

**LEAVING THE ENCLAVE: HISTORICAL EVIDENCE ON
IMMIGRANT MOBILITY FROM THE INDUSTRIAL
REMOVAL OFFICE**

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Abstract: We study a program that funded 39,000 Jewish households in New York City to leave enclave neighborhoods circa 1910. Compared to their neighbors with the same occupation and income score at baseline, program participants earned 4 percent more ten years after removal, and these gains persisted to the next generation. Men who left enclaves also married spouses with less Jewish names, but they did not choose less Jewish names for their children. Gains were largest for men who spent more years outside of an enclave. Our results suggest that leaving ethnic neighborhoods could facilitate economic advancement and assimilation into the broader society, but might make it more difficult to retain cultural identity.

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1. Introduction

During the Age of Mass Migration (1850-1913), many immigrants to the United States lived in immigrant neighborhoods and relied on ethnic networks.¹ At the time, both pro- and anti-immigration voices expressed concerns about poor conditions in immigrant neighborhoods, and how the isolation of immigrant enclaves might impede assimilation.² This paper studies the economic and cultural assimilation of one immigrant group that moved to the US in the early twentieth century – Eastern European Jews – and asks whether leaving enclave neighborhoods generated upward mobility. To do so, we leverage a unique program called the Industrial Removal Office (IRO) that financed 39,000 Jewish immigrant households to move out of Jewish enclaves in New York City between 1900-1919.

We start by documenting broader patterns of economic and cultural assimilation for Jewish immigrants from 1900 to 1920. Two million Jews settled in the United States during this period, coming from Europe both for economic opportunity and to escape persecution. Using a new Jewish Names Index, we construct a new longitudinal dataset of Jewish immigrants in the US from 1900 to 1920. We document that Jewish immigrants had high occupation-based earnings relative to the US-born even upon first arrival, primarily due to their concentration in urban, semi-skilled jobs. Jewish immigrants experienced rapid earnings growth and significant cultural assimilation, as measured by names selected for their children, with time spent in the US.

We then ask whether the process of moving out of immigrant enclaves contributed to upward economic mobility. We focus on the IRO program, which encouraged poor Jewish immigrants to relocate from enclave neighborhoods to destinations around the country that were often pre-selected by program officers. Because Jewish enclaves in New York City were

¹ In 1910, the average immigrant from Southern or Eastern Europe lived in a neighborhood that was made up of at least 50 percent first- or second-generation immigrants, compared to only 10 percent of neighbors for the typical US-born household head (Eriksson and Ward, 2018, Figure 5).

² Jacob Riis, a photographer who publicized the conditions in the crowded tenements in New York City in *How the Other Half Lives* (1890), lamented that many of the children in immigrant neighborhoods “...had never seen the Brooklyn Bridge that was scarcely five minutes’ walk away... The street, with its ash-barrels and its dirt, the river that runs foul with mud, are their domain.” Henry Cabot Lodge, one of the lead champions of border restriction likewise emphasized that immigrants lived “in congested masses in our great cities. They furnish... a large proportion of the population of the slums” (Lodge, 1896).

characterized by overcrowding and concentrated poverty, the program combined relocation to neighborhoods with a lower co-ethnic share and a higher socio-economic status. Participants received moving expenses and short-term lodging in exchange for leaving New York City.³

Our analysis is based on newly digitized records for IRO program participants that we recovered from the American Jewish Historical Society. We compare IRO program participants to other Jewish immigrants who lived in the same set of enclave neighborhoods in the 1910 census and who arrived in the US in the same period. First, we link IRO participants and comparison men forward to the 1920 census to study economic and cultural assimilation. Then, we follow the sons of both groups to the 1940 census to examine intergenerational transmission.⁴

We find that immigrants who left a New York City enclave experienced faster earnings growth than their neighbors. Because the census did not collect income data before 1940, we compute a proxy for individual-level income (“income scores”) based on detailed information on occupation, age, country of birth and state of residence. IRO participants earned 4 percent more by our income proxy in 1920 than a comparison group who held the same occupation and started in the same income quintile at baseline. Furthermore, these advantages persisted to the second generation, with the sons of IRO participants earning 7 percent more than the sons of non-participants in 1940.⁵

By leaving enclave areas, IRO participants also assimilated into the broader US society while retaining some of their Jewish identity. IRO participants married spouses with less distinctively Jewish first names, many of whom were probably from assimilated Jewish backgrounds but some of whom could have been non-Jews. Yet these couples did not select less Jewish names for their children, suggesting that men encountered a different pool of marriageable women outside of enclaves but nevertheless maintained their own cultural identity. Furthermore,

³ Total monetary benefits of the program were small, the equivalent of around two weeks of pay for a low-skilled worker.

⁴ Because women often change their surnames at marriage, we follow the literature by only attempting to link men who were moved through the IRO program and their sons.

⁵ It is possible that the comparison households also benefited by having some men removed from their neighborhoods by the IRO program. However, relative to the size of Jewish enclaves in New York City at the time (750,000 Jewish residents), the program was small, representing only 5 percent of the population over a twenty-year period. Furthermore, if remaining residents did experience gains through lessened competition, we will under-estimate the benefits of leaving the enclave.

we find that men who themselves had distinctively Jewish names, implying a higher degree of association with Jewish culture, were most likely to move back to New York City after a period of relocation, suggesting that enclaves offered cultural amenities that some immigrants found to be a counterbalance to their economic costs.

Men who left New York City through the IRO program may have been different from their neighbors in unobservable ways – i.e., more resourceful or talented – and these attributes may have allowed them to move up the ladder even without mobility assistance. Although we lack a situation with random assignment into the IRO program (i.e., the program was never allocated by lottery), we provide suggestive evidence that participating in the IRO program conferred economic benefits. First, we compare IRO participants who were relocated earlier versus later in the program’s history; men who moved earlier had more exposure to life outside of an enclave neighborhood by our follow-up date (1920). Men who moved earlier had the largest gain in income score, while men who moved latest in the program did not gain at all. We find no evidence that differential gains are driven by early movers being higher skilled at baseline, or having more family connections. Second, we compare IRO participants who moved with program assistance to men who left New York City enclaves on their own; these self-financed movers may have been selected for a similar set of attributes (e.g., risk tolerance) that encourage mobility. When we compare men with the same initial occupation, IRO participants and self-financed movers experience the same gains.

