japanese-American interment provides useful variation: not only was there variation in who was interned, but, conditional on original location, Japanese-American families were mostly exogenously assigned to one of 10 War Relocation Authority (WRA) camps throughout the U.S. We leverage this variation to explore how conditions associated with internment impacted Japanese-Americans' subsequent attitudes and behavior.

We find several factors suggesting that this racist state-sanctioned activity has had extensive downstream repercussions. First, exploring which individuals did or did not live within the exclusion zone, we find that being interned or having family members who were interned has had a lasting, large, and significant depressing effect on political engagement—one that cannot be explained by factors that plausibly covary with internment status (such as lack of military service or differential income). Second, conditional on internment, we find that an additional year of internment is associated with a sizable decrease in political trust and engagement. Third, to explore the mechanism by which internment could be demobilizing, we examine the nature of the camps themselves. We find that being assigned to a camp in which violence or unrest erupted resulted in greater political disengagement among those interned and their families. This suggests

to us that a key pathway to disengagement is the condition of the camp itself—with more unrest among internees being linked to lower engagement. Surprisingly, we do not find similar effects for exposure to militaristic environments or the cultural or political environment in camps' surrounding areas. This suggests that the collective negative social experience of internees, rather than exposure to a particular environment, forms the foundation of the demobilizing effect.

This paper makes several contributions. First, we link disparate literatures from comparative politics with scholarship on American politics, explaining how state-sponsored racial targeting—even within a large liberal democracy—can have negative downstream consequences on political engagement among those directly affected and also their families. Second, our research shows that the severity of these experiences matters, such that more adverse collective conditions in sites of state captivity produce more sizable effects over time. Third, our study provides an unprecedented opportunity to assess theories of "carceral contact" using a case where the psychological linkages between the internment and the government's role are strong. Our findings therefore have strong implications for governments' current-day use of detention centers, including those confining migrants. Lastly, our findings engage a growing literature on Japanese-American public opinion, questioning and complicating the link between Japanese-Americans' historically high political engagement and the history of internment.

This paper proceeds as follows. We first provide a review of the comparative literature on ethnic conflict, in tandem with a growing literature on the political consequences of the American "carceral state" as well as the literature on Japanese-American political behavior. We then provide context on the WWII Japanese-American internment process and provide an overview of our data, which include novel data on the conditions of the camps and their surroundings. We next present our main results showing that direct and family exposure to the internment process predicts subsequent political disengagement and that, among those interned, the length of the internment does as well. These results are not explained by alternative explanations, such as differences in terms of military service or economic success. We next provide an analysis showing that social unrest appears to play a key role in furthering this demobilization. Finally, we demonstrate that

our design assumptions are robust to several challenges as well as alternative explanations, such as the possibility that findings are being driven by exposure to surrounding camp environments. We conclude by noting how our work informs other findings on carceral policies in western democracies.

2 How Ethnically Targeted State Activity Could Impact Minority Group Behavior

States have historically exercised control over ethnic minority groups via targeted repression and violence during periods of social upheaval, mass migration, or war (Levy, 1988; Arendt, 1973; Reeves, 2015). Scholars have explored the political and psychological effects of these efforts, with some studies revealing that repression can politically mobilize targeted minority groups (LeBas, 2006; Davenport, 2005; Blattman, 2009; Bellows and Miguel, 2009) and others suggesting demobilizing effects (Lyall, 2009). While this literature has shed light on the potential political impacts of violence and repression on targeted communities, recent work has drawn attention to the importance of regime strength and whether repression efforts are carried out by state actors. As Zhukov and Talibova (2018) note, empirical examinations of the repression-mobilization link have traditionally selected cases of ethnic targeting perpetrated by non-state actors or weak states.

However, we know surprisingly little about the impact of large-scale repression efforts by "strong" states on minority groups' political participation. Focusing on the mass deportation of ethnic minorities during the Stalin Era, Lupu and Peisakhin (2017) and Rozenas, Schutte and Zhukov (2017) find that punitive encounters with the Soviet Union during the 20th century continue to have positive and durable effects on group attachments and support for minority leaders among targeted groups.[1] Moreover, these studies find that exposure to forced migration durably

[1]Among the various methods states use to repress ethnic groups, their capacity to move groups is especially effective. As McGarry (1998) notes, forced migration can dilute the power of ethnic minorities, allow states to reassert sovereignty over disputed territories, increase protection against external threats, and reduce ethnic conflict. Unsurprisingly, then, studies exploring the effects of state-directed ethnic targeting have gravitated toward forced migration as a relevant opportunity to test theories of repression.

affects voting patterns, such that those who experienced violence in the past report lower levels of support for pro-Russian parties in the contemporary period.

2.1 Ethnic Targeting in Liberal Democracies

A potential limitation of this literature has been its focus on authoritarian regimes—where opportunities to express grievances are limited and state repercussion can be severe. Even within liberal democracies, however, minority groups have collided with punitive institutions and experienced the consequences of ethnic targeting (Zakaria, 1997; Wahab Twibell, 2004; Mann, 2005; Soss and Schram, 2007; Fouka, 2019).[2] For example, an arena that possibly speaks to our inquiry is the contemporary status of Jews in Europe, who were targets of ethnic atrocities by fascist Germany but many of whom continue to live in Europe today. Although some recent work has found effects of concentration camps on local non-Jewish populations (Homola, Pereira and Tavits, 2020), the literature on the downstream effects of the Holocaust on Jewish political participation is scarce; what limited scholarship there is suggests that Jews may be more politically engaged than the general population (e.g., Schnapper, Bordes-Benayoun and Raphael, 2011).

Moving to the present day, the past few decades have seen liberal democracies banning forms of religious dress (Nanwani, 2011), increasing the surveillance and deportation of migrants (Kalhan, 2013), and sidestepping due process in the name of national security (Radack, 2004). The detention of migrants in camps has become more widespread across Australia, the U.S., and Europe (Sampson and Mitchell, 2013). Despite increases in such policies targeting particular ethnic groups, it is unclear whether the effects of ethnic targeting and repression translate across different kinds of political systems. On the one hand, "threat-mobilization" studies in the U.S. have shown that policies targeting ethnic groups can increase minority political participation (Pantoja, Ramirez and Segura, 2001; Bowler, Nicholson and Segura, 2006; White, 2016). Consistent with some of the existing literature on ethnic violence (Lupu and Peisakhin, 2017), prominent theories posit that

[2]Fouka (2019), for instance, studies the impact of discrimination on assimilation patterns among immigrants, and finds that German immigrants during World War I were more likely to increase their "assimilation investments" relative to other immigrant groups.

punitive policies increase the salience of ethnic identities, thereby strengthening the link between group attachments and voting patterns (White, 2016).[3]

On the other hand, research on "custodial citizenship"—which includes literature exploring the downstream consequences of state-sponsored incarceration on minority populations—has found that direct experience with racially disparate criminal justice policies can have demobilizing effects (Uggen and Manza, 2002; Hjalmarsson and Lopez, 2010; Burch, 2013; Lee, Porter and Comfort, 2014; Epp, Maynard-Moody and Haider-Markel, 2014; Walker, 2014; Kupchik and Catlaw, 2015). According to this perspective, contact with the criminal justice system serves as a political socialization experience that erodes trust in government by exposing minority groups to aggressive elements of otherwise democratic systems (Lerman and Weaver, 2014).

We note that, in contrast to cases of ethnic targeting in comparative contexts (where state-directed efforts are intended to encompass entire groups), this literature in American politics has focused on policies that target subsets of minority populations.[4] However, this complicates our ability to assess the political effects of punitive policies because "policy recipients" often differ in important ways from other group members who do not directly experience the policy (Maltby, 2017). The potential mismatch between contemporary cases of racial or ethnic targeting in liberal democracies and similar cases in comparative politics muddles the transportability of theories and expectations across political systems.

In addition, existing studies assess contact with punitive institutions in a binary fashion (i.e., contact v. no contact). However, as Weaver, Hacker and Wildeman (2014, 19) note, scholars have yet to "move beyond treating incarceration as a uniform treatment" and leverage "variation in the character of custodial interactions, not just their incidence." We take up this invitation in our analyses below.

[3] See Dehdari and Gehring (2018) for a similar argument in the context of regional identities.

[4] For instance, despite significant racial disparities in the criminal justice system (Soss and Weaver, 2017), a minority of blacks and Latinos experience incarceration. Moreover, restrictionist immigration policies tend to target unauthorized immigrants who comprise a relatively small proportion of the broader immigrant population.

2.2 Japanese-Americans, Internment, and Expectations

The case of Japanese-American internment during WWII makes an urgent and compelling case to study these topics. First, unlike instances in authoritarian regimes, the Japanese-American case took place in a relatively well-functioning, modern, liberal democracy.[5] Second, the internment of Japanese Americans involved the explicit targeting of an entire ethnic group, a contrast with ostensibly race-neutral targeting (such as policing or even immigrant detention centers). Lastly, Japanese Americans remained in the U.S. following their internment, enabling us to evaluate subsequent political participation.

In this regard, Japanese-American political behavior has been the subject of robust scholarly research, with several studies finding a high baseline level of political engagement. For example, Wong et al. (2011, p. 18-20), report that, among Asian Americans, "Japanese Americans are the likeliest group to be registered to vote and to report voting," with registration, turnout, and engagement rates far outpacing U.S. averages (Wong et al. (2011), Table 1.1). This activism has translated into political officeholding: "many Japanese Americans ran for elected office and, by 2008, the majority of Asian American elected officials were of Japanese descent" (Wong et al., 2011, p. 48). However, the same authors also report that "among national-origin groups…Japanese (11 percent) are the most likely to report being a victim of a hate crime" (Wong et al., 2011, p. 169).

Several scholars have linked political engagement among Japanese Americans back to internment. For example,Wong et al. (2011) argue that Japanese Americans as a group "are characterized by relatively high socioeconomic status, as well as stark historical experiences of racial discrimination, including the internment of Japanese Americans during World War II" (p. 180). Within cultural anthropology, Takezawa (1991) points to the movement for redress as a unifying event for many third-generation Japanese Americans; "the ethnicity of the Sansei today is constructed not merely from racial and cultural markers of pre-war days," she notes, "but from a sense of

[5]Scholars have noted subnational authoritarianism in the U.S. South during the early 20th Century (e.g., Mickey, 2015), not to mention the proceeding 250 years of chattel slavery. In addition, American history is replete with other examples of racial targeting, including the targeting of Native Americans, Asian immigrants, and Latino/as, as well as more localized targeting of certain white immigrants. Religious minorities (for example, Mormons) also have historically been targeted by the state.

suffering of their forebears who experienced internment" (p. 41). As one of the Japanese Americans that she interviews notes, "My parents went to the camp, that affected me indirectly. Now I have become closer to the Japanese American community, whereas originally before the redress [movement], I wouldn't say that I was very much close to the community" (quoted in Takezawa, 1991, p. 48).

That both direct and indirect experience with internment should yield lasting effects across generations is supported by other literature. General orientations toward politics such as partisanship and political cynicism are correlated across generations (Jennings and Niemi, 1968; Jennings, Stoker and Bowers, 2009). Moreover, as research on pre-adult political socialization has shown, intensive exposure to highly salient political events can crystallize attitudes toward policies and candidates, closing attitudinal gaps between adolescents and adults (Sears and Valentino, 1997). Furthermore, in the aftermath of state violence and repression, group attachments, perceptions of victimhood, and feelings of threat are correlated across generations, despite a lack of direct experience with oppression among younger generations (Lupu and Peisakhin, 2017). For example, although conversations within families about internment were brief and infrequent, children still sensed the "racial implications of the internment" (Nagata and Cheng, 2003, 268). There is also evidence in the U.S. that experience with the carceral state can have demobilizating effects on family members, albeit ones that are not long-lasting (White, 2019*a*).

Taken together, the literature is in agreement that internment was, and is likely to be, an important political event for those who experienced it as well as their family. However, in terms of impact, the literature suggests two divergent possibilities. First, in line with the comparative politics literature as well as the literature on Asian American public opinion, we expect that the internment experience had a *galvanizing effect* on Japanese-American political involvement. That is, internment was a seminal event that fomented a shared ethnic identity and raised Japanese-American political consciousness, leading to greater political engagement, activism, and political leadership. On the other hand, more in line with the literature on state-sponsored incarceration and its impact on American minority communities, we may expect that Japanese-American in-

ternment resonates more strongly with findings on custodial citizenship, creating a *depressive effect* on subsequent political engagement among those affected and also their families. We may also expect that such a depressive effect could vary according to camp conditions, echoing theorizing in the carceral effects literature. Ultimately, both of these are highly important: as the U.S. and other liberal democracies turn to internment in targeting largely minority migrant populations, scholars and policymakers must be aware of the possible downstream political consequences of these actions for these communities. We now turn to investigating these possible outcomes.

3 Japanese-American Internment Context and Data

Following the attack by Japanese air forces on Pearl Harbor, Hawaii, the United States entered World War II on December 8, 1941. On February 19, 1942, President Franklin Roosevelt, operating with the encouragement of top military officials, signed Executive Order 9066, authorizing the militarized internment of people of Japanese ancestry. Thus began the forcible relocation of more than 110,000 people of Japanese descent—the majority (62 percent) of whom were American citizens—into years-long internment into military camps.

Public Proclamation 1, issued on March 2, 1942, by pro-internment U.S. Army General John L. Dewitt, created two distinct military "exclusion zones." Military Area 1 included swaths of the American west coast deemed sensitive for military purposes. This encompassed the coasts of California, Washington, and Oregon, as well as the southern sections of California and Arizona (Kashima et al., 2012). The remainder of these states constituted Military Area 2. Initially, all of those Japanese Americans living in Area 1 were under mandatory evacuation, but all California-based people of Japanese ancestry eventually fell under the military order. People of Japanese descent living further inland in Washington and Oregon, as well as those living away from the coast were not subject to evacuation or internment.[6]

[6]Hawaii, with a large Japanese-American population at the time, presents a very different case. While approximately 2,000 Japanese and Japanese Americans living in Hawaii were interned in the continental U.S., the U.S. Army did not forcibly relocate the vast majority of Hawaii's approximately 158,000 ethnically Japanese residents (Scheiber, Scheiber and Jones, 2009; Kashima, 2003). Instead, Hawaii's governor declared martial law in 1941, placing the terri-

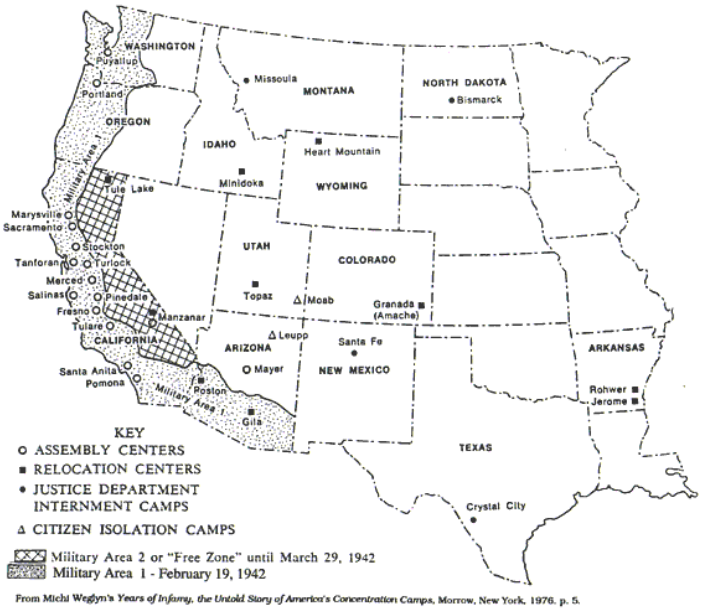


Figure 1: Historical map of the two military areas as well as the location of the Japanese-American internment camps. Source: Weglyn (1976).

