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Backlash Against Repression: Evidence from Refugees Fleeing the Former Soviet Bloc

Valentin Bolotnyy
Stanford University

Mayya Komisarchik
University of Rochester

Brian Libgober
Northwestern University and IPR

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Abstract

The authors study the effects of state repression on political behavior. Applying a within-family research design to a unique administrative dataset of refugees fleeing former Soviet Bloc countries, they examine the impact of having lived longer in the Soviet Bloc on political behavior once in the United States. Siblings who lived longer in Soviet Bloc countries are significantly more likely to vote in U.S. elections and to affiliate with the Republican Party. The authors document evidence of a backlash against repression mechanism and find supportive evidence for their conclusions in Israel, one of the other major recipients of this refugee population.

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For those lucky enough to survive, living through episodes of mass state repression can have profound impacts on political behavior. A growing literature in political economy has begun to document these impacts, providing evidence of contemporary political consequences for repression, deportation and famine under Stalin (Lupu and Peisakhin, 2017; Rozenas, Schutte and Zhukov, 2017; Zhukov and Talibova, 2018; Rozenas and Zhukov, 2019), German military occupation in Greece during World War II (WWII) (Fouka and Voth, 2023), the deportation and mass murder of Jews living under Soviet rule during the Holocaust (Acemoglu, Hassan and Robinson, 2011), the Khmer Rouge genocide in Cambodia (Bühler and Madestam, 2023), and the internment of Japanese Americans in the United States (US) during WWII (Komisarchik, Sen and Velez, 2022). Broadly, this research suggests that extreme, devastating state interventions in the lives of civilians have a demobilizing impact that may last for generations. Public trust, turnout, and other measures of political participation tend to be lower in the most severely affected places. Predictably, surviving victims of state violence and repression also harbor hostility toward the perpetrating regimes and pass these sentiments down to their descendants.

This vein of research has provided valuable insight into the persistent effects of state violence and repression, but its focus has thus far centered largely on affected places and the people who remained in them after a particular episode of state repression drew to a close. Yet for millions across the globe, repression is a powerful impetus to leave—even if the risks associated with emigration are enormous. Members of targeted minority groups, in particular, may flee in the face of systematic threats of violence or deprivation. For instance, almost 90% of the Jewish population under Soviet rule that had survived the Holodomor famine in Ukraine (1932-1933), arrest and execution under Stalin's Terror (1936-1938), imprisonment in gulags (1936-1953), and mass murder during the Holocaust (1933-1945) ultimately emigrated out of the Former Soviet Union (FSU)[1] after 1970 (Tolts, 2019). While an extensive body of research has suggested state repression imbues groups with deep, collective memories and affects the way members perceive one another, society, and the state (Dessí, 2008; Nunn and Wantchekon, 2011; Superti and Gidron, 2021; Bühler and Madestam, 2023; Fouka and Voth, 2023), researchers almost never get a chance to track the individuals who leave repressive states in order to evaluate state repression's diasporic

[1] Armenia, Azerbaijan, Belarus, Estonia, Georgia, Kazakhstan, Kyrgyzstan, Latvia, Lithuania, Moldova, Russia, Tajikistan, Turkmenistan, Ukraine, and Uzbekistan.

political consequences.

In this paper, we use a novel data source to provide a first look at how living under a repressive, authoritarian communist regime affects political participation and party affiliation for immigrants who come to the US. We rely on records of refugees resettled in the US with help from the Hebrew Immigrant Aid Society (HIAS), a nonprofit organization that has provided humanitarian aid and assistance to refugees entering the US since 1881. HIAS was originally founded to help Jews fleeing discrimination and violence throughout tsarist Russia. The clients they serve consist primarily of people leaving countries in the FSU. These countries—along with several other Soviet Bloc[2] states from which HIAS-assisted refugees fled to the US—were all under communist authoritarian rule throughout most of our observation period and all targeted the members of their Jewish populations with violence, imprisonment, and restrictions on education, employment, Communist Party membership, residence permits, worship, speech, and other aspects of civic life well into the 1990s. Our data, made available by the American Jewish Historical Society (AJHS), consists of more than 600,000 records of immigrants resettled in the United States between the 1950s and 2016, making it one of the largest sources of information on immigrants ever used in studies of migration.

Crucially, this data set identifies individuals, family connections, dates of birth, countries of origin, and dates of emigration, which allows us to address the question of how living under a repressive authoritarian regime pre-migration affects individual immigrants' political lives in diaspora after they have resettled in a democratic regime where their personal and civil rights are generally protected. Access to information beyond individuals' self-reported immigration status is very rare. While governments often keep even more extensive data on immigrants than resettlement organizations do, these records are typically embargoed until long after these individuals are deceased. For example, the US National Archives only makes available comparable records 100 years after an individual's birth[3] and these are provided in hard copy. Under such rigid information constraints, social scientists have seldom been able to trace the migration process in much detail.

We merge our extensive data on refugees who emigrated as children to commercially avail-

[2]In addition to the 15 states that constituted the FSU this includes Albania, Bulgaria, Czechoslovakia, East Germany, Hungary, Poland, Romania, and Yugoslavia.

[3]https://www.archives.gov/research/immigration/aliens

able individual voter files to track the effects of coming of age under a repressive regime on voting (conditional on registration) and party identification after migration. In contrast to the pattern of demobilization documented in the literature surrounding the effects of state repression, we find that children who spend more time living under a repressive regime are, on average, more likely to participate in presidential and midterm elections and to register as Republicans in the US. Our findings hold within our simplest within-family design, as well as within variations on this design that examine the possibly confounding role of family structures and eldest sibling effects; variations that look cross-sectionally across families; and versions that look across different immigration cohorts affected by changing macroeconomic circumstances. Moreover, we establish similar patterns for refugees from the FSU in a different national context (Israel) using data from a large multi-election survey.

Our research design marks a notable advance for the literature, as it allows us to provide evidence that the political behaviors we observe among refugees after resettlement are likely to be a function of exposure to repression experienced prior to emigration rather than pre-immigration partisanship, the immigration experience, or the assimilation process in the destination country. While a number of studies done on Eastern European immigrant populations in Europe, Canada, and Australia have pointed out their distinctively right-leaning partisan character and low levels of expressed public trust (Wüst, 2000, 2004; Strijbis, 2014; McAllister and Makkai, 1991; Bird, Saalfeld and Wüst, 2010), their findings are difficult to attribute to the effects of living under repressive regimes for two reasons. Like most surveys of immigrant populations, these surveys rely on mixed samples of people who emigrated as refugees fleeing repressive regimes and people who emigrated for other reasons, like to reunite with a spouse or to start a new job. Both types of immigrants may come from the same country in the same year, making it difficult to tell what the result of having been repressed by the state might be. Additionally, surveys of immigrant populations are snapshots of post-migration political attitudes and behavior, making it difficult to rule out the possibility that any patterns we observe in immigrant preferences or behaviors are themselves the factors that drove migration in the first place (Turcu and Urbatsch, 2022; Lim, 2022). Immigrants who support right-wing parties in their settlement country, for instance, may have also been disposed to right-wing parties before migration.

Our research design guards against the possibility of this type of selection by (1) focusing

on children in immigrant families because children are not typically responsible for migration decisions and (2) comparing the "doses" of exposure each sibling would have received to the repressive regime they were born in, with the majority of other factors fixed. Consider, for example, a family from Moscow with two children, aged ten and five upon emigration to the US in 1992. At the moment of migration, these siblings will have received a five-year difference in the levels of direct and indirect exposure to the FSU, with most other features of life constant across children. On the US side, these children will have typically lived in the same household, attended the same schools (though not necessarily exactly at the same time), done many of the same activities, and shared in their parents' economic fortunes. Most importantly, by 2024, both siblings will have spent exactly the same 32 years living in the US, making it considerably more likely that differences in their political behavior result from different levels of exposure received pre-migration. This focus on children within the same families allows us to hold fixed potential confounding that might arise from genetics, family structure, and the migration experience itself (e.g., Griliches (1979); Böhlmark (2008); Currie et al. (2010); Chetty and Hendren (2018)).

In the context of this study, we conceptualize treatment as being the subject of state repression. In the Soviet Union, Jews were required to carry identification documents with their nationality listed as "Jew", which facilitated rampant discrimination in employment and education, while the state also suppressed Jewish religious, cultural, and communal life (Remennick, 2007). The intensity of experiences of repression varied, of course, but in the Soviet Union Jews all experienced some degree of repression at the hands of the state. We are mindful of reports of similar political behaviors among refugees from Cuba, Venezuela, Vietnam, and other communist authoritarian countries (Collet, 2000; Takaki, 1989; Girard, Grenier and Gladwin, 2012; Wong et al., 2011), which suggests that the treatment is more acutely exposure to *communist* repression. While our design does not let us further disentangle which types of repression drive the effects we observe, it does allow us to establish that the reason we observe these effects has more to do with socialization in immigrants' countries of origin rather than assimilation to their settlement country environments or the tendency of individuals with political predisposition to choose to migrate.

Our findings about political participation and political orientation speak to a persistent gap in the immigrant socialization literature, and are broadly relevant to a variety of other litera-

tures on immigration, authoritarianism, and political behavior. We provide empirical evidence on the long-term effects of authoritarian regimes on individual political behaviors, contributing to the understanding of how different types of political systems influence citizens' political engagement and attitudes even after they leave those systems. We likewise add to the literature on political socialization by demonstrating the enduring impact of early political experiences and the role of personal and familial narratives in shaping political identity and behaviors in a new democratic context. Finally, this study underscores the importance of understanding the diverse backgrounds of participants in democratic societies, suggesting that experiences of repression may motivate higher levels of political engagement, which is critical for the vitality of democracies. These results help allay the fears voiced in Handlin (1951) and, to some extent, resurfacing in more contemporary literature on immigration (see Just (2019) for an overview), where authors voice normative concerns that immigrants socialized in undemocratic states might either shun democratic participation or attempt to undermine democracy.

1 Jewish Refugees from the Former Soviet Bloc

In the late 1890s, Russian census data suggest that almost five million Jews lived in the Pale, a strip of territory including Belorussia, Bessarabia, the Crimean Peninsula, Lithuania, all of Russian-controlled Poland, and most of Ukraine, which remained home to most of the Jews in Europe until their mass murder during World War II (Grosfeld, Rodnyansky and Zhuravskaya, 2013). Between 1881 and 1924, a large number of Jews fled Eastern Europe, with approximately two million arriving in the US alone. In 1924, however, the US severely restricted immigration with the Johnson-Reed Act, while the ruling Communist Party also drastically restricted emigration in the 1920s, closing borders under its control and investing heavily in border security to restrict movement out (Gitelman, 1977). Jewish migration from Eastern Europe to the US slowed to a trickle. Due to growing international pressure in the wake of the holocaust, the FSU began allowing limited Jewish emigration to Israel. The process of obtaining visas was initially cumbersome and arbitrary, with applicants often risking dismissal from their work assignments and retaliation by the government (Remennick, 2007). In 1973, the US passed the Jackson-Vanik Amendment, which made permitting emigration a human rights issue and a condition for trade

agreements between the US and FSU. After some consternation, restrictions on emigration out of the Soviet Union were lifted under Gorbachev beginning in 1986, when ethnic Germans were permitted to return to west Germany and Jews were allowed to go to Israel or the US. Despite the serious obstacles to emigrating, nearly 300,000 Jews left the FSU between 1970 and 1988, with a slim majority heading to Israel (Tolts, 2019).

Jews who remained in the FSU in the second half of the 20th century were still widely subject to official and unofficial discrimination, including restrictions on university entry, employment, and membership of the Communist Party, along with a legacy of religious repression held over from earlier Soviet regimes that had banned religious practice and destroyed spaces for public worship (Altshuler, 1987). For instance, the requirement that Jews throughout the FSU register with Jewish "nationality" on passports, medical records, and library cards - established under Stalin's rule in 1932 - remained in full force until 1997 (Remennick, 2007). Ironically, the liberalization of free expression rules under Gorbachev's Perestroika heralded an increase in public anti-semitic demonstrations (Gitelman, 1991), all prompting subsequent waves of emigration out of the Soviet Union that would continue after the Soviet Union dissolved in 1991.

The exodus associated with the collapse of the Soviet Union is a particularly remarkable one in its scale and near-completeness. Approximately 2.2 million Jews lived in the Soviet Union in 1970 – a population that fell to 1.5 million in 1989 and dwindled to just 248,000 by the beginning of 2019 (Tolts, 2019). Of the approximately 1.6 million Jews born in the FSU who now live elsewhere, an estimated 750,000 currently live in the US. This group constitutes approximately 10% of the American Jewish population, but looks profoundly different politically. As we discuss in the subsequent section, while the records we have obtained from HIAS go so far back as to include individuals born in the 1880s, the people we can confidently merge to contemporary voting records belong largely to waves of immigrants leaving after 1985.

Given how late into its existence explicit restrictions on Jewish participation in public and private life persisted in the FSU, nearly every HIAS-assisted refugee in our data will have been a survivor of some form of political repression. The intensity of repression these refugees might have experienced directly, however, likely varies with age. The oldest immigrants in our data are likely to have been survivors of both the mass murder of approximately 3 million Jews under Soviet rule during the Holocaust (Altshuler, 1987) and Stalin's cleansing campaigns that arrested

and executed high-ranking Jewish Communist Party members as well as intellectual leaders and dissidents both before and after World War II (Martin, 1998; Remennick, 2007). The youngest immigrants in our data are less likely to have directly experienced formal restrictions on work or education, but would have experienced formal restraints on religious and cultural practice, while others may have witnessed anti-Semitic demonstrations or had uncomfortable experiences in early educational settings. More indirect or second-hand experiences of repression would have been ubiquitous.

Most of the historical immigration patterns we address in this section apply to the FSU specifically, while the population in our data emigrated from the Soviet Bloc more broadly. We focus our historical discussion on the FSU because the vast majority of emigrants in our data (84.6%) emigrated out of this set of countries. It is worth pointing out, however, that the force we describe as the main driver of emigration out of the FSU — being targeted by the state for persecution on ethnic and religious grounds — was a similarly important driver of emigration across the Soviet Bloc despite differences in specific policy approaches and regime survival lengths. Many regimes in the Soviet Bloc (but outside the FSU) had very small surviving Jewish populations after the end of WWII and generally did not restrict their immigration to Israel or other countries. Romania's government, for instance, did not oppose the mass migration of its surviving Jewish population to Israel beginning virtually with Israel's independence in 1948. Similarly, approximately one-third of Hungary's Jewish population emigrated to Israel between 1945 and 1949 — and emigration would continue through the Hungarian communist party's collapse in 1989. Other regimes forcibly drove out their surviving Jewish populations. Poland's communist government forcibly expelled over 15,000 Jews in 1968. While emigration was not restricted everywhere, Jews fled countries throughout the Soviet Bloc en masse to escape significant episodes of ethnic targeting by their ruling regimes (Tolts, 2019).

2 The Imprint of Repression

How might experiencing repression at the hands of a communist authoritarian state affect the political lives of survivors who emigrate to democracies? The extant literature suggests a "backlash effect" for victims who remain in affected areas: victims are more hostile to the per-

petrating regime and persistently less likely to express loyalty unless the regime can credibly threaten them again (Rozenas and Zhukov, 2019). It is reasonable to suggest that this dynamic holds for surviving victims who emigrate since emigration itself is a powerful rejection of the ruling regime. Because emigration makes it exceedingly difficult for a repressive regime to credibly threaten those who leave, the backlash effect has straightforward predictions for what emigrants might do once they resettle in a democratic country: affiliate with parties they perceive to be most unlike the ruling regime that persecuted them in terms of ideology or character.

As we discuss below, that is a view consistent with other studies of immigrants fleeing repression under communist totalitarian regimes, press interviews with members of the refugee population fleeing formerly communist states throughout the Soviet Bloc, and our own data. The implications of this argument for post-migration political participation are less clear. Living through episodes of severe repression by the state may so destroy victims' sense of public trust that they come to fear engagement with any state, but it might also motivate survivors to participate more post-migration to fulfill pent up demand for political voice, cast votes against parties similar to oppressive ones in their birth countries, or a number of other possible psychological mechanisms beyond the scope of this study. We map out theoretical implications for both partisanship and participation in detail below.

2.1 Participation

In contemporary authoritarian states, either as a result of limited exposure to democracy or due to messaging deployed by governments seeking to disparage democracy, residents might have limited trust in the general public and skepticism of deliberative democratic institutions themselves. In fact, several surveys across western democracies have suggested that immigrants from authoritarian regimes are more likely to express skepticism of democracy or openness to other forms of government (McAllister and Makkai, 1991; Bilodeau, McAllister and Kanji, 2010; Bilodeau, 2014, 2016; Just, 2019). This certainly appears to be the case among immigrants from the Soviet Bloc, who report low levels of public trust and faith in institutions (DiFranceisco and Gitelman, 1984; Rose, Mishler and Haerpfer, 1998; Holmes, 1997). Immigrants from Vietnam have also expressed high levels of distrust in the government relative to members of other Asian-American subgroups emigrating from less repressive regimes (Collet, 2000). Given the well-documented

positive correlation between political trust and turnout (see Kelly, Tilley and Oskarsson (2026) for an overview), we might expect that immigrants with limited faith in political institutions would participate less in the democratic process.

Researchers have pointed out that a lack of familiarity with voting may not be the issue, as various left-wing authoritarian regimes have either allowed or encouraged some form of political participation from citizens (Just, 2019). However, in authoritarian regimes political participation is often compulsory and performative; citizens may be expected to join the governing party, attend rallies and political events, take part in political debates, or even cast ballots — all without any expectation of exerting real influence over government (DiFranceisco and Gitelman, 1984; Kuran, 1991). The compulsory nature of political participation across authoritarian regimes, along with the fact that this participation rarely produces responsive government, might lead us to think that citizens who lived under these systems might be reluctant to engage in the political process.

Taken together, pre-migration distrust in public political institutions, aversion to engagement with the state after being forced to do it ceremonially, or outright oppression at the hands of the state might all suggest that authoritarian regimes politically demobilize their citizens (Just, 2019). That is likely to represent at least a portion of the direct effect that living under such regimes has on immigrants and non-immigrants alike. However, it remains important to account for the theoretical implications of both selection and socialization. It is possible that those who leave authoritarian regimes are precisely those people with the greatest desire to participate in the democratic process. Accordingly, immigrants who obtain citizenship in new countries may actually be more likely to participate in elections despite the disincentives to do this in their countries of origin.