2. Contributions to Literature

Our findings suggest that leaving enclave neighborhoods facilitated immigrant advancement in the early twentieth century, but at the cost of lessening their cultural attachment. As a result, some immigrants who were more ethnically identified chose to move back to enclave neighborhoods. In addition, we offer a major advance on the historical literature on the Age of Mass Migration by producing primary data on a group – Jewish immigrants – who have heretofore not been separately identifiable in large datasets. Given the lack of information on religious affiliation in the census, studies of historical immigrant assimilation focus almost exclusively on country of origin, rather than religious group (Lieberson, 1980; Abramitzky, Boustan, Eriksson,

2014, Eriksson and Ward, 2018; Abramitzky, Boustan, Jácome, Perez, 2019; Ward, 2019).⁶ Our new Jewish Names Index, which we will make broadly available to other scholars going forward, provides a window into historical Jewish communities.⁷

Our paper also contributes to the literature on the costs and benefits of living in immigrant enclaves. First, we provide some of the first evidence on the inter-generational effects of leaving immigrant enclaves. We follow the sons of participants into the labor market and find that the economic benefits of leaving an enclave persist to the second generation. Second, we focus on large enclaves that are more representative of immigrant neighborhoods today.⁸ To date, the best causal evidence on immigrant enclaves is based on refugee assignment programs, mostly in Scandinavia (Edin, Fredriksson and Aslund 2003, 2004; Damm 2009, 2014; Beaman 2012). Refugee enclaves tend to be small: for example, the average refugee in Edin, Fredriksson and Aslund (2003) was settled in a municipality whose residents were only 8 percent foreign born overall (1 percent from the refugee's own home country). Refugees appear to gain from assignment to an area with others from their home country. This pattern has been interpreted as evidence that immigrant enclaves confer economic benefits more broadly.⁹ By contrast, we document some economic costs of staying in large enclave areas.¹⁰

⁶ The few studies that identify Jewish immigrants do so by using Yiddish speakers observed with the “mother tongue” question available in some census years (Pagnini and Morgan, 1990; Chiswick, 1983, 1991). Chiswick (1992) also uses historical information on Jewish immigrants in the Dillingham Commission report. Much of the literature on contemporary Jewish acculturation use the National Jewish Population Survey (Himmelfarb and Loar, 1984; Amyot and Sigelman, 1996; Kivisto and Nefzger, 1993; Burstein, 2007; Chiswick and Huang, 2008).

⁷ Our new Jewish Names Index complements recent work by Zhang, Zuckerman and Obhukova (2016) and Fermaglich (2019) which analyze novel sources like World War I service records and name change petitions to document innovation and creativity in Jewish naming practices as a means to assimilate into US culture.

⁸ Before relocation, the average IRO participant lived in a neighborhood that was at least 55 percent Jewish, on par with some large immigrant enclaves today (e.g., Mexicans in East Los Angeles).

⁹ Beaman (2012) finds that recently arrived co-ethnics constitute labor market competition, whereas more established immigrants can provide assistance. Earlier work emphasizes the importance of immigrant enclaves in providing informal social insurance (Cohen, 1991), information about access to social services (Bertrand, Luttmer, and Mullianathan, 2000) and employment assistance (Munshi, 2003).

¹⁰ Our results are consistent with recent historical work by Connor (2020) and Eriksson (Forthcoming) and with contemporary studies in the US (Borjas, 2000; Xie and Gough, 2011).

Finally, our paper contributes to the literature on mobility programs. We find a large out-migration response to a small financial incentive, similar to the effectiveness of small payments to encourage seasonal migration in Bangladesh (Bryan, Chowdury and Mobarak, 2014). However, many IRO participants eventually returned to New York City at a rate similar to the 10-15 year persistence rate in the Moving to Opportunity (MTO) study (Sanbonmatsu, et al., 2011, Exhibit ES-2).¹¹ Consistent with Bazzi, et al.'s (2019) study of a resettlement program in Indonesia, we find that leaving an enclave promotes cultural assimilation.

3. Patterns of Jewish Assimilation

More than two million Jewish immigrants moved from Europe to the United States during the Age of Mass Migration. The first large wave of Jewish migration from Germany in the 1860s was followed from 1880-1920 by poorer Jewish immigrants from the Russian Empire and other parts of Eastern Europe. Some Jewish immigrants were fleeing from anti-Jewish violence in Europe, while others were pulled to the US by economic opportunity (Kuznets, 1975; Boustan, 2007; Spitzer, 2013; Zipperstein, 2018). Jewish immigration slowed after the US border was restricted to new entry in the 1920s (Abramitzky and Boustan, 2017).

We start by documenting new facts about the economic and cultural assimilation of Jewish immigrants from 1900 to 1920. These facts rely on our new index of Jewish identity and on an “income score” variable that proxies for individual income. We explain the Jewish index and the income score in more detail in section 4.

First, Jewish immigrants had higher earnings (“income score”) than the US-born even upon first arrival, primarily due to their concentration in semi-skilled urban occupations. Russian Jews experienced further earnings growth relative to the US-born with additional years spent in the US. We summarize these results in **Figure 1**, which presents coefficients from a regression of log income score on indicators for time spent in the US by country of origin or Jewish ethnicity. Coefficients are relative to US-born men, the omitted category. The panel sample of immigrants

¹¹ Immediately after receiving vouchers to move to low-poverty neighborhoods, MTO participants lived in census tracts with poverty rates 43 percentage points below the control group. However, after 4-7 years, the poverty rate gap between MTO compliers and control households fell to 20 points, and, after 10-15 years, the gap further declined to 10 points.

and US-born workers is observed in the 1900, 1910 and 1920 censuses (compare to Abramitzky, Boustan Eriksson, 2014; Figure 3; $N = 1.85$ million, with 44,000 likely Jews).¹²

Second, Russian Jews experienced the fastest cultural assimilation of any immigrant group during the Age of Mass Migration period. Here we define cultural assimilation as giving less foreign-sounding names to children born after spending more time in the US. **Figure 2** reports estimates by ethnicity or country of origin of the implied effect of spending 20 years in the US on the foreignness index of a child's name (compare to Abramitzky, Boustan and Eriksson, 2020; Figure 2 (Panel A); $N = 6.9$ million, with 406,000 likely Jews). Russian Jews are the group that shift their name choice the most with time spent in the US, with other Jewish immigrants in the middle of the pack.

Overall, Jewish immigrants arrived in the US with skills that allowed them to enter highly-paid occupations, and they continued to advance up the occupational ladder with time in the US. Our results are thus most relevant to high-skilled immigrant groups, of which there were many both in the past (see Figure 1) and the present (e.g., immigrants from China and India).