Thus, as the historical map in Figure 1 shows, the eventual exclusion zone covered the entire state of California along with western sections of Washington, Oregon, and Nevada. Because people of Japanese ancestry disproportionately lived in California and other coastal areas, this had the effect of including the vast majority of people of Japanese ancestry in the exclusion zone. Specifically, over 110,000—more than 86 percent—of Japanese Americans in the continental U.S. resided within the final exclusion zone and were thus relocated from their homes; California alone contained around 73 percent of the total Japanese American population living in the continental U.S. (Ruggles et al., 2019).

tory under the command of the U.S. Army and subjecting the entire Hawaiian population to strict curfews and other restrictions. Because only a very small, targeted fraction—around 2 percent—of Hawaiians of Japanese descent were interned, we set Hawaii aside in our analyses. Additional qualitative context about Hawaiian internment is provided in Appendix Section 8.1. See also Kashima (2003, ch. 4).

Table 1: Internment Camp Characteristics

Camp	State	Peak Pop.	Guard Towers	Strikes	Use of Force	Violence
Amache	Colorado	7,318	6	0	0	0
Crystal City	Texas	4,000		0	0	0
Jerome	Arkansas	8,497	7	0	0	1
Fort Sill	Oklahoma	707		0	1	0
Heart Mountain	Wyoming	10,767	9	1	0	0
Minidoka	Idaho	9,397	8	0	0	0
Livingston	Louisiana	1,123		1	0	0
Lordsburg	New Mexico	2,500		1	1	0
Manzanar	California	10,046	8	0	1	1
Rohwer	Arkansas	8,475	8	0	0	0
Tule Lake	California	18,789	19	1	1	0
Poston	Arizona	17,814	0	1	0	1
Gila River	Arizona	13,348	1	0	0	0
Topaz	Utah	8,130	7	0	1	0

Sources: Ishizuka 2016, Burton, et. al. 2002, Densho Encyclopedia
(see `http://encyclopedia.densho.org/Fort_Sill_(detention_facility)/`),
New Mexico Office of the State Historian,
(see `http://newmexicohistory.org/places/lordsburg-internment-pow-camp`)
Note: Information on guard towers is not available for all facilities.

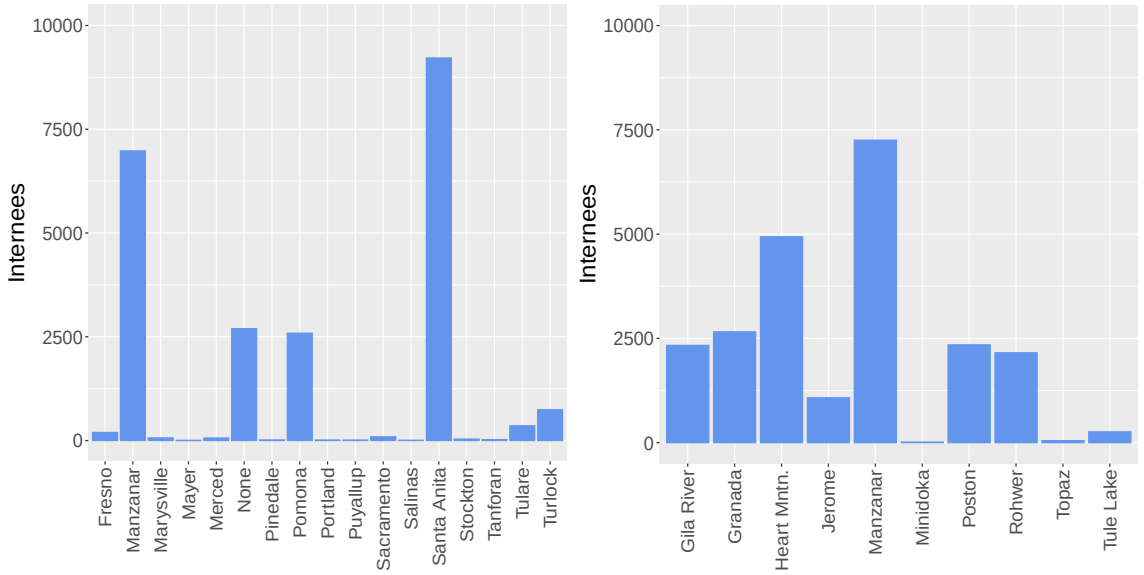
Internment Camp Site Selection. The U.S. Army began to scout locations that could collectively accommodate large concentrations of people in the spring of 1942. Sites suitable for internment had to be far from strategic military targets on the West Coast, large enough to house thousands of internees, and connected to transportation and public utilities. Because of these pressures, the U.S. Army focused on developing sites with these features already in place. Accordingly, all but four assembly and relocation centers were located in pre-existing fairgrounds, stockyards, and exposition centers (Ng, 2002), all in remote locations.

As we note above, a key motivation is to assess the extent to which camp-specific environments impacted subsequent political engagement. Figure 1 shows the locations of the 10 major "War Relocation Centers," (the largest internment camps) while Table 1 summarizes several camp characteristics. As the table shows, some camps had more military-style infrastructure, including watch towers and buildings dedicated to military use. We describe how these variables were coded in the data discussion, below.

Camp Assignment. The U.S. Army began evacuations on March 31, 1942 using a consistent procedure (Daniels, 1993). The larger military zones depicted in Figure 1 consisted of 108 Civilian Exclusion Zones encompassing approximately 1,000 people each. Army personnel posted signs displaying Civilian Exclusion Orders in public places (such as post offices, telephone poles, and store windows) throughout each of the 108 zones. Exclusion Orders informed people with Japanese ancestry (in practice, people who were at least 1/16 Japanese) that they were required to register themselves and their families and prepare for transfer. In the two days following, the head of each household would report to a control center near his or her home, register, and receive instructions for relocation. Evacuees were given six days to travel to one of multiple assembly centers located throughout California, Oregon, and Washington. Internees spent an average of 100 days in an assembly center before being transferred to a permanent internment camp (Kashima et al., 2012).

Importantly for our analyses, with rare exceptions, evacuees were transferred from assembly centers to internment camps according to criteria unlikely to be correlated with their personal, cultural, or economic backgrounds. Evacuees in assembly centers with the most dangerous conditions (e.g., no indoor plumbing) were moved to internment camps first (Burton, 2000). U.S. Army records suggest that it made efforts to move evacuees waiting at assembly centers to the nearest internment camps with climates closest to what they had known at home (Burton, 2000). Beyond these two concerns, families were assigned—together when possible—to internment camps that were (1) sufficiently complete in terms of construction to house evacuees and (2) had room for them. We see direct evidence of this in the War Relocation Authority's records. Figure 2 shows that internees from Los Angeles County were primarily sent to the nearby assembly centers (left) in Manzanar, Pomona, and Santa Anita. The same internees were distributed across a variety of internment camps (right) in Arizona (Gila River, Poston), Colorado (Granada), Wyoming (Hearth Mountain), and Arkansas (Rowher, Jerome). Thus, conditional on initial pre-internment location, final camp assignment was exogenous to family characteristics.

Figure 2: Distribution of Assembly Center and Internment Camp Locations among L.A. Internees



(a) Assembly Centers

(b) Internment Camps

3.1 Data Sources and Key Variables

Our primary interest is in how the internment experience impacted the political behavior and attitudes of those interned and their direct descendants. For this, we draw on the Japanese American Research Project (JARP), a nationally representative multi-wave survey of 4,153 mainland Japanese Americans that was conducted in 1967. (The survey excludes Hawaii-based people of Japanese descent. As we discuss above and in Appendix Section 8.1, this is not a problem for our substantive inferences as only a very small fraction of Hawaiians of Japanese ancestry were interned.) Although these data are older, we use JARP for several reasons. First, these data were collected at a time period when many of those who were interned were still alive and possibly politically active, thus allowing us to evaluate the political engagement of those who experienced internment first-hand. (Making inferences about the direct internment experience using data gathered today would be impossible; as of our writing, those interned as very young infants

would be in their late 70s.) Second, other surveys of Asian Americans tend to be underpowered with regards to Japanese Americans. Third, JARP includes information about where respondents lived between 1932 and 1941, which allows us to leverage conditional exogenous variation in camp assignment (following Shoag and Carollo, 2016). Lastly, the study allows us to evaluate the effects of internment on political engagement across generations because it includes three immigration cohorts. These are (1) Issei (Japan-born immigrants, or first-generation immigrants from Japan), (2) Nisei (individuals born to Japan-born immigrants, or second generation), (3) Sansei (third generation). Given the cross-cohort nature of the survey, we selected outcomes that were present in at least two of the questionnaire forms (i.e., Issei, Nisei, or Sansei). Key features of the JARP data are summarized in Table 2.

The JARP data are, however, constrained in that camp assignment is only available for Issei, despite the fact that many Nisei and even some Sansei were interned (Table 2). In the results that follow, we link respondents in JARP using the survey's family identifier field and assign subsequent generations the internment camp reported by Issei members. That is, we assume that all members of the same family were interned at the same place.[7] In addition, in all of our main analyses, we include a fixed effect for immigration generation (Issei, Nisei, or Sansei) since JARP used different questionnaires for each cohort. This also makes substantive sense: cultural and political differences across these immigration cohorts could have not only impacted the internment experience, but also influenced political attitudes.

In addition to JARP, we use the War Relocation Authority's ("WRA") records of Japanese internment in order to validate our results and provide richer descriptions of the internment experience.[8] The WRA and the Department of Justice recorded detailed personal information about internees, including name, age, gender, addresses prior to internment, family units, education,

[7]This assumption is plausible. First, the U.S. Army prioritized keeping families together. Second, families living close together were likely to be in the same Civilian Exclusion Zone and thus ordered to the same assembly centers. Assembly centers would often send their whole populations to single internment camps when capacity was available. We recognize, however, that this assumption is potentially incorrect for some Nisei children living far enough from their parents to have been interned separately.

[8]See the Database of Japanese American Evacuees, Record Group 210, National Archives, `https://www.archives.gov/research/JapaneseAmericans/wra`.

Table 2: JARP Sample Demographic Information

Generation	Observations	Age	Gender	Married	California	Oregon	Washington	Other	Interned	Families
Issei	1,047	72	0.66	0.57	723	47	127	150	0.82	0.70
Nisei	2,304	41	0.52	0.81	1,470	101	266	467	0.76	0.86
Sansei	802	22	0.47	0.30	104	2	24	672	0.21	0.85
Total	4,153	45	0.55	0.65	2,297	150	417	1,289	0.67	0.82

Notes: Age represents mean age; gender represents proportion male, and married represents proportion married. California, Oregon, Washington, and Other represent counts of Japanese Americans living in each location on the eve of the internment period. Interned represents the proportion of respondents in each generation who were interned Families represents the proportion respondents in a family where at least one member was interned

and occupation. These records contained assembly center and internment camp assignment information for 109,384 Japanese Americans interned during WWII.

Internment Status. Given that the data span several immigration cohorts, we operationalized exposure to internment in three ways:

1. *Direct Exposure*: Respondents who were themselves interned, either solo or alongside members of their family
2. *Family-Only Exposure*: Respondents who were *not themselves* interned but had at least one family member in the sample who experienced internment[9]
3. *Baseline*: Respondents who were not interned *and* did not have an interned family member in the JARP.

In total, 67% of respondents in the JARP (across Issei, Nisei, and Sansei cohorts) fall into the first category of direct exposure, 18% into the second category of family-only exposure, and 15% fall into the baseline or control group of no exposure whatsoever.

Individual covariates. We also include respondent gender and age, both recorded when respondents participated in the JARP survey waves. Age and gender are pre-treatment features that

[9]Each family in the JARP was assigned a family identifier, which can be used to calculate how many respondents within a given family were interned.

may also affect our outcomes of interest. (Age, for instance, likely affects political engagement independently of the internment experience.) Finally, our analysis of camp-level effects relies on controls for respondents' place of residence on the eve of internment. Since the internment camp to which people were ultimately confined was orthogonal to pre-treatment characteristics, it was largely a function of where they lived at the onset of WWII. All of our analyses of camp effects therefore control for pre-treatment area of residence.

Camp environment covariates. As we note above, part of our contribution lies in exploring whether the conditions of internment had potentially depressive effects, echoing what has been suggested by the carceral state literature (e.g., Weaver, Hacker and Wildeman, 2014). We do so by gathering new data on the conditions of the WRA camps themselves. First, we operationalize the level of militarism in camps by recording the numbers of watch towers per 1,000 people in each camp. Second, we also draw upon multiple historical resources to identify relocation centers that experienced violence among internees, violence between internees and civilians, or the use of force by military personnel against internees. The strikes, violence, and force columns in Table 1 represent indicator variables for whether each event occurred in the camps. ("Violence" represents violence between internees or violence between internees and the surrounding civilian population.) Lastly, we also incorporate other contextual features such as the percent of the surrounding county that was white (according to the 1940 U.S. Census) and the percent of the county-level vote-share won by Franklin Roosevelt in the 1940 presidential election. We use these camp characteristics in Section 6.1.

Outcome Variables. Our main outcomes of interest concern political engagement, a topic on which we have several JARP questions.[10] First, "political interest" is measured using a four-item

[10]We do not present results on turnout and vote choice. Information on both items appears in the JARP, and our analysis suggests that internment experience has no detectable effect on either turnout or vote choice. However, both of these are expected outcomes given the previous literature describing internees. Regarding voter turnout, over 40% of Issei respondents in our data report not having U.S. citizenship. In addition, over 40% of Sansei in the JARP data were under 21 at the time the survey was administered, making them ineligible to vote. As a result, the JARP provides us with limited power to investigate this question. The same concerns apply to voter preference, but, in addition, the direction in which internment might affect vote choice between candidates from the two major parties is unclear.

ordinal scale ranging from "No interest at all" (0) to a "A great deal" (3) ($\bar{x}$ = 1.29; s = .83).[11] Second, "political engagement" is measured using a binary item asking respondents whether non-family members have asked them for advice regarding politics ($\hat{p}$ = .17). Third, "faith in government" is measured using a binary item asking whether respondents disagreed that "most people in government are not really interested in problems of the average man." ($\hat{p}$ = .43), and finally, preferences for dissent are measured using a trichotomous variable that captures whether respondents would have preferred a leadership strategy that emphasized dissent (-1), accommodation (1), or neither (0) ($\bar{x}$ = .61; s = .77).

For this question, JARP asked whether Japanese-American leadership should have, in retrospect, protested the circumstances of internment or cooperated with the U.S. government. Respondents could have answered that the best leadership strategy at the time would have been to protest internment (which we code using the value -1, for simplicity); they could have indicated that they were not sure what the best strategy was or that they couldn't generalize on this point (0), or they could have claimed that the best leadership during the period would have sought a peaceful and orderly transition into internment facilities (1). Seventy-nine percent of respondents who were asked this question agreed that a peaceful and orderly leadership approach was preferable.

4 Effects of Internment Status

We first investigate the possible role internment status—that is, the relationship between direct exposure to internment or family-only exposure to internment—played in shaping political engagement and political attitudes. For this, we leverage the fact that nearly 86 percent of people of Japanese descent living in the continental U.S. were residing in the exclusion zone, but 14 percent were not. Although not causal, the variation allows us to assess the relationship between

Internment was ordered by a Democratic president, but prominent Republicans had lobbied the federal government for restrictions on Japanese immigration and subsequent internment.

[11] The Issei version of this question had different response options, allowing for "No" or "Yes" responses. We code these responses as 0 and 1 on a 0-3 scale.