It is similarly possible that, despite a learned reluctance to engage in politics in their countries of birth, immigrants might think of the US, Canada, Australia, Israel, or other mature democracies as places where political participation is fruitful and welcome any encouragement that they receive to obtain citizenship and engage in politics upon arrival. In other words, conditional on knowing that they might be settling in a democratic country, immigrants may adopt positive views of the democratic process. There is some evidence that this might be the case: Acemoglu et al. (2021) find that immigrants who spend long periods in well-functioning democracies express support for democratic institutions. Similarly, scholars of migration to the US during the

late 19^{th} and early 20^{th} centuries have pointed out that immigrants (including Eastern European immigrants) hailing from undemocratic regimes were often aggressively and successfully recruited into Democratic Party politics, particularly in large urban centers (Dahl, 1961).

These countervailing forces may help explain why empirical findings attempting to relate political participation to country of birth among immigrants to Western democracies are so mixed. There appears to be no clear empirical relationship between turnout and country of origin for immigrants to Canada (White, 2017) or Australia (Bilodeau, McAllister and Kanji, 2010). Using surveys of first generation immigrants to the US spanning 1994-2000, Ramakrishnan (2005) finds that the relationship between coming of age in a repressive regime and turnout varies by ethnicity, changing signs from negative for white and non-Cuban LatinX immigrants to positive for Asian immigrants. Earlier studies of Cuban immigrants to the US have suggested higher rates of political participation relative to immigrants from other LatinX subgroups who were not fleeing repressive regimes (Portes and Mozo, 1985; Arvizu and Garcia, 1996).

We will show evidence that, for the population we study, longer periods of exposure to left-wing authoritarian regimes increase the likelihood of political participation. It is worth pointing out one important caveat in the context of theoretical predictions. As we discuss in Appendix B, our data constrain us to look at people who turn out conditional on having registered to vote. We will not be able to address the question of whether or not living under authoritarian regimes makes people more or less likely to register to vote. The possibility that coming of age under authoritarianism is broadly demobilizing in the sense that smaller proportions of immigrants coming from authoritarian countries register relative to native-born people or other immigrants remains open for future research.

2.2 Party Affiliation

In the post-World War II world, most of the regimes that have engaged in mass political repression have been communist authoritarian regimes (Davenport, 2007). Immigrants hailing from left-wing authoritarian regimes across continents, religions, races, and ethnicities have tended to affiliate with right-wing parties over left-wing parties — at least at much higher rates than immigrants coming from other regime types. Scholars working in the comparative literature have observed that this backlash against left-wing authoritarian governments is affective more than it

is ideological. Pervasive public anti-communism in communist, authoritarian regimes is rooted in opposition to party control over civic, political, and economic life; disappointment at the deprivation and economic inefficiency introduced by central planning; and anger over violent repression at the hands of the state (Kuran, 1991; Darden and Grzymala-Busse, 2006; Just, 2019). Sentiments like these abound in popular press accounts from Eastern European-Jewish refugees to the US, who are heavily represented in our data. "They have experienced socialism and communism in a totalitarian regime," the director of Russian-Jewish Community Affairs at the American Jewish Committee explained to *The Atlantic*; "anything that remotely resembles that, they hate it, they despise it" (Khazan, 2016). In explaining his own shift toward Donald Trump and the political right, Google co-founder Sergey Brin told the *New York Times* "I fled socialism with my family in 1979 and know the devastating, oppressive society it created in the Soviet Union. I don't want California to end up in the same place" (Schleifer and Conger, 2026). Immigrants from the Soviet Bloc are not alone. Examples of similarly fervent anti-communism appear in studies of immigrants from China, Vietnam, and Cuba (Takaki, 1989; Girard, Grenier and Gladwin, 2012; Wong et al., 2011).

An abundance of descriptive survey evidence points to the possibility that reflexive rejection of communist authoritarian parties helps guide political orientation for immigrants fleeing these regimes. Many studies have documented the tendency for immigrants from left-wing authoritarian regimes to identify with right-wing parties in democratic countries - even when those parties are anti-immigrant (Spies et al., 2023). Studies have shown this is the case among Eastern European immigrants to Germany (Wüst, 2000, 2004), Switzerland (Strijbis, 2014), and Australia (McAllister and Makkai, 1991), as well as Romanian immigrants to Spain (Bird, Saalfeld and Wüst, 2010). Several studies have pointed out similar trends among Vietnamese and Cuban immigrants to the US (Cain, Kiewiet and Uhlaner, 1991; Hill and Moreno, 1996; Alvarez and Bedolla, 2003; Lien, Conway and Wong, 2004; Hajnal and Lee, 2006).

There is evidence for this in the US voting behavior of refugees from the Soviet Bloc as well. Some of the most extensive existing surveys of the Russian-speaking Jewish immigrant population suggested that 60-70% would support Donald Trump in the 2016 general election (Khazan, 2016). In fact, Donald Trump ultimately won 84% of the Republican primary vote in Brooklyn's Brighton Beach neighborhood, historically an enclave for Russian-speaking immigrants (Bagri,

2016). Even before Trump, Russian-speaking Jewish immigrants reported casting predominantly Republican ballots in the 2004, 2008, and 2012 presidential elections (Bagri, 2016).

For immigrants from the Soviet Bloc, this is more likely to be evidence of a backlash effect against a persecuting regime than a case where immigrants are directly mapping pre-migration political ideologies to new political circumstances. Unlike refugees from Castro's Cuba, who were right-wing partisans prior to migration and largely retain right-of-center party affiliations after migration to the US (Portes and Mozo, 1985), Jews in the FSU were prominent supporters of a communist ruling state and were over-represented in the highest ranks of the Communist Party until Stalin's regime launched systematic cleansing campaigns of the Party's ranks and the country's intellectual centers to remove them (Martin, 1998; Slezkine, 2004; Remennick, 2007). While individual-level pre-migration surveys of this population are unavailable, qualitative and historical evidence suggests it is unlikely that this group of refugees would have been right-wing relative to others living in the FSU prior to migration.

3 Empirical Strategy and Results

3.1 Sample Characteristics

The American Jewish Historical Society provided on their website scanned index cards describing the identity of HIAS clients who immigrated prior to 1980 and also a digital database of records created after 1980. We manually coded the scanned index cards and scraped the digital records, yielding information on 635,964 individuals across 226,577 family case files. We merged this database of HIAS clients to the L2 Voter file. Our matching process differed slightly depending on whether the client information originally came from paper or electronic records, since the data available for matching also differed depending on the original format. Full details on our merge process are available in Appendix B. In total, we confidently match 97,650 refugees from the original HIAS data to L2's voter files — an overall merge rate of 15.4%. The L2 data provides us with additional information about these individuals, including gender, current state of residence, voting history, party affiliation, and, for immigrants after 1980, current age, a covariate that we can reasonably infer from information provided for immigrants before 1980.

The 97,650 matched refugees include people who arrived in the US as children and people who arrived in the US as adults. The main analysis we present below uses a subset of these individuals. First, we restrict the sample of refugees we analyzed to people from Soviet Bloc[4] countries. We do this because we want to look at the effects of targeted repression under a series of ideologically similar regimes pursuing a relatively similar set of policies against their Jewish minority populations in the period before the collapse of the Soviet Union. This allows us to interpret any effects we observe as likely functions of the forms of repression these regimes engaged in, rather than something else about national institutions, culture, or policy. While some of the refugees in the HIAS data hailed from India, Mexico, Peru, Equador, Iraq, and many other countries around the world, these countries were governed by fundamentally different regimes that did not, as far as we are aware, engage in comprehensive programs of targeted repression against their Jewish minority populations. Including refugees from these parts of the world would make it more difficult to figure out what exactly drove any effects we observe.

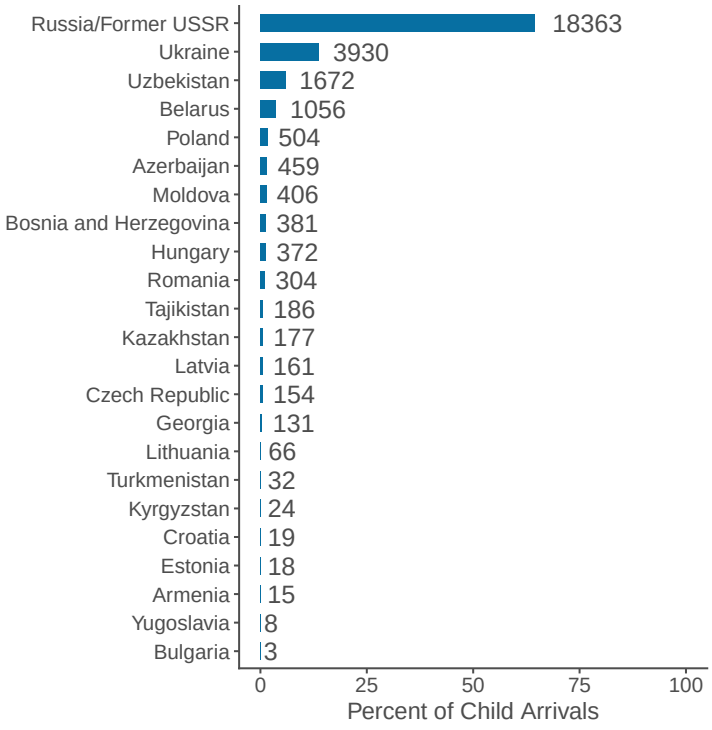
In practice, however, this restriction has a relatively minimal impact on our analysis. The reason for this is that, while refugees we matched from the HIAS data represent over 50 different countries, the overwhelming majority of them came from former Soviet Bloc countries (pre- or post- collapse). 84.7% of the refugees in the original HIAS data come from the former Soviet Bloc, which is unsurprising given HIAS's organizational mission. Fully 92.7% of the refugees we successfully matched to the voter files similarly came from the Soviet Bloc. Thus, while we explicitly restrict our main analysis to this 92.7%, in practice we prune very few matched observations. Figure 1 summarizes the top countries of origin for the refugees we do include in our main analysis; most come from the former USSR (or Russia for those emigrating after 1991), Ukraine, Uzbekistan, and Belarus.

Given our research design, the second important restriction we impose on our data is to limit our analysis to refugees who arrived as children and came from families that had more than one child. We define children as individuals who were younger than 21[5] at the time of immigration.

[4]Countries in the FSU are itemized earlier in the paper. In addition to these, the Soviet Bloc refers to Bosnia and Herzegovina, Bulgaria, Czechoslovakia, Croatia, Hungary, Poland, Romania, and Yugoslavia. We include immigrants who came from Soviet Bloc countries after the Soviet Bloc dissolved and the relevant communist regimes were toppled, so the sample includes people emigrating from Russia as well as the USSR, Croatia and Bosnia and Herzegovina (formerly part of Yugoslavia), etc.

[5]We chose this threshold because it maximizes the amount of variation we could see in ages of immigration throughout childhood, but still corresponds to an age at which a young person generally has not yet completed her

Figure 1: Countries of Origin for Refugees Arriving as Children



This allows us to do two things. First, the focus on people who arrived as children lets us weaken selection effects. If we were worried that some latent characteristic drove *both* the decision to emigrate *and* downstream political behavior, then it would be harder to interpret our effects as responses to a repressive state's behavior. In the worse case scenario, *all* of the parents who chose emigration exhibit that latent characteristic — evidenced by their decisions to leave their countries of birth. Yet, unless we think we are in the unlikely situation that this characteristic is perfectly heritable, the children of these parents may not have that latent characteristic and indeed neither drove the choice to emigrate that we observe in this data nor would necessarily be the types to choose emigration in the future.

Second, this facilitates the within-family comparisons that allow us to look at outcomes for siblings whose pre-migration, migration, and post-migration circumstances are largely similar, but whose differing ages resulted in different levels of both direct exposure to targeted repression from the states they lived in and indirect communication about that repression from parents or

college education and likely still lives with parents or other family members. However, as we show in Appendix F, our results are not sensitive to this choice of threshold.

other family members. Within-family comparisons between parents are technically possible, but these are essentially perfectly confounded by gender. Similarly, comparisons between parents and children in cases where parents have just one child are largely confounded by the difference in within-family roles that these statuses imply. Still, we do look separately at levels of similarity in political behavior between parents and children in Appendix H; results for adult arrivals look markedly different for those who arrived as children.

In the complete HIAS data, the modal number of members associated with a case file is 2 — usually a couple without children. The mean number of people per case file in the original data is 2.81 (see Appendix A for more detail). A minority of refugees in the data had children at all and fewer still had more than one child. Figure 2 summarizes the distribution of children per family, conditional on having children at all, at the time of immigration in our merged data. Thus, while we successfully merged 28,441 refugees across 24,636 families who arrived in the US as children, our analysis relies on the 7,265 children from families with more than one child.

Figure 2: Number of Children Per Case File

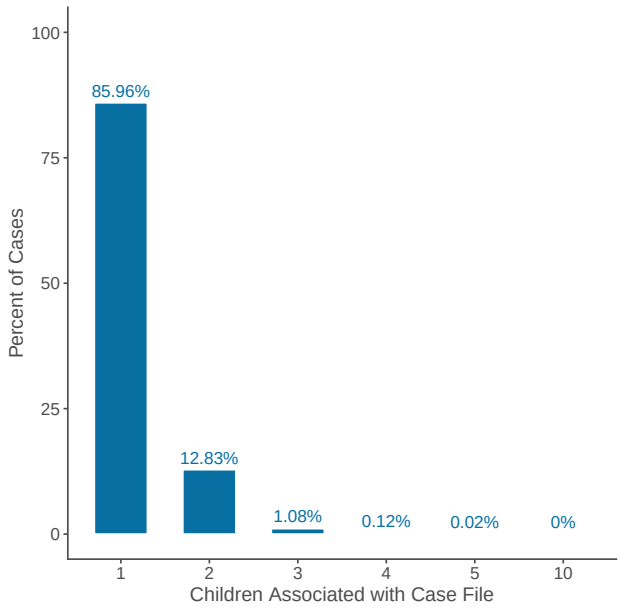


Figure 3: Sibling Age Differences in Years

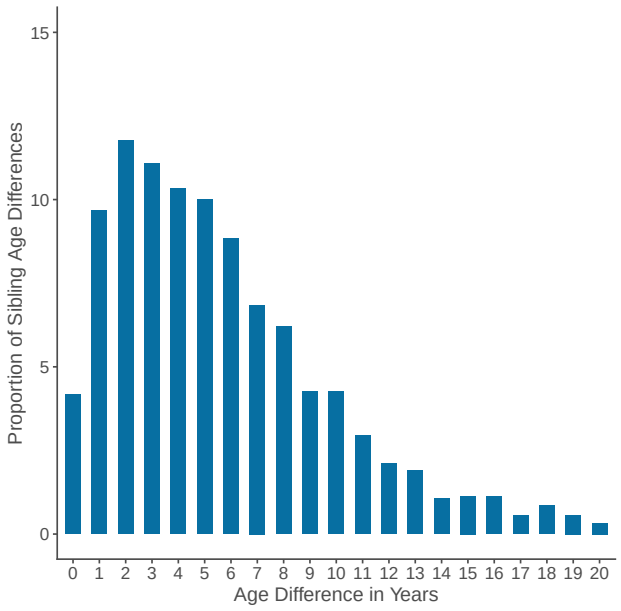


Figure 3 summarizes the distribution of age differences between siblings in our merged data. We interpret these age differences as the difference in doses of direct and indirect exposure to state repression that each sibling would have received. More than half of children in our data have siblings who are within 5 years of their own age, and about 85% have siblings who are

within 10 years. Some gaps are much larger, and might occur in cases where cousins rather than siblings appear in a case file together, which we know to be rare in the more detailed pre-1980 case files. Excluding siblings with larger age gaps from our analysis does not substantively affect the results we will report below, as we show in Appendix D.

Figure 4 depicts the distribution of arrival ages for people who arrived as children and appeared in the L2 data. While we have comparatively fewer people who arrived either as infants or as young adults aged just under 21, the distribution is relatively uniform across ages in between. While this limits our ability to say much about outcomes for the very youngest, ample observations across the rest of the age range make it unlikely that our results will be driven by a paucity or overabundance of arrivals at a particular age.

Figure 4: Age At Arrival for Child Arrivals Matched to Voter Files

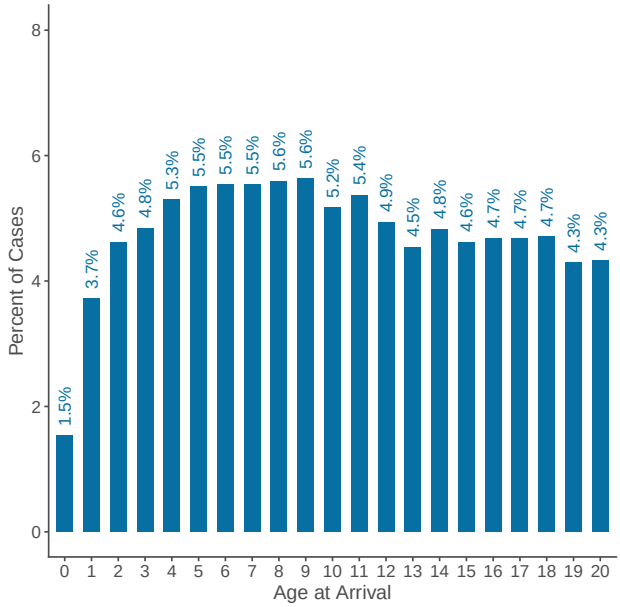
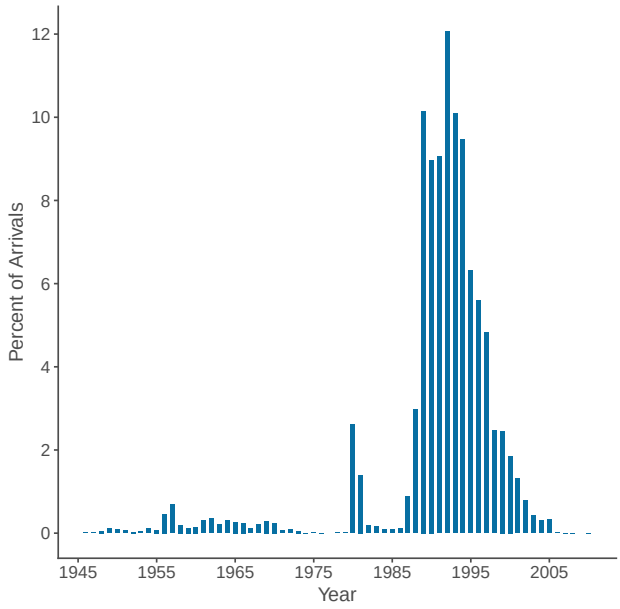


Figure 5 summarizes the distribution of arrival years for our effective sample of people who arrived as children and appeared in the L2 data. The vast majority of our sample of child arrivals arrived in the US between 1986 and 2000. This is representative of the broader HIAS data, which has a nearly identical pattern of arrival years (Appendix C). This distribution of arrival years also has the advantage that it includes arrivals before and after the collapse of the Soviet Union in 1991. We show that our results are robust to refugees leaving in both periods in Appendix J.

