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RIDING TO OPPORTUNITY: GEOGRAPHIC AND HOUSEHOLD EFFECTS FROM THE ORPHAN TRAINS

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ABSTRACT

Between 1854 and 1929, approximately 200,000 children were transported from East Coast cities to new homes in the American West, motivated by the theory that a change of geography and household environment can transform lives. We leverage quasi-experimental variation in child placements to evaluate both whether being sent West improved outcomes relative to remaining in New York institutions, and whether variation in destination and foster household characteristics affected later life success. Linking tens of thousands of ``orphan train'' riders and comparable non-relocated children to Census records through 1940, we find no systematic evidence that relocation itself improved adult economic outcomes. Among children sent West, substantial variation in county-level economic opportunities also did not predict adult success. In contrast, the individual foster household income predicts children's later incomes, with an estimated intergenerational elasticity of about 0.2.

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1 Introduction

> "...when the alternative is the streets of New York or a good place in the country, there is no room for hesitation."
>
> —New York Juvenile Asylum, *Second Annual Report* (1853, p. 12)

On October 1, 1854, a train pulled into the depot at Dowagiac, Michigan bearing 45 children sent from New York City. At the local tavern, they were distributed among the townspeople to begin new lives in the West. Through similar journeys over the next 75 years, charitable organizations relocated up to 200,000 children from the urban Northeast to locations throughout the United States in what became known as the "orphan train" movement. These journeys were driven by a strong belief among sponsoring organizations that a change of location–from the city to the country, from the streets to the farm–could transform the future social and economic outcomes of the children.

Understanding how childhood environment affects adult economic outcomes remains central to modern debates about human capital development, foster care, housing mobility programs, and place-based interventions. We leverage the historical episode of the orphan trains to investigate how childhood economic environment characteristics causally affect adult outcomes. By linking tens of thousands of orphan train riders and similar non-relocated children to their decennial census entries in adulthood, we exploit quasi-random variation in which children were sent West and in their destination counties and households to identify how specific local characteristics influence long-term economic welfare.

A fundamental challenge in estimating place effects is that residential location—and relocation—is typically endogenous. Most individuals or families select where to live and when to move based on unobservable characteristics that may also influence economic outcomes (Chetty and Hendren 2018b; Harding et al. 2023). The Moving to Opportunity (MTO) experiment addressed this through randomized housing vouchers, but faced important limitations: incomplete take-up, selective relocation choices, relatively short-distance moves within metropolitan areas, and a primary focus on the specific effect of variation in local poverty rates (Ludwig et al. 2013; Chetty et al. 2016). More expansive investigations of administrative records consider a broader range of features of local environments (Chetty et al. 2020), but again must contend with the endogeneity of location choice. Studies of forced migrations (Chyn 2018), while solving the selection-into-moving problem, introduce potential confounding impacts of the displacement itself (Becker and Ferrara 2019; Becker et al. 2020).

The orphan train movement provides a unique natural experiment to overcome these empirical challenges. Children who shared very similar origins and backgrounds in New York were transported by railroad to a wide range of locations spread across the country. Our analysis confirms there was no significant relationship between child characteristics and destination county characteristics, strengthening our identification strategy.

Unlike most migration, orphan train placements featured extensive variation in destination characteristics with limited self-selection. Unlike MTO participants, these children moved across substantially greater distances that prevented regular interaction with origin environments, allowing for a potentially stronger impact of place effects distinct from origin influence. Unlike forced migrations caused by natural disasters or conflict, these relocations were intentional interventions designed to improve welfare, avoiding the confounding influences of catastrophic displacement (Deryugina et al. 2018; Nakamura et al. 2022).

While the orphan train movement offers an ideal setting for examining place effects given the extensive variation in destination characteristics for the orphan train riders, a notable limitation is that children experienced changes in both location and family environment, unlike studies where families move together. This introduces the challenge of disentangling place effects from family effects, which some research suggests may be the dominant factor in determining outcomes (Oreopoulos 2003). We partly account for household traits and, in doing so, demonstrate their importance, by also linking assigned foster heads of household to their census records and showing how they directly relate to orphan train rider outcomes. Thus this limitation is also an advantage, given the simultaneous reassignment of both location and household, whereas most children studied in the literature move with their current family unit.

Empirically, we address two primary research questions: First, does relocating disadvantaged children to areas with different economic and social characteristics significantly improve their adult outcomes? Second, which specific location characteristics most strongly predict economic mobility for these vulnerable children? Our empirical strategy exploits the conditional quasi-random assignment of children to destination counties. On the extensive margin, we use an instrumental variables approach to account for non-random selection into being sent West, using the probability of being sent West depending on year of admission as an instrument. To study outcomes conditional on going West, we implement an intent-to-treat approach consistent with contemporary mobility literature (Chetty et al. 2016), comparing adult outcomes of children sent to locations with different characteristics while controlling for numerous child- and time-level observables.

To investigate these questions, we combine digitization of novel historical data on the individual orphan train riders and their assigned destinations with a matching methodology to identify these individuals in the complete count decennial U.S. census from 1860 to 1940. Our data includes the full set children set West by the New York Juvenile Asylum (NYJA), a 20% sample of NYJA children not sent West, and about 15,000 additional children sent West by the Children's Aid Society (CAS). The NYJA records contain detailed information about each child, including birth year, birth place, religion, parent occupation and temperance, literacy, numeracy, reason for commitment, which we use to estimate selection into relocation, and we use the sample of children not sent to estimate the extensive margin effect of going West. The CAS ledgers do not record detailed child characteristics but add significant variation in destination counties, strengthening the

empirical identification of place-based effects conditional on relocation.

Our analysis yields three main findings that enhance our understanding of location and household effects. First, we find no evidence that being sent West causally improved adult economic outcomes relative to remaining in New York institutions. Point estimates suggest small negative effects on occupational status, though these are not statistically significant. Second, among children sent West, substantial variation in county-level economic opportunities, including differences in urbanization, industrialization, income, and educational infrastructure, has minimal detectable effect on adult success. Third, foster household income, proxied by occupational status, emerges as a significant predictor of children's later incomes, with intergenerational elasticity estimates of about 0.15 to 0.20.

It is not random who became an orphan train rider, and this endogenous selection correlates with later outcomes. The children chosen to be sent West were statistically older, more literate and numerate, less likely to have been institutionalized for misconduct, more likely to have deceased parents, and more likely to have intemperate parents. After relocation, these children selected to be sent West were 17% more likely to be later found working in agriculture. Their occupational income scores were 3.1 points lower, they were 4.6 points more likely to be homeowners, and they were 3.8 points less likely to be married. This last result aligns with the theory of social displacement as in Barnhardt et al. (2017).

When we isolate exogenous variation in the probability of being sent West by instrumenting for relocation with the overall sending rate by year of admission to the sending organization, the statistical relationship for the extensive margin of being sent West on adult outcomes disappears, except for the fact that children sent West are much more likely to be enrolled in school in the first decennial census after relocation. The results do not vary by a child's age upon relocation, unlike the results in Chetty and Hendren (2018a), Deutscher (2020), or Wodtke (2013), and they are not sensitive to the match quality threshold used to construct the sample.

On the intensive margin, we find a null relationship between county-level characteristics and later outcomes for our matched children sent West. The strongest relationship we observe is that children sent to counties with a greater share of children enrolled in school are themselves much more likely to be enrolled in school. Neither urbanicity, manufacturing intensity, income, nor population predicts a child's later adult income or occupational income score, and once more there is no heterogeneity in effect by age at relocation. These null results persist even as we limit the sample to only the children linked most robustly to the Census.

We also find that the *household* within which a child is placed seems to have a strong effect on their outcomes. When we locate the census records of the assigned foster parents of the children sent West, we find that the head of household's occupational income score is a strong statistical predictor of a child's later occupational income score, with an estimated elasticity of about 0.15 to 0.20—with estimates increasing in

the degree of confidence in match accuracy. Although there is some evidence that assignment of specific children to specific fosters households is not entirely random, these estimates are robust to controlling for the child characteristics on which matches are potentially selected.

Our paper contributes several insights to the literature on place effects and intergenerational mobility. First, we extend research on neighborhood effects and economic mobility (Chetty and Hendren 2018b; Chyn and Katz 2021) by examining a wider range of place characteristics across more diverse geographic settings than typically studied in contemporary contexts. Second, we provide new evidence on the long-run effects of early childhood interventions, complementing work on critical periods in development (Almond et al. 2018; Currie and Almond 2011). Third, we contribute to understanding historical migration patterns and their economic consequences, connecting to literature on the Great Migration and other population movements (Derenoncourt 2022; Tan 2023). Finally, we provide the first comprehensive quantitative analysis of the orphan train movement's economic legacy, offering insights into how geographic mobility shaped opportunity for disadvantaged children in American history.

The remainder of the paper is organized as follows: Section 2 provides historical background on the orphan train movement. Section 3 describes our data collection from historical records. Section 4 presents our estimation of selection into going West and the causal impact of relocation. Section 5 details the process of matching orphan train riders to their census records. Section 6 investigates the impact of place characteristics on economic mobility. Section 7 investigates the role of foster parents characteristics. Section 8 discusses implications and concludes.