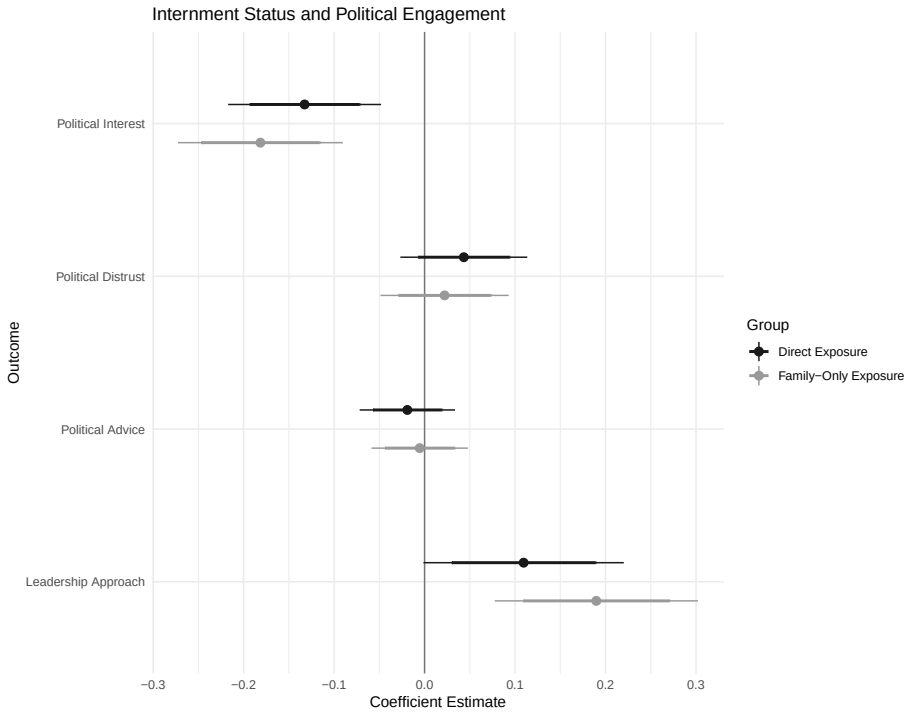


Figure 3: The relationship between internment status and various indicators of political engagement, conditional on age, gender, generation, and pre-internment residential locations. A measure of political interest was present in all three generation forms, whereas the other three questions were only present in the Nisei and Sansei forms. 84% and 95% confidence intervals are presented along with coefficient estimates. 84% confidence intervals (heavy bar) allow for visual tests of equality across coefficients (Julious, 2004). Full model results are presented in the supplemental appendix.

whether a person or one of their family members was sent to a camp and their subsequent political attitudes—a relationship that many scholars believe could have galvanized Japanese Americans' political engagement (Wong, Ramakrishnan, Lee and Junn, 2011).

Figure A.5 reports results from a linear model regressing indicators of political engagement on measures of direct exposure and family-only exposure to internment with pre-internment residential locations and generational identifiers included as fixed effects. (The full specification is provided in Appendix Table A.5.) The figure shows that those who were interned are about 13% of a scale point ($\pm$ 9% of a scale point) *less likely* to report an interest in American politics than those

who were not, a statistically significant difference. These patterns are similar among Japanese-Americans in our sample who themselves were not interned but who had family that were. These individuals are about 18% of a scale point ($\pm$ 9% of a scale point) less likely to express an interest in politics. These estimates correspond to a movement of approximately 3% and 4% along a three-point scale, respectively. For both the distrust and political advice, estimates are in the expected direction, but there is considerable uncertainty. As for the leadership approach outcome, those who had direct exposure to internment are about 11% of a scale point ($\pm$ 11% of a scale point) more likely to support a "peaceful and orderly" leadership approach during internment than one employing protest and dissent, relative to others. Among those who were not interned themselves but had family who were, this difference is approximately 19% of a scale point ($\pm$ 11% of a scale point). These two estimates reflect a 3% and 6% movement across a three-point scale, respectively.

Consistent with an intergenerational transmission mechanism, coefficient estimates for both measures of internment status are strikingly similar across outcomes. Formal tests of differences between the two never reach conventional levels of statistical significance (Appendix Table A.6).

Alternative Explanations in Military Service and Earning Potential. These results provide evidence that internment suppressed political interest. But could other explanations drive this finding? One possibility is that there is unexplained confounding between those of Japanese ancestry who were interned (or had family interned) and those that did not. However, using data from the 1940 Census, we fail to reject the null hypothesis of no mean difference between Japanese Americans inside and outside of the exclusion zone with respect to gender, marital status, age, education, employment rates, and occupational class (see Appendix Section 8.7.2).

What of other explanations stemming from the internment experience? We consider a likely possibility to be military service. During WWII, Japanese-American men of draft age were initially ineligible for military service. The U.S. government reversed this policy in 1943 and, ultimately, over 30,000 Japanese-Americans served in WWII. In this regard, studies have shown that those who served have higher levels of political participation (e.g., Mettler, 2002; Teigen, 2006) and that there is an inter-generational component (Campante and Yanagizawa-Drott, 2015). Thus, an

alternative explanation is that those who were interned had lower rates of military service and this, not internment, drives our findings.

We investigate this by examining a JARP question that asked Nisei and Sansei respondents whether they (or their spouses) had ever served in the U.S. Armed Forces. While 55% of Nisei respondents and 34% of Sansei respondents reported serving in the military, we find no significant differences between the baseline, direct exposure, and family-only exposure groups in terms of military service (see Appendix Table A.21). This suggests that Japanese-Americans who were interned are comparable to those who were not in terms of their willingness to serve in the U.S. military.

Another possible explanation is that those interned suffered financially in ways that have persisted over time—perhaps as a result of lost professional opportunities or other lost income. Given that people who have fewer financial resources tend to be less political engaged, this income differential (and not the internment experience itself) could possibly explain our findings. Though we find differences in total family income at the time of the JARP survey between the baseline group and those affected by internment (Appendix Table A.22), estimates for the direct exposure and family-only exposure group run in opposite directions, with higher incomes among those personally interned and lower incomes among those with family-only exposure. Given this inconsistency, and given the surprising positive finding among those directly interned, income differentials are unlikely to explain the general pattern of disengagement for both groups.

5 Effects of Internment Length

We next evaluate the association between internment *length* and political engagement. We would suspect, in accordance with the literature on ethnic targeting (Lupu and Peisakhin, 2017) and carceral contact in the United States (Weaver and Lerman, 2010), that longer interments would more strongly demobilize and depress civic engagement. After all, extremely brief detainments may have little effect, but longer internments may expose internees to ongoing injustice, recalibrating their political views toward the U.S., and perhaps souring them on future engagement

Table 3: Internment Length - Summary Statistics

Internment Length	N	Percentage of Interned Sample
Less than 1 Year	313	11.50
1 - 2 Years	498	18.30
2 - 3 Years	678	24.90
3 - 4 Years	1,064	39.10
4 - 5 Years	157	5.80
5 + Years	8	0.30

(Weaver and Lerman, 2010). Table 3 provides summary statistics on internment length. Roughly 12 percent of internees in our sample were released within one year, while 45 percent were detained longer than three years.

We operationalize this using simple ordinary least squares and subsetting the data to only those with direct experience with internment—i.e., those who were themselves interned (either on their own or alongside family members). The data show that those interned for longer periods had greater attenuation in political engagement, shown in Figure 4. (The full specification is provided in Appendix Table A.7). An additional year of being interned is associated with approximately 1.4% of a scale point decrease in political interest ($\pm$ 2.6% of a scale point, so narrowly insignificant), a 4.2 pp increase in distrust ($\pm$ 2.2 pp), a 3.4 pp decrease in the likelihood of being sought out for political advice ($\pm$ 1.6 pp), and 4.3% of a scale point ($\pm$ 3.4% of a scale point) increase in supporting a "peaceful and orderly" leadership approach during the internment process. To put this into context, those who were interned for four years or more (6% of the interned sample) are approximately 4% of a scale point less likely to report an interest in American politics than those who were interned for less than one year (12% of the interned subsample). Moreover, they are approximately 17 pp more likely to express distrust in government, 14 pp less likely to be sought out for political advice, and 17% of a scale point more likely to support a "peaceful and orderly" leadership strategy. This corresponds to a movement of about 6% across the the three-point scale.

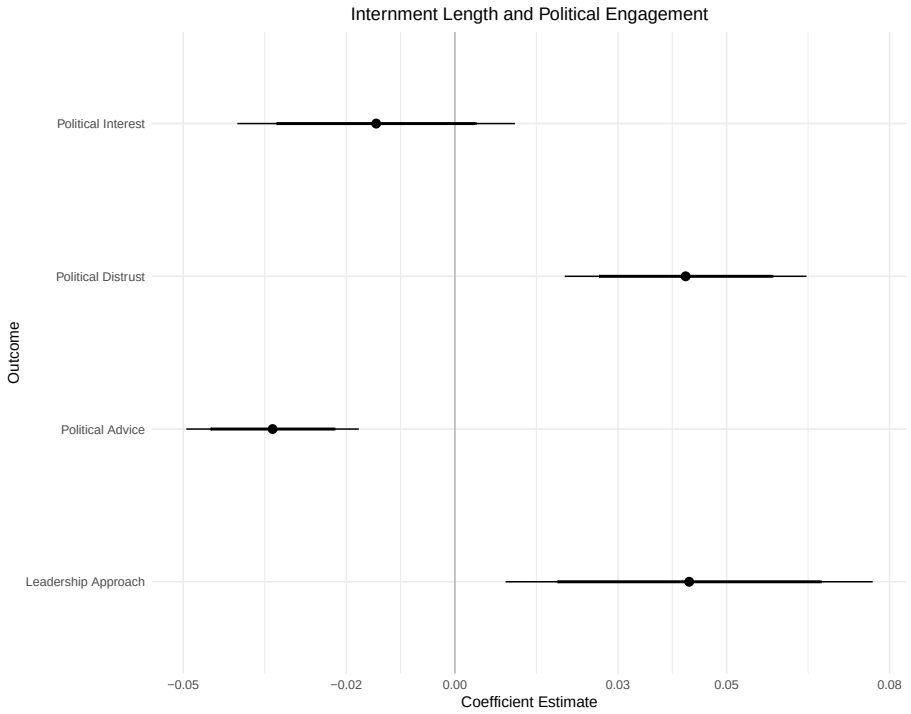


Figure 4: The relationship between internment length and various indicators of political engagement, conditional on age, gender, and generation among those who were personally interned. A measure of political interest was present in all three generation forms, whereas the other three measures were only present in the Nisei and Sansei forms. 84% and 95% confidence intervals are presented along with coefficient estimates. Full model results are presented in the supplemental appendix.

6 Effects Associated With Internment Experience

These findings show broadly demobilizing effects associated with internment status and internment length. In this section, we examine the internment experience more closely to try to understand the explanation behind these findings. Not only does doing so shed light on how internment can lead to demobilization, but as as we explained in Section 3, conditional on initial place of residence, assignment to one of the 10 major internment camps was unrelated to individual or family attributes (Shoag and Carollo, 2016). This allows us to estimate the causal effects of exposure to specific camp conditions, conditional on internment, on downstream political

behavior in order to gauge possible mechanisms.

We expect that these conditions are substantively important in shaping political attitudes. As suggested by the literature on custodial citizenship (Weaver, Hacker and Wildeman, 2014), internees in camps that experienced more unrest, strikes, or backlash may have experienced greater demobilizing effects. First-person accounts of the internment experience frequently emphasize two socially pertinent features: (1) the struggle to access basic resources and (2) the reporting of (sometimes violent) widespread unrest among internees. On the first point, shortages of basic necessities were widespread. Flimsy living quarters meant that internees had little protection from vermin and extreme weather (Pistol, 2017). Grievances over basic needs sparked strikes and demonstrations across several camps, inflaming tensions among internee factions. At Tule Lake, agricultural workers protested authorities' unwillingness to compensate the widow of a worker killed in a trucking accident; the camp's project director responded by using Poston and Topaz internees as strike breakers (Burton, 2000). At Manzanar, union leader Harry Ueno led an investigation of supply shortages and founded the Mess Hall Workers Union in 1942. Tensions between Ueno's union and the broadly pro-American Japanese-American Citizens League led to larger-scale violence among internees on multiple occasions (Burton, 2000). These activities impacted large shares of affected camp populations.

We expect that being imprisoned in a camp that witnessed strikes or violence among internees might be demobilizing for several reasons. Internees who went on strike did so because they believed that camp authorities—the arm of the state with which they interacted most—were not committed to providing basic needs. At Manzanar, for example, Ueno's committee found evidence that shortages were caused in part by a camp official smuggling out supplies. Using the literature on the carceral state as a guide (Lerman and Weaver, 2014), experiences like this may have dampened faith in government and reduced the motivation to participate in politics. Violent disagreements among internees would similarly have depressed political activity by straining communication and fostering resentments in the community.

We explore these possible effects in Figures 5 (strikes) and 6 (violence). These show the results

of OLS regressions of binary outcomes on whether the respondent was affiliated with an internment camp in which a strike or violent event took place. In both sets of analyses, we include fixed effects for immigration cohort/survey wave (Issei, Nissei and Sansei), as well as controls for pre-internment location, age, and gender. (JARP includes case internment location only for Issei; we assume that interned Nisei and Sansei were sent to the same camps as their Issei relatives, an assumption we discuss above.) As before, we assess camp-treatment effects across two groups: (1) Direct Exposure (people who were themselves interned, either solo or alongside family) or (2) Family-Only Exposure (people who were not interned, but had Issei family members who were). We do not estimate effects on those who were not interned and had no family interned.

Strikes. We first examine the causal effect of being assigned to a camp that experienced strikes. Figure 5 shows that respondents who were themselves interned in camps where strikes took place were 5% of a scale point ($\pm$ 8% of a scale point) less likely to report being interested in American politics, although this is narrowly insignificant. Among those who were not themselves interned, but had family members who were, this treatment effect is of a larger magnitude (nearly 9% of a scale point), but insignificant ($\pm$ 11% of a scale point). In terms of trust in government, respondents who were interned themselves were approximately 8 pp ($\pm$ 5 pp) more likely to say that the government is not concerned with the issues facing everyday people; results are similar for respondents who only experience internment via family. Both are significant.

As for political advice, respondents interned at camps that witnessed strikes were 2.4 pp ($\pm$ 2 pp) less likely to report having been approached for political advice, though this finding is significant only for those who themselves were interned (and not for those who only had family interned). Lastly, the results suggest that internees sent to camps that experienced strikes were significantly more likely to favor leaders who espoused peaceful transitions, perhaps because such respondents did not believe protesting would yield concessions or because they feared retribution. On this point, those directly interned are 0.08 ($\pm$ 0.05) scale points (relative to the -1, 0, 1 scale) more likely to prefer leaders who fostered orderly transitions. The findings are similar are roughly similar in magnitude but insignificant for those who were not themselves interned but who had

family who were.

Overall, the results lend support to the hypothesis that labor unrest and relative deprivation at camps has a demobilizing effect on internees. We mostly detect slightly stronger findings for those individuals themselves interned, although formal tests looking at the difference in treatment effect between direct versus family-only exposure are insignificant. (Appendix 8.6). This provides suggestive evidence in favor of an inter-generational transmission of attitudes, consistent with our findings above.

Violence. We next examine the differences in outcomes in terms of whether internees were assigned to camps that witnessed violence. These results are presented in Figure 6. In terms of political interest, respondents who lived through violent episodes while they were interned express significantly lower levels of interest in American politics than counterparts who did not experience violence. These respondents were 7.5% of a scale point ($\pm$ 6% of a scale point) less likely to report an interest in American politics. However, the effect is only significant for those who themselves were interned, either on their own or alongside family. For individuals who only experienced the effects of internment via interned family, the effect is in a similar direction but not significant.

For trust in government, internees imprisoned in camps that witnessed violent episodes amongst internees themselves were 2 pp ($\pm$ 7 pp) more likely to report believing that the government had little concern for the problems faced by average people. Respondents who only had family interned were significantly more likely to express skepticism of the government than other internees. This group was 10pp ($\pm$ 4pp) more likely to report high levels of distrust.