4. The Industrial Removal Office

Despite some economic successes, on average, many Jewish immigrants in the early twentieth century lived in enclave neighborhoods characterized by overcrowding and poor health conditions. Housing in the Lower East Side, in particular, was considered to be “stifling, unhealthy and miserable” (Maffi, 1995, p. 119). Together, charitable organizations like the B'nai Brith, the United Hebrew Charities and the Baron de Hirsch Foundation founded the Industrial Removal Office with the intention of “dispersing the immigrants [to] alleviate some of [the] problems [of]... filth, poor sanitation, disease, and soaring rates of delinquency and crime” (Rockaway, 1998, p. 1-

¹² The graph differs from Abramitzky, Boustan and Eriksson, 2014 (Figure 3) in four ways: first, we use the newly available complete-count census data, rather than a smaller sample initially compiled by the authors. Second, we use an improved crosswalk between original census occupation records and occupation-based income measures. Third, we define Jewish and non-Jewish immigrants in a mutually exclusive fashion, so that (for example) Russian, Austrian and German coefficients here are based only on non-Jewish immigrants. Fourth, our outcome variable is an “income score” estimated from the 1940 census, and applied to the men in our sample (1900-1920), rather than the 1950-based “occupation score” provided by IPUMS. See our website (<https://ranabr.people.stanford.edu/matching-codes>) for a replication of Abramitzky, Boustan and Eriksson (2014) using these improvements made since that paper was published.

3). Raising awareness for these efforts, Cyrus Sulzberger, the president of the Jewish Agricultural and Industrial Aid Society, addressed the National Conference for Jewish Charities in 1901 saying “go back to your communities and tell them.... to take these thousands of newcomers off New York’s hands” (cited in Diner, 2000, p. 151).¹³

The first moves financed by the IRO program occurred in 1900. **Figure 3** graphs the number of IRO removals in each year of operation by country of birth. The program was most active from 1903 until the Panic of 1907, which led to a drop in overall immigration to the US and a decline in the willingness of communities around the country to accept and assist IRO participants. A second round of moves took place in 1912 and 1913. The program ceased operation after the closing of the US border to Eastern European migration in the 1920s.

The IRO targeted young Jewish immigrants experiencing economic hardship. Internal IRO documents reported that, in nine out of ten cases, applicants had experienced spotty employment for up to twelve weeks in the year before removal (IRO, 1911, p. 6). Participants learned about the program through public lectures, newspapers, referrals from other Jewish charities, or by meeting IRO agents that were stationed at Ellis Island. The IRO program maintained a storefront recruiting center in the Lower East Side. As an incentive for participation, the IRO offered moving expenses, as well as short-term lodging and help with job search at the destination. The average stipend for moving expenses was \$15, the equivalent of around two week’s pay for a low-skilled worker in the 1901 Cost of Living Survey.

Table 1 reports demographic and economic characteristics of the 39,000 household heads in the IRO records (we describe the dataset in more detail below). 79 percent of participants were men, most of whom moved alone, and the average age at removal was 28 years old. Around half of the cases were processed as “direct removals” comprising individuals with “no definite place to which they desire to be sent and who [left] the selection of the place to the judgement and discretion of the officials of the office.” (IRO, 1911, p. 8). Other participants stated a locational preference – for example because they were moving to meet family.

¹³ The IRO was one of many Jewish social assistance programs in New York; other agencies focused on poor relief and support for widows and orphans (Fridkis, 1981; Szajkowski, 1973). It also joined a wider effort aimed at *immigrant deflection* – as it was called – which sought to lessen overcrowding in New York City by re-routing Jewish immigrants through other ports of entry, including Galveston, TX (Eisenberg, 1995; Marinbach, 2012; Aaronson, Davis and Schulze, 2018).

We transcribed additional information from the IRO records for our complete linked sample of 2,362 individuals (the linking procedure is explained below).¹⁴ At the time of their departure from New York City, 18 percent of participants reported having ‘no trade,’ a category that might reflect being an unskilled laborer. Other common occupations include semi-skilled positions like tailors, carpenters, blacksmiths and operators, which together represent around 25 percent of the sample. The majority of participants reported Russia as their country of birth (75 percent), with other Southern and Eastern European countries making up the balance. Relative to comparison households, IRO participants were somewhat more likely to be born in Russia and less likely to be born in Austria. We re-weight the data in our analysis to account for these differences in place of birth.¹⁵

IRO participants were assigned to more than 1,000 locations around the country, although participants were not compelled to stay in their assigned location, and our longer-term follow-up suggests that few of them did. Diner (2000, p. 152) summarizes these scattered locations, writing “The IRO sent Jewish immigrants to small communities – Champaign, Illinois; La Crosse, Wisconsin; Gary, Indiana; Galveston, Texas; Cedar Rapids, Iowa – all places quite unlike the Lower East Side in terms of Jewish numbers, density, and diversity. But the IRO also sent New York’s Jewish newcomers to Cleveland, St. Louis and Chicago, places that had attracted immigrant Jews directly from eastern Europe.”¹⁶

Figure 4 maps the share of IRO participants assigned to each location (Panel A). IRO participants were sent to the Midwest, the upper South and the Pacific states, with very few being resettled on the Eastern seaboard. Internal IRO documentation reports that around 90 percent of

¹⁴ Data Appendix Table 1 explains how observations are lost in creating the linked sample. We drop women because they cannot be linked forward in the census. We also drop men with incomplete information on name and age, men whose names are below a certain threshold on the Jewish Names Index, men who are not unique in the 1910 census and thus cannot be matched forward, etc. Our final linked sample has 3,612 observations, of which around 65 percent have complete information on occupation and neighborhood.

¹⁵ The place of birth distribution in our comparison sample is 65 percent Russian-born, 21 percent Austrian-born, 5.5 percent Romanian-born and 8.5 percent from other countries of origin.

¹⁶ The IRO identified target locations through intermittent surveys and informal correspondence with established, but typically small, Jewish communities. Local contacts were charged with documenting the arrival of IRO participants, distributing stipends, and assisting with job search and accommodation. The extent of this assistance varied, depending on the in-kind offers of the local community.

participants were residing at the assignment location in the first year. However, by 1920, we find that only 10 percent of IRO households remain in the State Economic Area (county group) to which they were assigned.¹⁷ The lack of “stickiness” in initial assigned location makes it impossible for us to use assigned location as an instrument for actual location, as in papers based on refugee assignments. Panel B shows that, by 1920, some portion of IRO participants had returned to the New York metropolitan area or other cities on the East Coast. However, in contrast to other Jewish households who had lived in an enclave in New York City in 1910 (Panel C), IRO participants remained more dispersed throughout the US, with large clusters in cities like Minneapolis, Denver, Portland and Los Angeles, that otherwise had yet to attract major Jewish settlement.