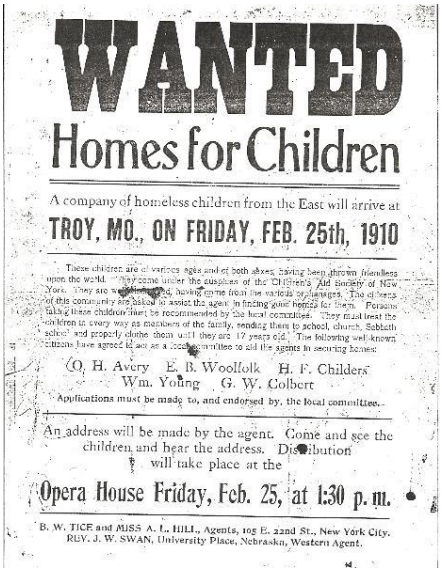
2 The Orphan Train Movement

As America urbanized in the mid-19th century, major cities became home to an increasing number of children living on the streets. An 1849 report by New York Police Chief George W. Matsell focused attention on the social dangers of these children, both for the welfare of the children themselves, as well as for other citizens for whom "the degrading and disgusting practices of these almost infants, in the school of vice, prostitution and rowdyism, would certainly be beyond belief." Until this point, the primary policy response to child abandonment, delinquency and destitution was the establishment of large institutions where children were confined until either claimed by relatives or adopted. However, by the 1850s the institutional care system was increasingly criticized both for its inhumane treatment of children, as well as its inability to control the problem of unhoused children (O'Connor 2004).

These evident social ills attracted a variety of social reformers, of whom the best know may be Charles Loring Brace. Brace founded the Children's Aid Society (CAS), at the time the largest charitable organization

devoted to improving the welfare of New York Street Children (Brace 1880; O'Connor 2004; Staller 2020). The CAS engaged in a wide variety of activities, and pioneered the "placing out" of New York children into new households in the American West, a program that came to be known as the Orphan Trains. The care of orphans or abandoned children by volunteer households, though now commonly known as the foster system, was a radical idea in its day and attracted substantial controversy. Nevertheless, its low cost and (apparent) success attracted many other charitable organizations to create their own placing out programs. From the first cohort of children sent West in 1854 until the program ended in the 1920's, scholars have estimated that approximately 250,000 children were transported from East Coast cities to new homes further west.[1]

Most orphan trains operated by charitable organizations shared several key features (Holt 1994). Children embarked on a train headed West, usually from New York, with no knowledge of their destination or future household. After several days of travel, they disembarked in a rural town, and were taken to a public location, often a church or meeting hall, where local inhabitants had been informed by an advance agent of the organization that they might find children in need of a home. Children were then assigned to households based upon the household's preference, which was often highly arbitrary. We illustrate this process in Figure 1, which shows a typical local newspaper ad placed by the CAS in advance of a train's arrival.


WANTED

Homes for Children

A company of homeless children from the East will arrive at

TROY, MO., ON FRIDAY, FEB. 25th, 1910

These children are of various ages and of both sexes, having been thrown friendless upon the world. They come under the auspices of the Children's Aid Society of New York. They are well-[illegible], having come from the various orphanages. The citizens of this community are asked to assist the agent in finding good homes for them. Persons taking these children must be recommended by the local committee. They must treat the children in every way as members of the family, sending them to school, church, Sabbath school and properly clothe them until they are 12 years old. The following well-known citizens have agreed to act as a local committee to aid the agents in securing homes:

O. H. Avery E. B. Woolfolk H. F. Childers
Wm. Young G. W. Colbert

Applications must be made to, and endorsed by, the local committee.

An address will be made by the agent. Come and see the children, and hear the address. Distribution will take place at the

Opera House Friday, Feb. 25, at 1:30 p. m.

B. W. TICE and MISS A. L. HILL, Agents, 105 E. 22nd St., New York City.
REV. J. W. SWAN, University Place, Nebraska, Western Agent.

Figure 1: Example local newspaper advertisement placed by the CAS soliciting homes for orphan train riders. Agents such as those listed at the bottom were responsible for generating interest in pending arrivals and arranging for local committees to vet applicants interested in taking a child, though applicants were not matched to specific children ahead of time.

To modern sensibilities the foster child-household matching process appears extraordinarily informal and

[1]As this figure has no traceable origin, we prefer to report one closer to 200,000 based on aggregate totals reported in historical documents from the NYJA, CAS, and NYFH.

rapid. Since no official procedure existed, we must rely on the written narratives of the children themselves (Warren 2001). A common theme is that children were selected based upon physical appearance; in other cases children recall that they were asked a few question about education, or housekeeping skills. While the children had no official role in the process, narratives frequently highlight how they could influence the process, for instance by biting the hand of a unappealing adult, or running away after a few hours to re-board the train (Holt 1994). Thus while there was no premeditated matching of children with placement households, we cannot rule out that some endogenous matching may have occurred during the short period of contact after the children disembarked from the train.

The role of the child within the foster household was left deliberately ambiguous by the sending organizations. It was expected that, as with many children in rural households, the re-located child would work on the family farm, business, or in the household, and the prospect of "free" labor was likely an important incentive for households to accept a orphan train child. However, all agencies required foster households to agree, in writing, to provide a minimum standard of care and education to foster children. Sending organizations employed agents to visit children to verify that these standards were met, although the agent's abilities to do so may have been highly limited. The NYJA typically legally indentured children to receiving households, while the CAS required no formal contract.

The orphan train movement attracted intense study from policymakers during its operation, and has continued to be continued to be the topic of historical works. As early as 1880, widespread complaints from Western states that the CAS was transporting (proto) criminals to their lands prompted a follow-up study by Hart (1884), tracing the outcomes of children placed by the CAS in Minnesota. Hart's work and a similar study by Georgia Ralph in 1928 both argued that, although the triumphant anecdotes published by the CAS overstated the program's success, there was no evidence that orphan train riders were more criminal than the surrounding population. While there has been a substantial narrative, historical, and genealogical exploration of the orphan train movement, to date there has been no systematic evaluation of the economic impact of the program on the children sent West. Our current work builds upon these primarily qualitative contemporaneous studies, using modern econometrics and census-linking tools to generate the first quantitative evaluation of the orphan train program.

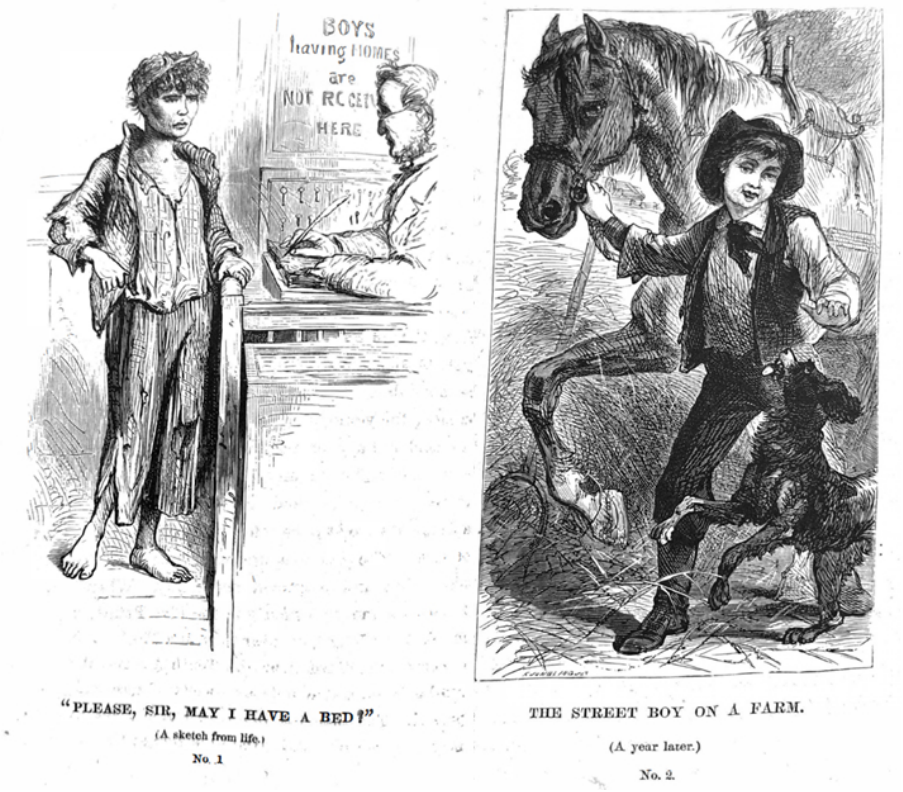


Figure 2: An illustration of the mental model of transformation held by sending agencies (Brace 1880). Organizers believed that physical and social environment in New York City was a detrimental influence, removal from which meant children would be free to live up to their potential. This model derived from the contemporary romantic ideal of the American West as the land of opportunity.