Figure 5: Years of Arrival for Child Arrivals Matched to Voter Files



3.2 Voting Behavior

We are interested in understanding whether there is a relationship between age at arrival in the US and an individual's political engagement and preferences later in life. Numerous studies have shown a causal relationship between exposure to a particular environment, especially in childhood, and one's income, health, and education, among other outcomes (Chetty and Hendren, 2018). We use our data to understand whether environment shapes political preferences and behaviors.

We start by looking at the unconditional relationship between age at arrival and the probability of turning out in a presidential election (Figure 6) or a midterm election (Figure 7). To do this, we split our merged sample of child arrivals from multi-child families into one-year age bins. Within each age bin, we report the proportion of child arrivals in the sample who turned out in any presidential or midterm election. In Figures 6 and 7, the point at 5, for instance, represents the proportion of child arrivals who were between 4 and 5 when they immigrated who turned out in any presidential or midterm election between 2012 and 2022. We plot descriptive ordinary least squares regression lines regressing the proportion who turned out in each age bin on age at arrival, and report the slopes that describe the expected average increase in turnout associated with arriving one year older on the plots. Both slopes are positive and statistically

distinguishable from 0; p-values associated with each are effectively 0. These estimates have no causal interpretation, but they suggest that, in general, children in this data who were older when they arrived tend to participate more often than children who arrived younger. The estimates we obtain after applying our identification strategy will similarly suggest that older arrivals are more likely to participate — though with a slightly different interpretation. We show Figures 6 and 7 here to assure readers that this pattern is "native" to the data rather than an artifact of our main specification.

Figure 6: Presidential Elections

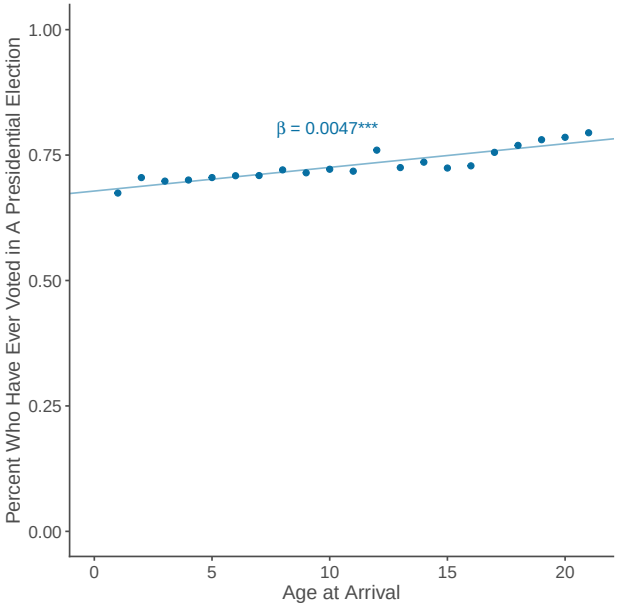
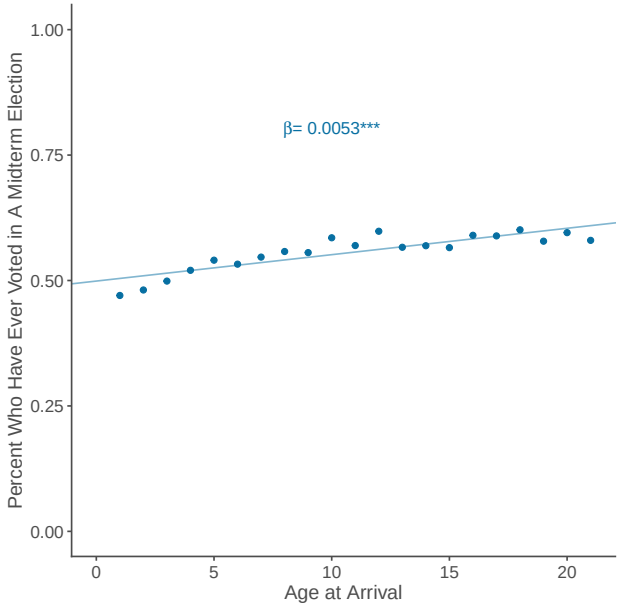


Figure 7: Midterm Elections



To see whether there is indeed a causal relationship between age at arrival and the probability of voting, we restrict our attention to childhood arrivals from multi-child families and estimate family fixed effects regressions of the following form:

$$y_{ij} = \alpha + \beta ArrivalAge_{ij} + \theta Female_{ij} + \phi_j + \eta_{ij} \quad (1)$$

where y_{ij} is the outcome of child i in family j, $Female_{ij}$ is a dummy variable for the child's gender, $ArrivalAge_{ij}$ is the child's age at arrival in the US, and ϕ_j is a family fixed effect that captures unobserved family characteristics that are common to all siblings in the same family. η_{ij} denotes the error term. We do not explicitly control for arrival year because year of arrival is

almost universally uniform across family members for all families in our data.[6] We likewise do not explicitly control for current state of residence despite the fact that this is likely to have effects on both participation levels and party affiliation. The reason for this is that the current states of residence recorded for resettled refugees reflect post-treatment choices about where to reside, and would likely bias our estimates (Acharya, Blackwell and Sen, 2016). We cluster standard errors at the state level in all reported results to account for within-state correlations in voting behavior. We report the distribution of states in which refugees in our sample currently reside in Appendix K.

Our chief quantity of interest in this specification is β, the coefficient that modifies age at arrival for children in this data. Since we include family fixed effects, the way to interpret this coefficient for a given family is to think of it as the effect of being an additional year older relative to one's own sibling(s) on the outcome — whether that's voting or the probability of identifying with a particular party. More broadly, we can think of β as the average effect of being a year older than one's siblings across all families with multiple children. If we assume the family fixed effect adequately accounts for heterogeneity in pre- and post- migration environments, as well as the migration experience itself, since those are effectively common across family members, then we can also believe that β corresponds to the additional year of exposure to targeted repression — since that's the only difference between siblings via age in this context. We will discuss threats to inference and present a variety of robustness checks that bolster this interpretation of β throughout the paper.

We present our results for this quantity across election years in Table 1. These effects are positive and relatively consistent across election years, and statistically distinguishable from 0 in every year except for 2018. These effects suggest that, on average, being a year older than one's own siblings at the time of immigration makes it more likely that child arrivals in this data would participate in midterm and presidential elections.

We provide some intuition about effect sizes in Table 2. We report our effect sizes by election year (Column 2) and compare them to the average turnout in our merged sample of child arrivals for the same year (Column 4). The Scaled Effect column (Column 3) reports our effect sizes as

[6]There are 84 families in the merged data who arrive in waves such that groups of members have different arrival dates. We drop these families before estimating the results presented in this section, though including them does not substantively impact our results.

Table 1: Effect of Age at Arrival on Participation

	Turnout					
	2012	2014	2016	2018	2020	2022
Age at Arrival	0.008*	0.004**	0.007**	0.004	0.007***	0.006*
	(0.004)	(0.002)	(0.003)	(0.004)	(0.002)	(0.003)
Female	−0.019	−0.028**	−0.022	−0.055**	−0.037*	−0.075***
	(0.029)	(0.014)	(0.027)	(0.027)	(0.020)	(0.022)
Family Fixed Effect	✓	✓	✓	✓	✓	✓
Observations	3,785	3,785	3,785	3,785	3,785	3,785
Adjusted R^2	0.224	0.266	0.135	0.234	0.080	0.133
Residual Std. Error (df = 3783)	0.404	0.291	0.461	0.417	0.453	0.464

Note: *p<0.1; **p<0.05; ***p<0.01
Standard errors clustered by state.

a percentage of the average overall turnout in our merged sample of children; national turnout rates (Column 5) come from data provided by the U.S. Elections Project (McDonald, 2026). Thus, as a percentage of average turnout in the merged sample of child arrivals, our effect sizes range from approximately 1.1% to 3.3%. In percentage terms relative to average sample turnout, the effects are at least as large in midterm elections as they are for presidential elections. This is counterintuitive given the larger salience of presidential elections and higher overall turnout associated with them, but may indicate a consistent commitment to participation in the sense that older arrivals are likely to be engaged enough to follow midterm election cycles as well as presidential ones.

Additionally, it is worth pointing out that participation rates in our matched sample are lower than they are in the general US population until 2022. There are some caveats to this comparison since the match process is not error-free. We likely failed to match some portion of refugees who did register due to transcription errors or as a result of post-marital surname changes, which suggests that true rates of participation in this group may be higher than what we report here.[7]

[7]There is some evidence of this point, since our final matched sample is 57% male and 43% female. While there is no way to find the women who would have changed their last names between immigration and voter registration, we show that our findings replicate on a subsample of male child arrivals since we are less likely to lose observations to surname change in this group. These results appear in Appendix E.

Table 2: Evaluating Effect Sizes Relative to Sample Mean and Population Turnout

Year	Effect	Scaled Effect	Mean Turnout in Sample	Turnout in U.S. Population
2012	0.008	0.026	0.301	0.586
2014	0.004	0.033	0.132	0.367
2016	0.007	0.017	0.437	0.601
2018	0.004	0.012	0.349	0.494
2020	0.007	0.011	0.663	0.666
2022	0.006	0.012	0.463	0.462

Turnout rates based on national election data from the U.S. Elections Project.

3.3 Party Affiliation

We next examine the relationship between exposure to a repressive regime and post-migration party affiliation using the same approach as above, but now turning the outcome variable into a dummy variable for an individual's party registration. Before delving into these effects, however, it is worth mentioning that party registration in our matched sample of child arrivals is not overwhelmingly Republican. Just 28% of the child arrivals we successfully matched to the voter file are registered as Republicans; 30.1% are Democrats and 38.5% have non-partisan registrations. As of 2025, 31.01% of registered voters nationwide registered as Republicans, 36.84% as Democrats, and 32.16% were independents or registered with third parties (McDonald, 2026). While our main concern in this study centers on how exposure to state repression affects refugees' decisions about which parties to affiliate with rather than these levels themselves, this snapshot of registration data does not bear out the impression conveyed in popular press writing suggesting that this group is predominately Republican. In fact, they seem to have a similar overall split to the general population. That said, since reports suggest two thirds of Jewish Americans are Democrats (Tighe, Parmer and Saxe, 2020), the child arrivals we match are notably more likely to register as Republicans than their co-ethnics with deeper roots in the United States.

We again begin by looking at the unconditional relationships between age at arrival and party affiliation. Here, it becomes apparent that while this group of refugees looks similar to the general population in terms of the levels at which they affiliate as Republicans, Democrats or as independents/nonpartisans or members of third parties, older arrivals tend to be more likely to register as Republicans or independents/nonpartisans than younger arrivals, and significantly less likely to register as Democrats. Each relationship is statistically distinguishable from 0, where p-values

associated with the coefficients we report in Figures 8-10 are near 0. We will show that this pattern similarly holds for our causally identified results, but this is likely what generates the impression of Soviet-Bloc refugees as an uncommonly Republican-leaning group; those who got more exposure to repression for being Jewish under Soviet rule are more likely to say they want to affiliate with the party least like the ruling party they experienced in their youth and to want to keep the political status quo from shifting in the direction of the world they grew up in.

Figure 8: Republicans

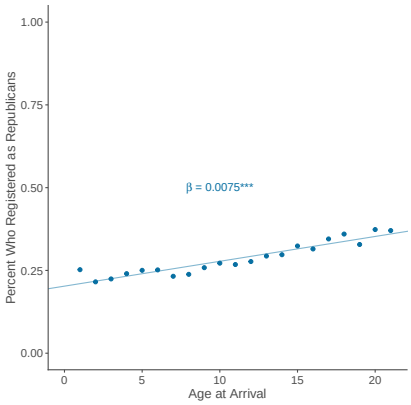


Figure 9: Democrats

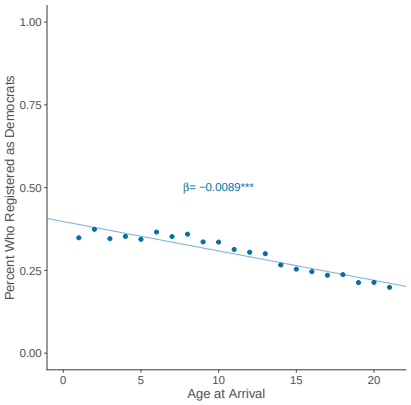


Figure 10: Nonpartisans

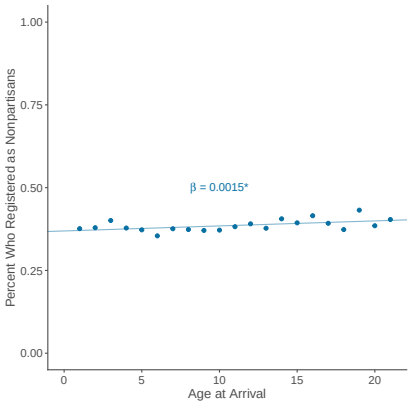


Table 3 summarizes the results we obtain when we estimate equation (1) with Democratic, Republican, and Independent or nonpartisan registration indicators as outcomes, respectively. An additional year of exposure to ethnic targeting and repression in the Soviet Bloc produces, on average, a 0.008 percentage point increase in the probability of registering as a Republican and a nearly equivalent decline in the probability of registering as a Democrat. Looking back at the overall proportions of our sample registered as Republicans and Democrats above, this is a 2.8% increase relative to the mean proportion of Republicans and a 2.1% decrease relative to the mean proportion who are Democrats. This is consistent with the backlash effects reported in previous research and press interviews with members of this population; refugees with more direct and indirect exposure to targeting are more likely to affiliate with the right-leaning party in the US than its left-leaning counterpart. While the point estimate for registering as an independent, with a third party, or as a nonpartisan is negative, here we cannot statistically distinguish this estimate from 0 and report no visible effects of regime or persecution exposure on nonpartisan registration. Since virtually none of the theories about how refugees from repressive regimes who targeted them might affiliate in democratic environments suggest more (or less) commitment

to nonpartisanship, we can think of the near-zero effects of exposure on nonpartisanship as a placebo test that supports our hypothesis about backlash against the left. If there were strong effects in nonpartisanship here, arguments about either backlash in the form of affiliating with the political right or assimilation in the form of affiliation with the political left[8] would make considerably less sense.

Table 3: Effect of Age at Arrival on Party Affiliation

	Party Affiliation		
	Democrat	Republican	Nonpartisan
Age at Arrival	−0.007**	0.008*	−0.001
	(0.003)	(0.005)	(0.003)
Female	0.038**	−0.030	−0.003
	(0.019)	(0.032)	(0.031)
Family Fixed Effect	✓	✓	✓
Observations	3,785	3,785	3,785
Adjusted R^2	0.175	0.139	0.112
Residual Std. Error (df = 3783)	0.417	0.419	0.459
Note:	*p<0.1; **p<0.05; ***p<0.01		
	Standard errors clustered by state.		

4 Discussion

Both across and within families, age at arrival is associated with heightened tendency to participate in politics via voting and also to orient in a more conservative direction through affiliation with the Republican Party. These findings come in notable contrast to some scholarly expectations about the political behavior of those experiencing repression. That said, attributing these differences in behavior to differences in experiences of repression is not obvious and there are a number of potential alternative explanations to consider. While not intended to fully exhaust all possibilities, in the subsections that follow we examine the possible role of changing economic circumstances, family structure and eldest sibling effects, and the national context where these

[8]Recent surveys, like Pew's in 2021, suggest that 71% of American Jews reported identifying themselves as Democrats and just 26% reported identifying as Republicans. Thus, to the extent that assimilation would mean looking like the native Jewish population, if we believe the driving force in party identification within the US is assimilation, we would expect to see large positive effects in favor of Democratic Party affiliations.

refugees settle.

4.1 Repression or Economic Collapse?

By the late 1980s, the Soviet economy was collapsing: Gross National Product (GNP) growth was negative, shortages of food and consumer products were rampant, trade suffered as firms across the globe refused to do business at artificially low prices fixed by the state (Shleifer and Vishny, 1991). Rapid and uneven privatization in the late 1980s and early 1990s led to skyrocketing prices, job losses, and salary freezes, perpetuating "the emergence of an economic crisis that threatened the welfare of the middle class" — to which the majority of the FSU's Jewish minority belonged (Litwin and Leshem, 2008). There is no question that concerns about economic prospects, alongside hardships imposed by decades of state targeting and ethnic repression, pushed Jews living throughout the Soviet Bloc to emigrate. There is similarly little question that refugees from the Soviet Bloc blame the ruling regime(s) for widespread financial and economic destruction and that these experiences shaped their views of American politics (Berger, 2012).

Precise weights describing how much of our observed effects are attributable to targeting by the state versus backlash to economic mismanagement are impossible to recover, since most refugees in our sample emigrated in the late 1980s or later and experienced economic hardship and uncertainty along with state repression. We can, however, demonstrate that the effects we report are not exclusively the product of experiencing sudden economic hardship. We split our sample into refugees who emigrated prior to 1980, before the Soviet economic contraction was fully underway, and those who emigrated after 1980 and would have experienced the effects of both repression and economic freefall.

Appendix I shows that the effects we report in Section 3 manifest among pre-1980 refugees as well as post-1980 refugees. While the pre-1980 regression coefficients fall short of conventional levels of statistical significance because the FSU's restrictive pre-1980 emigration regime limited the number of people who could exit and consequently constrained our sample size for this period, they provide suggestive evidence, through comparable coefficients, that our conclusions would hold even if the economic crisis of the 1980s and 1990s did not hit the FSU. Before 1980, emigrants' choice of settlement country reflected religious and ideological motivations over economic ones:

emigrants leaving the FSU in the 1970s were more than twice as likely to go to Israel than the US (Rosenberg, 2014). The fact that the effects we measure are comparable for this group of refugees to the effects we measure for those who leave when repression is coupled with economic crisis suggests that repression is the dominant mechanism behind the effects we document in this paper.