Figure 5 summarizes the effectiveness of the IRO program in removing participants from enclave neighborhoods. IRO participants were twice as likely as comparison households who lived in enclaves in New York City in 1910 to live outside of the New York area in 1920 (54 percent versus 28 percent), and 10 percentage points less likely to live in a Jewish enclave (defined here for descriptive purposes as an enumeration district that was at least 40 percent Jewish; results look similar using other thresholds). Note that enumeration districts have around 300 residents on average, around the size of a modern census block group. **Appendix Figure 1** graphs the full distribution of neighborhood Jewish share for IRO and non-IRO participants before and after relocation.¹⁸

Immigrant enclaves – both in New York City and other large metropolitan areas – were characterized by not only having a large foreign-born population but also having residents of lower socio-economic status. **Table 2** documents that – not surprisingly – immigrant enclaves in the 10 largest metropolitan areas had a higher immigrant share, but also fewer homeowners and fewer residents working in white collar positions. The IRO program was thus a “bundled” treatment,

¹⁷ The IRO archive contains a number of disgruntled letters from participants complaining about their placement location. Rockaway (2018) quotes from this letter, dated August 23, 1905. *“Murderers! What did you want from us? Why did you send us to South Bend? We are going around hungry, and no work is found for us. We will die from hunger. The agent doesn’t care at all. Now there is no other way for us, but to go straight to the river.”*

¹⁸ Before relocation, both groups were highly concentrated in neighborhoods that were above 60 percent Jewish. By 1920, many IRO participants had moved out of enclaves, whereas comparison households exhibited a bimodal distribution split between enclaves and integrated neighborhoods.

shifting participants to neighborhoods with fewer co-ethnics and more higher status neighbors. **Figure 6** confirms that, by 1920, IRO participants lived in neighborhoods with a lower Jewish share (by 7 percentage points) and also a higher white-collar share, a higher English speaking share and a higher homeownership rate.¹⁹

5. Data

We combine a series of historical sources to collect information on IRO participants before and after their relocation from New York City, and comparable information on non-participating households. We compiled the dataset in four steps: First, we identified IRO participants in the original program records, which were housed at and partially digitized by the American Jewish Historical Society. Second, we constructed comparison groups from the 1910 census of other Jews living in Jewish enclaves in New York City who did not participate in the program. Third, we linked IRO participants and comparison households forward to the 1920 census. We then link the children in these 1920 households forward to the 1940 census. Fourth, we define outcome variables, including measures of occupational mobility and cultural assimilation. We explain each step in turn.

5.1. *The IRO records*

We obtained records of IRO participants from the American Jewish Historical Society (AJHS), which digitized some of the information originally collected by the IRO program in order to facilitate genealogical research. In particular, the AJHS created an online searchable database with the following information on each program participant: first name, last name, year of removal, age at removal, and city of assignment. We augment this database by transcribing additional variables from the IRO ledgers for each participant, including birthplace, pre-participation occupation, and street address prior to leaving New York. **Figure 7** depicts an image of the records that we used to construct our dataset. Each of the variables that we added to the data is relevant for our analysis. We use birthplace as a characteristic in our census linking procedure (alongside name and age). Pre-program occupation allows us to examine who selected into the IRO program.

¹⁹ Figure 6 is based on versions of equations (2) and (3), each using an enumeration district characteristic as our outcome variables.

Finally, we use street address to map participants to census geography in order to measure initial neighborhood characteristics and to find comparison households who lived nearby before removal.

We develop a geolocation procedure to map IRO participants into 1910 enumeration districts; a detailed description of this method is presented in Section 3 of the **Data Appendix**. Contemporary GIS software does not work well for this historical application given that street names, numbering systems and enumeration boundaries have changed across many US cities over the past century (Shertzer and Walsh, 2019; Connor, et al., 2019). Instead, we performed a fuzzy match between reported addresses in the IRO records and addresses in the 1910 census, which then allowed us to link each address to an enumeration district. Our method is similar in spirit to Akbar, Li, Shertzer and Walsh (2020) but was developed independently. In total, we match 71% of the street addresses in the IRO records. **Figure 8** identifies the 720 enumeration districts that define the boundaries of the four Jewish enclaves in New York City in 1910. On average, these districts were 44 percent Jewish by our measure, compared to the balance of enumeration districts in New York City, which were 6 percent Jewish.²⁰ This figure is a likely underestimate of Jewish population share given that our index will not capture all Jews (and very rarely classifies a non-Jewish person as Jewish; see our discussion below).

5.2. Constructing comparison groups using the new Jewish Names Index

Our main analysis compares IRO participants to other Jewish households who lived in a Jewish enclave in 1910. Because the census does not ask about religious affiliation, we identify likely Jews according to our new Jewish Names Index, which is based on the first and last name of household heads.

Our Jewish Names Index calculates the relative probability in the complete count censuses of 1920 and 1930 of a name (first or last) being held by a speaker of Jewish languages (Yiddish or Hebrew), relative to a speaker of non-Jewish languages. These relative probabilities are then normalized between zero and one for first and last names separately according to this formula:

²⁰ Diner (2000, p. 42) emphasizes that the boundaries of Jewish enclaves were not entirely clear. She cites the *WPA Guide to New York City* from 1939 as defining the neighborhood as “Fulton St. (South St. to Pearl St.) and Franklin St. (Baxter St. to Broadway) on the south to 14th St. on the north; from the East River west to Pearl St. and Broadway; excluding Chinatown.”

$$Jewish\ Index_{name} = \frac{\frac{\# Yiddish\ speakers_{name}}{total\ \# Yiddish\ speakers}}{\frac{\# Yiddish\ speakers_{name}}{total\ \# Yiddish\ speakers} + \frac{\# nonYiddish\ speakers_{name}}{total\ \# nonYiddish\ speakers}}$$

Index values close to two (adding first and last name) are most associated with speakers of Jewish languages and names with values close to zero have no Jewish attachment. We then assign index values to all respondents in the 1910 census by first and last name (94 percent have a non-missing index value).²¹ Our approach follows Fryer and Levitt's (2004) construction of a Black Names Index and Abramitzky, Boustan, and Eriksson's (2020) more general index of name foreignness.

We choose the threshold value of 1.4 on the Jewish Names Index above which individuals are considered likely Jews in order to match external estimates of the total size of the Jewish population from Kuznets (1975) (we consider robustness to alternative threshold values below). **Table 3** lists a set of names from the 1910 census that rank either very high or very low on the index, or around the threshold value. Individuals with traditional Jewish first and last names – such as Hyman or Abraham for first names and Cohen or Kaplan for last names – rank highly on our index. Individuals at the threshold have names like Jennie Snyder or Joseph Rich that could belong either to Jews or non-Jews.