Respondents interned in camps that experienced violence among internees also reported lower rates (2 pp) of being sought out for political advice, but the finding is not significant either for those directly interned or those who only had family interned. Lastly, the respondents directly interned in camps that experienced violence were 0.08 scale points ($\pm$ 0.07) more likely to support leaders who did not favor protest. The effect for those were not themselves interned but who had family members who were are similar in magnitude, but narrowly insignificant.

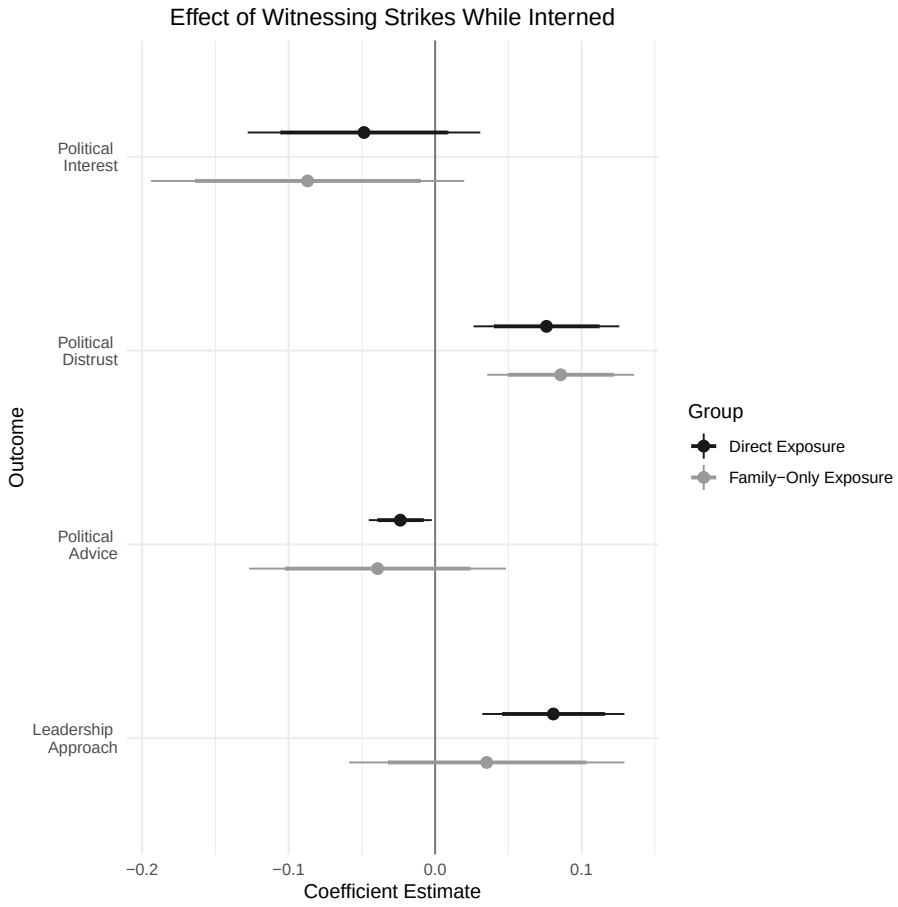


Figure 5: Effects associated with strike conditions in a camp. 95% (narrow bar) and 84% (heavy bar) confidence intervals are shown. Controls include age, gender, pre-internment residential location, and survey wave (Issei, Nisei, Sansei). Political interest models rely on data from all three generations; political advice, leadership approach, and political distrust models are based on the Nisei and Sansei waves. Sample sizes are reported in Table A.9 of the supplemental appendix.

Overall, these effects are slightly more modest than those we found regarding strikes. However, they provide suggestive evidence that violence had a demobilizing effect. In addition, we cannot rule out differences in treatment effects transmitted across generations through family experiences (shown in Appendix 8.6), although our findings are mostly significant for those who experienced internment themselves.

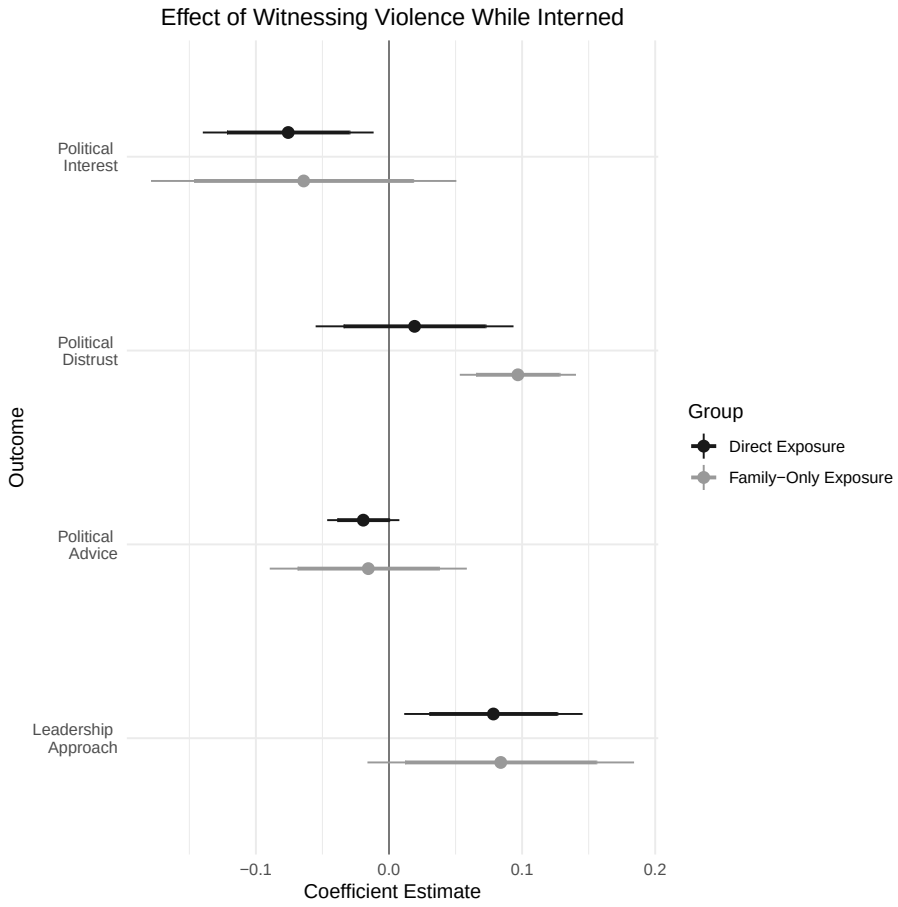


Figure 6: Effects associated with violent conditions in a camp. 95% (narrow bar) and 84% (heavy bar) confidence intervals are shown. Controls include age, gender, pre-internment residential location, and survey wave (Issei, Nisei, Sansei). Political interest models rely on data from all three generations; political advice, leadership approach, and political distrust models are based on the Nisei and Sansei waves. Sample sizes are reported in Table A.8 of the supplemental appendix.

6.1 Robustness of the Results

In sum, our results lend support to a broad demobilizing effect associated with internment, with particularly strong effects among those who were interned longer and those with direct connections to camps exhibiting labor conflict or violence. We also find a corresponding, albeit slightly weaker effect among those with no direct internment experience but who had family

members interned, lending evidence to the possibility of inter-generational transmission of attitudes.

To this extent, our findings not only support the broader literature on custodial citizenship in the U.S. but expand it by confirming that internment length and conditions are an important pathway in the demobilizing effect. We note, however, some threats to this analysis. These are (1) confounding in camp assignment and (2) differences in pre-internment locations.

Robustness to Unobserved Confounders in Camp Assignment. Relying on the historical record and following other work on internment (e.g., Shoag and Carollo, 2016), we assume that, for those individuals who were themselves interned (either solo or alongside family members), camp assignment is orthogonal to pre-internment characteristics at the individual level. However, internees may differ on important characteristics impacting political engagement, even conditional on location.

One possible threat to our strategy is the possibility that the government reassigned or sequestered internees based on their level of resistance after their initial camp assignment. An example of this is the WRA's loyalty questionnaire, which was administered to men aged 17 and over beginning in 1943. Respondents who answered that they would not register for selective service and could not pledge unconditional loyalty to the U.S. were labeled "disloyal" and sequestered at the Tule Lake camp. We address possible confounding by replicating the analyses subsetting to JARP respondents who were younger than 17 in 1945. This group would not have been subjected to the questionnaire and is less likely to have expressed other resistance owing to their age. The results from that analysis are consistent with the effects of internment that we report in this section. (See Appendix Figures A.15 through A.18 for these results.)

We also assess whether camp assignment is related to any pre-treatment controls (such as age, gender, or occupation) that could also influence political engagement. We do so only for those who themselves were interned. Since internment camps are discrete units, we model camp assignment as a multinomial logistic function of age, gender, and pre-internment location—the most relevant pre-treatment control variables we have access to in JARP. Values in Table 4 correspond

to z-scores, calculated by dividing coefficients from the multinomial logit by their corresponding standard errors, for each covariate. In the age and gender columns, no camp is associated with a test statistic greater than 1.96 or less than -1.96. (Pre-internment location, on the other hand, *is* significantly associated with camp assignment in our model, but this is what we expect given the way camp assignment was carried out.[12])

Table 4: Covariate Balance Across Internment Camps

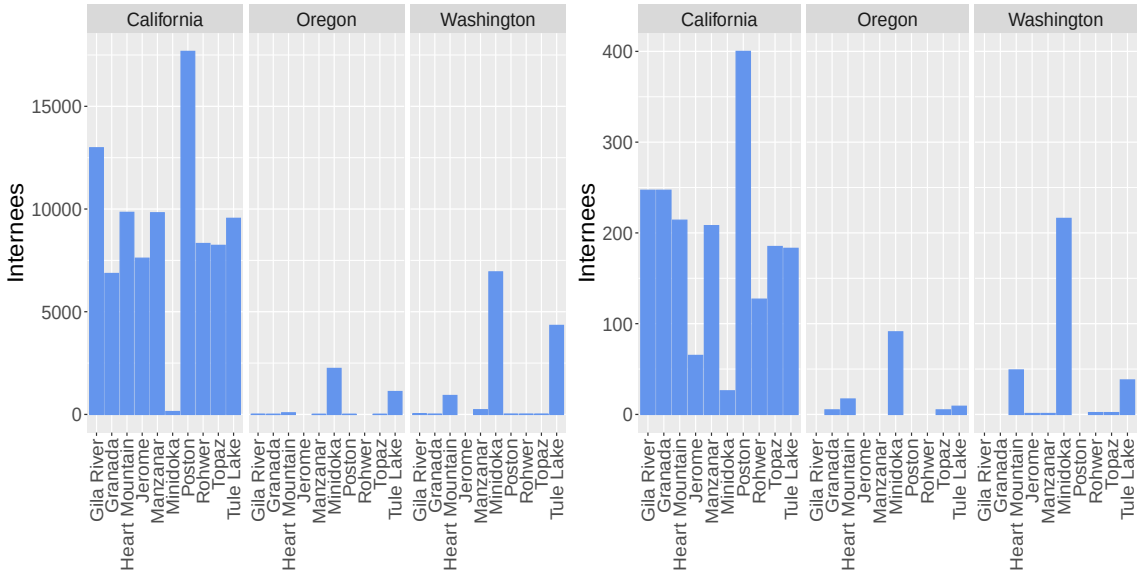
	Age	Gender (Male)	Pre-Camp Location
Jerome	-.88	1.27	-1.43
Heart Mountain	.38	-.54	-2.74
Minidoka	.14	.31	-1.97
Manzanar	-.18	1.58	-2.73
Rohwer	1.37	.15	-4.17
Tule Lake	-.49	.68	-1.07
Poston	-.09	.12	-2.06
Gila River	.04	-.64	-1.41
Topaz	.67	-.33	2.34

Note: Reference category is Granada (Amache)

Finally, plots of age, gender, and pre-internment occupations for internees held at different camps (displayed in Appendix Figures A.9 and A.10 and Appendix Table A.14) reveal no significant differences in these distributions across different internment locations. This is confirmed in a series of Kolmogorov-Smirnov (KS) tests comparing the distributions, presented in the Appendix. These provide evidence that internee camp assignment was a function of location (per Table 4) but not pre-treatment characteristics like age and gender. This gives us good reason to think that

[12]These results only include internment camps to which at least 30 respondents in the JARP reported being assigned; estimates for camps with fewer assigned respondents are unlikely to be reliable. In the JARP data, pre-camp location is a three-digit numeric code, which we leave as a numeric variable in this model to preserve power; locations in the same region and state will be close in value and locations across states will differ considerably in value, which provides reasonable distinctions between different pre-camp locations.

Figure 7: Assignment to Internment Camp by State of Residence



(a) WRA Data

(b) JARP Data

assignment was also orthogonal to other pre-treatment traits.

Location Prior to Internment. Identifying the effect of camp environment relies on the assumption that every person of Japanese ancestry who lived in roughly the same area on the eve of internment was treated similarly by the WRA, which prioritized rapid evacuation and proximity. Figure 7 provides evidence that supports this assignment mechanism. Each panel in Figure 7 displays the distribution of internees living in California, Oregon, and Washington on the eve of WWII. Panel (a) describes this information using the War Relocation Authority's records, while Panel (b) shows the same distribution using JARP data. This is again, only for those individuals who themselves were interned. If the assumption that the U.S. Army prioritized proximity and rapid transportation from assembly centers to camps holds, then most internees from a given state should have been held in camps closest to their state of residence.

This is borne out by the two figures. For example, Japanese Americans living in Oregon and

Washington were sent to the northern-most camps in California (Tule Lake), Wyoming (Heart Mountain), and Idaho (Minidoka). Those living in California were sent to camps located near them—these included Manzanar, Poston, and Gila River for people living in southern California and Tule Lake for people living in northern California. Lastly, we note strong correspondence between the two figures, giving us assurance that JARP self-reported data paints an accurate portrayal of internment assignment.

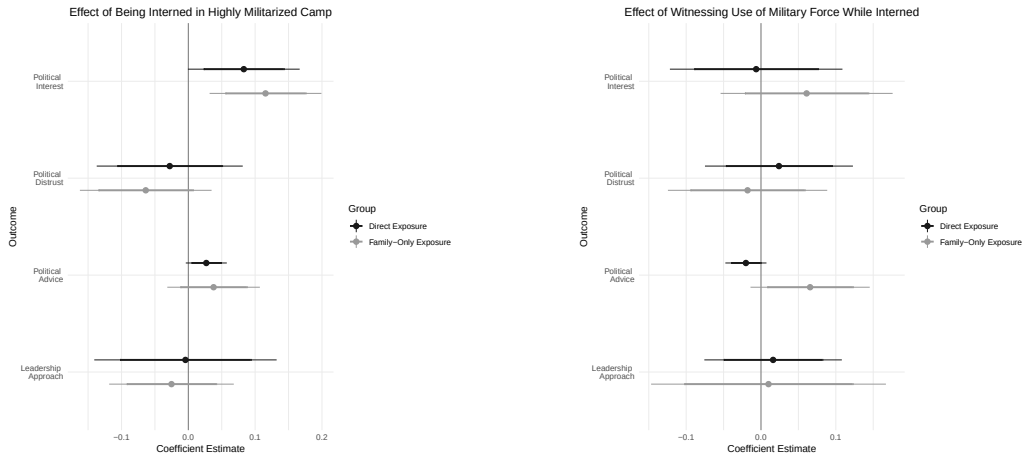
6.2 Alternative Mechanisms of Demobilization

Our analyses suggest that differences in political engagement among Japanese Americans varied not just by internment status, but also by features of the camps themselves, with exposure to unrest being an important mechanism behind demobilization. However, one potential challenge to this substantive interpretation is the possibility that any camp effects are actually the result of another factor correlated with internment location. Here, we examine the three likeliest alternative factors: (1) use of force by the state, (2) a militarized environment, and (3) the camps' surrounding racial and political environments.