We try to isolate the repression mechanism further in Appendix J, where we estimate our main results separately for people who arrived before and after the collapse of the Soviet Union itself in 1991. While our results are robust in the sense that the magnitudes and directions for each effect we report in Section 3 appears for both pre- and post- 1991 arrivals, the partisanship results are much stronger for the group who arrived *before* 1991 despite the fact that this is a somewhat smaller sample than the post-1991 arrivals. This is consistent with a story about backlash to repression rather than economics or civic culture. The group who left before 1991 is comprised of people who spent more of their lives living under Soviet repression than the group who would have spent more time living under less consolidated totalitarian governments with less organized capacity and less interest in engaging in organized state campaigns against Jews.

4.2 All in the Family?

We rely on family fixed effects to identify the impact of living under an authoritarian regime. Because families with children tend to live together pre- and post-migration and make the move together, this allows us to hold fixed the vast majority of socioeconomic, religious, traditional, biological, and migration-related confounders that might affect political outcomes. Identification under a family fixed effect relies on the less explicit assumption that family organization, structure, and parenting are likewise relatively constant across children within the same family.

If parents treated elder children systematically differently than they treated younger children — and did this in a way that produced more political engagement and conservative-leaning political orientation in older children — we could not interpret our results as a function of each child's level of exposure to the political context prevailing in their country of birth. Instead, differences between younger and older children would result from differences in the treatment they received from parents rather than from reactions to spending more or less time under authoritarian regimes. One practical example of this that appears in the comparative and development economics literature is families who disproportionately rely on older children to work or con-

tribute to the raising of younger children.

This particular scenario is unlikely to apply to our sample of refugees; data detailing demographic characteristics of the Jewish refugee population from the Soviet Bloc suggest that families were very small (averaging fewer than two children), making it unlikely that parents would be overextended enough at home to rely on older children to raise younger ones. Additionally, most adults in this immigrant group reported having college or advanced degrees themselves and were likely to prefer that their children study rather than earn an income while they were dependents (Chiswick, 1993; Simon, 1985).

Still, we test for evidence of the possibility that older children might have been treated differently explicitly in Appendix D. First, we replicate our main results on a restricted sample of just families with two children. This precludes the possibility of large families in which older children might necessarily have more responsibility for younger children because parents are overextended; our results are consistent with Section 3. We also test for the converse of this situation. We restrict our sample to just families that have more than two children and drop the oldest child. This tests the possibility that the effects we report are simply the effects of being the oldest child made more engaged and conservative via the endowment of more general responsibility for family welfare. Our results remain unchanged in this specification too, represented in Tables D.6 and D.7, suggesting that this is more consistent with exposure than an "oldest child" effect since it is just as likely to appear in second oldest children relative to younger siblings.

Finally, we replicate our results on a sample of families with children relatively close in age. We do this because we expect relatively few differences in how parents might bring up children who are close in age and at similar stages of development at any given time. While some studies have suggested that parents are indeed likely to supervise older children more intensely than younger children (Averett, Argys and Rees, 2011), there is little evidence that parental supervision itself produces a systematically different outlook or behavior among children. The majority of empirical research into the relationships between birth order and psychological, sociological, or political outcomes has consistently found no systematic patterns connecting birth order to any of these (Ernst and Angst, 1983). Researchers examining the connection between birth order and ideology, policy views, and political engagement have pointed out that birth order is no more predictive of any of these than other covariates like age or gender (Urbatsch, 2014). Even if we

think that persistent differences in relationships between parents and children of different ages exist on average, we can reduce the potential that this might confound our results by looking at families with children very close in age, because parents will not be meaningfully older or likely to be facing considerably different circumstances. We report these results in Tables D.8 and D.9, and they are also consistent with our main results in Section 3.

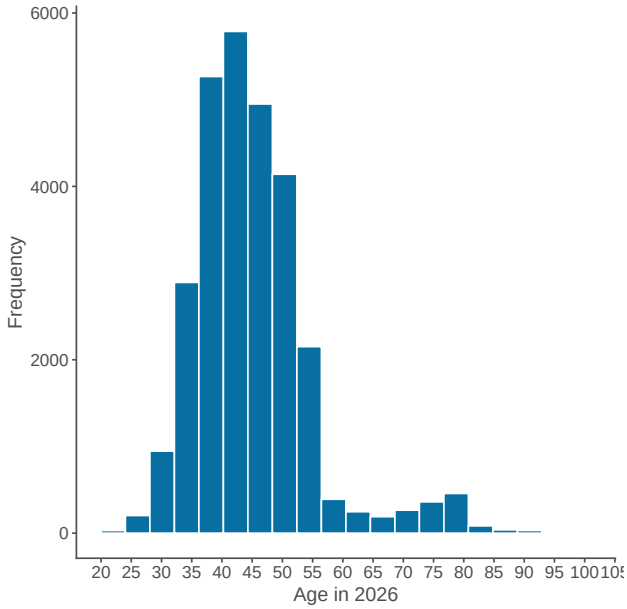
4.3 Are We All Just Getting Old? The Secular Effects of Aging on Participation and Partisanship

The robustness checks we discuss above also help allay another threat to inference, which arises in the possibility that the effects we observe are just a secular effect of aging. The effects we report are, mechanically, age differences between siblings at the time of emigration, and we interpret them as the different doses of exposure to state repression that each sibling received. But age differences between siblings remain exactly parallel throughout their lives, so one particular concern about this identification strategy might be that increased participation and willingness to affiliate with right wing parties are functions of something that happens later in life. Older siblings, in other words, might just be more likely to turn out and to register as Republicans *just because they are older.* Several researchers have documented the positive relationships between age and both turnout and Republican registration in aggregate US national survey and registration data (Fowler, 2017; Fraga, 2018; Hillygus and Holbein, 2020).

There are several reasons we believe that our results are driven by exposure to state repression rather than aging. Firstly, although the refugees who arrived as children range from 23 to almost 99 years old in 2026, the vast majority of these child arrivals throughout the years currently fall within a relatively narrow age bandwidth: 30-55. Figure 11 summarizes the age distribution for child arrivals we matched to the L2 data. This helps guard against one version of the aging hypothesis, where what we observe is a result of a sample disproportionately tilted toward refugees who are older today. Figure 11 shows that this is not the case.

Secondly, it is important to remember that our research design inherently focuses on age differences *within families.* As we show in the previous section, we are typically comparing siblings who are within 10 years of age from one another, and we show that our effects are

Figure 11: 2026 Ages of Refugees Arriving as Children



robust if we restrict our sample to siblings who are within 5 years of one another in Appendix D. This helps guard against the possibility that our effects are driven by a secular effect of aging precisely because people do not do a great deal of aging, at least in political terms, in 5 year bandwidths. That is, while we typically think that there are major differences in the likelihood of voting between someone who is 20 and someone who is 45, we would expect much smaller differences in the same likelihood between someone who is 20 and someone who is 25. The same goes for party affiliation. To believe that a positive difference in the likelihood of turnout (or affiliating with a particular party) between someone who is 20 and someone who is 21 is an effect of aging, a skeptic would have to believe that an individual's probability of turnout (or affiliation with a particular party) linearly increases (or decreases) *every year*.

While the potential of aging to confound inference is an important concern, we do not believe aging drives the effects we report in Section 3. We present additional evidence against the possibility that our effects might result from the aging process itself in Appendix G, where we show that our effects are robust to estimation within narrower cohorts of current age, and generally show that the relationships between age, participation, and party affiliation are not linearly positive in this data.

4.4 Over Here or Over There? Evidence from Immigrants to Israel

Some readers might wonder if the effect that we wish to attribute to time spent in the FSU might not be better understood as a difference in time spent in America. Put differently, supposing the treatment effect is correctly estimated, is the treatment we describe what the treatment actually is? While this is a difficult question that often appears in observational (and even sometimes experimental) work, we do have some evidence to justify the view that these effects are likely driven by what was happening in country of departure rather than the country of arrival.

First, and most simply, the effects we estimate are between siblings in families who immigrated at the same time, therefore there is no variation in duration of exposure to country of arrival. A family with a three year old and thirteen year old who arrived in America the day the Berlin Wall fell would have children that are roughly 30 and 40 years old today, both of whom would have experienced 27 years in the United States and 3 years behind the Iron Curtain, with the older sibling getting an additional ten years of life lived in the FSU. With no variation in time of exposure to the country of arrival between siblings, it appears implausible to attribute the effects we observe to the time spent in country of arrival.

While the argument that explanation follows variation is straightforward, it also proceeds a bit too quickly. Even if there is no meaningful variation in the quantity of exposure to the receiving national environment, there are certainly important differences in the ages at which each individual was exposed. Continuing the example above, the older sibling experiences ages 3-13 in FSU, while the younger sibling experiences ages 3-13 in USA. The difference in which years are spent where likely matters for all sorts of phenomena: for example, the older sibling is more likely to speak with a detectable accent than the younger one on account of early childhood being an important window for language acquisition. Our argument against this sort of interpretation is not that "critical ages" are implausible theoretically for this (or any other) phenomena. Rather, our argument is that we find no evidence to support a critical age hypothesis after extensive investigation. Appendix F reviews our findings. If the particular age at which one immigrates does not appear to matter for the political behaviors we investigate, but differences in age at arrival do, it supports the view that it is the additional exposure to the country of origin that matters.

As a more direct test of our argument, we examine another national context which received a similar population and where we have survey evidence to investigate the existence of similar patterns. After the United States, the most frequent destination for Jewish refugees from the Soviet Bloc was Israel. Today, these refugees make up approximately 15% of Israel's 7.7 million inhabitants. Using data from the Israel Polarization Panel Dataset (Gidron, Sheffer and Mor, 2022), we replicate our analysis of the relationship between age upon arrival to a democratic settlement country, political participation, and party affiliation. We provide more detail on the survey, our estimation strategy, and additional analysis in Appendix L.

Table 4: Age at Arrival and Turnout (Israel)

	Dependent variable:			
	Voted 2015	Voted 2019	Voted 2020	Voted 2021
Age at Arrival	0.006***	0.001	0.002	0.001
	(0.002)	(0.002)	(0.002)	(0.002)
Male	0.069	−0.052	−0.020	0.098
	(0.049)	(0.035)	(0.015)	(0.069)
Immigration Year Dummy	✓	✓	✓	✓
Observations	282	102	180	134
R^2	0.191	0.466	0.276	0.223

Note: *p<0.1; **p<0.05; ***p<0.01
Standard errors clustered by region of residence.

Table 4 summarizes the relationship between age at arrival and turnout in a series of election years for immigrants from the Soviet Bloc to Israel. In each election year, outcomes are just binary indicator variables for whether respondents reported voting in that year. Bearing in mind the restrictions on power we face in this sample relative to our US data — especially for survey waves asking about elections after 2015 — these results are consistent with US data. Each additional year of exposure to the political repression in the Soviet Bloc (that is, arriving to Israel one year later) is associated with a slightly higher probability of turning out in each election year. In 2015, the 0.006 estimate is 0.7% of average turnout (77.5%).

Table 5: Age at Arrival and Party Affiliation (Israel)

	Dependent variable:	
	Would Vote for Right Wing Party	Did Vote for Right Wing Party
	(1)	(2)
Age at Arrival	0.008***	0.008**
	(0.002)	(0.004)
Male	0.086**	0.112*
	(0.041)	(0.062)
Immigration Year Dummy	✓	✓
Observations	282	282
R^2	0.221	0.198

Note: *p<0.1; **p<0.05; ***p<0.01
Standard errors clustered by region of residence.

Respondents in the Israel Polarization Panel were asked which party or parties they might support "if Knesset elections were held today" in waves 1 (2019), 2 (2019), and 8 (2021) of the study. Additionally, respondents were asked which party or parties they supported in the most recent Knesset elections relative to the panel month. Table 5 summarizes the relationship between age at arrival and a binary indicator for respondents who said they might or did support a right-wing party[9] in any wave of the survey.

Here, too, people who arrived from the Soviet Bloc at older ages are more likely to report supporting right-wing political parties. This is approximately a 1% effect for each additional year of exposure to the Soviet Bloc relative to overall support for right-wing parties in the sample. All specifications reported in Tables 4 and 5 are estimated using ordinary least squares regression; using logistic regressions that constrain outcome variables to be binary, as they are in our data, does not change our substantive conclusions (see Appendix L). These results are unique to immigrants from the Soviet Bloc; Appendix L shows that immigrant respondents from countries outside of the Soviet Bloc who emigrated at older ages are significantly less likely to support right-wing parties. The fact that arriving later in life, after having had more extensive exposure to life under a repressive authoritarian government, manifests in higher levels of political en-

[9]We classify Habayit Hayehudi, Hazionut Hadatit, Kulanu, Tikvah Hadasha, Yahadut Hatorah, Yemina, Zehut, Israel Beitenu, Likud, and Shas as right-wing parties based on Hazan (2021), but our results replicate if we restrict this list just to people who reported supporting either just Likud, Israel's primary right-of-center party, or Likud and Israel Beitenu (the latter party advocating for Russian speakers' interests in the country).

gagement and willingness to support right-of-center parties in Israel, as well as the US, makes it less likely that the main effects we report are a function of something idiosyncratic to the US.

5 Conclusion

Approximately three million refugees have entered the US since the federal Refugee Resettlement Program was established in 1980, making it the largest refugee resettlement program in the world (Budiman, 2020). People seeking refuge from war, environmental or humanitarian crises, or states that targeted and oppressed them had been arriving in considerable numbers since long before the US established "refugees" as a legal category of immigrants in the wake of World War II. Almost by definition, this means that a significant proportion of both refugees and the larger immigrant population settling in the US came from countries without broad civil rights protections or participatory democratic institutions. In fact, approximately 65% of the 482,579 refugees resettled in the US between 2011 and 2022 came from countries Freedom House categorizes as "not free" in the sense that they do not preserve citizens' civil or political rights (Refugee Processing Center, 2022), and refugees from many of these countries were subjected to severe political repression before emigrating.

This group of Americans has enormous potential to impact US politics. Indeed, they already have. By recent estimates, 23.2 million of the people eligible to vote in the 2020 presidential election, or one-in-ten eligible voters, were naturalized immigrant citizens — a number that has more than doubled since 2000 (Budiman, Noe-Bustamante and Lopez, 2020). In 2020, 50.7% of foreign-born respondents to the US Current Population Survey who claimed to have voted in the 2020 general election listed countries of birth with Polity scores below 6, the cutoff the Center for Systemic Peace typically uses to indicate democracies (Flood et al., 2022; Marshall and Gurr, 2020).

While registration and turnout estimates specific to refugees are unavailable, we know that rates of naturalization for refugees and asylees have been high relative to other immigrant subgroups. For instance, 66% of refugee arrivals between 2000 and 2010 attained citizenship by 2015 (Mossaad et al., 2018). Once immigrants are naturalized, they become eligible to register and vote in federal and state elections and to donate to political campaigns.

Despite the size and significance of migrant populations, not just in the US but also in many national contexts, social scientists are only beginning to understand the way that their pre-migration experiences shape the political attitudes and behaviors they adopt upon settlement. Scholars of political socialization have long understood that the places in which we grow up, as well as the social, political, and economic contexts prevailing in them during our formative years, have an enormous effect on our levels of political engagement and our political affiliations. Going through formative experiences such as military conscription, state repression, unrest, or public prejudice can have lasting effects on political views for whole cohorts (Harvey, 1972; Sebert, Jennings and Niemi, 1974; Beck, 1977; Tedin, 1980; Lajevardi, 2020).

Our results, based on one of the largest samples of information on refugees to the US analyzed to date, shed considerable light on the way suffering targeted political repression affects those who go on to emigrate to democratic countries. Under the assumption that families generally stick together before, during, and after migration, we considerably expand on the extant literature by making it possible to identify the effect of an additional year spent under a repressive regime on voting and party affiliation. Our findings on party affiliation in this population are broadly consistent with a growing survey literature on the political attitudes and behaviors of immigrants coming to democracies from various types of left-wing authoritarian regimes.

We recognize that, even in light of the research presented here, a vast amount of heterogeneity in the regimes that drive refugees out remains to be explored. These regimes subject their citizens to different cultures, educational systems, levels of repression and a host of other "treatments" before those citizens have a chance to emigrate. All of these elements can have deep and persistent effects on the political and social worldviews refugees take with them when they leave (Goldenberg and Saxe, 1996). Ascertaining which features of life under authoritarian regimes are the primary drivers of the political behaviors we can observe in immigrants' new home countries is a fruitful direction for future research.

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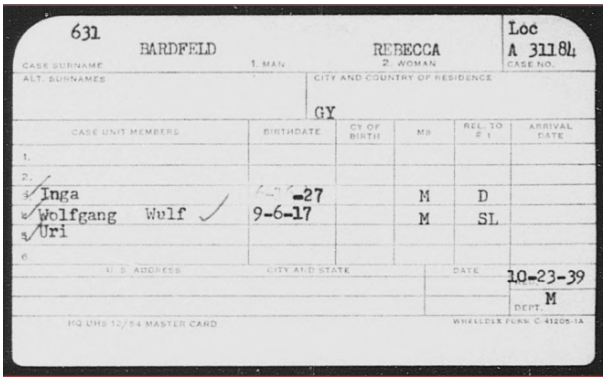
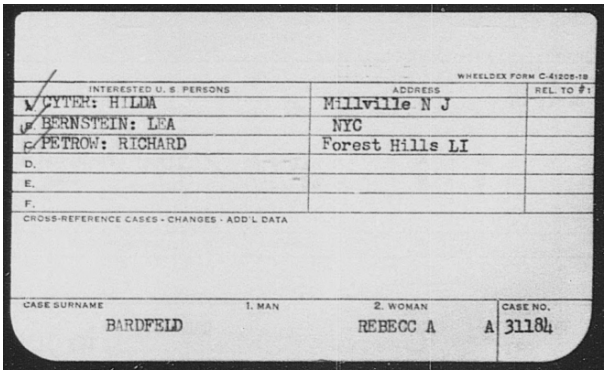
Appendices

A HIAS Refugee Records

Figure A.1 provides a sample HIAS record for refugees immigrating prior to 1980. This contains case number, names, dates of birth, gender, relationships to the primary record-holder, arrival dates, country of residence, and interested persons (most likely sponsors) along with addresses.