We validate our index in the Canadian census of 1911, which includes information on both names and reported religion. Our index classifies as Jewish 53 percent of Jews and less than one percent of Christians (Catholics and Protestants). Thus, our index is very good at distinguishing Jews from non-Jews (low false positive rate), but is unable to capture all Jews in the population (some false negatives).

The Jewish Names index allows us to define a set of comparison households who lived in one of the four Jewish enclaves in New York City in 1910. Our comparison households must be likely Jews (index value > 1.4), and also male household heads, foreign-born, and between the ages of 16 and 49. Because our method will be less likely to identify Jews who were given (or selected for themselves) non-Jewish sounding names, we apply the same threshold value of the Jewish Names Index to our IRO sample as well for symmetry. **Data Appendix Table 1** reports

²¹ Missing values occur because some individuals have only a first initial and because some very rare names are present in 1910 but not in the 1920 and 1930 censuses used to create the index.

the number of men in each subsample before the census linking procedure, including 25,130 IRO participants and 145,287 comparison households heads in a New York City enclave.

5.3. Record linkage

We estimate the effect of participation in the IRO program on later outcomes by following IRO participants and comparison households to subsequent censuses. We create two linked samples: one that links baseline measures to the complete-count 1920 census, and one that links sons observed in the 1920 households to the complete-count 1940 census. Baseline data is drawn from the IRO records in the year of removal (median year = 1907) for IRO participants and from the 1910 census for comparison households.²² The datasets used in our matching procedure, and the match rates and sample sizes achieved are diagrammed in **Figure 9**.

Our matched samples are based on an automated algorithm developed by Abramitzky, Boustan, and Eriksson (2012, 2014) that creates links by first name, last name, age and state or country of birth.²³ For robustness, we also consider samples that apply the NYSIIS name standardization procedure before linking, or that take a more conservative approach and require individuals to be unique by name and birthplace within a five year age band.²⁴ Abramitzky, Boustan, Eriksson, Feigenbaum and Perez (2019a) demonstrates that these algorithms are on the frontier spanning the tradeoff between creating false links (type I errors) and missing true links (type II errors): the more conservative algorithm substantially reduces potential type I errors, and the less conservative algorithm reduces type II errors.

²² We use the IRO records as a baseline observation for IRO participants rather than the 1910 census for two reasons. First, half of the removals took place after 1910, so many participants were not yet living in the US by the enumeration of the 1910 census. Second, finding IRO participants in the 1910 census would require that every IRO record is double matched (both to the 1910 and the 1920 census), which would limit sample size and would impose an asymmetric matching requirement on the treatment and comparison groups.

²³ The first step of the ABE algorithm screens the initial data for uniqueness by all linking attributes (first name, last name, age and country of birth). In our setting, we start by appending the IRO data to the 1910 census. We then create a sample that includes only unique observations, defined for IRO participants as being either (a) present as a singular observation in the IRO records or (b) present once in the IRO record and once in the 1910 census. Note that, because some IRO removals occur after 1910, we would not expect all IRO participants to be present in the 1910 census.

²⁴ Our main sample matches on exact name but first converts nicknames or name truncations to the likely extended version of the name (e.g. Abe to Abraham, Joe to Joseph).

Data Appendix Tables 1 and 2 report the sample sizes and match rates using alternative linking procedures. We link 3,612 (14 percent) of the IRO records to the 1920 US census and 27,904 (19 percent) of comparison households living in a Jewish enclave in 1910. These match rates are typical for foreign-born cases circa 1900.²⁵ We observe 4,285 sons living in IRO households in 1920, and 21,535 sons living in comparison households, and link 29 percent (31 percent) of these sons forward to the 1940 census.²⁶

One concern with census linking is that it is easier to find a unique match for men who had an uncommon name or who reported an accurate age to the enumerator. Men with these characteristics often have higher socio-economic status than the general population (Abramitzky, et al., 2019a). **Data Appendix Tables 3 and 4** compare men in our matched sample to men in the IRO records (or to men in the 1910 census) who cannot be matched to 1920. Matched men score higher on the Jewish Names Index and our income proxy. To improve external validity, our main results are reweighted by baseline characteristics to match the full population. Column 4 in **Data Appendix Table 5** demonstrates that the reweighting procedure substantially balances the matched sample with the unmatched segment of the population.²⁷ We report unweighted results in the appendix.

5.4 Outcome variables for household heads and their sons

Table 4 includes summary statistics for IRO participants and other sets of comparison households from our linked sample. Recent immigrant arrivals were more likely to volunteer for relocation. The typical IRO participant in our sample arrived in the US in 1903, compared to an average arrival year of 1900 for other residents of Jewish enclaves and of 1896 for Jewish

²⁵ We suspect that the quality of the IRO records can explain the disparity in match rates between the IRO and the census samples, given that match rates for the sons linked from the 1920 census are more comparable (match disparity = 26 percent [=5/19] in the father generation and only 6 percent [=2/29] in the son generation).

²⁶ As is typical of historical linked samples, the match rates improve over census years, perhaps because the handwriting is easier for modern transcribers to decipher.

²⁷ Coefficients are weighted by the propensity of being matched $P_i(M_i = 1|X_i)$, which is calculated from a probit of match status on the covariates (e.g., age, farm status). Observations are reweighted by $(1 - P_i(M_i = 1|X_i))/P_i(M_i = 1|X_i) \times q/(1 - q)$, where q is the proportion of records linked.

households outside of enclave neighborhoods. We thus control flexibly for year of arrival in the US in our analysis.²⁸

At the time of removal, IRO participants had substantially lower income scores than comparison households (earning \$720 in 1940 dollars, relative to \$990 for other enclave residents). Because the 1920 census does not contain individual earnings information, we use this income score as our main economic outcome. Our income score is based on a statistical model predicting income from covariates in the 1940 census (the first year with income data), and then using this model to assign income for men in earlier years. In particular, we regress log income in 1940 on fixed effects for 3-digit occupation, age, country of birth, and current state of residence, as well as all interactions.²⁹

Both IRO participants and other residents of Jewish enclaves have similarly Jewishly-identified names by our Jewish Names Index (index = 1.83-1.84), whereas Jews that lived in other parts of New York City or in the rest of the country had less Jewishly-identified names (index = 1.76-1.77). All IRO participants and other enclave residents lived in the New York metropolitan area before removal.³⁰ By 1920, 13 years after the median removal, 54 percent of IRO participants remained outside of the New York area, compared to only 28 percent of comparison households. Before removal, IRO participants lived in similarly Jewish neighborhoods (54 percent versus 55 percent Jewish). By 1920, both participants and non-participants were living in more integrated neighborhoods, but this trend was particularly pronounced for IRO participants (a decline from 54 percent to 33 percent Jewish).