3 Data Sources

The orphan train rider data analyzed here comes from two sending organizations. The first source is the paper "Company Books" kept by CAS, the largest sending organization in the orphan train movement. For those ledgers that we were able to access in the archives of the New York Historical Society, we scanned and fully digitized the available information pertaining to each individual child.[2] The primary recorded information includes the child's, name, age, birthplace, religion, and parental status, and their departure date, intended destination, and the name of the foster to whom they were allocated. Collectively, this sample contains the records of 18,358 children sent West between 1855 and 1896.

The second data source is the records of the New York Juvenile Asylum (NYJA). The children assisted by the NYJA were similar to those placed through the CAS, and at times the two organizations collaborated

[2]Mid-way through our scanning process, the New York Historical Society (since rebranded as the "New York Historical") closed its archives to researchers for a multi-year renovation, preventing us from obtaining the complete set of potential records.

in their placing out programs (Kidder 2023). The NYJA sent comparatively few children West: we have data on 6,599 unique such children. However the NYJA data have a unique advantage: detailed information on the child's background at the time of admission has been preserved and is publicly available in the "reception book" records (Kidder 2021).[3] This includes information on the reason that the child was admitted to the NYJA, the child's characteristics, and the mortality, occupation, nativity, address, and sobriety of their parents. These data give us a valuable insight into the demographic composition of the street children of New York, as well as allow us to verify that the geographical placement of the children was indeed orthogonal to their observable characteristics.

The NYJA collected this rich demographic information on all children who were admitted to the institution, whether or not they were ultimately sent West. The children who remained in New York can then serve as a comparison group that informs us about the distinctive characteristics of those that NYJA decided to place out. For this paper, we also digitized the reception book records for approximately 20% of children who were not sent West, distributed evenly across all years of the NYJA orphan train program.

An example page from the NYJA House of Reception book is provided in Figure 3. A record includes detailed demographic information about a child, their parents, and their reason for being committed, as well as any later updates. In this example, the child was admitted in March 1899 and sent West in November of that year.

[3]These data may be available for some children sent West by other agencies, however access is restricted and information is typically released only to direct descendants.

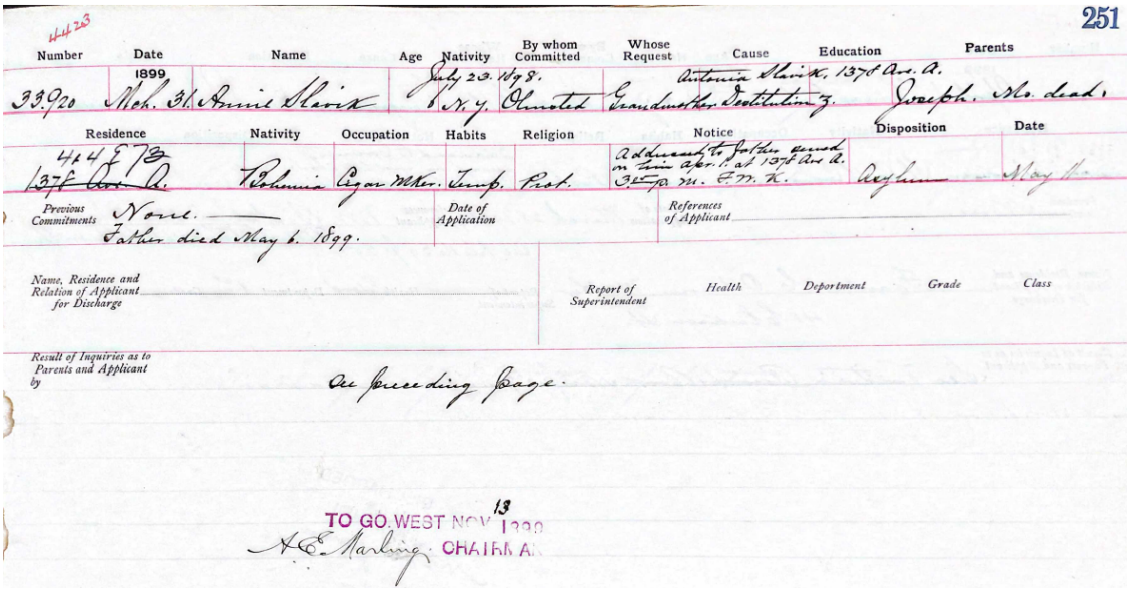


Figure 3: Example page from the NYJA House of Reception book (Source: Clark Kidder). The stamp at the bottom indicates that this child was designated to be sent West in November 1890, having been admitted to the NYJA in March 1899. This particular child was a true orphan at the time of sending, though many others had living biological parents.

To link the children in the NYJA list of those sent West with their admissions records and personal characteristics, we manually searched for every child in the "company books" of orphan train riders in the "reception books" of information collected on children when admitted to the NYJA. Out of the 6,599 children reportedly sent West, we were able to link 5,542 to the reception books.[4] We then gathered information about the destinations to which the children were sent from the company books, which contain both the destination county and the name of the foster parents. In many cases children shifted from one household to another over time; however to maintain the integrity of the natural experiment we exclusively use the characteristics of the county into which the child was initially placed by the NYJA. This "intention-to-treat" approach to causal inference is consistent with that employed in research on the Moving to Opportunity experiment (Chetty et al. 2016).

Geographic variation in the destinations of the orphan train riders is presented in Figures 4, a choropleth illustrating the number of children placed out to each continental U.S. county based on our sample of orphan train riders from the NYJA and CAS. Children from the NYJA were sent primarily to locations in the Midwest, especially Illinois, Indiana, and Iowa. Children from CAS went to a broader range of destinations, and there is is wide variation in the number of counties with at least one placement (905 unique counties in total).

[4]Several years of reception books were not preserved, accounting for 777 of the un-linked children.

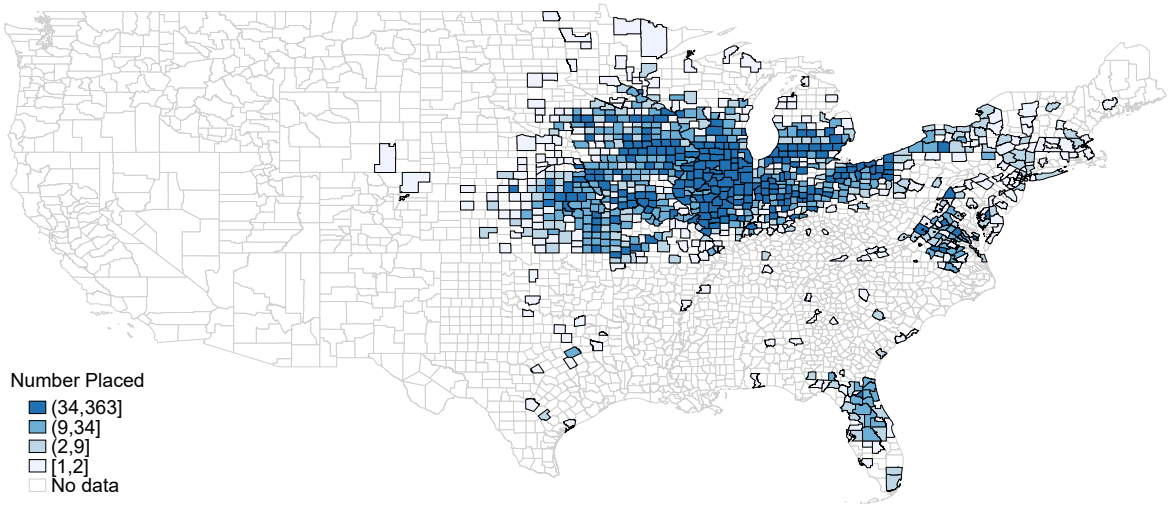


Figure 4: Map of continental U.S. counties where NYJA and CAS orphan train riders were placed. Many went to Midwest locations, though there are 905 unique counties with at least one placement.

To measure variation in economic opportunity, we assemble a rich set of data about the destinations for the orphan train riders. The literature on the geographical determinants of long-term individual outcomes has highlighted a variety of local attributes that might affect upward mobility. Following Chetty and Hendren (2018b) we collect average household wealth, the Gini coefficient of wealth, the number of colleges, the fraction of White children aged 5-20 in school[5], and the number of patents granted in the county prior to the date the child arrived. We expand the set of covariates to include the fraction of the county population in a city over 25,000, the fraction living on a farm, and the share working in manufacturing. Finally, we also include an indicator for the number of years the county has been on the frontier (Bazzi et al. 2020), and the railroad distance of the county from New York City (Donaldson and Hornbeck 2016) and the share of religious congregations in the county that are Catholic. We use these characteristics to measure selection into destinations in Section 6, and we then demonstrate the feasibility of our approach to measuring outcomes with regressions using a subset of these county characteristics in Section 6.

Finally, to locate the orphan train riders in adulthood, we use the complete count decennial US census records from 1860 to 1940 as harmonized by (Ruggles et al. 2024b). Details and results from the procedure to link the individuals in our records compiled from the CAS and NYJA to their corresponding records in the census follow in Section 5.