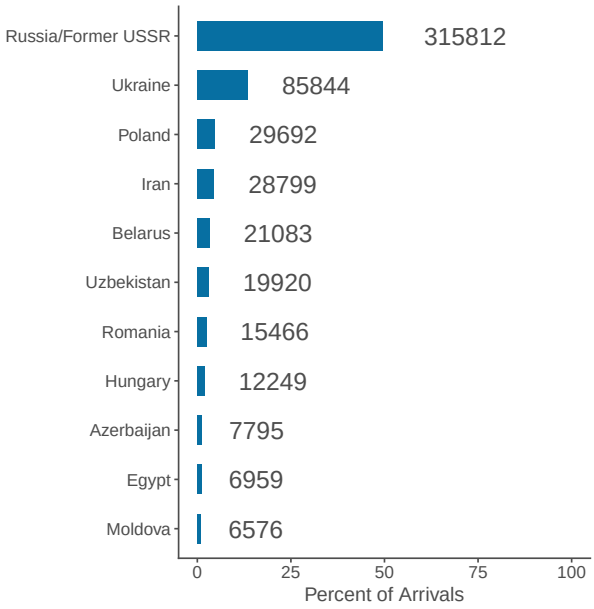
(a) Front



(b) Back

Figure A.1: An example card from the administrative files, 1955-1980. This sample record is publicly available via HIAS's weblog describing the digitization of the organization's records.

Figure A.2: Most Common Countries of Birth for HIAS Arrivals



B Matching HIAS/AJHS Client Files to the L2 Voter File

B.1 Narrative Overview

The HIAS organization keeps detailed administrative records describing refugee families that it has helped resettle since its founding in 1881. Our project relies on these records; subsets of the information they contain have been digitized through a collaboration with the AJHS. For refugee clients[10] who immigrated (roughly) from 1980-2016, the AJHS database provides information that includes: first and last name, country of birth, arrival date, and case number. By definition, individuals with common case numbers are people who immigrate together.[11] Manual inspection of these files revealed that single case numbers almost always correspond to nuclear families. In some cases, however, these include cousins, in-laws, or more distant relations.

For clients who immigrated between (roughly) 1955 and 1980, the AJHS database contains more extensive information in addition to what is available for post-1980 arrivals. These older records also include: birth date, marital status, relationship to the head of household, arrival address, and the names of related cases and interested persons. For records of refugees who

[10]Visa status is not explicitly available in the HIAS data. The first call for countries around the world to develop formal processes for admitting refugees came from the United Nations following the refugee crisis precipitated by World War II. Many of the HIAS records pre-date the creation of the United Nations itself, and certainly pre-date the US's responses in the form of the 1953 Refugee Relief Act and Refugee Act of 1980. However, internal HIAS memoranda refer to the clients for whom they process immigration applications as refugees, so we assume that the majority of people in this data have this status. This is especially likely for post-1990 arrivals, to whom the Lautenberg Amendment presumptively granted refugee status.

[11]Just 5.5% of case numbers in the original HIAS data are associated with more than one arrival date, and 0.11% of case files in the data we successfully merge to L2 have multiple dates. We drop case files with more than one arrival date from our analysis.

arrived before 1980, The AJHS also provides scanned copies of the original index cards HIAS used to record the data, making it possible to recode the originals and resolve ambiguities. We provide an example of these index cards, as well as summary statistics characterizing the original HIAS data, in Appendix A.

The digitized HIAS records describing pre- and post- 1980 arrivals contain information on 635,964 individuals across 226,577 case files (families). We merge HIAS and AJHS refugee records to individual voter records made commercially available by L2. L2 curates records of approximately 208 million U.S. voters, providing a large list of covariates describing each voter, including: full name, registration status (active or inactive), birth date, age, race, gender, address, turnout history, and party affiliation. Our L2 data covers the years 2014-2022. Like all voter records, L2's data are limited to U.S. citizens who have, at some point in the past, registered to vote. Residents who have never registered to vote do not appear in voter files, so there is no way to obtain information on non-voters from these files or to strictly distinguish between people who have truly never voted, people who do not match to the files as a result of having been purged off voter rolls, people whose names had been mis-transcribed in either data set, or people who changed their names at some point after immigration and registered under new names.

We engage in a step-wise matching procedure for both the pre-1980 and post-1980 immigrants. For each of the post-1980 immigrants, we search through the voter files for individuals with active registration that have the same first and last name. If we find a unique match, we accept that match and remove it from the pool of immigrants we are attempting to match. With the new, smaller set of unmatched immigrants, we look for unique exact matches on first and last name, now including inactive registrations as well. Successfully matched names are removed from the pool. With the remaining unmatched immigrants, we again look through the entire voter file for individuals with the same last name and one character edit to the first name to account for possible transcription errors. At this point, we stop searching for more tenuous matches of the post-1980 cohort. Our strategy for finding the pre-1980s immigrants proceeds similarly with a search for unique exact matches according to some restrictive set of criteria, removing successful finds from the pool, and then matching the leftovers against some less restrictive criteria. We iterate this process through more filters than in the case of the post-1980 group, because we have more relevant data. Full details on our step-wise procedures are available in Appendix B.

Since the L2 data does not contain countries of birth for voters, our merging procedure relies on first and last name for the post-1980 refugee cohort and on names and birthdates for the pre-1980 cohort. This is, admittedly, less information than we might like to have in order to merge across data sets. We are, however, still confident in the quality of this effort to merge using names for several reasons. First, the immigrants in the HIAS data had idiosyncratic surnames as compared with American natives, particularly those largely Soviet-Bloc immigrants that arrived post-1980. For example, Appendix Figure B.3 shows that almost 70% of surnames in our data are shared with fewer than 100 registered voters, whereas almost 90% of registered voters share a surname with more than 100 people. To reach an equal or better level of distinctiveness as one finds with these last names in the general voter population one would need to condition on birth year and birth month as well. Even within the HIAS data, 83.4% of first and last name combinations are unique. If we include information on arrival years, 94.5% of first name, last name,

and arrival year combinations for refugees in the HIAS data do not have duplicate entries. This also makes the merging exercise easier in the sense that we have few duplicates in the original data for whom we need unique matches and further suggests that this group of immigrants has largely distinctive names. Additionally, HIAS often recorded birth date for pre-1980 arrivals. The matching exercise for this group can take advantage of the fact birth dates are highly discriminating and surnames are fairly discriminating, while for post-1980 the matching exercise can take advantage of the fact that surnames for this group are particularly unique. Finally, in all cases, we can use arrival dates to remove likely false positives arising from cases where birth dates (from the voter file) post-date arrival dates (from the HIAS data).[12]

B.2 Matching Process in Detail

Our matching procedure for both the pre-1980 and post-1980 immigrants proceeds in a stepwise fashion. For each of the post-1980 immigrants, we search through the voter files for individuals with active registration that have the same first and last name. If we find a unique match, we accept that match and remove it from the pool of immigrants we are attempting to match. With the new, smaller set of unmatched immigrants, we look for unique exact matches on first and last name, now including inactive registrations as well.[13] Successfully matched names are removed from the pool. With the remaining unmatched immigrants, we again look through the entire voter file for individuals with the same last name, but now allow for one character edit to the first name. Particularly with foreign names, minor transcription errors are fairly common. Any successful unique matches are added to our dataset. At this point, we stop searching for more tenuous matches of the post-1980 cohort. Table B.1 shows the number of matches we obtain at each stage. In total, we match 119,603 individuals, a success rate of around 26%.

Table B.1: Matching process post 1980.

Stage	Unique	Given Name	Surname	Voters	Clients	Matches
1	✓	Exact	Exact	Only Active	All Post 1980	100,763
2	✓	Exact	Exact	All	Stage 1 Misses	4,339
3	✓	One letter edit	Exact	All	Stage 2 Misses	14,501

Our strategy for finding pre-1980 immigrants proceeds similarly: we search for unique exact matches according to some restrictive set of criteria, remove successful finds from the pool, and then match the leftovers against some less restrictive criteria. We iterate this process through more filters than the post-1980 group, because we have more relevant data. In particular, the index cards more or less exactly record birth dates. Individuals are with some frequency recorded as having two names. Sometimes these two names appear to be first and middle, while other times it appears to be a more anglicized alternative (e.g. Dawid vs. David).

[12] This is only 1.6% of cases in the merged data and we drop them from our analysis.

[13] If we did not distinguish between active and inactive in this way, then no individual with multiple registrations, some active and others inactive, would end up being included in our sample.

In such cases, it is hard to know which given name to search for in the voter file; if an index card describes an immigrant named Ben David, does one expect to find that person registered as Ben, David, or Ben David? Therefore, we consider the possible variations on the name that are contained in the cards. In particular, we apply our iterative procedure for searching for unique exact matches against the following pieces of information: (1) birth date, transcribed given name, transcribed surname; (2) birth year, given name, surname; (3) given name, surname, birthday, birth month, birth year $\pm$ 1 or birth year $\pm$ 2; (4) surname, birthdate, plausible variations on the given name; (5) birth year, surname, variations on given; (6) surname, variations on given, birthday, birth month, and birth year ± 1 or ± 2; (7) birth date, surname, two character edits to the given name, (8) birth date, surname, two character edit to variations on the transcribed given name. Table B.2 describes how many matches each step generates. In total, we match 6,140 individuals, a success rate of around 5%. The much lower success rate is not surprising given the median age of a pre-1980 immigrant at present writing is 96.

Table B.2: Matching process pre-1980.

Stage	Unique	Given	Surname	Birth Day/Month	Birth Year	Voters	Clients	Matches
1	✓	Exact	Exact	Exact	Exact	All	Pre-1980	2,664
2	✓	Exact	Exact	.	Exact	All	Stage 1 Misses	1,360
3	✓	Exact	Exact	Day and Month	$\leq \pm 2$	All	Stage 2 Misses	127
4	✓	Variations	Exact	Exact	Exact	All	Stage 3 Misses	439
5	✓	Variations	Exact	.	Exact	All	Stage 4 Misses	258
6	✓	Variations	Exact	.	$\leq \pm 2$	All	Stage 5 Misses	36
7	✓	2 Edits to Exact	Exact	Exact	Exact	All	Stage 6 Misses	1,078
8	✓	2 Edits to Variations	Exact	Exact	Exact	All	Stage 7 Misses	178

B.3 Representativeness

One important question is whether and how focusing on individuals who match to a voter file differ from those in the larger client population. Table B.3 examines how the sample of matched immigrants differs from the sample of unmatched immigrants in the administrative file for the pre-1980 cohort. While similar analysis is desirable for the post-1980 population, we simply lack the necessary information to do this analysis. The table reveals that the interaction of marriage with naming conventions has a substantial impact on the sub-sample. There are fewer women in the matched sample than the administrative file. If an individual was separated at the time of immigration, they are relatively more likely to match. The fact that people known to be married are a smaller part of the matched sub-sample is initially curious, however it is important to recognize that someone who was already married sometime in the 1955-1980 time frame is likely relatively old by 2018, when we search for them in the voter files. The more likely a person is to be deceased, the less likely they are to appear in the voter file. Indeed, immigrants who are in families with children are relatively more common in the matched sample than in the initial administrative file, which again makes sense given aging dynamics. Finally, it seems that matching against another set of records has induced some selection on administrative data quality. 18% of the individuals

in the administrative files have no gender indicated, whereas only 3% of the matched sample are missing gender in the HIAS file.

Table B.3: Sample characteristics of the pre-1980 client population before and after matching

	Mean (Unmatched)	Mean (Matched)	Difference (Standardized)
Deceased	0.003	0.000	-0.079
Divorced	0.014	0.003	-0.113
Engaged	0.000	0.000	-0.005
Married	0.693	0.411	-0.593
Separated	0.258	0.584	0.701
Widowed	0.032	0.001	-0.240
Unknown Marital Status	0.379	0.390	0.023
Female	0.420	0.311	-0.228
Unknown Gender	0.179	0.032	-0.492
Family Size	3.433	4.118	0.467

On the one hand, the difference between the matched and unmatched samples may lead to concerns about how representative the families we study are as compared with the typical family assisted by HIAS. We discuss external validity concerns at greater length in the manuscript, but it is worth noting here that the national origin of the typical HIAS immigrant changes drastically over decades, so the representative immigrant family is a strained notion to begin with. Moreover, we find that our results are robust to these drastic changes in national origin of the client population.

On the other hand, the difference observed in the balance table may raise questions about match quality. It is difficult to directly test the proposition, but we do have some indirect tests we can do. For one, the administrative case files describe a small percentage of deceased individuals. Encouragingly, none of our immigrants known to be deceased prior to 1980 appear as active or inactive voters. Another indirect test is that voter files often include gender and so do the HIAS administrative files, but gender is not used in the matching procedure. In greater than 98% of the matched cases, these two genders are concordant. It would be surprising, given the possibility of transcription and intake errors in both files, if the number of matches was 100%.

B.4 How Distinctive Are Soviet Jewish Names?

Several important considerations about the underlying data merit mention in thinking about how to implement the name matching. First, the immigrants that HIAS helped come to the US often had idiosyncratic surnames as compared with American natives, particularly those largely FSU-born immigrants that arrived post-1980.

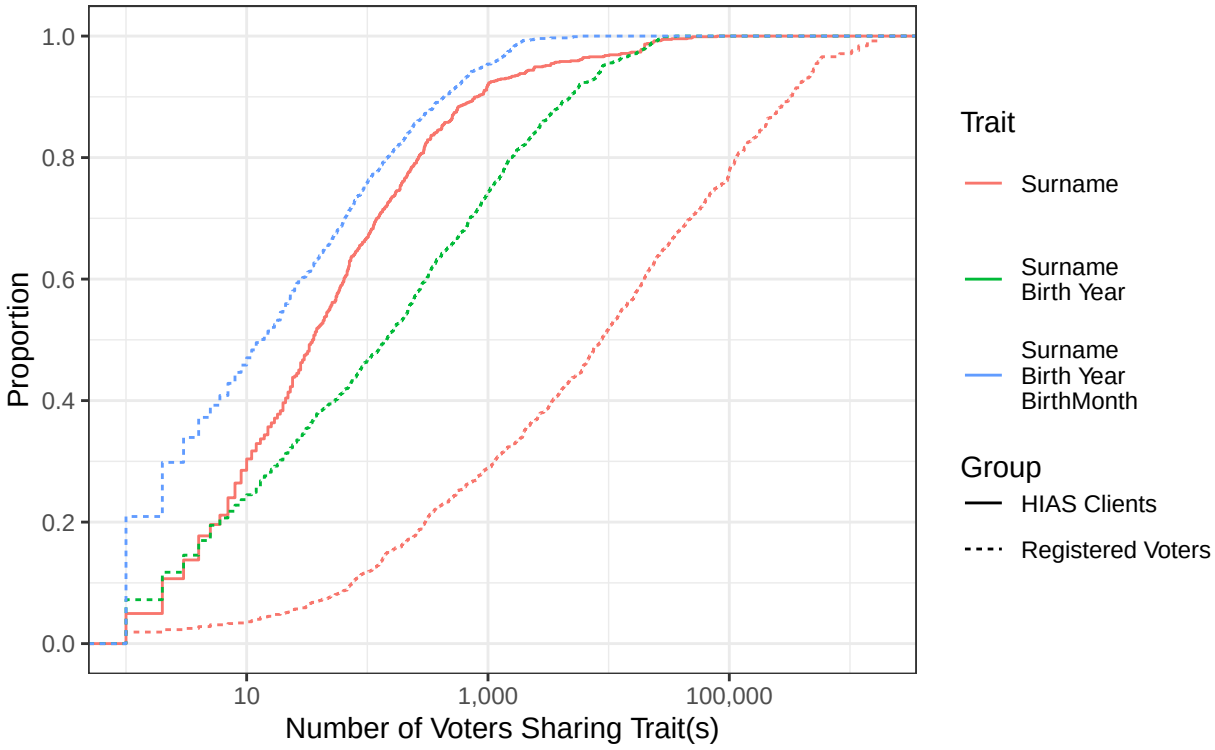
To take a concrete example, there are fewer Shteyngarts in the voter file than Steins, or even Steins born in 1972. While it is certainly possible that there are multiple individuals named Gary Shteyngart in the US, one or more registered and perhaps some not, it is also possible for individuals named Gary Stein to share the same birthday or other matching characteristics. Our analysis

shows, however, that the vast majority of the in-sample surnames are shared by fewer than 100 voters, and as a result it does not surprise us that when we do have birth dates and surnames, the former does not actually do all that much to change which immigrants we are able to match to voter files and which we cannot. This leads to our second point about matching, which is that while HIAS was not always perfectly reliable about filing out all available fields in their old index card system, they did often record birth-dates using that system. Pre-1980, the matching exercise to voter files can take advantage of the fact that birthdates are very frequently available. While it is of course the case that better matching might be possible with more data on individuals, especially birth dates for the post-1980 cohort, generally these data are exceedingly hard to find. The government's version of these administrative files will only become available to scholars when these individuals are older than 100, which is to say in 30 years for the oldest immigrants in our files.

Our matching approach for post-1980 immigrants relies explicitly upon the assumption that the surnames in this group are distinctive. While this is not necessarily true for all individuals in this client population, it is often true. In presenting our work, we have sometimes been asked how to consider how distinct these last names really are. Figure B.3 presents a thought experiment. We can think about how distinctive an registered voter's last name typically is by considering the number of other individuals sharing that name. As the figure shows, the typical voter shares a surname with a few thousand other voters in the US, it is rare for a voter to share a last name with only a few dozen others, and one in five has a surname such as "Johnson" shared by hundreds of thousands of other voters. If we consider the combination of surname and birth year, the distinctiveness of voters is several orders of magnitude higher. One in five voters will share a surname and birth year with roughly 1,000 other voters, while for the median voter the number of individuals sharing a last name and birth year could fit in a typical classroom. If we think about the combination of birth year, birth month and surname, the median registered voter would share this combination of traits with about 10 people. Only about 5% of registered voters share this trait with 1,000 others or more.

Calculating similar statistics for the last names found post 1980, we see that such surnames have a discriminating power that is close to the power of conditioning on last name, birth year, and birth month in the general population. 70% of these immigrant last names are possessed by 100 registered voters or fewer. This exercise provides some confidence that our matching approach, based additionally on first names and uniqueness constraints, is quite conservative in the sense that our matches are very likely true. At the same time, these statistics may give some indication why relaxing the uniqueness, first or last name constraints give us pause.