²⁸ Some of the difference in arrival years is mechanical, because all comparison households must have arrived by 1910 in order to be enumerated in the 1910 census, whereas some IRO participants arrived and were relocated after 1910.

²⁹ This method follows Abramitzky, et al. (2020) and is similar to the machine-learning approach for computing income scores proposed by Saavedra and Twinam (2018). Note that, for all interaction terms, we interact covariates with census division, instead of state. In addition, the 1940 census does not record farm income. We compute income for farmers following Collins and Wanamaker (2017). Specifically, we compute farmer incomes by multiplying the income of farm laborers in 1940 with the ratio of earnings for farmers versus farm laborers in the 1960 census, by region and immigration status. Few men in our sample are farmers.

³⁰ We use the State Economic Area boundaries to define living in the New York City area in 1920. This definition includes the five boroughs of New York City, along with Nassau, Rockland, Suffolk and Westchester counties.

6. Effects of participation in the IRO program

6.1 Estimation strategy

To study economic assimilation, we compare the income score of IRO participants to other residents of Jewish enclaves in New York City in the 1910 census, both before and after relocation. We stack data from two periods: before and after removal. Data before removal comes from the IRO records for program participants (median year = 1907) or from the 1910 census for comparison households. Post-removal observations are from the 1920 census. We then estimate:

$$y_{it} = \alpha + \beta_1(IRO_i \times After_t) + \beta_2(IRO_i) + \beta_3(After_t) + X_i\Gamma + \varepsilon_{it} \quad (1)$$

where the outcome variables y for household i include the logarithm of income score for fathers in 1920 or sons in 1940. The coefficient β_2 represents differences between program participants and comparison households before removal. We expect that $\beta_2 < 0$ if IRO attracted men who had poor labor market prospects. The coefficient β_3 represents income growth between 1910 and 1920; we expect our income score to be higher in 1920 ($\beta_3 > 0$). Our coefficient of interest is β_1 , which tests whether IRO participants experienced greater earnings gains relative to non-participants after removal. If leaving New York led to improved earnings, we expect $\beta_1 > 0$.

Our preferred specification adds a set of fixed effects for initial occupation and for placement in the initial income score distribution (in quintiles), along with interactions with the $After_t$ indicator to allow for differential trends in earnings growth by initial economic characteristics.³¹ These interactions are included in the vector X_i , along with fixed effects for a series of demographic attributes, including individual years of birth, individual years of arrival in the US, and birth place (Russian/not). In each case, we interact controls with the indicator for being in the 1920 census to allow for differential trends by group. In some specifications, we add a set of 1910 enumeration district fixed effects and their interaction with the $After_t$ indicator.

³¹ We include 20 occupational fixed effects, one for each of the 19 most common occupations and then a 20th category for the remaining observations (which accounts for 16-18 percent of the data). Note that occupation and income score are not identical because the income score is also based on age, state of residence, and country of birth.

For our cultural assimilation measures, we observe the Jewish Names Index for a man's wife and children in 1920. We also compare the Jewish Names Index of participants and non-participants at baseline (circa 1910) to assess selection into the program on cultural attributes:

$$\text{Own name index}_{i1910} = \alpha + \beta_4 \text{IRO}_i + X_i \Gamma + \varepsilon_i \quad (2)$$

$$\text{Wife/child name index}_{i1920} = \alpha + \beta_5 \text{IRO}_i + X_i \Gamma + \varepsilon_i \quad (3)$$

β_4 measures whether IRO program participants had a stronger Jewish identity as measure by the Jewish Names Index, and β_5 indicates whether program participants were more likely to marry wives with distinctively Jewish first names or to give their children Jewish names. Equation (3) includes a control for a man's own Jewish Names Index in the vector X . Therefore, the coefficient β_5 reveals whether an IRO participant married a less Jewishly-identified spouse relative to a comparison individual with a similarly Jewish name. If living outside of New York exposed participants to a wider range of cultural influences and expanded their pool of marriageable women, we expect $\beta_5 < 0$.

6.2 Occupational attainment and cultural assimilation after removal

We now turn to understanding the effect of leaving enclave neighborhoods on economic and cultural assimilation. If living in an enclave hinders occupational attainment, we expect IRO participants to move up the occupational ladder after removal from the Jewish neighborhoods in New York City thereby increasing our income proxy. However, we note that participation in IRO entailed leaving a highly-paid state (New York) to move elsewhere in the country, which might dampen the effect of the program on our income score. Furthermore, if immigrants engage in social interactions primarily in their local neighborhoods, we would expect that IRO participants would encounter more assimilated (or even non-Jewish) spouses in their more integrated neighborhoods after removal. If retaining their cultural identity becomes less important to them, we would also expect IRO participants to choose less Jewish names for their children after removal.

We start in **Table 5** by comparing the income score of IRO participants and other residents of Jewish enclaves before and after removal. Consistent with the program's goals of assisting poor immigrants, individuals who availed themselves of the program had 18 percent lower earnings at

baseline (column 1). By 1920, around 10 years after removal, participants in the IRO program had converged almost completely with comparison households (column 2). As these two snapshots would suggest, the difference-in-differences coefficient implies that IRO participants gained 23 percentage points more in occupation-based earnings relative to comparison households in the decade after removal (column 3). This gain is apparent even after controlling for enumeration district (baseline neighborhood) trends, which is not surprising because we select our comparison households to reside in the same set of neighborhoods from which IRO participants are drawn (column 4).

Because IRO participants started out with lower earnings, the observed growth in earnings may simply reflect a process of convergence or economic assimilation driven by factors beyond neighborhood mobility. Our preferred specification in Column 5 thus compares men who held the same occupation and were situated in the same quintile of the income score distribution at baseline. A large portion of what appears to be gains from the IRO program was indeed driven by general convergence experienced by men in lower-paid occupations. Yet, we continue to find that IRO participants earned 4 percent more than others with the same socio-economic characteristics at baseline. We will use this specification for the rest of the paper.

Panel 2 of Table 5 follows families forward to 1940 – 30 years after the average removal – to observe their sons in the labor market at around age 30. In columns 1 and 2, we reproduce the father’s regressions for men whose sons contribute to the analysis (that is, men who have sons in the 1920 census who can be followed forward to 1940). IRO participants were also negatively selected in this subsample – although slightly less so, earning 10 percent less than comparison households – and completely converged by 1920. In the 1940 census, we find that the sons of IRO participants in 1940 earned 7 percent more than the sons of comparison households whose fathers held the same occupation at baseline. Thus, the gains experienced by IRO households are retained into the second generation and possibly even expand, but the standard errors are large enough that we cannot rule out that the program gains are the same in the two generations.