[5]Virtually all of the children sent on orphan trains were White.

4 The Decision to Send a Child West, and Where

We emphasize, and the results below show, that the decision of whether or not to send a child West was not random, and is in fact highly correlated with the attributes of the child. In contrast, we show that the town (or county) where the child was placed *does* appear to be uncorrelated with the observable characteristics of the child.

Table 1 presents descriptive statistics on the population of children admitted to the NYJA. Since we collect data on all children sent West, but only on a sample of those not sent West, results are weighted by the inverse sampling probability in each group.

Overall, only 16% of children admitted to the NYJA were placed out in Western households. The average admitted child was 11 years old, and most were male (83%) and could read, write and do simple math. Most children had both parents living, and only 6% were literally orphans. The NYJA clerk also recorded that 14% of children had at least one "intemperate" (alcoholic) parent. The modal child was a Protestant born in New York city, though a substantial number were Catholic (21%) or Jewish (14%), and many were born abroad (21%). Among foreign born children, the most common country of national origin is Germany. Finally, the admissions clerk recorded the reason for the child's admission to the NYJA. By far the largest category related to crime or misconduct (59%), although contemporary social commentators (Brace 1880) frequently argued that many of these crimes, such as stealing food or sleeping in the property of others, were the direct result of extreme poverty. A small share of children (6%) were admitted by parents to the NYJA with the specific goal of taking advantage of the opportunities offered by relocation.

Table 1: Summary statistics of children admitted to NYJA

	Mean	N
Sent West	0.15	11,706
Age when admitted	11.07	11,677
Female	0.17	11,568
Can read & write	0.76	11,706
Can read, write, & calculate	0.55	11,706
Mother dead	0.22	11,706
Father dead	0.30	11,706
Orphan	0.06	11,706
Catholic	0.21	11,706
Jewish	0.14	11,706
Born in NYC	0.73	11,471
Born abroad	0.21	11,471
≥ 1 parent intemperate	0.13	11,706
Admitted for crime or misconduct	0.59	11,706
Admitted for economic opportunity	0.06	11,706

The NYJA did not select children to be sent West randomly. We document this selection process in a series of regressions in which we estimate linear probability models of an indicator for whether a child was sent West, $SentWest_i$ on that child's characteristics X_i and a fixed effect for the year when the child was admitted to the NYJA, γ_t:

$$SentWest_i = X_i\beta + \gamma_t + \epsilon_{it}$$

The results, in Table 2, provide a detailed insight into the selection process. The NYJA slightly favored older children and girls, perhaps reflecting the fact that these children were easier to place. Children born in NYC were more likely to be sent West, and children born abroad less likely, relative to the baseline category of American-born children not from NYC. The education of the child has no impact on their emigration outcomes, but we find a very large effect of the death of parent, particularly for children without a living biological mother, and orphans. Overall, orphans were 30 percentage points more likely to be placed on an "orphan train", relative to a baseline of 16%. Children with at least one alcoholic parent were also much more likely to be sent west.

Table 2: Selection into Placing Out - NYJA

	Sent West									
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Age when admitted	0.0028***									0.0062***
	(0.0010)									(0.0014)
Female		0.041***								-0.0031
		(0.0076)								(0.0082)
Born in NYC			0.00075							0.0095
			(0.011)							(0.012)
Born abroad			-0.047***							-0.0092
			(0.012)							(0.013)
Can read & write				-0.012						-0.017*
				(0.0079)						(0.0086)
Can read, write, & calculate				0.013*						0.018**
				(0.0072)						(0.0079)
Catholic					-0.070***					-0.056***
					(0.0062)					(0.0067)
Jewish					-0.10***					-0.035***
					(0.0063)					(0.0072)
Admitted for crime or misconduct						-0.089***				-0.073***
						(0.0056)				(0.0066)
Admitted for economic opportunity						0.21***				0.17***
						(0.017)				(0.018)
Mother dead							0.15***			0.14***
							(0.0084)			(0.0088)
Father dead							0.042***			0.049***
							(0.0057)			(0.0067)
Orphan							0.13***			0.11***
							(0.020)			(0.024)
≥ 1 parent intemperate								0.14***		0.090***
								(0.0099)		(0.011)
Occupational income score									0.0013***	0.0011***
									(0.00030)	(0.00034)

Standard errors in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

The CAS and NYJA received bitter criticism from Catholic organizations for placing children from Catholic

backgrounds into Protestant households in the West, and it is likely that Jewish parents or organizations were also apprehensive of these organizations' Protestant ideals. It is thus not surprising to find that Catholic and Jewish children were significantly less likely to be sent West.[6] Children who were admitted for bad behavior were also significantly less likely to be included on an orphan train, relative to the baseline category of children admitted due to abandonment, destitution, or child abuse. Predictably, children who were turned over to the NYJA to gain economic opportunities were much more likely to be sent West.

While there is strong evidence that the NYJA staff selected which children would be sent West, there is little suggestion that the agents in charge of placing children made any effort to match children to specific locations according to their preferences or characteristics. Instead, the location where a child was sent seems to have been determined by coincidence. Most East Coast charitable organizations had western agents, who both checked in with previously placed children and scouted for promising rural towns into which future children could be placed. Once a group of towns was identified, and local reception committees put together, the East Coast headquarters of the organization would send those children had been released by their parents, or whose parents had not responded to inquiries for long enough that the child was considered abandoned. Thus a child's placement location seems to have been determined by the interaction of the western agent's independent scouting for host towns, and the progression of the child through the administration of the NYJA. The only evidence of selective placement in the literature are accounts that certain, presumably undesirable, children who were not selected for fostering at the first towns visited by the group of emigrants, and were thus place further down the line (Warren 2001).

We test the null hypothesis of random placement more rigorously by estimating a series of tests in which we regress the characteristics of the receiving county at the time the child arrives, y_{ct}, on the qualities of children sent West x_i, and a fixed effect for the year in which the child was admitted to the NYJA, γ_t. A typical specification might be the regression of the share of population of a county that is urban, on an indicator for whether the child is male or female.

Since we examine 12 qualities of children and 12 placement county-level characteristics, there is a strong risk of a type I error due to multiple hypothesis testing. We therefore adjust the p-values on the child-quality coefficient using the Romano and Wolf (2016) method to control the family-wise false discovery rate at 5%. Tables 3 and 4 reports the results of these regressions, with each cell containing the estimates from a single bivariate regression of the county characteristic in the column on the child characteristic in the row. Multiple-hypothesis adjusted p-values are reported in curly brackets.

In Table 3 we present correlations between children's qualities and county characteristics derived from

[6] A Catholic organization, the New York Foundling Hospital, also implemented a large scale placing out program (Creagh 2012). We are in the process of digitizing these records.

US Census data. We select mostly census variables that can be used to create a balanced county panel; the limiting factor is thus the availability of data from at the county level from the 1890 Census, where microdata was not preserved. Since household wealth is not measured in most years, we use 1870 or 1940 data on average household wealth and inequality for children placed in any year. Regardless of the outcome, the results are highly consistent: there is no evidence of any correlation between the observable characteristics of the child, and the location to which they were sent by the NYJA. Virtually all coefficients are extremely small in magnitude, and most p-values are so high they are rounded to 1.

Table 4 repeats this analysis, now using county characteristics drawn from sources other than the Census. Once again, we find no significant evidence of selection with respect to county characteristic. The closest to statistically significant results relate to the placement of Jewish and NYC-born children in counties with colleges, but even the economic magnitude of these results is small. We conclude that there is no evidence to suggest any selective matching between NYJA children and specific host communities.