Use of Force By the State. Previous work on the carceral state implies that the use of force by the state would have politically demobilizing effects via instilling fear of the state, which could possibly explain our earlier findings. However, this prediction is not borne out, as shown in Figure 8b, which analyzes camp effects according to whether the camp experienced at least one instance use of force by military personnel against internees. The point estimates of this effect on political distrust, advice, interest and preferences over leadership are close to zero. Only among those with family exposure only is a single significant finding (on political interest); all other findings are insignificant.

These non-findings are at first glance puzzling. One reason we might observe demobilizing effects for violence among internees (discussed in our previous section), but not for cases in which military personnel used force against internees, is the difference in scale. Episodes of violence

Figure 8: Camp Effects: Militarized Conditions



(a) Militarism (b) Use of Force

Note: 84% and 95% confidence intervals are shown. Controls include age, gender, and pre-internment residential locations. Political interest models rely on data from all three generations; political advice, leadership approach, and political distrust models are based on the Nisei and Sansei sample.

among internees generally tended to precede or follow large demonstrations or involve large groups. Incidents in which guards used force against internees, in contrast, tended to be more isolated and rare. To give one example, one such incident coded in our data involves a guard at Topaz camp fatally shooting an elderly internee for standing too close to a perimeter fence (Burton, 2000). Violent confrontations between large groups of internees would have exposed more internees to unrest than isolated shootings such as this, which involved few individuals.

Militarized Space. Another jarring feature of the internment experience was living under militarized conditions. These conditions manifested in physical space through the use of guard towers, barbed wire fencing, and barracks-style housing for internees. These physical elements served as "reminders of [internees'] lack of freedom" (Burton, 2000).

We operationalize the militarization of space as the number of guard towers per one thousand internees at peak camp population. The results of an analysis looking at the effect of these on

political engagement appear in Figure 8a. Respondents interned in more militarized camps (or with relatives who were) were 8% of a scale point ($\pm$ 8% of a scale point) *more* likely to express interest in American politics, a finding in the opposite direction from what we would expect. This effect is, however, only significant respondents who themselves were not interned but who had family members who were. Other findings are, across the board, insignificant and with point estimates close to zero. This suggest that the demobilization patterns observed in the JARP sample is due to the internees' collective exposure to unrest, rather than differences in the camps' physical features.

Surrounding Political and Racial Environment. Although camp locations were remote and internees had limited contact with civilians living nearby, a possibility is that exposure to local culture or local political attitudes provide a possible alternative conduit for the long term effects we report in Section 6, particularly for those who themselves were interned. For example, if internees were sent to camps located in nearly exclusively white areas, this might have led them to have lowered feelings of belonging, thereby suppressing overall political engagement. Similarly, internment in camps located in extremely conservative areas might have had a depressing impact on political engagement in a what is a more liberal-leaning group (Wong, Ramakrishnan, Lee and Junn, 2011).

We test for this possible alternative explanation by analyzing the effects of (1) the proportion of the internment camp's county population that was white in 1940 and (2) the political climate, as measured by Franklin Roosevelt's 1940 share of the two-party vote. To the first point, Appendix Tables A.17 through A.20 present the results of models that include the percent of the camp's county that was white and the percentage of the two-party vote received by Franklin Roosevelt in 1940 as the primary explanatory factors. As the analyses show, county demographics do not consistently have a significant effect on the various political engagement outcomes (showing a significant relationship in only two out of 16 specifications), meaning that this is unlikely driver behind our results. (Including this factor as a control in the same model as camp strikes and violent episodes does not change either the magnitude or significance of those findings, as shown

in Tables A.17 and A.18.) For local Democratic-party vote share, we see mixed findings, with some evidence that area conservativeness is negatively liked with some political engagement (for example on political advice); however, the results are not consistent across groups or outcomes. In addition, including Democratic-party vote share as a control in the same model as strikes or violence does not change the fact that those are statistically and substantively significant. The overall findings are consistent with the interpretation we presented in Section 6, which is that the interpersonal, on-the-ground environment was important in terms of the overall depressive effect of internment.

7 Conclusion

After Pearl Harbor, raw racism and xenophobia led over a hundred thousand people of Japanese ancestry to be transported to militarized internment camps, where they were held—against their will and unconstitutionally—for years over the course of WWII. In this study, we explored the downstream consequences of this seminal, racist event. Did the targeting by the state of one minority group have downstream consequences on that group's political attitudes and political engagement?

Our findings suggest that the internment of people of Japanese ancestry did indeed impact their subsequent political behavior, in ways that speak not just to scholarly discussions but also to ongoing current events. First, we examine the effects of such internment by comparing political engagement among those who were themselves interned (or had family interned) to those who were not. Conditional on pre-internment residential location, we find that those with direct and family-only experience with internment are less likely to express an interest in politics and prefer a confrontational strategy toward the government relative to those who were not interned. Second, we explored the impact of internment itself, conditional on people being interned. Leveraging the fact that camp assignment was exogenous to individual or family characteristics conditional on pre-internment location, we find that internment length and camp conditions are associated with decreases in political engagement. This suggests that longer and more hostile

contacts with the state contributed to larger demobilization effects among those connected to the experience. Lastly, across every measure of political engagement, we cannot find meaningful differences in the effect of internment among those directly interned versus those who experience internment through a family member. This suggests a strong depressing effect of internment on Japanese Americans' political engagement that extend to even those with more indirect exposure.

Our study makes several contributions to ongoing scholarly discussions. From the perspective of the comparative politics literature, our paper highlights the potential demobilizing effects of punitive interactions with the state in the unusual context of a liberal democracy. This is an important inquiry: to date, existing work on the topic of repression, violence, and ethnic targeting has mostly focused on weak states, with more recent studies considering the effects of authoritarian strong states such as the Soviet Union during Stalin's reign. Our findings dovetail with those of Lupu and Peisakhin (2017) and Rozenas, Schutte and Zhukov (2017) who find that repression and violence increases distrust toward the state. (However, in contrast to Lupu and Peisakhin (2017), we do not observe increased mobilization and political engagement among members of the targeted group.) Future research could explore whether these differences between democracies and autocratic regimes are shaped by institutional variation or the nature of repression efforts.

We also contribute to research in American politics examining the political consequences of growth in the carceral state. While much of this growing literature has viewed exposure to penal institutions in a binary fashion (contact versus no contact), we find evidence that conditions within these institutions matter, such that harsher conditions increase the likelihood of demobilization, distrust, and disengagement. Potential avenues for future research research on the "carceral state" could assess whether variation in the severity and duration of punitive encounters with the state yield more dire consequences for affected groups.

Lastly, our paper contributes to an important and growing literature on Asian-American public opinion and political behavior. Previous work has demonstrated that Japanese Americans have high rates of political participation and leadership, with scholars positing that the group's previous history of internment may have had a galvanizing effect (Wong, Ramakrishnan, Lee and

Junn, 2011). Our findings call this latter explanation into question. Indeed, our findings suggest that those with intimate or secondary contact with internment are, if anything, less likely to be engaged, trustful of the government, and politically participatory. This leaves open several questions that are worthy of further research, including whether internment indirectly had galvanizing effects via the movement for redress in the 1970s and 80s (Takezawa, 1991).

We conclude by noting that our findings have high relevance beyond scholarly discourse. Throughout liberal democracies, governments are increasingly turning to the detention of minority groups as a public policy (Sampson and Mitchell, 2013). This includes state-sponsored detentions of migrants, group-based repatriation, refugee camps, and carceral policies that disproportionately impact members of minority groups. Our study—which suggests that detentions that happen in the modern-day could suppress political engagement among these groups for generations to come—urges caution in this approach.

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8 Appendix

8.1 People of Japanese Descent Living in Hawaii

Our analysis excludes the large number of people of Japanese descent living in Hawaii at the time. We exclude these individuals on substantive grounds, which we further explain in this section.[13]

During the mid-twentieth century, substantial numbers of people of Japanese descent lived in Hawaii, reflecting the close historical contact between Hawaii and Japan. In the late 1930s and early 1940s, this population included approximately 158,000 people of Japanese descent (citizens and non-citizens), accounting for around 37 percent of the territory's population. By contrast, there were around 130,000 people of Japanese descent living in the entire mainland U.S., much fewer in terms of the absolute number and also as a share of the population.

Given the large shares of Japanese and Japanese-Americans in Hawaii, large-scale evacuation and internment would be difficult. Some individuals in the military and in the government nonetheless recommended removing all Issei and their citizen children to the mainland or taking them hostage (Kashima, 2003, p. 68); however, others with deeper knowledge of the situation in Hawaii—such as Delos Emmons, head of Western Defense Command during the war—warned that removing such a large population would prove logistically intractable and leave gaping needs in terms of much needed war-time labor (Kashima, 2003, p. 75).

The latter view eventually won out. Following the attack by the Japanese on Pearl Harbor on December 7, 1941, the entirety of Hawaii was placed under martial law, with control of power transferred from the civilian territorial governor the military head of the Hawaii command. (This lasted some three years, until October of 1944.) The introduction of martial law affected all residents of Hawaii, including citizens and non-citizens, Japanese-Americans and others.

However, there were some detentions and evacuations. For example, several hundred Japanese or Japanese Americans on a special FBI "custodial detention list" were quickly taken into custody.

[13] For more on the differences in the internment of Hawaii-based people of Japanese descent and those living on the mainland, see Kashima (2003, ch. 4).

This included "businesspeople, Japanese consular officials, Japanese language school teachers and principals, Buddhist and Shinto priests, and 'others of no particular affiliation but who by reason of their extreme nationalist sentiments would be a danger to our security as well as others who have seen Japanese military service" (Kashima, 2003, p. 68). There was also additional wartime detention and relocation of some 1,500 Kibei residents—that is, Japanese-Americans living in Hawaii who had been educated in Japan, spoke primarily Japanese, or had other close personal or financial interests in Japan.

The result was that by far smaller numbers and shares of Hawaii's Japanese or Japanese-American population were interned than on the mainland. Kashima (2003, pp. 86–87) summarizes:

> Under this selective policy, an additional 1,217 persons eventually were shipped to WRA relocation camps from November 23, 1942 to July 1945. The largest group, numbering 1,037, arrived between November 1942 and March 14, 1943. [T]he army put all these Hawaii residents in WRA camps; most of them went to Jerome, Arkansas, or Topaz, Utah, but some ended up at Minidoka, Idaho, and Tule Lake, California. [A]dding this count of 1,217 to the 875 predominately male Issei produces a total of 2,092 persons brought to the mainland. Along with the estimated 300 persons who remained in Hawaii, the total number of Hawaii residents of Japanese ancestry were detained in permanent imprisonment facilities comes to 2,392. Compared to the prisoner population of Nikkei on the mainland, this is a very small number. The contrast in treatment between these two groups is stark and revealing.

For our purposes, the number of people interned—around 2,300 in total—represents around 2 percent of the overall Hawaiian Japanese or Japanese American population. By comparison, around 120,000 Japanese Americans—or around 85 percent—of people of Japanese descent living in the mainland were interned. In sum, there was never wholesale evacuation and internment of people of Japanese descent, like there was on the west coast of the United States. For these reasons, we set Hawaii aside in our analyses.

8.2 From Assembly Centers to Internment Camps

Over 90,000 of the 120,000 Japanese Americans in WRA custody during WWII were transferred from assembly centers to internment camps throughout the fall of 1942. Notably, not all Japanese Americans who were interned transferred from assembly centers. Japanese-Americans forced to report to the Owens Valley (10,000) and Parker Dam (12,000) reception centers were not transferred to internment camps because Owens Valley was expanded to become the Manzanar internment camp and Parker Dam became Poston. Over 17,000 internees were taken to internment camps directly from their homes, while 5,918 were born in internment camps, and over 4,500 were transferred to internment camps from Immigration and Naturalization Service (INS) facilities, prisons, and other institutions.[14]

By July 1, 1942, the WRA had completed construction on just three of the ten planned Japanese Internment camps: Manzanar, Poston, and Tule Lake. Manzanar had reached its population capacity by summer 1942, but the other camps began receiving groups of internees virtually as soon as each was operational - and sometimes before they were fully constructed (WRA, 1942). The WRA's objective was to "relocate the evacuated people as quickly as possible in order that their removal from private life and productive work would be brief" (WRA, 1943). Internees were transported to the nearest internment camps accessible by train as soon as those camps were open. In addition to speed, the WRA prioritized keeping families and communities together - the reason for this is that the Army envisioned internment camps as self contained communities where internees would work, farm, worship, run their own schools, and maintain some responsibility over self governance (WRA, 1943). To this end, the Army tried to relocate each assembly center's whole population to a single nearby internment camp in one mass evacuation per assembly center. The WRA only deviated from this when an assembly center's population was too large for any single internment camp to accommodate (see WRA (1943), p. 3).

Internees "boarded trains at the assembly centers and travelled hundreds of miles farther

[14]National Japanese American Memorial Foundation. "Forced Removal and Incarceration." `https://www.njamemorial.org/internment-overview`

inland to the partially completed relocation centers" (WRA, 1942). In interviews and oral histories, internees shared powerful memories of mass transport. "There were guards at the end of each car and the shades were drawn," recalls Kanji Sahara, now 82, of his removal from the Santa Anita assembly center to the internment camp in Jerome, Arkansas. Sahara and his family had reported to their local church under the initial evacuation orders and travelled to Santa Anita in a caravan of "10 or 15 buses;" the family stayed in the horse stables at Santa Anita for six months before being moved to Arkansas (Fares, 2017).

8.3 Internment Status (Table)

Table A.5: Internment Status and Political Engagement

	Dependent variable:			
	Political Interest	Political Distrust	Political Advice	Leadership Approach
	OLS	*OLS*	*OLS*	*OLS*
	(1)	(2)	(3)	(4)
Direct Exposure to Internment	$-.133^{***}$	.043	$-.019$	$.109^{*}$
	(.043)	(.036)	(.027)	(.056)
Family-Only Exposure to Internment	$-.181^{***}$	.022	$-.005$	$.190^{***}$
	(.046)	(.036)	(.027)	(.057)
Generations	Issei, Nisei, and Sansei	Nisei and Sansei	Nisei and Sansei	Nisei and Sansei
Generation Fixed Effects	✓	✓	✓	✓
Pre-Internment Location Fixed Effects	✓	✓	✓	✓
Controls	Age, Gender	Age, Gender	Age, Gender	Age, Gender
Observations	4,101	3,068	3,069	2,982
R^2	.336	.011	.029	.038

Note: *p<0.1; **p<0.05; ***p<0.01

8.4 Formal Tests of Coefficient Equality

Table A.6: Formal Tests of Coefficient Equality (Direct Exposure = Family-Only Exposure)

Model	Residual DF	RSS	df	Sum of Sq.	F-statistic	p
Political Interest	4062.00	1899.90				
Political Interest	4061.00	1899.14	1.00	0.77	1.64	0.20
Political Distrust	3035.00	743.05				
Political Distrust	3034.00	742.92	1.00	0.13	0.54	0.46
Political Advice	3036.00	417.96				
Political Advice	3035.00	417.91	1.00	0.05	0.40	0.53
Leadership Approach	2951.00	1716.64				
Leadership Approach	2950.00	1714.86	1.00	1.78	3.06	0.08

8.5 Internment Length (Table)

Table A.7: Internment Length and Political Engagement

	Dependent variable:			
	Political Interest	Distrust	Political Advice	Leadership Approach
	OLS	*OLS*	*OLS*	*OLS*
	(1)	(2)	(3)	(4)
Internment Length	−.014	.042***	−.034***	.043**
	(.013)	(.011)	(.008)	(.017)
Generations	Issei, Nisei, and Sansei	Nisei and Sansei	Nisei and Sansei	Nisei and Sansei
Generation Fixed Effects	✓	✓	✓	✓
Controls	Age, Gender	Age, Gender	Age, Gender	Age, Gender
Observations	2,690	1,891	1,892	1,858
R^2	.360	.011	.030	.027
Note:				*p<0.1; **p<0.05; ***p<0.01

8.6 Effects of Internment Experience (Table)

Table A.8: Effects of Experiencing Violence while Interned

	Dependent variable:			
	Political Interest	Political Distrust	Political Advice	Leadership Approach
	(1)	(2)	(3)	(4)
Violence	−.064	.097***	−.016	.084
	(.059)	(.022)	(.038)	(.051)
Interned	−.011	.034	−.021	−.079*
	(.063)	(.032)	(.031)	(.047)
Gender (Male)	.135***	−.014	.076***	−.135***
	(.026)	(.016)	(.021)	(.019)
Age	−.001	−.001	−.004**	.009***
	(.003)	(.001)	(.002)	(.002)
Violence x Interned	−.012	−.078***	−.004	−.006
	(.072)	(.027)	(.044)	(.060)
Generations	Issei, Nisei, Sansei	Nisei and Sansei	Nisei and Sansei	Nisei and Sansei
Generation Fixed Effects	✓	✓	✓	✓
Pre-Internment Location Fixed Effects	✓	✓	✓	✓
Observations	3,188	2,392	2,396	2,334
R^2	.314	.009	.029	.027

Note: *p<0.1; **p<0.05; ***p<0.01

Note: Standard errors clustered by internment camp to which respondents are assigned.