Figure B.3: Distinctiveness of Surnames in Sample v. Registered Voters



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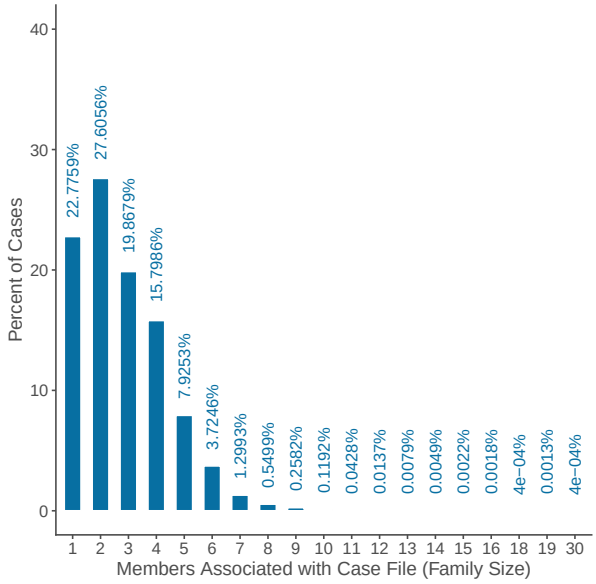
C Representativeness of the Merged Data

Figure C.4: Family Sizes

(a) HIAS Data



(b) Merged to L2

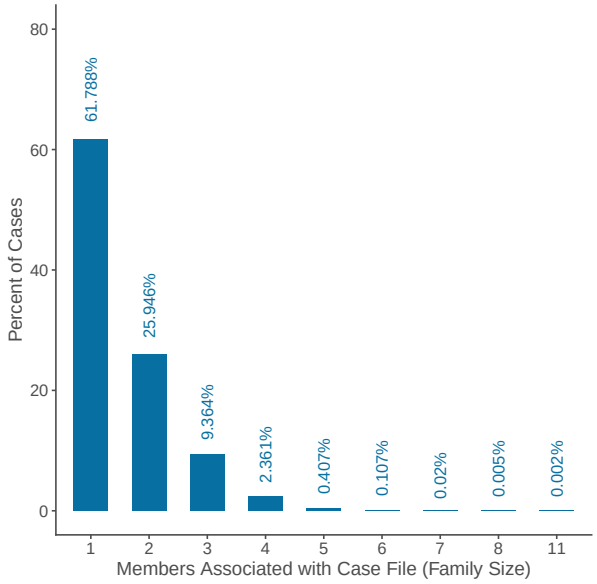
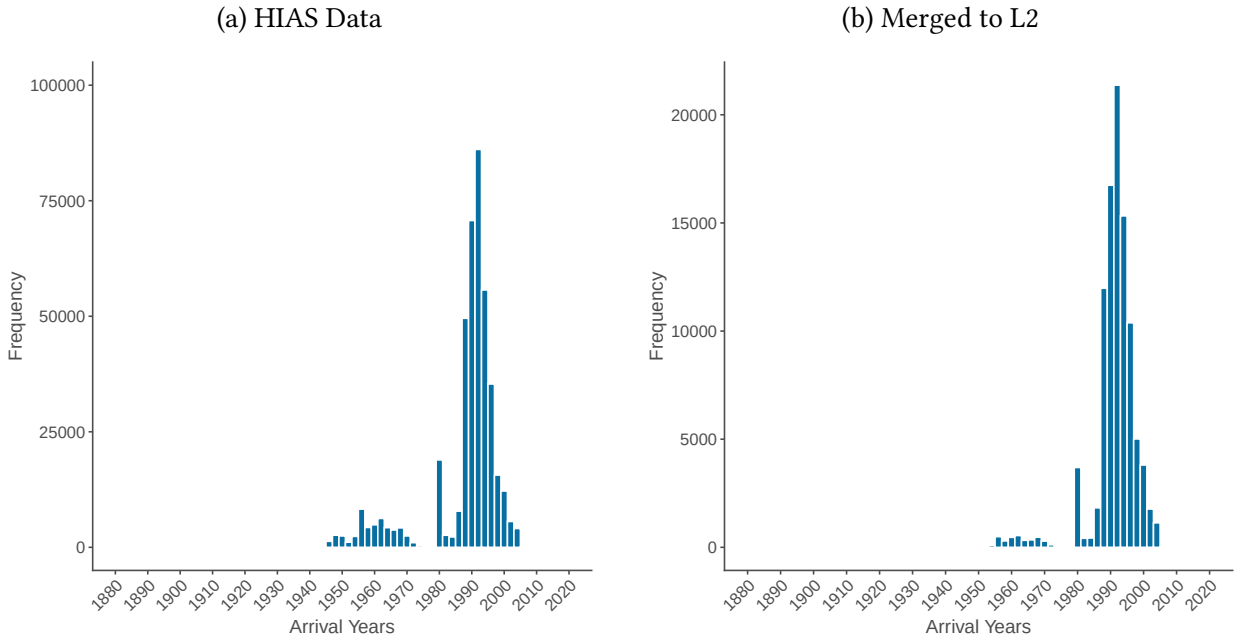


Figure C.5: Arrival Years



D Family Structure Robustness Checks

Tables D.4 and D.5 reports our main results from Section 3 on a sample of children from families with only two children. This restricted sample is one in which it is unlikely that oldest children bear disproportionate responsibility for tending to a large group of younger siblings. These findings are consistent with the results presented in the manuscript, which makes sense given that most families in this population of refugees had two or fewer children.

Table D.4: Effect of Age at Arrival on Participation for Families with 2 Children

	Turnout					
	2012	2014	2016	2018	2020	2022
Age at Arrival	0.007***	0.004***	0.007***	0.003*	0.007***	0.005***
	(0.002)	(0.001)	(0.002)	(0.002)	(0.001)	(0.002)
Female	−0.027*	−0.027***	−0.023	−0.052***	−0.047***	−0.076***
	(0.016)	(0.009)	(0.014)	(0.017)	(0.011)	(0.013)
Family Fixed Effect	✓	✓	✓	✓	✓	✓
Adjusted R^2	0.205	0.194	0.135	0.224	0.100	0.133
Residual Std. Error (df = 3143)	0.404	0.298	0.461	0.421	0.453	0.464
Note:						*p<0.1; **p<0.05; ***p<0.01
						Standard errors clustered by state.

Tables D.6 and D.7 report the effect of an additional year of exposure to a left-wing authoritarian regime on voting (conditional on registration) and party registration when we restrict

Table D.5: Effect of Age at Arrival on Party Registration for Families with 2 Children

	Party Affiliation		
	Republican	Democrat	Nonpartisan
Age at Arrival	0.009***	−0.008***	−0.001
	(0.002)	(0.001)	(0.002)
Female	−0.028**	0.039***	0.001
	(0.013)	(0.010)	(0.011)
Family Fixed Effect	✓	✓	✓
Adjusted R^2	0.163	0.197	0.112
Residual Std. Error (df = 3143)	0.414	0.413	0.457
Note:			*p<0.1; **p<0.05; ***p<0.01
			Standard errors clustered by state.

our sample to families with three or more children and drop the oldest child in order to assess whether the main effects we report in Section 3 are just the effect of being the oldest child rather than the effects of prolonged exposure to these regimes. These results suggest that our effects are not restricted to oldest siblings. Since most families in our sample have two or fewer children we lose considerable power and most results in Table D.6 fall short of statistical significance at the 5% level, but they are consistent with the main results. Children who are older (but not the oldest in their families) are still more likely to turn out and significantly more likely to register as Republicans than younger siblings. Results in Tables D.6 and D.8 are calculated using families from countries of origin that are socialist dictatorships. Controls include gender, year of arrival, state of residence, and family fixed effects.

Table D.6: Effect of Age at Arrival on Participation without Oldest Children

	Turnout					
	2012	2014	2016	2018	2020	2022
Age at Arrival	0.013	0.010	0.015	0.010	0.006	0.011
	(0.010)	(0.007)	(0.015)	(0.009)	(0.017)	(0.020)
Female	−0.041	−0.037	−0.048	−0.098	−0.074	−0.094
	(0.075)	(0.042)	(0.130)	(0.101)	(0.140)	(0.097)
Family Fixed Effect	✓	✓	✓	✓	✓	✓
Adjusted R^2	0.176	0.355	0.120	0.257	0.114	0.117
Residual Std. Error (df = 321)	0.396	0.247	0.460	0.401	0.456	0.465
Note:						*p<0.1; **p<0.05; ***p<0.01
						Standard errors clustered by state.

Tables D.8 and D.9 replicate our main results while restricting the sample to families in which age differences are relatively small: 5 years or fewer. This exercise should lower the likelihood

Table D.7: Effect of Age at Arrival on Party Registration without Oldest Children

	Party Affiliation		
	Republican	Democrat	Nonpartisan
Age at Arrival	0.008	−0.006	−0.001
	(0.013)	(0.013)	(0.013)
Female	−0.005	−0.029	−0.019
	(0.124)	(0.113)	(0.155)
Family Fixed Effect	✓	✓	✓
Adjusted R^2	0.012	0.191	0.068
Residual Std. Error (df = 321)	0.443	0.420	0.469

Note: *p<0.1; **p<0.05; ***p<0.01
Standard errors clustered by state.

that we are comparing cousins rather than siblings in the data. The fact that the results replicate in their direction and general magnitude is encouraging.

Table D.8: Effect of Age at Arrival on Participation for Siblings with Age Differences of 5 Years or Smaller

	Turnout					
	2012	2014	2016	2018	2020	2022
Age at Arrival	0.012^{***}	0.004^{***}	0.012^{***}	0.008^{***}	0.009^{***}	0.011^{***}
	(0.003)	(0.001)	(0.003)	(0.002)	(0.002)	(0.003)
Female	−0.012	-0.031^{**}	−0.032	-0.085^{***}	-0.041^{**}	-0.068^{***}
	(0.015)	(0.013)	(0.021)	(0.027)	(0.019)	(0.014)
Family Fixed Effect	✓	✓	✓	✓	✓	✓
Adjusted R^2	0.193	0.215	0.139	0.238	0.118	0.157
Residual Std. Error (df = 2403)	0.406	0.285	0.459	0.409	0.450	0.457

Note: *p<0.1; **p<0.05; ***p<0.01
Standard errors clustered by state.

Table D.9: Effect of Age at Arrival on Party Registration for Siblings with Age Differences of 5 Years or Smaller

	Party Affiliation		
	Republican	Democrat	Nonpartisan
Age at Arrival	0.006^{***}	-0.003	-0.004^{**}
	(0.002)	(0.002)	(0.002)
Female	-0.024	0.049^{***}	-0.027
	(0.023)	(0.015)	(0.026)
Family Fixed Effect	✓	✓	✓
Adjusted R^2	0.157	0.196	0.103
Residual Std. Error (df = 2403)	0.412	0.417	0.458
Note:			*p<0.1; **p<0.05; ***p<0.01
			Standard errors clustered by state.

E Gender Balance

One potential concern given the gender imbalance in our sample is that women who we successfully merge are fundamentally unrepresentative by virtue of choosing either to not get married or keep their surnames after getting married. While we cannot compare the women in our merged sample directly to women in the HIAS data who were not merged, we can see if our effects appear in families with exclusively male children. Since men are much less likely to change their names post-migration as a result of marriage than women are, this is a subset of our data where we may (1) be more confident in the quality of our merges to the L2 data and (2) be more confident in the representativeness of the data. These results appear in Tables E.10 and E.11. While these fall short of conventional levels of statistical significance because we have cut our power by more than half, these results have similar magnitudes and directions to the results we report in Section 3, suggesting that the estimates we observe are unlikely to be driven by some kind of merge error in the female half (or so) of the data.

Table E.10: Effect of Age at Arrival on Participation for Families with Only Male Children

	Turnout					
	2012	2014	2016	2018	2020	2022
Age at Arrival	0.008	0.004	0.005	0.005	0.007	0.005
	(0.009)	(0.004)	(0.010)	(0.009)	(0.006)	(0.007)
Family Fixed Effect	✓	✓	✓	✓	✓	✓
Observations	14,606	14,606	14,606	14,606	14,606	14,606
Adjusted R^2	0.165	0.202	0.111	0.236	0.104	0.159
Residual Std. Error (df = 1202)	0.427	0.325	0.469	0.423	0.441	0.459

Note: *p<0.1; **p<0.05; ***p<0.01
Standard errors clustered by state.

Table E.11: Effect of Age at Arrival on Party Registration for Families with Only Male Children

	Party Affiliation		
	Republican	Democrat	Nonpartisan
Age at Arrival	0.013	−0.007	−0.005
	(0.009)	(0.005)	(0.009)
Family Fixed Effect	✓	✓	✓
Observations	14,606	14,606	14,606
Adjusted R^2	0.133	0.224	0.128
Residual Std. Error (df = 1202)	0.431	0.393	0.455

Note: *p<0.1; **p<0.05; ***p<0.01
Standard errors clustered by state.

F The Critical Age Hypothesis

One possibility we have not yet addressed is the idea of a "critical age" or an "impressionable years" hypothesis. In general, this idea refers to the possibility that individuals form particular identities not incrementally over time, but during a particularly formative period in their youth. In the context of immigration, this hypothesis would suggest that children who live through that particularly impressionable period in their country of birth are more likely to have their political consciousness shaped by that country's political culture and institutions, while those who spend their most impressionable years in a new home will likely be most influenced by the forces of their chosen country. Thus, rather than a linear function of age, political behavior and identity begins to cement only once an individual hits a critical age threshold.

Evaluating this hypothesis directly is a challenge for a few reasons. First, there is no consensus in the literature about when exactly people enter their most impressionable years. Most agree that this happens sometime in their teenage years, but the years that bound the beginning and the end of this period remain opaque. Second, thresholds are harder to think about in the context of our within-family analysis because of the need to compare siblings. If we want the effect of

exposure rather than being over the threshold (or not), we would ideally choose families in which all of the siblings are past the threshold (or where none are, for comparison) so that the resulting differences really do represent the difference in exposure rather than the effect of crossing into a critical age realm. Again, however, this would require us to make a decision about the exact years that create the critical period.

To avoid making arbitrary decisions about thresholds, we instead estimate our main results from Section 3 with just children at every age from 5 to 21 or younger iteratively to see if the effect sizes of age at arrival relative to ones own siblings meaningfully changes at some given year. For instance, in Figures F.6 below, the leftmost point is the effect of age at arrival (relative to one's siblings) on voting in the 2016 or 2020 presidential elections for children who arrived at age 5 or younger. The next point over estimates the same thing, but includes 6 year-olds (and their siblings, provided they are similarly 6 or younger), etc. The existence of a critical age range, say 14, might produce a marked shift in the point estimates.

We find limited evidence for the possibility of a critical age effect. We gain precision as we allow older siblings into the sample, so error bars necessarily get narrower as we look across the plots from left to right. Yet effects for both participation and party affiliation remain similar across ages. There are no "jumps" that signal discontinuities at a particular age. To the extent that variation occurs, it happens around age 10, when effect sizes tend to stabilize. This is before the teenage period most writers focusing on the critical age hypothesis suggest is the relevant set of years.

Figure F.6: Presidential

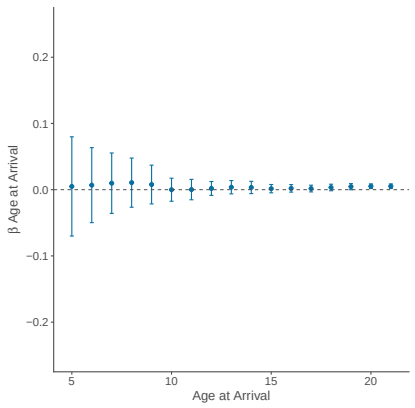


Figure F.7: Midterm

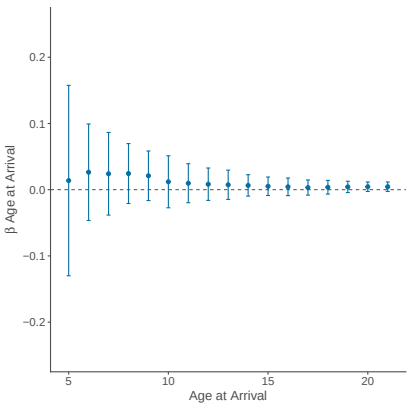
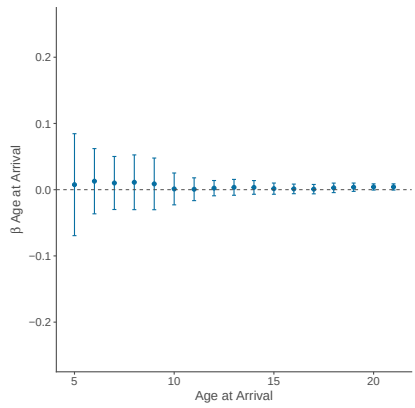


Figure F.8: Ever Voting



Another version of this hypothesis is more institutional. One structural discontinuity in age occurs when children start school and encounter, en masse, the first state institution they will likely remember interacting with. Several authors have suggested that schooling itself is a major tool of totalitarian regimes, whereby these regimes attempt to socialize citizens to the behavior they desire (Paglayan, 2024). We can test this directly to see if our effects obtain for children who would have been too young to start formal schooling prior to emigration (children under 5). These results appear in Tables F.12 and F.13. While we have considerably less power here with few very young arrivals, these results are much weaker than the results we present in Section 3, with several years of turnout effects pointing in the wrong direction and the signs for party

Figure F.9: Democrat

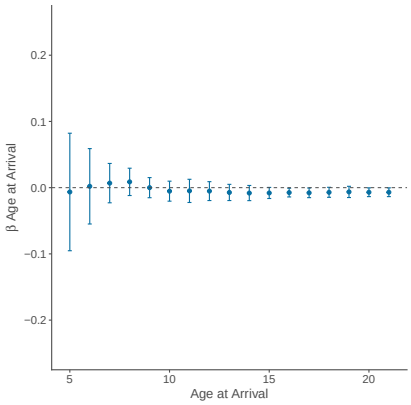


Figure F.10: Republican

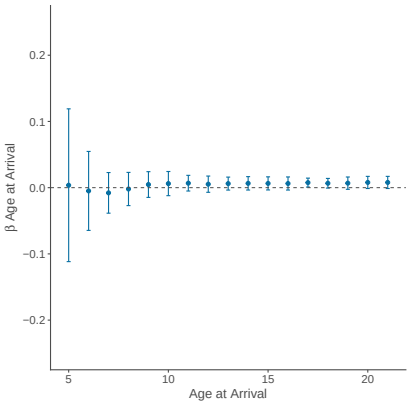
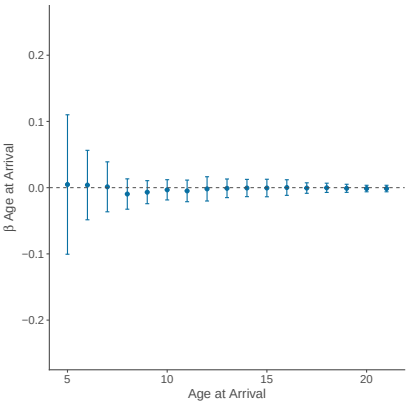


Figure F.11: Nonpartisan



affiliation reversed relative to the main results. Keeping in mind caveats about power, this may suggest that schooling and other institutions are a powerful conduit of information about the state, its willingness to repress its citizens and how it perceives various groups. While young children would have likely gotten indirect exposure to state repression via communication from their families even post-migration, the effects on them are much weaker than they are for children who stayed in Soviet Bloc countries long enough to attend schools.