Table 3: Orphan Train Destination and Child Characteristics: I

	(1) In school, ages 5-20, White	(2) Share urban	(3) Share farm households	(4) Share manufacturing workers	(5) Share born abroad	(6) Log household wealth, c. 1870	(7) Wealth Gini, c. 1870
Age when admitted	0.000	0.001	-0.001	0.000	0.001	0.007	-0.000
	(0.000)	(0.001)	(0.001)	(0.000)	(0.000)	(0.003)	(0.000)
	{1.000}	{1.000}	{1.000}	{1.000}	{0.723}	{0.723}	{1.000}
Female	0.001	0.010	-0.006	-0.000	-0.004	0.053	0.001
	(0.002)	(0.005)	(0.004)	(0.001)	(0.002)	(0.018)	(0.001)
	{1.000}	{0.941}	{1.000}	{1.000}	{0.980}	{0.257}	{0.990}
Can read & write	0.002	-0.001	0.001	-0.000	0.002	0.020	0.001
	(0.002)	(0.005)	(0.004)	(0.000)	(0.002)	(0.017)	(0.001)
	{1.000}	{1.000}	{1.000}	{1.000}	{1.000}	{1.000}	{1.000}
Mother dead	0.002	0.003	-0.002	0.001	0.000	-0.005	-0.000
	(0.002)	(0.004)	(0.003)	(0.000)	(0.002)	(0.015)	(0.001)
	{1.000}	{1.000}	{1.000}	{0.574}	{1.000}	{1.000}	{1.000}
Orphan	-0.001	0.006	-0.006	0.001	0.006	0.026	0.001
	(0.003)	(0.006)	(0.004)	(0.001)	(0.003)	(0.020)	(0.001)
	{1.000}	{1.000}	{1.000}	{0.851}	{0.782}	{1.000}	{1.000}
Catholic	-0.002	-0.010	0.005	-0.000	-0.004	-0.017	-0.001
	(0.002)	(0.006)	(0.004)	(0.001)	(0.003)	(0.021)	(0.001)
	{1.000}	{0.980}	{1.000}	{1.000}	{0.980}	{1.000}	{1.000}
Jewish	0.002	0.009	-0.002	0.003	0.008	0.022	0.001
	(0.004)	(0.011)	(0.008)	(0.001)	(0.005)	(0.037)	(0.001)
	{1.000}	{1.000}	{1.000}	{0.723}	{0.980}	{1.000}	{1.000}
Born in NYC	-0.003	-0.012	0.008	-0.001	-0.001	-0.038	-0.001
	(0.002)	(0.005)	(0.004)	(0.001)	(0.002)	(0.019)	(0.001)
	{1.000}	{0.832}	{0.812}	{0.545}	{1.000}	{0.901}	{0.950}
Born Abroad	0.002	0.012	-0.005	0.001	-0.001	0.034	0.002
	(0.003)	(0.006)	(0.004)	(0.001)	(0.003)	(0.021)	(0.001)
	{1.000}	{0.931}	{1.000}	{0.861}	{1.000}	{1.000}	{0.931}
≥ 1 parent intemperate	-0.002	-0.010	0.006	-0.001	0.000	-0.016	-0.000
	(0.002)	(0.005)	(0.004)	(0.001)	(0.002)	(0.017)	(0.001)
	{1.000}	{0.950}	{1.000}	{0.980}	{1.000}	{1.000}	{1.000}
Parent occupational score	0.000	-0.000	0.000	0.000	0.000	0.000	-0.000
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.001)	(0.000)
	{1.000}	{1.000}	{1.000}	{1.000}	{1.000}	{1.000}	{1.000}
Admitted for crime or misconduct	-0.001	-0.001	0.001	0.001	0.004	0.015	-0.000
	(0.002)	(0.004)	(0.003)	(0.000)	(0.002)	(0.016)	(0.001)
	{1.000}	{1.000}	{1.000}	{1.000}	{0.911}	{1.000}	{1.000}
Admitted for economic opportunity	0.005	0.004	-0.000	-0.001	0.002	-0.030	-0.001
	(0.003)	(0.006)	(0.004)	(0.001)	(0.003)	(0.021)	(0.001)
	{0.970}	{1.000}	{1.000}	{1.000}	{1.000}	{1.000}	{1.000}

All models include NYJA admission year fixed effects. Heteroskedasticity robust standard errors in parentheses. Multiple hypothesis adjusted p-values in curly brackets.

Table 4: Orphan Train Destination and Child Characteristics: II

	(1) Years of frontier exposure	(2) Total patents registered	(3) Share catholic	(4) Number of colleges	(5) Railroad distance to NYC
Age when admitted	0.000	10.861	0.002	0.000	-61.609
	(0.000)	(8.034)	(0.001)	(0.005)	(228.630)
	{1.000}	{1.000}	{0.750}	{1.000}	{1.000}
Female	-0.000	139.818	-0.004	0.077	3290.534
	(0.001)	(62.074)	(0.005)	(0.034)	(1113.099)
	{1.000}	{0.750}	{1.000}	{0.750}	{0.417}
Can read & write	-0.000	63.006	0.000	0.024	-1154.688
	(0.001)	(55.493)	(0.005)	(0.031)	(1408.146)
	{1.000}	{1.000}	{1.000}	{1.000}	{1.000}
Mother dead	-0.001	-35.260	-0.000	-0.008	688.054
	(0.001)	(48.334)	(0.004)	(0.027)	(1234.720)
	{1.000}	{1.000}	{1.000}	{1.000}	{1.000}
Orphan	-0.001	99.909	0.010	0.030	427.566
	(0.001)	(65.853)	(0.006)	(0.037)	(1631.056)
	{1.000}	{1.000}	{0.940}	{1.000}	{1.000}
Catholic	0.000	-25.650	-0.015	-0.038	-958.690
	(0.001)	(69.332)	(0.006)	(0.038)	(1107.600)
	{1.000}	{1.000}	{0.512}	{1.000}	{1.000}
Jewish	0.004	223.321	0.013	0.288	1241.471
	(0.002)	(125.892)	(0.010)	(0.070)	(3602.443)
	{0.750}	{0.940}	{1.000}	{0.119}	{1.000}
Born in NYC	-0.000	-181.666	0.000	-0.148	276.138
	(0.001)	(61.873)	(0.005)	(0.033)	(1079.128)
	{1.000}	{0.417}	{1.000}	{0.060}	{1.000}
Born abroad	0.000	86.957	-0.002	0.118	-1099.793
	(0.001)	(70.765)	(0.006)	(0.038)	(1269.145)
	{1.000}	{1.000}	{1.000}	{0.393}	{1.000}
≥ 1 parent intemperate	-0.000	-70.433	0.005	-0.017	-34.661
	(0.001)	(56.877)	(0.005)	(0.032)	(1391.648)
	{1.000}	{1.000}	{1.000}	{1.000}	{1.000}
Parent occupational score	0.000	0.442	-0.000	0.001	12.618
	(0.000)	(2.583)	(0.000)	(0.001)	(52.798)
	{0.762}	{1.000}	{1.000}	{1.000}	{1.000}
Admitted for crime or misconduct	0.000	-51.986	0.002	-0.070	-999.646
	(0.001)	(48.493)	(0.004)	(0.027)	(1397.825)
	{1.000}	{1.000}	{1.000}	{0.607}	{1.000}
Admitted for economic opportunity	0.001	-95.494	0.008	-0.075	-738.226
	(0.001)	(64.011)	(0.006)	(0.036)	(1835.730)
	{1.000}	{1.000}	{1.000}	{0.762}	{1.000}

All models include NYJA admission year fixed effects. Heteroskedasticity robust standard errors in parentheses. Multiple hypothesis adjusted p-values in curly brackets.

5 Census Matching

In order to obtain outcome measures for the children in our sample, we link them to their records in the historical U.S. censuses from 1860 to 1940 (Ruggles et al. 2024a,b). We attempt to link orphan train rider with Census observations based upon a range of characteristics, including first name, middle initial, last name, destination, last name of the foster family, relationship to the head of household, birth year, birth place, and gender. Potential matches are scored based on the degree of similarity between records, and we

keep the highest scoring match for each orphan train rider in each census year conditional on it being above a threshold determined from an examination of a sample of candidate matches.

We first impute gender for the NYJA and CAS children when it is not reported using a prediction algorithm based on historical records of name popularity by birth year (Blevins and Mullen 2015). We then clean the names in the NYJA and CAS data for uniformity, using a modified version of the process developed by (Abramitzky et al. 2019), adding multiple versions based on the U.S. Census Soundex methodology for fuzzy name matching. We repeat this process for each individual recorded in the complete count U.S. decennial censuses.

To match records between our source data and the censuses, we narrow the search to individuals with the same first and last initial born within age blocks of plus or minus five years. Using the "dtalink" methodology for probabilistic record linking from Kranker (2018), we weight potential matches based on similarity across a range of potential common covariates. Individuals sharing the same gender, middle initial, exact first name, exact last name, birth year, and birthplace receive higher points. We secondarily increase the score of a potential match if the Soundex versions of the names match, the location matches the reported destination state or county, or the head of household's surname matches the reported foster family. Finally, potential matches receive additional points if the head of household's surname differences from the candidate's, if the candidate's relationship to the head of household is listed as a boarder or nonrelative, and if the candidate's birthplace is listed as New York or overseas.

For each orphan train rider, we make a direct search from their name to every census between 1860 and 1940, beginning with the first census after the child was sent West, keeping the highest scoring match across all censuses unless no match exceeds a minimum threshold.[7] We repeat this process for the NYJA children sent West, the NYJA children not sent, the CAS children sent West, and all reported foster parents with whom children were first placed. For our candidate matches, we then conduct a secondary search from that census person across all other censuses using the links suggested by the Census Tree project (Buckles et al. 2023; Price et al. 2021). In cases where the Census Tree project does not suggest a linked person for our match but a Census observation appears to be a good match for that orphan train rider, we use our match.

Although we search for all orphan train riders, we limit our sample and analysis only to males due to difficulties in matching females across Census years [8]. Using this restriction and the matching methodology described above, we obtain plausible candidate matches for 6,540 unique individuals, including 1,334 orphan train riders from the NYJA, 2,003 orphan train riders from CAS, and 3,203 NYJA children who were not sent West. Across all individuals, this yields a candidate match rate of 26.1%, and after applying our census linking

[7] If the two highest score match candidates are tied, we select the earlier Census year. If matches are tied within Census year, we keep both.