Appendix Figure 2 more explicitly compares the intergenerational mobility of children of IRO participants with the children of non-participants whose parents had comparable baseline income as measured by their rank in the national income distribution (following Chetty et al. 2014 and Chetty et al. 2018, and Abramitzky et al. 2020). To do so, we first rank each son based on his income score relative to other sons born in the same year. Next, we rank fathers relative to all other

fathers with sons born in the same year. Ranks are based on position in the *national* income distribution. We then regress a son's rank on his father's rank, allowing both the slope and intercept to differ for sons of IRO participants and non-participants. We find a higher intercept for the sons of IRO participants relative to the sons of the non-participants, indicating higher levels of absolute mobility for IRO participants. Moreover, the slope for IRO participants is flatter than for non-participants, suggesting a weaker association between the initial rank of father and sons among IRO participants.³² The intergenerational mobility associated with the IRO program is most apparent for families that started out below the median of the income distribution, suggesting that leaving enclaves allowed some families to move out of poverty.

IRO participants lived in more integrated neighborhoods in 1920, and thus may have had more interactions with non-Jewish neighbors, hastening the process of cultural assimilation. In **Table 6**, we ask whether IRO participants were less likely to marry Jewishly-identified spouses (as measured by Jewish Names Index), and to give their children Jewish sounding names, relative to comparison households who lived in Jewish enclaves in 1910. IRO participants were not selected on Jewish identity; rather, they held similarly Jewish-sounding names at baseline to comparison households. Yet IRO participants married spouses with less Jewish-sounding names, which may be an indication that exposure to life outside the enclave introduced them to a different pool of marriageable women. IRO participants and their spouses appear to select *more* Jewish-sounding names for their children, which is not consistent with the idea of changing cultural values. We will show below that these patterns are strikingly different by exposure to time spent out of New York.

6.3 Addressing selection into the program

Men who chose to participate in the IRO program may have been different from their neighbors in unobservable ways – i.e., more resourceful or talented – and perhaps these attributes, rather than the mobility assistance through the IRO program, can explain their occupational attainment after removal (akin to a classic Ashenfelter (1978) dip).

³² **Appendix Figure 3** produces a similar graph for intra-generational mobility, wherein we plot a father's position in the income distribution in 1920 relative to his rank at baseline. We find that IRO participants were more likely to move up the distribution relative to non-participants, and that this mobility occurred most prominently below the median.

We address the potential for selection on unobservable attributes in a number of ways. First, we consider variation in exposure to life outside of New York City among IRO participants. If leaving enclave neighborhoods was salutary, we would expect that men who had longer exposure to life outside of the city by our follow-up year (1920) would experience the strongest economic benefits from initial removal. Exposure to life outside of New York can increase on two margins: removal from the city earlier versus later in the program, and/or remaining outside New York City rather than returning to the city. We consider each in turn.

In the first panel of **Table 7**, we split IRO participants into three groups of roughly equal size based on when they were removed from the city: early removals (1900-06), middle removals (1907-11) and late removals (1912-1919). The three groups look similar in their initial income scores, with each earning 15-22 percent less than comparison households, but gains from the program monotonically increase with exposure to time outside of New York. Compared to men who held the same baseline occupation and were placed in the same quintile of the income distribution, IRO participants who were removed early earned 18 percent more than comparison households by 1920, men who were removed in the middle earned 6 percent more, and men who were removed late did not gain at all, and in fact appear to fall behind in income score (column 3). Together, these coefficients imply a 0.5-1.1 percent gain in income score for each year spent out of the immigrant enclave.³³

We interpret the fact that IRO participants who moved earlier experienced larger income gains as evidence that greater exposure to life outside of New York City yielded a positive return. However, men who joined the IRO program earlier may have been different in unobserved ways than men who joined later. For example, men who joined earlier may have had fewer family connections in New York City, or more family outside of the city. Alternatively, men who joined earlier may have moved out of a sense of adventure (not as a response to a negative shock).³⁴

We find no evidence for these alternative explanations. We still find higher gains for early movers in **Appendix Table 1**, which controls for the number of likely Jews who shared an individual's surname (column 3) as a proxy for having family in New York, or directly adding

³³ The middle period in this analysis contains the recession years of 1907 and 1908, which may influence selection into the IRO program and later success. Results look similar when dropping these two years.

³⁴ Recall that we control for arrival year in the US, and so here we compare two men who arrived in New York in the same year but left via the IRO program in different years.

surname fixed effects (column 4). The same is true in **Appendix Table 2**, which restricts the IRO sample to participants who are identified in the records as “direct removals” (i.e., those who were *not* leaving New York to meet family but instead were placed in locations by program officers). Late movers do not appear to be especially disadvantaged; instead, we find that late removals had the highest income score at baseline (Table 7). Furthermore, the benefits of early moves appear even when we drop men who moved soon after arrival in the US who may join the program for different reasons – e.g., out of sense of adventure (**Appendix Table 3**). Without strong evidence for alternative explanations, we conclude that year of removal is likely driven by the idiosyncratic timing of negative shocks that might prompt men to leave the city.

Panel B instead compares the occupational gains of IRO participants who returned to New York versus those that remained out of the city in 1920. Initially, these men look identical on income score. Yet, here we find the counterintuitive result that men who remained outside of New York earned 3 percentage points more than comparison households, whereas men who returned to New York earned 5 percentage points more. However, we show below that this gain is driven by regional differences in earnings: New York state was a high-earning location in 1940 relative to the rest of the country. We introduce an alternative income score below that does not assign different income levels by state of residence and find that men who remained out of New York gained more in movement up the occupational ladder (see Section 6). All other results in the paper are robust to the use of this alternative score.

Table 8 explores the effect of exposure to time outside of New York on cultural assimilation. Panel A does not find a strong relationship between time spent out of New York and spouse characteristics or names selected for children. These patterns are more consistent with a change in marriageable pool than with a shift in cultural attitudes, which would have taken some time to occur.