Table F.12: Effect of Age at Arrival on Participation for Children Under 5 at Immigration

	Turnout					
	2012	2014	2016	2018	2020	2022
Age at Arrival	−0.011	0.004	0.003	−0.012	0.015	0.029
	(0.067)	(0.080)	(0.059)	(0.060)	(0.096)	(0.141)
Gender	0.012	−0.042	−0.165	−0.029	−0.118	0.021
	(0.226)	(0.124)	(0.144)	(0.197)	(0.423)	(0.401)
Family Fixed Effect	✓	✓	✓	✓	✓	✓
Observations	5,673	5,673	5,673	5,673	5,673	5,673
Adjusted R^2	0.264	0.467	0.184	0.427	−0.133	0.124
Residual Std. Error (df = 181)	0.361	0.205	0.437	0.345	0.516	0.460

Note: *p<0.1; **p<0.05; ***p<0.01
Standard errors clustered by state.

Table F.13: Effect of Age at Arrival on Party Registration for Children Under 5 at Immigration

	Party Affiliation		
	Republican	Democrat	Nonpartisan
Age at Arrival	−0.011	0.005	0.003
	(0.086)	(0.092)	(0.088)
Gender	−0.077	−0.024	0.092
	(0.256)	(0.206)	(0.244)
Family Fixed Effect	✓	✓	✓
Observations	5,673	5,673	5,673
Adjusted R^2	0.051	0.191	0.220
Residual Std. Error (df = 181)	0.413	0.430	0.429
Note:			*p<0.1; **p<0.05; ***p<0.01
			Standard errors clustered by state.

G Age Cohorts

As figure 11 shows, the vast majority of the refugees arriving as children who we successfully merged to the L2 data were between 35 and 55 in 2026. That range spans just two age cohorts: Millennials, born roughly 1981-1996, and Gen X, birth years 1965-1980. On the one hand, that relatively narrow span makes us confident that the effects we report are not the effects of aging, since most people in the data are relatively close in age - and closer still if we compare siblings. On the other, this covers relatively few cohorts so looking at effects by age cohort becomes impossible for most cohorts. Just 1633 observations (4.6%) in the data represent people over 65, and only 25 (0.16%) are under 26.

If our objective is to bin the data into relatively narrow slices by current age in hopes of comparing individuals who are maximally close in age, we are also presented with the challenge of how to operationalize this without doing it arbitrarily or in a way that advantages us. There are many possible approaches, with the caveat that even in the age range where data is the densest, bins that are too narrow make estimates too imprecise to be meaningful since all siblings have to be within the narrow bin and not across bins even if they are close in age. Bins that are too wide may cover to much ground substantively and lead us to compare people who truly are in different stages of life. With that in mind, we present the results of our main specifications but for everyone under 30, then 10 year age bins for 30-50, and those over 60. These appear in Figures G.12 - G.17. These show very consistent effects across age bins, especially bearing in mind that the central three age bins in each plot contain most of the data. This suggests further that the results we present are not a function of secular aging. We do not see effects systematically shifting in magnitude as we go across to older or younger age bins, and results within each relatively narrow age band are consistent with what we report across current ages in Section 3.

We conclude this section of the Supplementary Information by providing some additional evidence and argumentation against the hypothesis that our effects are driven by the secular effects of aging. In partiuclar, while it is true that relatively linear patterns similar to what we

Figure G.12: Presidential

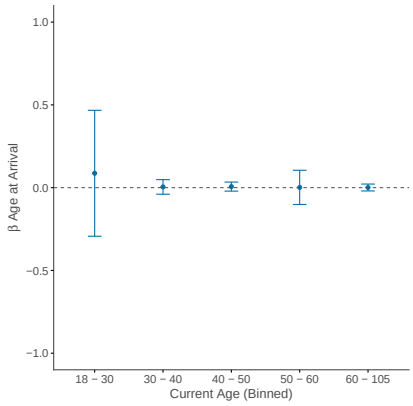


Figure G.13: Midterm

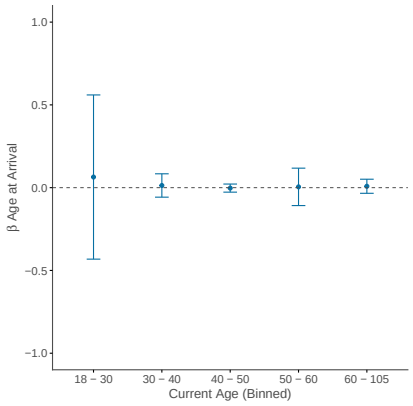


Figure G.14: Ever Voting

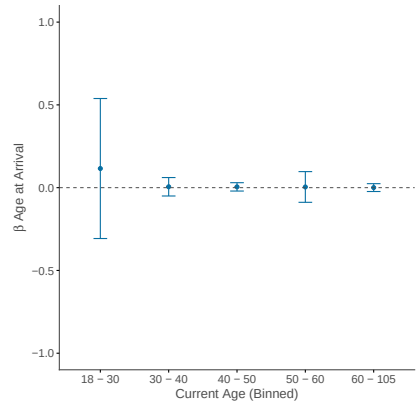


Figure G.15: Democrat

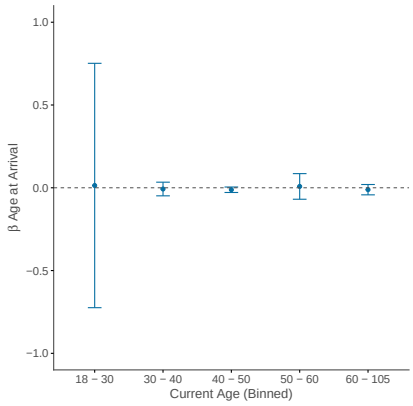


Figure G.16: Republican

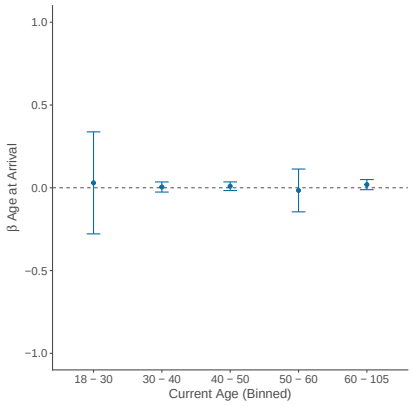
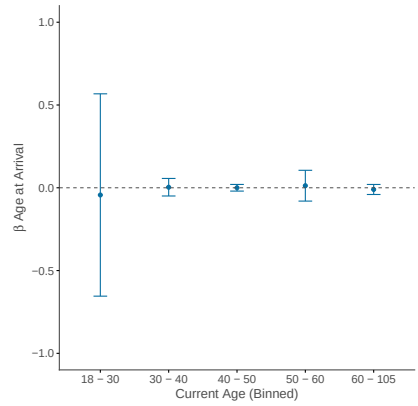


Figure G.17: Nonpartisan



describe do show up in cross-sectional data for specific elections (Fowler, 2017), cross-sections are not exactly the right comparison because the composition of the electorate at every age changes from election to election as people register or fall off the voter rolls. Te relevant question for our study is whether or not the probability of voting linearly increases with age *for the same person* - something researchers could only establish with a long-running panel data set. In addition, our own data on refugees (children and adults) merged to L2 does not bear out similarly linear patterns in participation or party affiliation by age. Figures G.18 - G.20 show the proportions of merged refugees in our data who turn out conditional on registration and register as either Republicans or Democrats. None of these patterns can be said to linearly increase with age. Participation follows a classic inverted-U shape, with the lowest levels occurring among the youngest and oldest and peak participation occurring in midlife. Similarly, the proportions registering as Republican start relatively low for those 40 and under, rise rapidly for those in their roughly 40-70s, and drop off sharply again for refugees over 80. The distribution of the proportions of refugees identifying as Democrats looks like the inversion of the distribution for Republican identifiers, but is similarly nonlinear. Even focusing on narrow windows of consecutive years across each distribution, it does not appear as though we see consistent linear increases with age; on average, the proportions of people either participating or registering with either party in the next age bin up are just

as often slightly lower than the proportions in the age bin directly below as they are slightly higher. Given these patterns in our data, along with the fact that our results in Appendix G show that restricting our data to people who are in the same age cohort *today* would not change our conclusions, make us confident that our main results are not due to any secular effects of aging on participation or party registration.

Figure G.18: Participation

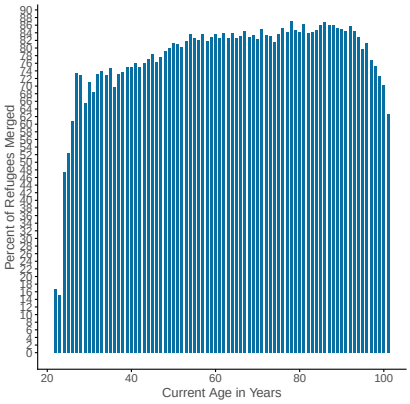


Figure G.19: Republicans

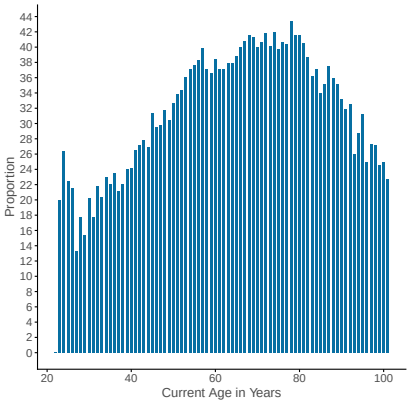
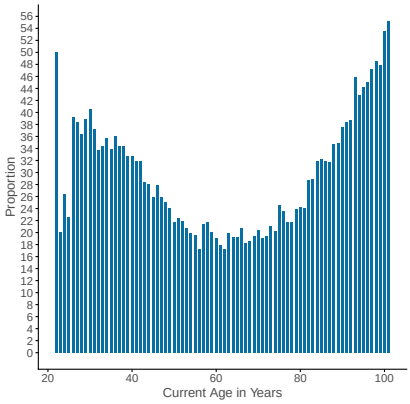


Figure G.20: Democrats



H Adults Only

Our focus on refugees who arrived as children allows us to fix factors common within families and interpret age differences between children as varying levels of exposure. Yet this approach leaves a considerable amount of data on the table. 84,530 of the HIAS-resettled refugees we successfully merged to L2 arrived in the US as adults, and we did observe relevant outcomes for these individuals. Accordingly, a natural question about this group might be whether or not they exhibit similar patterns to the child arrivals in our data. That is, do adult arrivals who come to the US older - and therefore with longer exposure periods to state repression in the Soviet Bloc - also participate more and affiliate with right wing parties more relative to younger arrivals?

We can shed some light on this question, but we have to use a slightly different approach to estimation than the one we adopt in Section 3. For adults, it makes little sense to include a family fixed effect. First, that ignores many single adults who came to the US without immediate family members. Second, those adults who did come to the US with adult family members typically came with spouses. Therefore comparisons between adults in the same family would largely be confounded by gender. While there are families in the data in which more than two adults with heterogeneous members immigrated simultaneously (e.g. families that arrived with grandparents or uncles and aunts), this is less common and estimates driven by these families might be local to a very specific type of family structure.

To get at this question, we instead estimate a model of a slightly different form:

$$y_{ij} = \alpha + \beta ArrivalAge_{ij} + \theta Female_{ij} + FamilySize_j + ProportionFemale_j + ArrivalYear_j + BirthCountry_j + \eta_{ij}$$

where y_{ij} is the outcome of individual i in family j, $Female_{ij}$ is a dummy variable for gender, $ArrivalAge_{ij}$ is age at arrival in the US, $FamilySize_j$ records the number of members in each family, $ProportionFemale_j$ reports the proportion of female family members in the family at the time of immigration, $ArrivalYear_j$ is a dummy for the family's arrival year, $BirthCountry_j$ is a dummy representing the individual's country of birth, and η_{ij} denotes the error term.

This specification breaks the family fixed effect, and is therefore open to possible confounding. However, it does allow us to do two things. First, it allows us to individually examine pre-migration family characteristics that would otherwise be obscured by family, such as family size and the proportion of women in the family. While we suppress the individual coefficients for year of arrival and birth country for brevity, this approach also allows us to control for both separately and examine any effects associated with time and country of birth. We report our results in Tables H.14 and H.15.

In these results, we separate adult arrivals (columns 1-3 in each table) from those who arrived as children (columns 4-6). There are several things worth pointing out about these results. First, even in this new specification, the coefficients describing the relationships between age at arrival and participation are positive and significant at the 5% level for child arrivals. So too is the coefficient describing the relationship between age at arrival for child arrivals and registration as a Republican, with the mirrored negative and significant relationship between age at arrival and registering as a Democrat. This suggests that our main results for child arrivals are reliable and robust to alternative specifications.

The results for people who arrived as adults, however, are essentially the opposite of those for people who arrived as children. For adults, the coefficient describing the relationship between age at arrival and participation is either *negative* or indistinguishable from zero despite the fact that we have considerably more power in this group. The results for party affiliation are all distinguishable from zero, but older arrivals tend to be significantly *less* likely to register as Republicans or nonpartisans and significantly *more* likely to register as Democrats than younger ones conditional on having immigrated as adults. It is difficult to speculate why this might be the case for adults conditional on controlling for their countries of birth and years of arrival. Without benchmarks for within-family comparison, this approach remains open to confounding from pre-migration characteristics like education, personal experiences with persecution prior to migration, and a host of other possibilities for which we cannot directly account. This does suggest, however, that the relationships between age at arrival and our outcomes of interest are fundamentally different for those who arrived as children and those who came as adults.

Table H.14: Age at Arrival and Participation without Family Fixed Effects

	Presidential: Adults	Midterm: Adults	Voted: Adults	Presidential: Children	Midterm: Children	Voted: Children
Intercept	0.5461*	0.2458	0.5036*	1.0390*	1.0213*	1.0147*
	[0.3439; 0.7483]	[−0.0490; 0.5406]	[0.2827; 0.7245]	[0.6812; 1.3968]	[0.6610; 1.3816]	[0.6405; 1.3889]
Age at Arrival	0.0003	0.0002	−0.0002	0.0051*	0.0053*	0.0041*
	[−0.0014; 0.0020]	[−0.0013; 0.0017]	[−0.0018; 0.0015]	[0.0032; 0.0070]	[0.0035; 0.0071]	[0.0025; 0.0057]
Female	−0.0084*	−0.0315*	−0.0096*	−0.0179	−0.0652*	−0.0258*
	[−0.0152; −0.0016]	[−0.0407; −0.0224]	[−0.0166; −0.0025]	[−0.0382; 0.0025]	[−0.0959; −0.0345]	[−0.0460; −0.0056]
Family Size	0.0117*	0.0122*	0.0115*	0.0128*	0.0120*	0.0101*
	[0.0085; 0.0149]	[0.0089; 0.0156]	[0.0083; 0.0148]	[0.0049; 0.0208]	[0.0073; 0.0167]	[0.0026; 0.0176]
Proportion Female in Family	0.0081	−0.0078	0.0054	0.0028	0.0291*	0.0090
	[−0.0021; 0.0182]	[−0.0173; 0.0017]	[−0.0041; 0.0149]	[−0.0202; 0.0259]	[0.0046; 0.0537]	[−0.0104; 0.0284]
R^2	0.0121	0.0258	0.0144	0.0239	0.0436	0.0225
Adj. R^2	0.0107	0.0243	0.0130	0.0209	0.0406	0.0194
Num. obs.	61935	61935	61935	28365	28365	28365
RMSE	0.3936	0.4788	0.3706	0.4386	0.4862	0.4117
N Clusters	51	51	51	50	50	50

All specifications include dummies for country of birth and arrival year. Standard errors clustered by state of residence.

Table H.15: Age at Arrival and Party Affiliation without Family Fixed Effects

	Republicans: Adults	Democrats: Adults	Nonpartisans: Adults	Republicans: Children	Democrats: Children	Nonpartisans: Children
Intercept	0.0290	0.6079*	0.2927	0.1252	0.9594*	0.0352
	[−0.0993; 0.1573]	[0.3742; 0.8415]	[−0.0036; 0.5891]	[−0.3160; 0.5664]	[0.6631; 1.2556]	[−0.4028; 0.4732]
Age at Arrival	−0.0018	0.0060*	−0.0042*	0.0082*	−0.0092*	0.0012
	[−0.0037; 0.0001]	[0.0032; 0.0089]	[−0.0058; −0.0026]	[0.0057; 0.0106]	[−0.0105; −0.0079]	[−0.0005; 0.0030]
Female	−0.0301*	0.0397*	−0.0082	−0.0411*	0.0645*	−0.0211*
	[−0.0371; −0.0231]	[0.0333; 0.0461]	[−0.0195; 0.0031]	[−0.0550; −0.0271]	[0.0500; 0.0791]	[−0.0414; −0.0008]
Family Size	0.0023	−0.0024	−0.0002	0.0068*	0.0055	−0.0119*
	[−0.0069; 0.0116]	[−0.0091; 0.0043]	[−0.0051; 0.0047]	[0.0034; 0.0103]	[−0.0011; 0.0121]	[−0.0201; −0.0037]
Proportion Female in Family	−0.0129*	0.0150*	−0.0039	−0.0173*	0.0033	0.0094
	[−0.0246; −0.0012]	[0.0047; 0.0253]	[−0.0139; 0.0061]	[−0.0333; −0.0013]	[−0.0095; 0.0160]	[−0.0093; 0.0281]
R^2	0.0239	0.0454	0.0216	0.0281	0.0341	0.0153
Adj. R^2	0.0224	0.0440	0.0201	0.0250	0.0311	0.0123
Num. obs.	61935	61935	61935	28365	28365	28365
RMSE	0.4804	0.4165	0.4730	0.4453	0.4516	0.4837
N Clusters	51	51	51	50	50	50

All specifications include dummies for country of birth and arrival year. Standard errors clustered by state of residence.