Table A.9: Effects of Experiencing Strikes while Interned

	Dependent variable:			
	Political Interest	Political Distrust	Political Advice	Leadership Approach
	(1)	(2)	(3)	(4)
Strikes	−.087	.086***	−.039	.035
	(.055)	(.026)	(.045)	(.048)
Interned	−.032	.016	−.029	−.095*
	(.056)	(.037)	(.035)	(.050)
Gender (Male)	.133***	−.014	.076***	−.133***
	(.026)	(.017)	(.021)	(.019)
Age	−.001	−.001	−.004**	.009***
	(.003)	(.001)	(.002)	(.002)
Strikes x Interned	.038	−.010	.015	.045
	(.079)	(.024)	(.049)	(.046)
Generations	Issei, Nisei, Sansei	Nisei and Sansei	Nisei and Sansei	Nisei and Sansei
Generation Fixed Effects	✓	✓	✓	✓
Pre-Internment Location Fixed Effects	✓	✓	✓	✓
Observations	3,188	2,392	2,396	2,334
R^2	.314	.013	.030	.027

Note: *p<0.1; **p<0.05; ***p<0.01

Note: Standard errors clustered by internment camp to which respondents are assigned.

Table A.10: Effects of Experiencing Use of Force while Interned

	Dependent variable:			
	Political Interest	Political Distrust	Political Advice	Leadership Approach
	(1)	(2)	(3)	(4)
Force	.061	−.018	.066	.010
	(.059)	(.054)	(.041)	(.080)
Interned	−.001	.003	−.002	−.078
	(.056)	(.032)	(.035)	(.049)
Gender (Male)	.133***	−.013	.076***	−.132***
	(.026)	(.018)	(.021)	(.019)
Age	−.001	−.001	−.004**	.009***
	(.003)	(.001)	(.002)	(.002)
Force x Interned	−.067	.042	−.086*	.006
	(.108)	(.047)	(.047)	(.071)
Generations	Issei, Nisei, Sansei	Nisei and Sansei	Nisei and Sansei	Nisei and Sansei
Generation Fixed Effects	✓	✓	✓	✓
Pre-Internment Location Fixed Effects	✓	✓	✓	✓
Observations	3,188	2,392	2,396	2,334
R^2	.313	.008	.030	.025

Note: *p<0.1; **p<0.05; ***p<0.01

Note: Standard errors clustered by internment camp to which respondents are assigned.

Table A.11: Effects of Militarized Camp Environment

	Dependent variable:			
	Political Interest	Political Distrust	Political Advice	Leadership Approach
	(1)	(2)	(3)	(4)
Militarism	.116***	−.064	.038	−.025
	(.043)	(.050)	(.035)	(.048)
Interned	−.001	−.012	−.016	−.115
	(.038)	(.032)	(.032)	(.089)
Gender (Male)	.135***	−.010	.077***	−.135***
	(.027)	(.017)	(.021)	(.019)
Age	−.001	−.001	−.004***	.009***
	(.003)	(.002)	(.002)	(.002)
Militarism x Interned	−.033	.036	−.011	.021
	(.071)	(.023)	(.046)	(.082)
Generations	Issei, Nisei, Sansei	Nisei and Sansei	Nisei and Sansei	Nisei and Sansei
Generation Fixed Effects	✓	✓	✓	✓
Pre-Internment Location Fixed Effects	✓	✓	✓	✓
Observations	3,135	2,357	2,360	2,299
R^2	.314	.009	.030	.028

Note: *p<0.1; **p<0.05; ***p<0.01

Note: Standard errors clustered by internment camp to which respondents are assigned.

8.7 Balance Test Results

8.7.1 K-S Tests

Plots of age, gender, and pre-internment occupations for internees held at different camps (displayed in Figures A.9 and A.10 and Table A.14) reveal no significant differences in these distribu-

tions across different internment locations. We further conduct a series of Kolmogorov-Smirnov (KS) tests of the null hypothesis that observed age and sector of employment for internees imprisoned across different camps are draws from the same distribution broadly support the idea that internees are comparable across these dimensions. Note that, since our objective is to investigate whether or not the groups of internees physically sent to various internment camps are in fact similar across a variety of observable characteristics, all of the analyses in this section condition on respondents who report having been interned themselves. Respondents who were not interned are not included in this analysis.

There are 45 possible pairs of the 10 major camps to which Japanese Americans were assigned during the internment period. For age, our KS test results reject the null hypothesis of a single parent distribution in two of 45 cases (internees in Topaz vs. Manzanar and Topaz vs. Jerome). This is with the defined Type I error rate of 0.05 for these tests. For employment sector, we treat the 3-digit designations for occupational sector as a continuous covariate and test the distributions of these sectors for Nisei respondents across camps. Here, we reject the null hypothesis of a single distribution at the 5% level in none of the 45 cases. Full results for these tests are presented in Tables A.12 and A.13. A series of t-tests for standardized differences in proportions never reject the null hypothesis that gender balance is nearly identical across all 45 possible pairs of internment camps at the 5% level.

The following figures and tables summarize balance in the distributions of age, gender, and employment sector among internees imprisoned across all 45 possible unique pairs of internment camps.

Table A.12: Kolmogorov-Smirnov Test Results for Age

Camp 1	Camp 2	p-value
Rivers, AZ	Poston, AZ	0.53
Amache, CO	Poston, AZ	0.66
Denson, AR	Poston, AZ	0.43
Manzanar, CA	Poston, AZ	0.26
McGehee, AR	Poston, AZ	0.66
Heart Mountain, WY	Poston, AZ	0.87
Hunt, ID	Poston, AZ	0.63
Newell, CA	Poston, AZ	0.78
Topaz, UT	Poston, AZ	0.08
Amache, CO	Rivers, AZ	0.52
Denson, AR	Rivers, AZ	0.77
Manzanar, CA	Rivers, AZ	1.00
McGehee, AR	Rivers, AZ	0.74
Heart Mountain, WY	Rivers, AZ	0.64
Hunt, ID	Rivers, AZ	0.64
Newell, CA	Rivers, AZ	0.87
Topaz, UT	Rivers, AZ	0.06
Denson, AR	Amache, CO	0.14
Manzanar, CA	Amache, CO	0.25
McGehee, AR	Amache, CO	0.72
Heart Mountain, WY	Amache, CO	1.00
Hunt, ID	Amache, CO	0.90
Newell, CA	Amache, CO	0.53
Topaz, UT	Amache, CO	0.52
Manzanar, CA	Denson, AR	0.81
McGehee, AR	Denson, AR	0.44
Heart Mountain, WY	Denson, AR	0.17
Hunt, ID	Denson, AR	0.16
Newell, CA	Denson, AR	0.46
Topaz, UT	Denson, AR	0.04
McGehee, AR	Manzanar, CA	0.61
Heart Mountain, WY	Manzanar, CA	0.37
Hunt, ID	Manzanar, CA	0.31
Newell, CA	Manzanar, CA	0.44
Topaz, UT	Manzanar, CA	0.05
Heart Mountain, WY	McGehee, AR	0.82
Hunt, ID	McGehee, AR	0.68
Newell, CA	McGehee, AR	0.49
Topaz, UT	McGehee, AR	0.22
Hunt, ID	Heart Mountain, WY	1.00
Newell, CA	Heart Mountain, WY	0.56
Topaz, UT	Heart Mountain, WY	0.15
Newell, CA	Hunt, ID	0.92
Topaz, UT	Hunt, ID	0.14
Topaz, UT	Newell, CA	0.06

Table A.13: Kolmogorov-Smirnov Test for Employment Sector

Camp 1	Camp 2	p-value
Rivers, AZ	Poston, AZ	0.09
Amache, CO	Poston, AZ	0.10
Denson, AR	Poston, AZ	0.24
Manzanar, CA	Poston, AZ	0.22
McGehee, AR	Poston, AZ	0.98
Heart Mountain, WY	Poston, AZ	0.15
Hunt, ID	Poston, AZ	0.10
Newell, CA	Poston, AZ	0.97
Topaz, UT	Poston, AZ	0.62
Amache, CO	Rivers, AZ	0.93
Denson, AR	Rivers, AZ	1.00
Manzanar, CA	Rivers, AZ	0.68
McGehee, AR	Rivers, AZ	0.46
Heart Mountain, WY	Rivers, AZ	1.00
Hunt, ID	Rivers, AZ	1.00
Newell, CA	Rivers, AZ	0.19
Topaz, UT	Rivers, AZ	0.09
Denson, AR	Amache, CO	1.00
Manzanar, CA	Amache, CO	1.00
McGehee, AR	Amache, CO	0.68
Heart Mountain, WY	Amache, CO	1.00
Hunt, ID	Amache, CO	1.00
Newell, CA	Amache, CO	0.12
Topaz, UT	Amache, CO	0.24
Manzanar, CA	Denson, AR	0.98
McGehee, AR	Denson, AR	0.73
Heart Mountain, WY	Denson, AR	1.00
Hunt, ID	Denson, AR	1.00
Newell, CA	Denson, AR	0.20
Topaz, UT	Denson, AR	0.52
McGehee, AR	Manzanar, CA	0.94
Heart Mountain, WY	Manzanar, CA	0.97
Hunt, ID	Manzanar, CA	0.96
Newell, CA	Manzanar, CA	0.25
Topaz, UT	Manzanar, CA	0.67
Heart Mountain, WY	McGehee, AR	0.53
Hunt, ID	McGehee, AR	0.66
Newell, CA	McGehee, AR	0.93
Topaz, UT	McGehee, AR	1.00
Hunt, ID	Heart Mountain, WY	1
Newell, CA	Heart Mountain, WY	0.15
Topaz, UT	Heart Mountain, WY	0.11
Newell, CA	Hunt, ID	0.12
Topaz, UT	Hunt, ID	0.10
Topaz, UT	Newell, CA	0.41

Table A.14: T-Tests for Differences in Gender Proportion by Camp

Camp 1	Camp 2	Difference in Means	Standard Error	T	p-value	Lower Bound	Upper Bound
Rivers, AZ	Poston, AZ	0.03	7.11	0.004	1.00	-13.99	14.00
Amache, CO	Poston, AZ	0.01	7.10	0.001	1.00	-13.97	13.97
Denson, AR	Poston, AZ	-0.07	5.24	-0.01	0.99	-10.37	10.35
Manzanar, CA	Poston, AZ	-0.06	6.83	-0.01	0.99	-13.45	13.43
McGehee, AR	Poston, AZ	-0.01	6.18	-0.001	1.00	-12.19	12.18
Heart Mountain, WY	Poston, AZ	0.03	7.35	0.003	1.00	-14.45	14.46
Hunt, ID	Poston, AZ	-0.01	7.58	-0.001	1.00	-14.91	14.90
Newell, CA	Poston, AZ	-0.02	6.95	-0.003	1.00	-13.68	13.67
Topaz, UT	Poston, AZ	0.01	6.60	0.002	1.00	-12.99	12.99
Amache, CO	Rivers, AZ	-0.02	6.49	-0.004	1.00	-12.76	12.76
Denson, AR	Rivers, AZ	-0.10	4.99	-0.02	0.98	-9.88	9.84
Manzanar, CA	Rivers, AZ	-0.09	6.27	-0.01	0.99	-12.36	12.33
McGehee, AR	Rivers, AZ	-0.04	5.77	-0.01	1.00	-11.38	11.37
Heart Mountain, WY	Rivers, AZ	-0.004	6.67	-0.001	1.00	-13.13	13.13
Hunt, ID	Rivers, AZ	-0.04	6.84	-0.01	1.00	-13.47	13.46
Newell, CA	Rivers, AZ	-0.05	6.37	-0.01	0.99	-12.55	12.53
Topaz, UT	Rivers, AZ	-0.02	6.10	-0.003	1.00	-12.01	12.00
Denson, AR	Amache, CO	-0.08	4.98	-0.02	0.99	-9.86	9.83
Manzanar, CA	Amache, CO	-0.07	6.26	-0.01	0.99	-12.33	12.31
McGehee, AR	Amache, CO	-0.01	5.76	-0.002	1.00	-11.35	11.35
Heart Mountain, WY	Amache, CO	0.02	6.66	0.003	1.00	-13.11	13.11
Hunt, ID	Amache, CO	-0.01	6.83	-0.002	1.00	-13.44	13.44
Newell, CA	Amache, CO	-0.03	6.36	-0.004	1.00	-12.52	12.51
Topaz, UT	Amache, CO	0.01	6.09	0.001	1.00	-11.99	11.99
Manzanar, CA	Denson, AR	0.01	4.83	0.002	1.00	-9.55	9.55
McGehee, AR	Denson, AR	0.06	4.62	0.01	0.99	-9.12	9.14
Heart Mountain, WY	Denson, AR	0.10	5.07	0.02	0.99	-10.00	10.04
Hunt, ID	Denson, AR	0.06	5.13	0.01	0.99	-10.14	10.16
Newell, CA	Denson, AR	0.05	4.91	0.01	0.99	-9.70	9.72
Topaz, UT	Denson, AR	0.08	4.80	0.02	0.99	-9.48	9.51
McGehee, AR	Manzanar, CA	0.06	5.59	0.01	0.99	-11.00	11.02
Heart Mountain, WY	Manzanar, CA	0.09	6.44	0.01	0.99	-12.66	12.68
Hunt, ID	Manzanar, CA	0.05	6.59	0.01	0.99	-12.95	12.97
Newell, CA	Manzanar, CA	0.04	6.14	0.01	0.99	-12.08	12.09
Topaz, UT	Manzanar, CA	0.07	5.91	0.01	0.99	-11.62	11.65
Heart Mountain, WY	McGehee, AR	0.03	5.89	0.01	1.00	-11.61	11.62
Hunt, ID	McGehee, AR	-0.002	6.01	-0.0004	1.00	-11.84	11.84
Newell, CA	McGehee, AR	-0.02	5.67	-0.003	1.00	-11.18	11.18
Topaz, UT	McGehee, AR	0.02	5.49	0.004	1.00	-10.81	10.82
Hunt, ID	Heart Mountain, WY	-0.03	7.05	-0.005	1.00	-13.88	13.87
Newell, CA	Heart Mountain, WY	-0.05	6.54	-0.01	0.99	-12.88	12.87
Topaz, UT	Heart Mountain, WY	-0.01	6.24	-0.002	1.00	-12.30	12.29
Newell, CA	Hunt, ID	-0.01	6.70	-0.002	1.00	-13.18	13.18
Topaz, UT	Hunt, ID	0.02	6.38	0.003	1.00	-12.56	12.57
Topaz, UT	Newell, CA	0.04	5.99	0.01	1.00	-11.80	11.81

The following figures and tables summarize balance in the distributions of age, and employment sector among internees imprisoned in the largest internment camps in our data.