[8] Expansion of the Census Tree project will likely allow a future analysis of economic mobility for females.

approach we have an average of between 4 and 5 decennial census observations per matched individual. Table 5 summarizes the match counts for males from the NYJA and CAS overall and by Census year. Additional details on the matching criteria and scoring method are described in the Appendix.

Table 5: Census Matching Results for Males from the NYJA and CAS

		Matches by Census Year										
Group	Males in Sample	1860	1870	1880	1900	1910	1920	1930	1940	Total Matches	Unique Matched	Match Rate
NYJA in NY	5,632	1,215	1,617	2,623	2,769	2,838	2,721	2,456	2,000	16,239	3,203	0.569
NYJA sent West	4,230	236	736	902	1,046	949	851	689	478	5,887	1,334	0.315
CAS sent West	15,201	897	1,807	1,631	1,370	1,237	1,039	727	354	9,062	2,003	0.132
Total	25,063	2,348	4,160	5,156	5,185	5,024	4,611	3,872	2,832	31,188	6,540	0.261

6 Results

We analyze the impact of the orphan train program on economic outcomes on two dimensions. First, we use an instrumental variables strategy to address the endogeneity in the choice to send a child, estimating among the NYJA sample the effect of being sent West versus remaining in New York. Second, we leverage cross-county variation across the destination counties to estimate, conditional on being sent West, the causal effect of local proxies for opportunity on later adult outcomes.

I: The Causal Effect of Going West

To estimate the causal effect of begin sent West on the extensive margin, we first present ordinary least squares regressions for the effect of being sent West relative to remaining in New York. We next account for endogeneity in the selection process for being sent West by using an instrumental variable strategy with the annual sending rates by year of admission to the NYJA as an instrument for being sent West.

Table 6 presents the results from a series of ordinary least squares regressions for the following estimation equation:

$$Y_i = \beta_0 + \beta_1 SentWest_i + X_i\beta + \gamma_t + \gamma_d + \epsilon_{it} \tag{1}$$

which represents the effect of being sent West on one of three socio-economic outcomes Y in adulthood: an indicator for living on a farm (Columns 1 and 4), occupational earnings score—the median reported earned income for Census respondents with that occupation in 1950, with a range of 0 to 80 (Columns 2 and 5)—and an indicator for being married (Columns 3 and 6).[9] *SentWest* is an indicator variable equal to one if a child was sent West. The covariate vector X in the first three models includes controls for a quadratic in age in the

[9]Sample sizes are reduced overall because we cannot calculate the probability of being sent West for most children admitted before 1860, and in Columns 4 to 6 due to missing covariates.

Census year, the γ_t are Census year fixed effects, and the γ_d are decade of admission fixed effects. The second three models add a suite of covariates for a child's characteristics upon admission to the NYJA, including age, whether they were an orphan, whether they were admitted for misconduct or having committed a crime, whether they had any education at the time, whether they were Jewish or Catholic, whether they were board abroad, and the occupational earnings score of a parent (usually the father).

Being sent West is a strong predictor of whether an individual is found living on a farm in adulthood (Columns 1 and 4). This is consistent with the structure of the placing out program, which predominantly fostered children in farming communities. As the occupational earnings scores for farmers are generally lower than for non-farmers, it is not surprising that being sent West is also predictive of having a lower occupational earnings score in adulthood (Columns 2 and 5). There is a negative relationship between being sent West and getting married (Columns 3 and 6), a reduction of about 4.5 percentage points. Among the controls, Jewish and Catholic children are less likely to end up on a farm, as are children who are born abroad.

Table 6: Effect of Going West on Outcomes in Adulthood (OLS)

	(1) On Farm	(2) Occ Score	(3) Married	(4) On Farm	(5) Occ Score	(6) Married
Sent West	0.118***	-2.708***	-0.0419***	0.116***	-2.550***	-0.0424***
	(0.00976)	(0.282)	(0.00814)	(0.0103)	(0.296)	(0.00878)
Age	-0.00224	0.373***	0.0552***	-0.000968	0.335***	0.0546***
	(0.00138)	(0.0498)	(0.00147)	(0.00147)	(0.0564)	(0.00163)
Age Squared	0.0000197**	-0.00419***	-0.000458***	0.0000145	-0.00410***	-0.000458***
	(0.00000926)	(0.000399)	(0.00000976)	(0.00000933)	(0.000403)	(0.00000990)
Admit Under Age 10				0.00603	-0.203	-0.00669
				(0.0103)	(0.385)	(0.0112)
Dead Both Parents				0.00267	-0.0342	-0.0112
				(0.0190)	(0.499)	(0.0174)
Crime/ Misconduct Commitment				0.00625	0.107	0.00611
				(0.00833)	(0.269)	(0.00871)
Any Education				0.00183	0.277	0.00741
				(0.0104)	(0.350)	(0.0109)
Jewish				-0.0708***	2.435***	-0.0270**
				(0.00838)	(0.396)	(0.0132)
Catholic				-0.0468***	0.756**	-0.0315***
				(0.00970)	(0.328)	(0.00963)
Born Abroad				-0.0192**	-0.139	0.0472***
				(0.00868)	(0.329)	(0.00969)
Constant	0.254***	15.89***	-0.779***	0.247***	16.17***	-0.768***
	(0.0291)	(0.991)	(0.0297)	(0.0330)	(1.160)	(0.0347)
N	18,351	15,507	18,351	17,902	15,134	17,902

Standard errors clustered by child in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. All models include fixed effects for Census year and decade of admission to the NYJA. Samples are restricted to observations at age 18 or older.

Because we have established in Section 6 that there was potential selection into which children were sent West, we next address this endogeneity by re-estimating Equation 1 with an instrumental variables strategy. Historical sources suggest that the NYJA's decisions to send children West was highly influenced by idiosyncratic administrative factors, such as the work of their western agents identifying and preparing communities, and the availability of space in the New York facility (Kidder 2021). For example, in 1908 the NYJA moved to a smaller, more rural establishment, requiring them to reduce their resident population by sending many children to Western homes. We take advantage of this exogenous variation to use the annual intensity of sending—the share of children from a given admission year who the NYJA ultimately sent West–as an instrument for whether an individual child was sent West. The overall variation in the instrument is shown in Figure 5. It is below five percent in some years (e.g., 1903) and above 25 percent in others (1880).

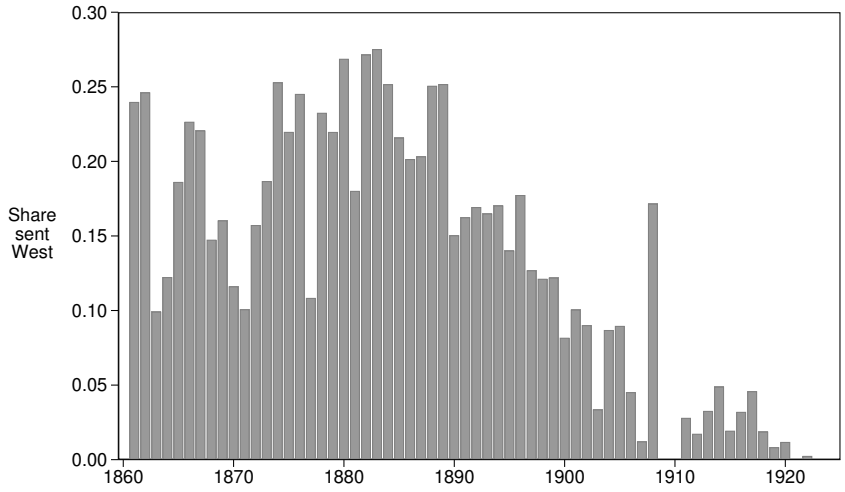


Figure 5: Histogram of the share of children sent West from the NYJA by year of admission. The high year-to-year exogenous variation in the probability of assignment to treatment is a visual representation of the strength of the first stage of our instrumental variables estimation strategy.

Table 7 reports two-stage least squares results for the same set of models as in Table 6, instrumenting for being sent West with the annual share of NYJA children sent. Compare the repeated OLS results in Column 1 with the same two IV specifications as in Equation 1 (Columns 2 and 3) along with an additional suite of outcomes including urban residence, home ownership, school attendance, and number of children, we see that the effect of going West becomes statistically insignificant for farming, occupation score, and marital status. The only consistent effect is on the probability of being in school, which is estimated as 0.34 points—much higher than the OLS estimate of 0.019.

For robustness, also report the results from estimating the same model as in Column 3 (IV with controls) on several alternative samples, including one only keeping matches in the top 50% of scores[10] (Column 4), only examining for each child the first census after they were admitted to the NYJA (Column 5), only examining census 20-40 years after admission to the NYJA (Column 6), and only looking at children who were age 10 or younger when admitted to the NYJA (Column 7). No clear systematic relationship is apparent between whether a child was sent West and their later socio-economic outcomes, and even the estimates for school attendance become statistically weak under the alternative specifications.