I Pre-1980 Arrivals

Tables I.16 - I.19 show our results separately for children who arrived with siblings before 1980 and for those who arrived in 1980 or later. Recall that the largest wave of immigration from the Soviet Bloc to the US occurs approximately 1985-1995. Thus, most of our power comes from refugees who emigrated during this period and the estimates for child arrivals who came before 1980 are considerably less precise. However, we show the pre-1980 results here to demonstrate that these have approximately the same magnitudes and point in the same direction as the post-1980 results, which suggests that the effects are likely to be similar for refugees who sought to leave the Soviet Union before and after the Soviet economic collapse was underway in earnest. This suggests that economic factors are not the primary driver of the effects we observe.

Table I.16: Effect of Age at Arrival on Participation for Siblings Arriving Before 1980

	Turnout					
	2012	2014	2016	2018	2020	2022
Age at Arrival	0.022	0.024	0.020	0.013	0.003	0.016
	(0.024)	(0.033)	(0.018)	(0.026)	(0.018)	(0.034)
Female	−0.044	−0.139	−0.045	0.067	0.050	0.020
	(0.144)	(0.230)	(0.246)	(0.183)	(0.098)	(0.226)
Family Fixed Effect	✓	✓	✓	✓	✓	✓
Adjusted R^2	0.046	0.139	0.183	0.228	0.306	0.175
Residual Std. Error (df = 130)	0.451	0.464	0.402	0.408	0.315	0.394

Note: *p<0.1; **p<0.05; ***p<0.01
Standard errors clustered by state.

Table I.17: Effect of Age at Arrival on Participation for Siblings Arriving After 1979

	Turnout					
	2012	2014	2016	2018	2020	2022
Age at Arrival	0.007*	0.004**	0.007**	0.004	0.008***	0.005*
	(0.004)	(0.002)	(0.003)	(0.003)	(0.002)	(0.003)
Female	−0.018	−0.026*	−0.022	−0.058**	−0.039*	−0.076***
	(0.029)	(0.015)	(0.029)	(0.028)	(0.020)	(0.022)
Family Fixed Effect	✓	✓	✓	✓	✓	✓
Adjusted R^2	0.199	0.206	0.120	0.213	0.075	0.119
Residual Std. Error (df = 3651)	0.402	0.282	0.463	0.418	0.458	0.467

Note: *p<0.1; **p<0.05; ***p<0.01
Standard errors clustered by state.

J Pre- and Post- Soviet Collapse

In this section, we break out our results from Section 3 for people arriving as children prior to the Soviet Union's official dissolution in 1991 and those arriving after the collapse. The effect of exposure to state repression in the Soviet Bloc, measured via age at arrival relative to one's own siblings, is similar for both groups. The effects of exposure on participation are positive and comparable in magnitude for every election year in our data, though these effects are stronger for the 1991 or later arrivals. We also show similar effects of exposure on partisanship for both groups: more exposure produces a greater tendency to affiliate with the Republican Party and a concomitant tendency to avoid affiliating with the Democratic Party. These effects, however, are stronger for pre-1991 arrivals than they are for those who emigrated after the collapse. This supports our interpretation of these effects. If we think of affiliating with a right wing party as a backlash effect against everything Soviet totalitarianism pursued and represented, this effect

Table I.18: Effect of Age at Arrival on Party Affiliation for Siblings Arriving Before 1980

	Party Affiliation		
	Democrat	Republican	Nonpartisan
Age at Arrival	−0.003	0.008	−0.009
	(0.018)	(0.017)	(0.012)
Female	0.182	−0.033	−0.151
	(0.183)	(0.197)	(0.158)
Family Fixed Effect	✓	✓	✓
Adjusted R^2	0.231	0.274	0.321
Residual Std. Error (df = 130)	0.438	0.389	0.331
Note:			*p<0.1; **p<0.05; ***p<0.01
			Standard errors clustered by state.

Table I.19: Effect of Age at Arrival on Party Affiliation for Siblings Arriving After 1979

	Party Affiliation		
	Democrat	Republican	Nonpartisan
Age at Arrival	−0.007**	0.008*	−0.001
	(0.004)	(0.005)	(0.002)
Female	0.035*	−0.030	−0.0003
	(0.020)	(0.034)	(0.032)
Family Fixed Effect	✓	✓	✓
Adjusted R^2	0.162	0.133	0.105
Residual Std. Error (df = 3651)	0.416	0.420	0.462
Note:			*p<0.1; **p<0.05; ***p<0.01
			Standard errors clustered by state.

should be stronger for those who lived under and left that regime than they would be for refugees leaving a less consolidated totalitarian state that made less organized effort to repress them.

Table J.20: Effect of Age at Arrival on Participation for Siblings Arriving Before 1991

	Turnout					
	2012	2014	2016	2018	2020	2022
Age at Arrival	0.006	0.009**	0.002	0.003	0.004	0.003
	(0.011)	(0.004)	(0.008)	(0.004)	(0.006)	(0.008)
Female	−0.054	−0.034	−0.046	−0.076	−0.039	−0.069
	(0.068)	(0.050)	(0.058)	(0.059)	(0.068)	(0.084)
Family Fixed Effect	✓	✓	✓	✓	✓	✓
Adjusted R^2	0.227	0.246	0.145	0.244	0.097	0.132
Residual Std. Error (df = 983)	0.431	0.352	0.462	0.432	0.426	0.464

Note: *p<0.1; **p<0.05; ***p<0.01
Standard errors clustered by state.

Table J.21: Effect of Age at Arrival on Participation for Siblings Arriving in 1991 or Later

	Turnout					
	2012	2014	2016	2018	2020	2022
Age at Arrival	0.008**	0.003	0.009**	0.004	0.009***	0.007**
	(0.004)	(0.002)	(0.004)	(0.004)	(0.003)	(0.003)
Female	−0.008	−0.026	−0.015	−0.049*	−0.037	−0.077**
	(0.025)	(0.016)	(0.030)	(0.028)	(0.029)	(0.030)
Family Fixed Effect	✓	✓	✓	✓	✓	✓
Adjusted R^2	0.179	0.191	0.108	0.197	0.078	0.118
Residual Std. Error (df = 2798)	0.394	0.265	0.461	0.412	0.463	0.465

Note: *p<0.1; **p<0.05; ***p<0.01
Standard errors clustered by state.

K Current States of Residence

Table J.22: Effect of Age at Arrival on Party Affiliation for Siblings Arriving Before 1991

	Party Affiliation		
	Democrat	Republican	Nonpartisan
Age at Arrival	−0.007	0.009	−0.002
	(0.005)	(0.007)	(0.005)
Female	−0.012	0.003	0.019
	(0.070)	(0.074)	(0.077)
Family Fixed Effect	✓	✓	✓
Adjusted R^2	0.170	0.143	0.132
Residual Std. Error (df = 983)	0.420	0.433	0.442
Note:			*p<0.1; **p<0.05; ***p<0.01
			Standard errors clustered by state.

Table J.23: Effect of Age at Arrival on Party Affiliation for Siblings Arriving in 1991 or Later

	Party Affiliation		
	Democrat	Republican	Nonpartisan
Age at Arrival	−0.007**	0.008*	−0.001
	(0.003)	(0.005)	(0.003)
Female	0.055**	−0.041	−0.010
	(0.025)	(0.037)	(0.034)
Family Fixed Effect	✓	✓	✓
Adjusted R^2	0.176	0.123	0.106
Residual Std. Error (df = 2798)	0.415	0.413	0.464
Note:			*p<0.1; **p<0.05; ***p<0.01
			Standard errors clustered by state.

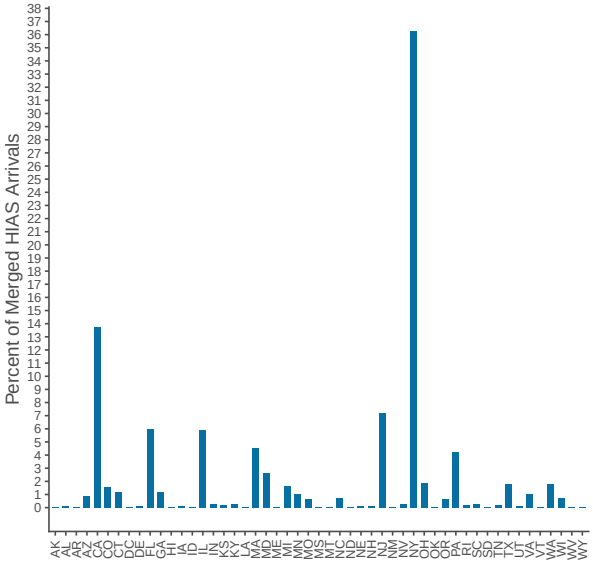
L Israel Survey Data

The Israel Polarization Panel Dataset is a 10-wave panel survey designed to be representative of the Israeli electorate as of 2015. Sampling was conducted by a public opinion survey firm between 2019 and 2021. All respondents to the panel were asked about turnout in 2015, while smaller subsets were asked about turnout in subsequent elections held 2019-2021. This dataset includes observations collected from 2,542 respondents (where observations for core demographic characteristics relevant to our study purposes are complete), 495 of whom report being foreign-born and 291 of whom report having been born in the FSU or the broader Soviet Bloc. This panel does not include family identifiers, but it does contain information on residents' country of birth, immigration year, age, gender, turnout in various years between 2015 and 2021, and indicators of support for the various political parties represented in the Knesset between 2019 and 2021.

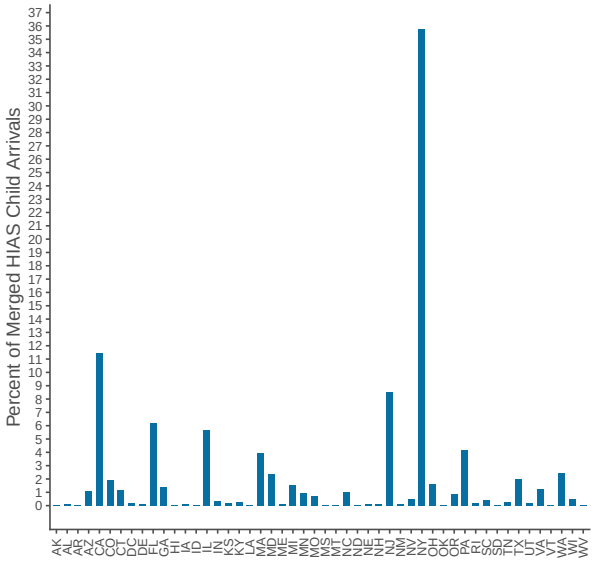
In the main manuscript, we use these survey data to run the most analogous specification to

Figure K.21: Current States of Residence

(a) All Merged Refugees



(b) Arrived As Children



Equation 1 that the data allow, though this time including a term for arrival year in the absence of information on family units:

$$y_i = \alpha + \beta ArrivalAge_i + \theta Female_i + \kappa ArrivalYear_i + \eta_i \tag{2}$$

where y_i represents a binary indicator for either turnout in a given year (we model turnout in 2015, 2019, 2020, and 2021 separately) or declared support for a right wing political party for respondent i depending on the specification. Arrival age, gender, and arrival year represent the corresponding self-reported fields for each individual respondent in the panel. As in the main results, we do not explicitly control for current region of residence[14] or other post-treatment socioeconomic characteristics available in the survey data, but we do cluster results by region to account for regional voting patterns. η_i represents the error term. We do not restrict this sample to people who immigrated as children in order to preserve power since only 230 respondents born in the Soviet Bloc were under 21 at the time they migrated, but doing this produces results consistent with those in Tables 4 and 5 since most people in this sample who emigrated from the Soviet Bloc came as children.

Tables L.24 and L.25 replicate the results reported in Tables 4 and 5 using logistic regression since all outcome variables are binary. These results are consistent with the results reported in the manuscript.

The Israel survey panel does not contain enough immigrants from outside of the FSU or other Soviet Bloc countries who turned out in any year to reliably estimate the relationship between age at arrival and turnout, but Table 4 shows the relationship between age at arrival and willingness to support right-wing parties for immigrants from outside this region. Contrary to a positive and

[14]Jerusalem, Northern Israel, Haifa, Central Israel, Tel Aviv, Southern Israel, Judea and Samaria, or living outside of Israel

Table L.24: Age at Arrival and Turnout, Logistic Regression

	Dependent variable:			
	Voted 2015	Voted 2019	Voted 2020	Voted 2021
	(1)	(2)	(3)	(4)
Age at Arrival	0.037**	0.068	0.074	−0.035
	(0.018)	(0.092)	(0.059)	(0.086)
Male	0.420	−0.991	−0.103	20.711
	(0.329)	(1.347)	(0.774)	(5,466.218)
Constant	17.921	23.152	20.345	22.673
	(6,522.638)	(48,196.140)	(17,730.370)	(48,196.160)
Immigration Year Dummy	✓	✓	✓	✓
Observations	282	217	181	134

Note: *p<0.1; **p<0.05; ***p<0.01
Classical standard errors. Insufficient power to estimate results within-cluster.

Table L.25: Age at Arrival and Party Affiliation, Logistic Regression

	Dependent variable:	
	Would Vote for Right Wing Party	Did Vote for Right Wing Party
	(1)	(2)
Age at Arrival	0.026**	0.023*
	(0.013)	(0.013)
Male	0.186	0.384
	(0.248)	(0.247)
Constant	0.036	−0.082
	(0.227)	(0.225)
Immigration Year Dummy	✓	✓
Observations	282	282

Note: *p<0.1; **p<0.05; ***p<0.01
Classical standard errors. Insufficient power to estimate results within-cluster.

significant one for immigrants from the FSU and Soviet Bloc reported in Table 5, this relationship is negative for immigrants outside of this region. This provides suggestive evidence that living under the repressive communist governments of the FSU and Soviet Bloc had a unique effect on Jewish immigrants to Israel as well as the US. Much like in the US case, further analysis by country of origin is impossible here because survey respondents who immigrated from all countries outside of the FSU or Soviet Bloc number fewer than 20 - and often fewer than 10 - per country.

Table L.26: Age at Arrival and Party Affiliation for Non-Soviet Bloc Immigrants to Israel

	Dependent variable:	
	Would Vote for Right Wing Party	Did Vote for Right Wing Party
	(1)	(2)
Age at Arrival	−0.008	−0.003
	(0.006)	(0.004)
Male	0.011	0.114
	(0.185)	(0.113)
Immigration Year Dummy	✓	✓
Observations	133	133
R^2	0.527	0.563
Note:		*p<0.1; **p<0.05; ***p<0.01

Figure L.22: Proportion Who Reported Voting in 2015 By Age

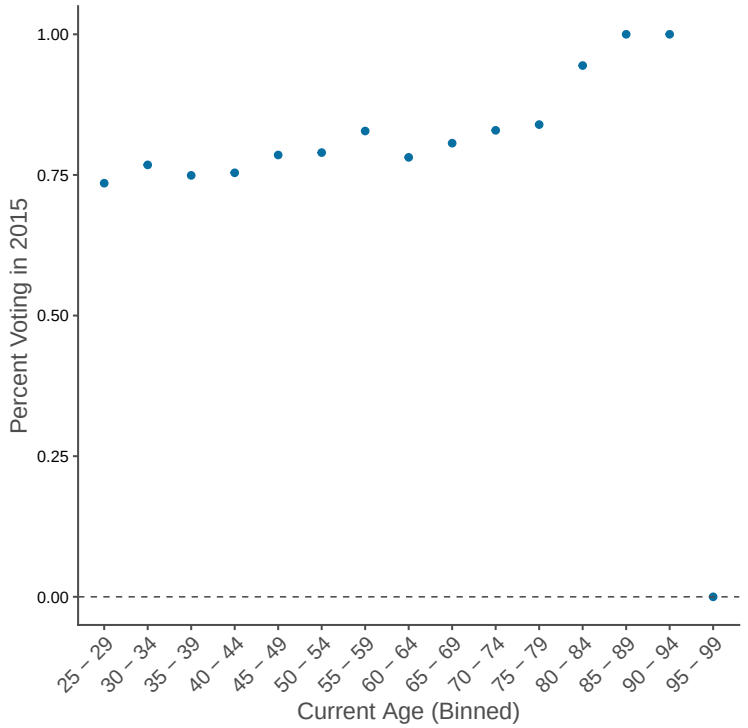


Figure L.23: Would Vote for Right Wing Party

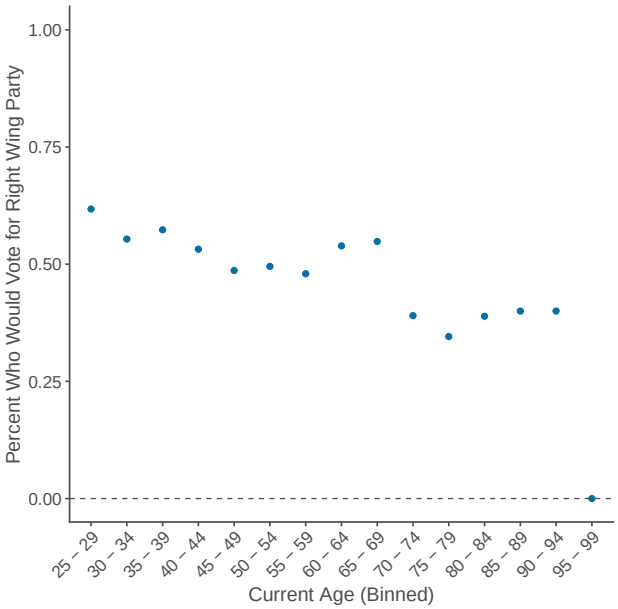


Figure L.24: Did Vote for Right Wing Party