Panel B shows that the decision to return to New York was highly selective on Jewish identity, which is an interesting finding in its own right. IRO participants who chose to come back to New York had names that were themselves 1.8 points higher on the Jewish Names Index than other enclave residents, and gave their own children substantially more Jewish names after return (4.3 points).³⁵ By contrast, IRO participants who remained out of the city looked similar to

³⁵ To put this magnitude in perspective, consider that, in 1910, men in enclave neighborhoods were married to wives that scored 9 points higher on the index than men outside of enclaves (Table 3).

comparison households at baseline but married spouses with less Jewish-sounding names (3.6 points). Despite marrying women with less Jewish names, these couples selected similarly Jewish names for their kids, suggesting again that leaving enclaves shifted the pool of potential spouses but did not change cultural attitudes.³⁶

If all men who chose to leave New York City (including through the IRO program) were more entrepreneurial or resourceful in some way than those who remained, a better comparison for IRO program participants might be other voluntary movers. **Table 9** subdivides non-participants who lived in a New York City enclave in 1910 into two groups: men who voluntarily left the enclave by 1920 and men who remained in the enclave by 1920 (the omitted category). We document two patterns. First, the IRO program allowed men who were otherwise struggling (i.e., who had low initial income scores) to leave the city, whereas self-financed movers held similar income scores at baseline to the comparison group. Second, IRO participants who remained outside of enclaves in 1920 gained the same amount as self-financed movers, each earning 4-5 percent more than comparison households who held the same occupation at baseline (column 3). Together, these patterns suggest that relocation through the IRO program generated a “return to mobility” similar to the benefits received by other voluntary movers. What made the IRO program distinctive was that it provided the funding and coordination necessary to prompt poorer residents to leave the enclave.

7. Robustness

We make a number of decisions with our data in order to produce main results. This section tests the robustness of our findings to each of these choices.

We start in **Appendix Table 4** by adding two comparison groups to our sample: Jewish residents of New York City who lived outside of enclave neighborhoods in 1910, and Jewish men outside of New York in 1910. At baseline, Jewish residents of non-enclave neighborhoods earned 10 percent more than comparison households and Jews outside of New York earned 13 percent less. Both groups converged with residents of enclaves (the omitted category) over time, but the

³⁶ Regressions underlying column 3 are estimated at the level of the individual child. As a result, men with multiple children in the 1920 household will enter the sample multiple times. Results look similar if we instead collapse the results to the level of the household head, instead using the average Jewish Names Index value as a dependent variable.

degree of convergence for IRO participants was faster. We report these results both weighted and unweighted by baseline characteristics.

Appendix Table 5 considers our definition of “likely Jews,” which is based on a threshold value on the Jewish Names Index. Our main results use a threshold of 1.4. Here, we use two more conservative values, threshold values of 1.6 and 1.8, which respectively eliminate 10 percent and then 30 percent of the sample. We find a similar pattern of initial selection and earnings convergence for IRO participants in each case, with participants earning 4-5 percent more by 1920 than comparison men with the same initial occupation and income quintile.³⁷

Our results are robust to our choice of matching algorithm. **Appendix Table 6** presents samples built using two alternative algorithms: a modified version of the ABE algorithm that standardizes names using the NYSIIS phonetic algorithm (rather than using exact names as recorded) and a more conservative version of the ABE algorithm that requires individuals to be unique by name and country of birth within a five-year age band. In both cases, we continue to find an earnings gain of 4-5 percentage points relative to men who held the same occupation and income quintile at baseline.

We next try dropping two groups of IRO participants in **Appendix Table 7**: men who have “no trade” listed in their occupational field (there is no equivalent “no trade” category in census data), and men who reported a preferred location to the IRO program officers. Neither subsample alters our results, with the remaining IRO participants earning 4-5 percent more than similar men from the comparison group. This finding suggests that the results are not being driven by the subset of IRO participants who left New York to join established family networks.

Appendix Table 8 considers two alternative income scores that do not vary by state of residence. Our main income score assigns different income levels to men who live in different states, according to the regional differences in wage levels in 1940. Here, we use the standard 1950 “occupation score” that assigns each individual the median earnings for his occupation from the 1950 census (panel A), as well as a modified version of our “income score” that allows earnings to vary only by occupation, age and country of birth (panel B). Because the variables have different scales, we compare across outcomes by considering the size of the percentage point gain (column 3), relative to the initial earnings gap (column 1). In both cases, the IRO program gain is twice as

³⁷ Results are also similar when we construct a Jewish Index based on last name alone because many Jews used common first names like Mary.

large as the main effect in Table 5, which incorporates the income losses associated with leaving New York. This pattern suggests that the true return to leaving enclaves alone (rather than combined effect of leaving an enclave neighborhood and a high-paying state) may be even larger than our main estimates suggest. **Appendix Table 9** uses our alternate income score to reproduce the results by year of removal (early/middle/late) and by return to New York (compare to Table 8). Most importantly, we find here that IRO participants who remained out of New York in 1920 enjoyed larger gains than participants who returned to the city (9 percentage points versus 4 percentage points), which supports the importance of exposure to time outside of the enclave.

8. Conclusion

Both today and in the past, many immigrants live in enclave neighborhoods, residentially segregated from the native born. We document the economic and cultural assimilation patterns of one such immigrant group during the Age of Mass Migration – Eastern European Jews – and study a unique program that relocated almost 40,000 Jewish households from enclave neighborhoods in New York City to more integrated areas around the country circa 1910. The Industrial Removal Office program provided the funding and coordination necessary to allow poorer residents to leave the enclave.

Overall, Jewish immigrants integrated into the broader economy and assimilated into society. We find that men who volunteered to be resettled through the IRO gained 4 percent more in income score by 1920 than comparison households that held the same occupation and income quintile at baseline, suggesting that leaving enclave neighborhoods contributed to this upward mobility. These benefits were transmitted to the next generation, as the sons of IRO participants earned more than the sons of comparable households in 1940. Ours is one of the first papers that documents the effect on adult outcomes for children that grew up in an immigrant enclave.

By leaving the large Jewish community in New York City, IRO participants were exposed to neighbors from more diverse backgrounds, and they married spouses with less distinctively Jewish names. However, these couples did not select less Jewish names for their children, emphasizing the importance that many immigrants place on retaining their cultural identity.

IRO participants who moved earlier in the program and thus spent more years out of New York City experienced the largest income score gains by 1920, and were especially likely to marry spouses with less distinctively Jewish names. Men who chose to eventually move back to New

York City were distinguished for having more Jewish names (a sign of cultural attachment). Returning to the enclave carried a cultural benefit through proximity with ethnic community, but may have come at an economic cost

Prior evidence from refugee resettlement finds that the small immigrant enclaves can be beneficial to their residents. By contrast, in the context of Jewish immigrants in the early 20th century, we document the economic costs of remaining in a Jewish immigrant enclave. This comparison raises the possibility that there is an “optimal” enclave size – namely, living with too few countrymen may limit ethnic networks, while living with too many may create isolation. Another possibility is that benefits of enclaves are heterogeneous across groups –unlike refugee populations, Jewish immigrants held relatively highly-paid occupations upon first arrival in the US. Understanding when and who are helped by enclave neighborhoods is a fruitful avenue for future research.

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