[10] 39 for children sent West and 42 for children not sent West.

Table 7: IV Estimates with Alternative Specifications

	OLS	IV					
Outcome:	(1)	(2)	With controls (3)	Best 50% matches (4)	1st census post-NYJA (5)	20-40 years post-NYJA (6)	Admitted at age≤10 (7)
Occupational education score	-1.24***	3.37	15.0	18.6	-14.8	-9.34	45.1
	(0.43)	(12.9)	(14.6)	(17.2)	(9.83)	(18.0)	(29.8)
Occupational income score	-2.70***	2.44	9.96	10.3	-0.36	-17.5	15.6
	(0.28)	(8.46)	(9.69)	(10.7)	(7.58)	(12.1)	(18.6)
Log 1940 income	-0.14	-3.67*	-4.71*	-6.55**	-6.55**	-1.55	-5.31
	(0.087)	(2.06)	(2.76)	(3.30)	(3.30)	(4.69)	(3.29)
Works in agriculture	0.14***	0.14	0.022	-0.53	-0.11	0.34	0.12
	(0.012)	(0.37)	(0.41)	(0.41)	(0.39)	(0.48)	(0.81)
Urban resident	-0.18***	-0.42	-0.33	0.097	0.037	0.15	-1.22
	(0.012)	(0.39)	(0.43)	(0.45)	(0.39)	(0.50)	(0.93)
In school	0.014***	-0.025	-0.015	-0.0071	-0.26	-0.099	-0.29
	(0.0028)	(0.076)	(0.084)	(0.10)	(0.23)	(0.12)	(0.22)
Homeowner	0.037***	-0.24	-0.72	-0.21	0.64	-0.049	-0.32
	(0.012)	(0.44)	(0.52)	(0.48)	(0.72)	(0.56)	(1.04)
Married	-0.042***	-0.14	-0.14	-0.45	0.13	-0.42	-0.29
	(0.0081)	(0.26)	(0.30)	(0.34)	(0.29)	(0.39)	(0.55)
Number own children in HH	-0.092**	0.80	0.58	0.90	0.73	1.27	-1.93
	(0.038)	(1.26)	(1.43)	(1.50)	(0.48)	(2.40)	(2.89)
N (max)	18,344	18,344	17,895	9,754	2,326	6,766	6,983

To examine the potential impact of our match quality threshold cutoff more systematically, we re-estimate the IV model several times, each time restricting the sample to matches with a score in the decile reported on the x-axes in Figure 6 or higher. Estimated coefficients and 90% confidence intervals are shown on the y-axes. The estimates are stable in sign and magnitude even when limited to only the highest confidence matches.

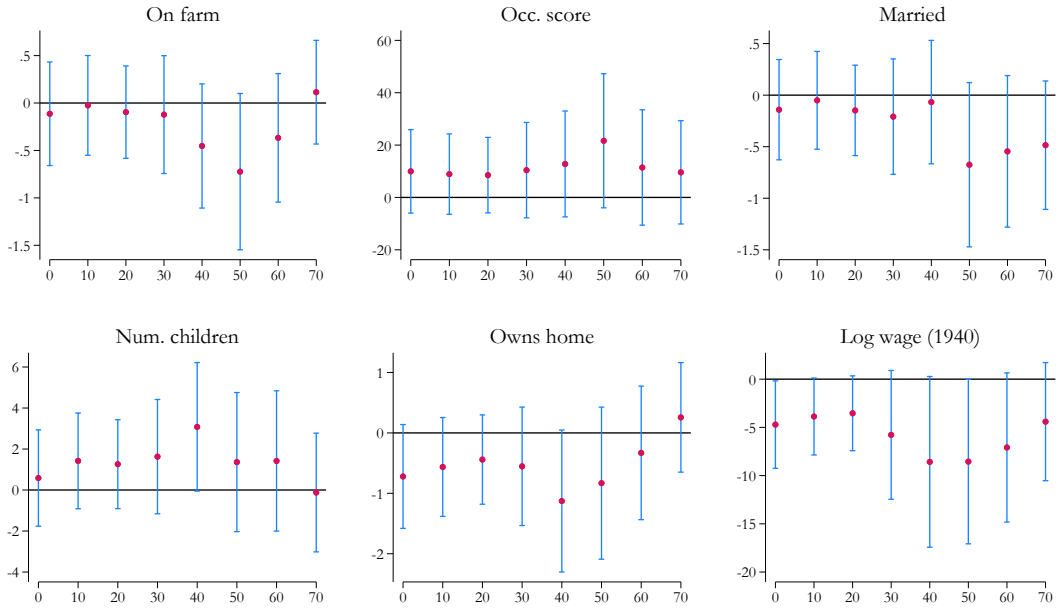


Figure 6: Coefficient plots for IV regressions limiting the sample to only matches with a score in the top d decile as reported along the x-axes. Estimated coefficients and 90% confidence intervals shown on the y-axes. Estimates are stable in sign and magnitude even when limited to only the highest confidence matches.

II: The Causal Effect of Destination County Characteristics

We turn now to the question of how geographic variation in the characteristics of the counties to which the children were sent determined their economic opportunity conditional on being sent West. We focus on a set of 6 socio-economic outcomes, including income (occupational income score), school enrollment, living on a farm, ever marrying, and number of children, with the estimates presented in Table 8. The basic estimating equation is

$$y_{it} = \beta_0 + \beta_1 CountyCharacteristic_c + X_{it}\beta + \gamma_t + \epsilon_{it} \tag{2}$$

In each model, we regress an individual's socio-economic when observed at age 18 or later on a set of 11 distinct county characteristics that are potentially correlated with the degree of local economic opportunity.[11] We first examine each factor individually (Table 8), then regress them jointly in multivariate regressions including all county characteristics together (Table 9). In each case, we control for a quadratic in age and fixed effects γ_t for the Census year in which we observe the adult child. County characteristics are taken for the county where the sending agency records report that the child was placed, in the first census year

[11] For the outcome of enrollment in school (Column 2), we instead limit the sample to the first census after the child went West.

after the child was sent. This approach is designed to capture the trajectory of the local opportunities that would have been available in the subsequent years. We examine separately, and then jointly, the relationship between each socio-economic and the degree of urbanization, the share of employment in manufacturing, the share in agriculture, the share of children ages 5-20 enrolled in school, the log (plus one) of the number of patents issued to that county over the preceding 10 years, log (plus one) population of the largest city in the county, the number of colleges in the area, and the average age, occupational income score, family size, and married rate of heads of households in each destination county in the first census after each child was sent West.

In the regressions including country characteristics one at a time, with coefficients collected in Table 8, there are few strong relationships with orphan train rider outcomes, with several exceptions. Average income (occupation score) in the destination county and patent intensity are positively associated with the child's later adult score, with coefficients of 0.245 and 0.129, respectively, shown in Column 1 (compared to a mean of about 21.7). Several measures significantly predict whether the child is found to be enrolled in school, including the urban share and manufacturing industry intensity (negative) and the agricultural share and prevailing school enrollment rate (positive), seen in Column 2.

When all county characteristics are combined in multivariate regressions, the results of which are presented in Table 9, most statistical relationships are not significantly different from zero, except for the positive relationship between general school enrollment rates and patent intensity in predicting whether an orphan train rider is found to be enrolled in school in the first census after they go West (Column 2). Children placed in manufacturing intensive regions have fewer children on average (Column 6), which could operate through the channel of lower rates of marriage (Column 5).

In general, however, the individual and joint results suggest little systematic relationship between county characteristics where the children were sent and the children's later adult socio-economic outcomes. This pattern tentatively contradicts several previous findings, based on more endogenously chosen locations, that location effects play an important role in shaping economic opportunity.

As previous literature has shown that some neighborhood effects are stronger when exposure occurs at a young age, we repeat the analysis limiting the sample to children placed out at age 10 or younger. The results, reported in Table 10 again generally show minimal relationship between county characteristics and adult outcomes. Younger children placed in areas with higher educational enrollment are even more likely to be enrolled in school themselves (Column 2), though this could be attributable in part to the fact that they are also younger at the time of the first census after they are sent West. Children placed in manufacturing intensive regions at young ages still have fewer children (Column 6) and lower rates of marriage (Column 5). Interestingly, the number of nearby colleges does become a significant predict of later adult occupation scores,

with an estimate of 0.307 points on the index per college, where the mean observed score in the sample is about 21.7.