Figure A.9: Age Distribution for Japanese Internees by Camp

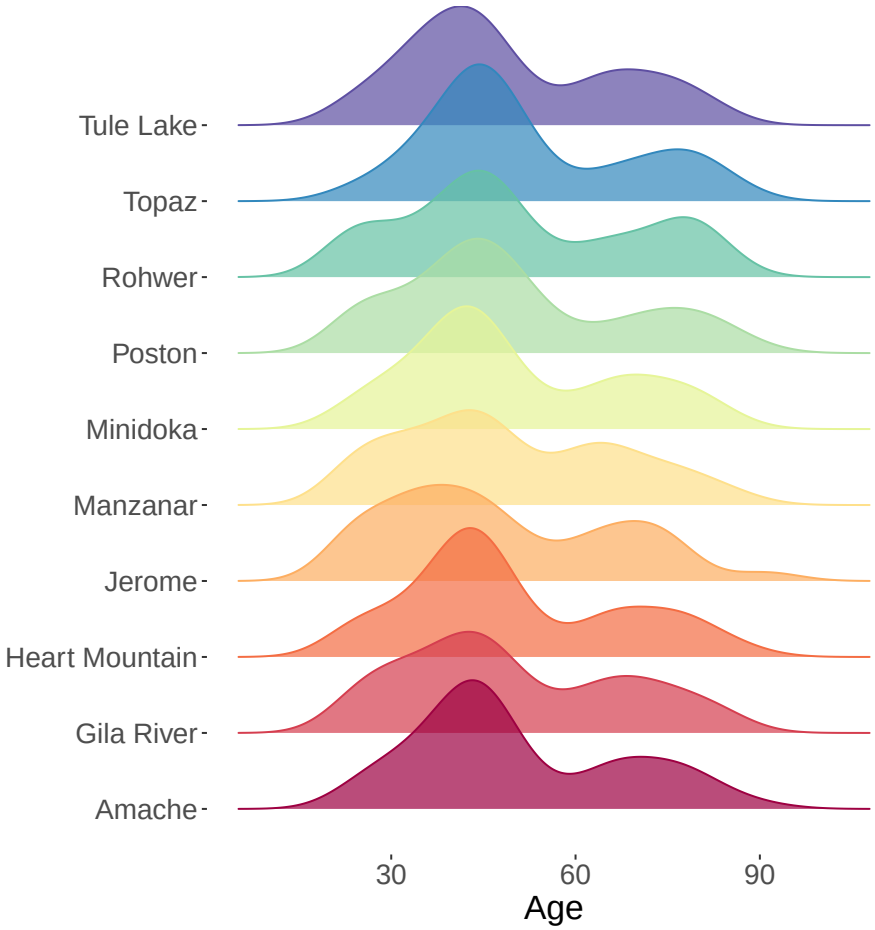
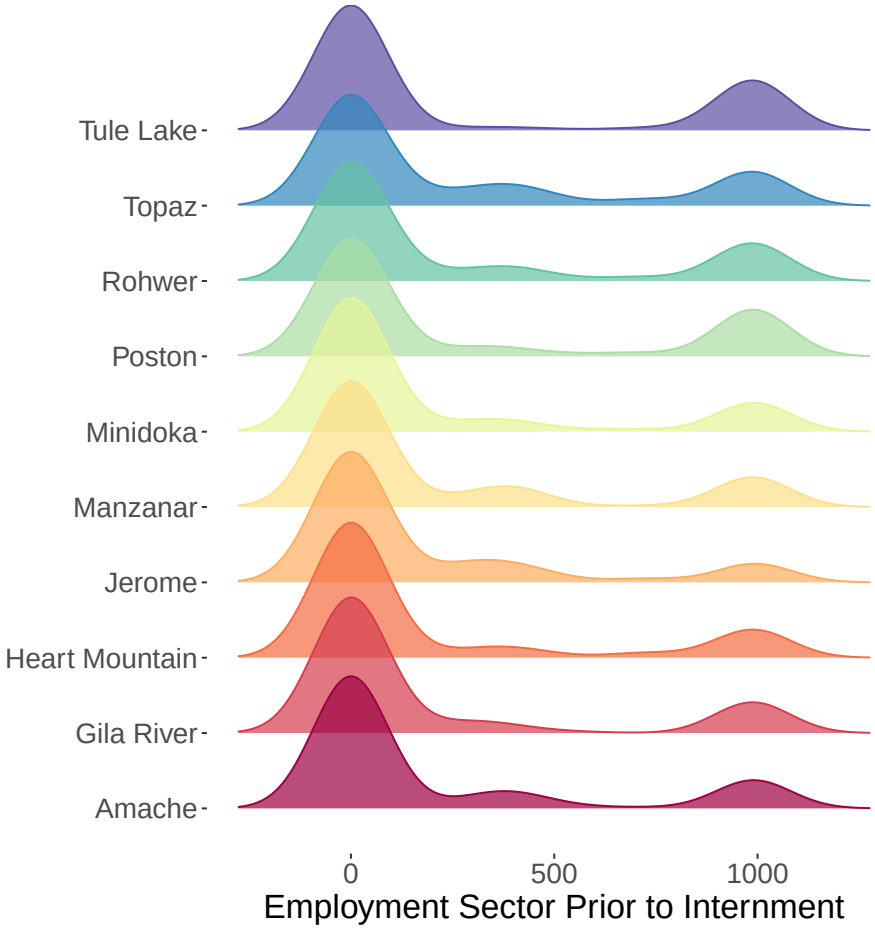


Figure A.10: Distribution of Pre-Internment Occupational Sectors by Camp



8.7.2 Comparing Japanese Americans Living In- and Outside of the Exclusion Zone in 1940 Using the Census

One important limitation of the JARP survey data is that it allows researchers only a narrow glimpse at respondent level characteristics assigned *before* respondents were interned (or not). We control for gender, age, and pre-internment locations throughout this study, but these controls still allow for the possibility that Japanese Americans who were interned still differ from Japanese Americans who were not along some important covariate. Because social scientists typically construe an individual's level of political engagement as a function of socioeconomic status (Verba, Schlozman and Brady, 1995), we might be particularly worried that internees and non-internees differ along other socioeconomic covariates such as education, marital status, or specific occupation. If the population of Japanese Americans living in the Exclusion Zones on the eve of the internment period was already less educated, less wealthy or less likely to be married, skeptical researchers might argue that this population would be less politically engaged than its counterpart further inland *regardless* of the internment experience.

In the analyses that follow, we use data from the 1940 U.S. Census to demonstrate that this is unlikely to be the case; Japanese Americans living in- and outside of the Exclusion Zone are comparable along a variety of important socioeconomic dimensions. These analyses rely on the 1940 Census summary report entitled "Characteristics of the Nonwhite Population by Race."[15] This summary report provides distributions of age, gender, marital status, education, employment status, and employment type for Japanese Americans living in California, Oregon, Washington State (inside the Exclusion Zone), Colorado, New York, and Utah (outside the Exclusion Zone). We code Washington and Oregon as states within the Exclusion Zone because, while internment orders did not cover the entirety of both states, most Japanese Americans living in these states would have lived close to the coast and in the Exclusion Zone. The Census summarizes data for these states because each contains a Japanese-American population of at least 2,000 people. While this data does not represent the totality of Japanese Americans living in the United States in 1940,

[15] United States Department of Commerce. Sixteenth Census of the United States. 1940. Tables 33-38.

these six states represent 119,835 of the 126,947 Japanese Americans living in the United States in 1940. We present summary statistics for Japanese Americans by state in Table A.15.

Table A.15: Japanese Americans by State, 1940

State	Exclusion Zone	Population	Prop. Male	Prop. Married	Prop. Under 20	Pop. Completed HS	Median Yrs. Ed.	Prop. Employed	Prop. Professional	Prop. Farmers
CA	1	93,717	56.07	49.84	25.49	22.83	8.60	55.75	2.38	14.34
CO	0	2,734	55.05	42.23	31.49	15.93	8.10	44.57	2.84	67.92
NY	0	2,538	70.17	50.83	17.14	21.72	12.30	63.93	6.64	0.59
OR	1	4,071	55.78	50.08	23.63	22.48	8.50	54.91	2.54	47.91
UT	0	2,210	57.15	44.45	29.05	17.57	8.10	47.72	2.04	25.95
WA	1	14,565	55.56	47.49	23.08	28.49	9	56.36	2.32	12.92

These summary statistics reveal no major differences between Japanese Americans living inside the Exclusion Zone and outside of the Exclusion Zone. Table A.16 displays the results of a more formal test of these differences in the form of a set of two sided hypothesis tests for the differences in means across a broad set of covariates that might influence downstream political behavior. The lowest p-value corresponding to any of these tests is 0.33, which suggests that Japanese Americans living in- and outside of the Exclusion Zone are not significantly different in terms of gender balance, marital status, age, education, or employment at the 95% level.

Table A.16: p Values from Two-Sided Difference-in-Means Tests on Exclusion Zone Boundary in 1940

Variable	p
Prop. Male	0.40
Prop. Married	0.33
Prop. Under 20	0.72
Prop. Completed HS	0.81
Median Yrs. Ed.	0.63
Prop. Employed	0.61
Prop. Professional	0.42
Prop. Farmers	0.79

8.8 Assignment to Places with Particular Political or Demographic Characteristics

One alternative pathway through which internment may have affected political outcomes for internees might have involved the political or demographic characteristics of the jurisdictions that housed internment camps. In a period when wartime uncertainty and racial anti-Japanese sentiment were at an all-time high in the mass public, skeptical readers might worry that Japanese-Americans interned in particularly white areas or areas that showed broad support for Franklin Roosevelt might have experienced more hostility. In short, the climate outside of the camps might have conveyed more of the effects we report than the climate within the camps themselves. Below, we provide evidence that this was unlikely to have been the case. First, qualitative accounts suggest that Japanese-Americans interned throughout the United States had extremely limited contact with the outside world. Internees would have little information about the surrounding people living in areas with even extremely distinct political or demographic characteristics. To rule out this possibility, we included indicators for Franklin D. Roosevelt's 1940 share of the two party vote and for the percentage white in the county of internment hosting each internment camp. These covariates themselves are rarely significant. The percentage white in each county of internment is only significantly associated with level of political distrust when we consider one type of camp experience (experiencing violence). While presidential vote share in 1940 seems to consistently and significantly influence political communication and views on leadership approach, these values are substantively small. More importantly, including these covariates does not change our substantive conclusions about the effects of experiencing violence or strikes while interned.

Table A.17: Effects of Experiencing Violence while Interned: Controlling for Surrounding Camp Environment

	Dependent variable:			
	Political Interest	Political Distrust	Political Advice	Leadership Approach
	(1)	(2)	(3)	(4)
Violence	−.057	.084***	−.022	.052
	(.063)	(.029)	(.035)	(.035)
Interned	−.010	.032	−.022	−.082*
	(.063)	(.033)	(.032)	(.047)
Gender (Male)	−.001***	.001	−.0004	.0003
	(.001)	(.001)	(.0003)	(.001)
Age	−.081*	.114	.033	.221***
	(.045)	(.113)	(.022)	(.066)
Violence x Interned	.135***	−.015	.075***	−.137***
	(.026)	(.016)	(.020)	(.020)
FDR 1940 Vote Share	−.001	−.0005	−.004**	.009***
	(.003)	(.002)	(.002)	(.002)
Pct. White 1940	−.012	−.077***	−.004	−.007
	(.070)	(.027)	(.044)	(.063)
Generations	Issei, Nisei, Sansei	Nisei and Sansei	Nisei and Sansei	Nisei and Sansei
Generation Fixed Effects	✓	✓	✓	✓
Pre-Internment Location Fixed Effects	✓	✓	✓	✓
Observations	3,188	2,392	2,396	2,334
R^2	.314	.011	.030	.030

Note: *p<0.1; **p<0.05; ***p<0.01

Note: Standard errors clustered by internment camp to which respondents are assigned.

Table A.18: Effects of Experiencing Strikes while Interned: Controlling for Surrounding Camp Environment

	Dependent variable:			
	Political Interest	Political Distrust	Political Advice	Leadership Approach
	(1)	(2)	(3)	(4)
Strikes	−.072	.079***	−.043	.020
	(.060)	(.028)	(.040)	(.037)
Interned	−.030	.014	−.030	−.103**
	(.055)	(.037)	(.035)	(.051)
Gender (Male)	−.001	.0001	.00004	−.0004
	(.001)	(.001)	(.001)	(.001)
Age	−.097*	.070	.041	.210***
	(.058)	(.061)	(.042)	(.047)
Strikes x Interned	.134***	−.015	.075***	−.136***
	(.026)	(.017)	(.020)	(.020)
FDR 1940 Vote Share	−.001	−.001	−.004**	.009***
	(.002)	(.001)	(.002)	(.002)
Pct. White 1940	.036	−.007	.017	.053
	(.076)	(.024)	(.048)	(.047)
Generations	Issei, Nisei, Sansei	Nisei and Sansei	Nisei and Sansei	Nisei and Sansei
Generation Fixed Effects	✓	✓	✓	✓
Pre-Internment Location Fixed Effects	✓	✓	✓	✓
Observations	3,188	2,392	2,396	2,334
R^2	.314	.014	.030	.030

Note: *p<0.1; **p<0.05; ***p<0.01

Note: Standard errors clustered by internment camp to which respondents are assigned.

Table A.19: Effects of Experiencing Use of Force while Interned: Controlling for Surrounding Camp Environment

	Dependent variable:			
	Political Interest	Political Distrust	Political Advice	Leadership Approach
	(1)	(2)	(3)	(4)
Force	.164*	−.089**	.068	−.108**
	(.093)	(.035)	(.045)	(.047)
Interned	.002	.0002	−.002	−.084*
	(.056)	(.032)	(.035)	(.050)
Gender (Male)	−.004***	.002***	−.0005	.002**
	(.001)	(.001)	(.001)	(.001)
Age	−.344***	.245**	.007	.463***
	(.068)	(.098)	(.065)	(.115)
Force x Interned	.135***	−.016	.076***	−.138***
	(.026)	(.017)	(.021)	(.020)
FDR 1940 Vote Share	−.001	−.0004	−.004**	.010***
	(.003)	(.002)	(.002)	(.002)
Pct. White 1940	−.061	.038	−.087*	−.004
	(.113)	(.045)	(.047)	(.069)
Generations	Issei, Nisei, Sansei	Nisei and Sansei	Nisei and Sansei	Nisei and Sansei
Generation Fixed Effects	✓	✓	✓	✓
Pre-Internment Location Fixed Effects	✓	✓	✓	✓
Observations	3,188	2,392	2,396	2,334
R^2	.315	.011	.031	.031

Note: *p<0.1; **p<0.05; ***p<0.01

Note: Standard errors clustered by internment camp to which respondents are assigned.

Table A.20: Effects of Militarized Camp Environment: Controlling for Surrounding Camp Environment

	Dependent variable:			
	Political Interest	Political Distrust	Political Advice	Leadership Approach
	(1)	(2)	(3)	(4)
Militarism	.118**	−.066	.038	−.027
	(.053)	(.042)	(.034)	(.032)
Interned	−.004	−.009	−.016	−.113
	(.038)	(.029)	(.033)	(.091)
Gender (Male)	−.002***	.001	−.001***	.0001
	(.0002)	(.001)	(.0001)	(.001)
Age	−.174***	.148	−.006	.248***
	(.030)	(.095)	(.021)	(.039)
Militarism x Interned	.136***	−.012	.077***	−.139***
	(.027)	(.017)	(.021)	(.020)
FDR 1940 Vote Share	−.001	−.001	−.004**	.010***
	(.003)	(.002)	(.002)	(.002)
Pct. White 1940	−.023	.028	−.011	.008
	(.069)	(.021)	(.046)	(.083)
Generations	Issei, Nisei, Sansei	Nisei and Sansei	Nisei and Sansei	Nisei and Sansei
Generation Fixed Effects	✓	✓	✓	✓
Pre-Internment Location Fixed Effects	✓	✓	✓	✓
Observations	3,135	2,357	2,360	2,299
R^2	.315	.011	.031	.032

Note: *p<0.1; **p<0.05; ***p<0.01

Note: Standard errors clustered by internment camp to which respondents are assigned.

8.9 Recording of Pre-Internment Location

One important limitation for the JARP data is the fact that pre-internment location, in most cases outside of California, is recorded at the state level.[16] While it is plausible to assume that people living in a single city or county area would have had similar probabilities of camp assignment, it is less clear that this assumption might hold for everyone living in a given state.

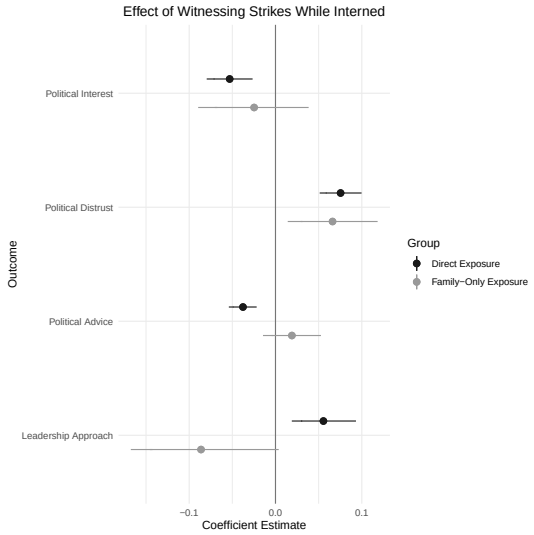
To address this issue, we examine individual internee records provided by the WRA. We use this data to simulate more specific pre-internment locations for JARP respondents. We took the following approach. First, we used the WRA data to calculate the empirical probabilities of having lived in specific areas within a state. For instance, according to the WRA records 6 Japanese Americans interned after 1941 had last-known addresses in Colorado before they were relocated: 4 in Rocky Falls, 1 in Fort Upton, and 1 in Greeley. For JARP respondents living in a given state, we drew a specific location from a multinomial distribution using these empirical probabilities at the family level. In our example, every member of a single family unit living in the state of Colorado would have a 67% probability of being assigned to Rocky Falls and 16% probabilities of being assigned to Fort Upton or Greely, respectively. Not all family members in the JARP shared a pre-internment location, but we assumed that family members who did share a pre-internment code in the JARP survey would share a specific location. This means that we assumed all members of a given family living in Washington state in the JARP data might be assigned to Seattle if this was the specific location drawn for their family ID (but members of the same family living in California would all get a different, single assigned location in California)[17].

We assigned every respondent in the JARP who was interned to a more specific pre-internment location using this procedure and re-estimated the effects of internment experience using the

[16]For California, JARP provides separate designations for people who lived in the state's largest Metropolitan Statistical Areas ("MSA") from people who lived in "rural clusters" throughout the state. These designations provide little additional information relative to the statewide code because they group jurisdictions across geographic areas. San Francisco and Los Angeles, for instance, are coded using the same value because both are large MSAs

[17]Specific location assignments for California respondents are generated naïvely, meaning that we use the raw empirical probabilities of living in any WRA-designated sub-area of California instead of mapping between the JARP's regional California codes and the WRA's. The reason for this is that the lists of locations within California do not match exactly between the WRA and the JARP, and it is unclear how to assign specific locations to the "other California" category listed in the JARP

Figure A.11: Effect of Witnessing Strikes While Interned Using WRA Location Data



same approach described in Section 6. We repeated this procedure 1,000 times to get a distribution of effect sizes under a variety of assumed pre-internment locations for JARP survey respondents.

Figures A.11 through A.14 display the results of these simulations. The means of each plotted distribution can be interpreted as the average effect of witnessing violence, force, strikes, or heavily militarized camp conditions across 10,000 possible distributions of internees in the JARP across specific pre-internment locations, where distributions across locations are based on the real places in which Japanese Americans lived on the eve of internment as recorded by the WRA. We compare the coefficients and standard errors (for these results, standard errors are the standard deviations of the distribution of coefficients generated for each of 10,000 simulations) for these simulations to our original results. The simulation produces very similar point estimates for the effects of various camp characteristics even under different assumed pre-internment locations, which provides support for plausible exogeneity of camp assignment.

Figure A.12: Effect of Witnessing Violence While Interned Using WRA Location Data

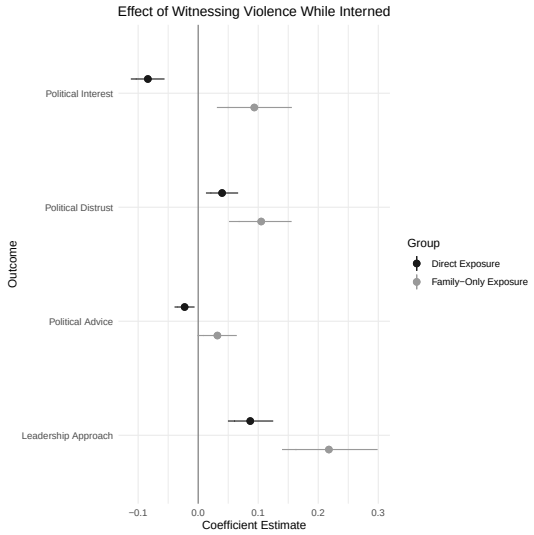


Figure A.13: Effect of Witnessing Use of Force While Interned Using WRA Location Data

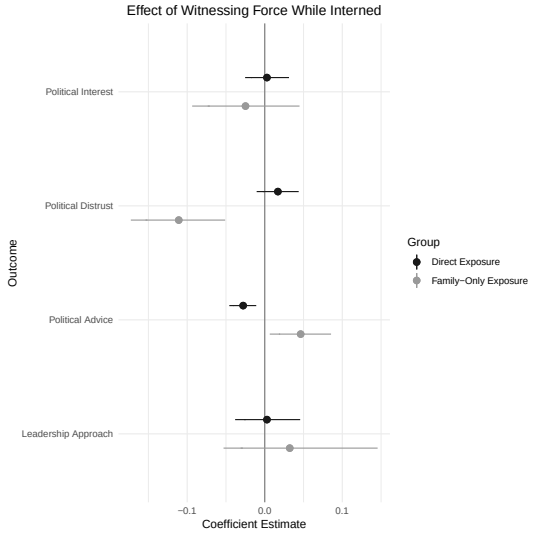
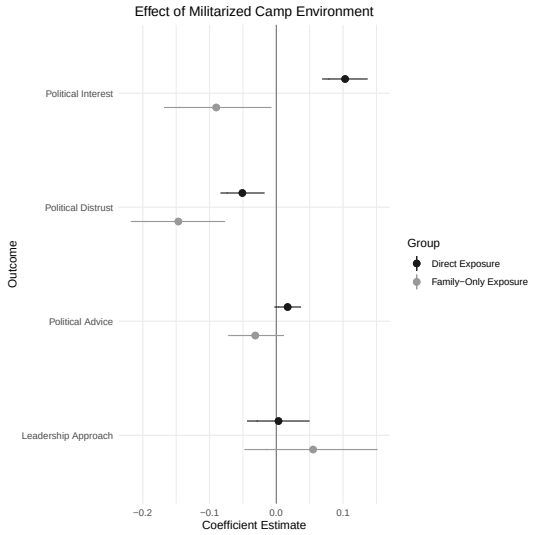


Figure A.14: Effect of Militarized Camp Environment using WRA Location Data



9 Additional Robustness Checks

9.1 Military Service

Nisei and Sansei respondents were asked whether they or their spouses had ever served in the U.S. armed forces. There is some missingness in this variable: 179 of 2,304 Nisei have no recorded response for military service, as do 347 of 802 Sansei. Our results do not suggest that specific features of internment camps produced meaningful differences in Japanese-American military service. Similarly, regressing this indicator for military service on measures of internment exposure suggests internment did not produce differences in rates of military service among interned and non-interned Japanese Americans. Moreover, the coefficient estimates for each group move in opposite directions, which cannot explain the directionally consistent estimates among both groups that we observe throughout the paper. This provides additional evidence for the fact that, prior to internment, Japanese Americans who were interned were similar to Japanese Americans who were not. One potential threat to inference might reside in the possibility that a latent form of patriotism may have both led some Japanese Americans to voluntarily relocate away from the

Table A.21: Internment Status and Post-Treatment Military Service

	Dependent variable:
	Military Service
	OLS
Direct Exposure to Internment	−.042
	(.028)
Family-Only Exposure to Internment	.051
	(.034)
Baseline	.613***
	(.025)
Observations	2,571
R^2	.005
Note:	*p<0.1; **p<0.05; ***p<0.01

coasts (thereby avoiding internment) and to serve in the military later. This does not appear to be the case in our data.

9.2 Income

Another possible threat to inference is the possibility that Japanese-Americans who were not interned had higher (or lower) earning potential than Japanese Americans who were. If so, that potential is likely to have translated into higher family income after internment. In Table A.22, we show that there are differences in income among those who personally and indirectly experienced internment, and these differences are statistically significant at the .05 level. However, these estimates run in the opposite direction, and thus, cannot explain the directionally consistent estimates that we observe for both groups in our models. Moreover, our analysis using 1940 Census data – which examined balance on *pre-treatment* socioeconomic characteristics and is presented in Table A.15 – suggests that those who lived within the exclusion zone and outside of it were comparable in terms of socioeconomic characteristics.

Table A.22: Internment Status and Post-Treatment Income

	Dependent variable:
	Post-Treatment Income
	OLS
Direct Exposure to Internment	$.605^{***}$
	(.114)
Family-Only Exposure to Internment	-1.136^{***}
	(.139)
Baseline	4.412^{***}
	(.104)
Observations	3,891
R^2	.068
Note:	*p<0.1; **p<0.05; ***p<0.01

9.3 Returning Home

Our data makes it difficult to identify how many people returned home after internment. The WRA data does not provide post-camp locations for internees. While the JARP does contain specific information on the places Japanese-American internees returned to after WWII, their pre-camp locations are recorded with less precision. Accordingly, all we can say is whether or not Issei (the only generation for whom post-camp location is recorded) returned home to the same state and sub-state area after internment. If the 849 Issei for whom post-camp location is recorded, 425, or just over 50%, returned to the same area within their state.

9.4 Selection into Internment Experience

The U.S. Army's records detailing the procedure by which Japanese-Americans were assigned and transferred to internment camps suggests that internees were not assigned to camp destinations based on their social, economic, or political characteristics. Still, one potential source of concern might be how responses to internment and interrogation among internees ultimately affected their experience *once they were assigned* to internment camps. For instance, readers might

worry that internees who resisted internment might have been assigned to more punitive or more militarized facilities. One prominent example of this includes the WRA's loyalty questionnaire - launched in the winter of 1943. Men over the age of 17 who were still incarcerated in WRA camps were asked to fill out a questionnaire that asked them whether they would comply with selective service if they were drafted into the U.S. military and whether they could pledge their unqualified allegiance to the United States. Approximately 12,000 of 78,000 respondents to this survey answered "no" to both questions, and were sequestered at the Tule Lake camp for "disloyalty" as a result. To address possible selection issues like this, we replicated our analysis with a restriction just to respondents under the age of 17. We do this because respondents under 17 at the time of internment (we set this to 1945, the final year of WWII) could not have taken the survey or been in the group of respondents who expressed dissatisfaction at the government's action in this way. Similarly, younger respondents would have been unlikely sources of resistance or punishment for it. Figures A.15 through A.18 display results for this population, numbering 1,173 to 1,192 respondents. Variation in observations is a result of missingness. These results are statistically and substantively consistent with the internment experience effects we report in the main body of this paper.

Figure A.15: Effect of Witnessing Strikes While Interned for Internees Younger than 17 in 1945

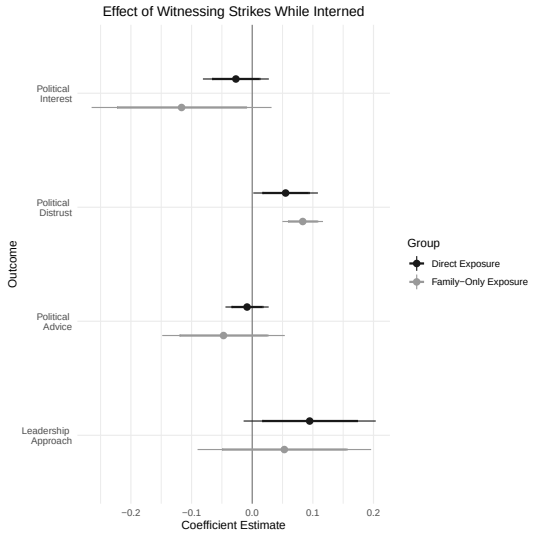


Figure A.16: Effect of Witnessing Violence While Interned for Internees Younger than 17 in 1945

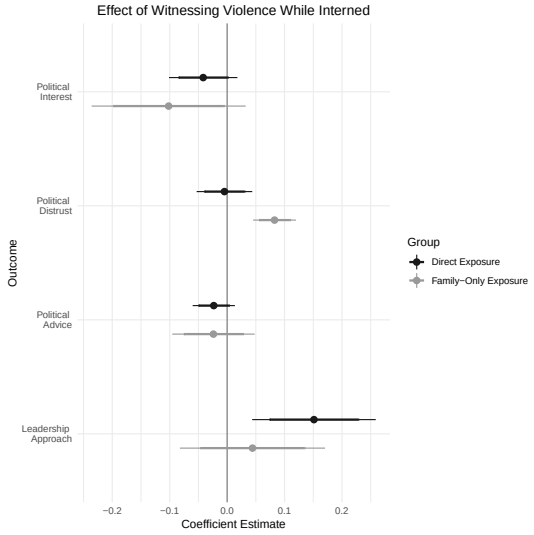


Figure A.17: Effect of Witnessing Use of Force While Interned for Internees Younger than 17 in 1945

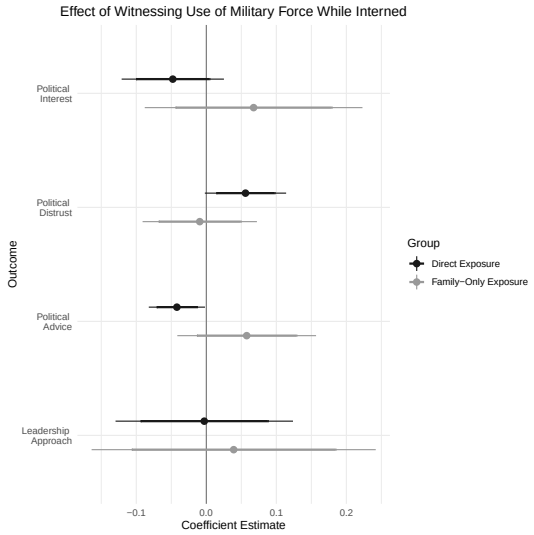


Figure A.18: Effect of Militarized Camp Environment for Internees Younger than 17 in 1945