Table 8: Adult Outcomes as a Function of Individual County Characteristics

	(1) Occ score	(2) In school	(3) On farm	(4) Log income	(5) Ever married	(6) Number children	(7) In original destination
Urban population share	0.879	-0.121***	-0.047**	0.500*	0.003	-0.007	0.026
	(0.636)	(0.038)	(0.023)	(0.286)	(0.019)	(0.100)	(0.022)
Manufacturing industry share	4.078	-1.452**	-0.241*	2.723*	-0.121	-0.741	0.106
	(4.393)	(0.564)	(0.130)	(1.570)	(0.123)	(0.608)	(0.114)
Agricultural industry share	-3.938	0.373**	0.252**	-2.077	-0.086	-0.125	-0.127
	(2.693)	(0.160)	(0.102)	(1.280)	(0.086)	(0.430)	(0.096)
Share age 5–20 in school	-0.238	0.379***	0.001	3.450*	-0.110	-0.029	-0.095
	(1.907)	(0.069)	(0.088)	(2.047)	(0.069)	(0.265)	(0.069)
Log patents per capita	0.245**	0.011*	-0.008**	0.070*	0.003	0.005	0.009**
	(0.104)	(0.006)	(0.004)	(0.041)	(0.003)	(0.016)	(0.004)
Log population	0.076	-0.008**	-0.003	0.050	0.001	-0.003	0.002
	(0.062)	(0.003)	(0.003)	(0.032)	(0.002)	(0.010)	(0.002)
Number of colleges	0.105*	-0.012*	-0.002	0.106	0.003***	0.011	0.006**
	(0.060)	(0.006)	(0.002)	(0.094)	(0.001)	(0.010)	(0.003)
County head age	0.047	0.002	-0.005*	-0.004	-0.000	0.019	-0.001
	(0.073)	(0.004)	(0.003)	(0.049)	(0.002)	(0.012)	(0.002)
County head occupation score	0.129*	-0.000	-0.001	0.172***	-0.004*	0.005	0.002
	(0.072)	(0.004)	(0.003)	(0.053)	(0.002)	(0.012)	(0.003)
County family size	-0.703	0.040	0.046***	-0.374	-0.019	-0.166**	-0.009
	(0.439)	(0.025)	(0.016)	(0.238)	(0.014)	(0.071)	(0.014)
County head married	-6.447*	0.751***	0.453***	0.816	-0.254**	-1.114**	0.123
	(3.359)	(0.181)	(0.132)	(2.313)	(0.116)	(0.548)	(0.101)

Each cell reports the coefficient on the county characteristic from a regression of the outcome listed in the column header on that characteristic plus controls for age, age-squared, and census year fixed effects. Samples for Columns 1 and 3-7 are restricted to age 18 or older. Log income is only observed in the 1940 census. The sample for Column 2 is limited to observations in the first census after a child was sent West. Standard errors are clustered by individual and reported in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table 9: Adult Outcomes as a Function of County Characteristics Jointly

	(1) Occ score	(2) In school	(3) On farm	(4) Log income	(5) Ever married	(6) Number children	(7) In original destination
Urban population share	-0.293	-0.165	-0.0394	-0.329	-0.0431	0.199	0.0237
	(2.040)	(0.128)	(0.0660)	(0.852)	(0.0571)	(0.279)	(0.0496)
Manufacturing industry share	-2.052	-0.454	0.174	3.482	-0.473**	-2.539***	-0.146
	(6.422)	(0.959)	(0.193)	(2.569)	(0.199)	(0.981)	(0.152)
Agricultural industry share	2.485	-0.0292	0.222	1.876	-0.313*	-0.643	-0.0270
	(5.238)	(0.272)	(0.204)	(3.303)	(0.171)	(0.778)	(0.150)
Share age 5–20 in school	0.933	0.304***	-0.0919	2.498	-0.103	-0.0513	-0.130*
	(2.068)	(0.0788)	(0.0931)	(2.386)	(0.0761)	(0.276)	(0.0713)
Log patents per capita	0.332*	0.0395***	-0.00484	-0.0756	0.00738	-0.000194	0.00823
	(0.177)	(0.00830)	(0.00724)	(0.110)	(0.00612)	(0.0264)	(0.00565)
Log population	-0.0483	-0.00452	0.00516	0.0362	0.00155	-0.0255	-0.00466
	(0.137)	(0.00767)	(0.00510)	(0.0818)	(0.00429)	(0.0197)	(0.00388)
Number of colleges	0.0255	-0.00957	0.000646	0.0323	0.00220	0.0133	0.00523*
	(0.0722)	(0.00786)	(0.00211)	(0.117)	(0.00144)	(0.0107)	(0.00283)
County head age	0.0411	-0.00898**	-0.00429	-0.0207	0.000789	0.0258*	-0.000279
	(0.0822)	(0.00421)	(0.00289)	(0.0579)	(0.00285)	(0.0136)	(0.00226)
County head occupation score	0.108	0.00385	0.00184	0.207**	-0.00753***	0.00519	-0.00208
	(0.0843)	(0.00481)	(0.00295)	(0.0821)	(0.00274)	(0.0143)	(0.00381)
County family size	0.0238	0.0295	0.0102	-0.541*	-0.00419	-0.127	-0.00946
	(0.557)	(0.0328)	(0.0214)	(0.311)	(0.0185)	(0.0861)	(0.0175)
County head married	-8.048*	0.0672	0.399**	-0.0755	-0.163	-0.630	0.278**
	(4.248)	(0.240)	(0.169)	(3.647)	(0.139)	(0.654)	(0.128)
F-stat	1.242	6.462	1.982	1.665	2.810	1.368	1.441
p-value	0.253	0.000	0.026	0.082	0.001	0.181	0.147
N	10408	4235	12702	252	12702	12701	12701
R^2	0.063	0.172	0.044	0.084	0.338	0.127	0.051

NOTE: Each column reports a separate regression with the dependent variable listed in the header. All models include a constant, a quadratic in age, and census year fixed effects. "County" variables are local averages in the first census year after a child was sent West. Standard errors are clustered by child and reported in parentheses. Samples for Columns 1 and 3-7 are restricted to age 18 or older. Log income is only observed in the 1940 census. The sample for Column 2 is limited to observations in the first census after a child was sent West. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table 10: Adult Outcomes as a Function of County Characteristics Jointly for Children Sent West at Age 10 or Younger

	(1) Occ score	(2) In school	(3) On farm	(4) Log income	(5) Ever married	(6) Number children	(7) In original destination
Urban population share	1.527	-0.120	0.0242	-1.198	-0.0266	0.389	0.0373
	(3.636)	(0.276)	(0.129)	(1.260)	(0.107)	(0.481)	(0.103)
Manufacturing industry share	-3.940	-3.553*	-0.273	-4.196	-0.613*	-4.257***	-0.391
	(11.64)	(2.030)	(0.366)	(5.939)	(0.333)	(1.405)	(0.299)
Agricultural industry share	-0.731	0.133	0.0362	-2.009	0.166	1.777	-0.358
	(11.41)	(0.571)	(0.428)	(5.583)	(0.368)	(1.560)	(0.365)
Share age 5–20 in school	0.125	0.690***	-0.0506	3.414	-0.379**	0.189	-0.0836
	(3.760)	(0.148)	(0.170)	(3.404)	(0.159)	(0.460)	(0.181)
Log patents per capita	0.0928	0.0260	-0.0211	0.168	0.00618	0.0127	0.0113
	(0.371)	(0.0194)	(0.0148)	(0.172)	(0.0129)	(0.0525)	(0.0125)
Log population	-0.0903	-0.00692	0.00439	-0.0849	0.00982	-0.00947	-0.00773
	(0.252)	(0.0162)	(0.0102)	(0.113)	(0.00839)	(0.0358)	(0.00811)
Number of colleges	0.307**	0.0258	0.00189	0.0785	0.00239	-0.0235	-0.00502
	(0.146)	(0.0219)	(0.00597)	(0.180)	(0.00501)	(0.0148)	(0.00332)
County head age	0.0730	0.000816	-0.00496	-0.0645	0.0119**	0.0317	-0.00330
	(0.167)	(0.00981)	(0.00623)	(0.0915)	(0.00544)	(0.0214)	(0.00500)
County head occupation score	-0.0124	0.0268***	0.00181	0.108	-0.00904**	-0.00105	-0.0133
	(0.161)	(0.00962)	(0.00573)	(0.138)	(0.00451)	(0.0270)	(0.0100)
County family size	-1.223	0.0763	0.0239	-0.0284	0.0468	-0.146	-0.0214
	(1.133)	(0.0729)	(0.0412)	(0.596)	(0.0364)	(0.153)	(0.0343)
County head married	8.716	-1.196**	-0.235	-2.411	0.000797	-1.749	0.0370
	(9.291)	(0.575)	(0.385)	(4.600)	(0.313)	(1.317)	(0.344)
F-stat	1.016	3.466	0.964	0.580	1.912	3.433	0.865
p-value	0.429	0.000	0.477	0.841	0.034	0.000	0.574
N	2925	1298	3505	100	3505	3504	3504
R^2	0.055	0.134	0.044	0.087	0.335	0.118	0.049

NOTE: Each column reports a separate regression with the dependent variable listed in the header. All models include a constant, a quadratic in age, and census year fixed effects. The sample is restricted to children who were sent West at age 10 or younger. "County" variables are local averages in the first census year after a child was sent West. Standard errors are clustered by child and reported in parentheses. Samples for Columns 1 and 3-7 are restricted to age 18 or older. Log income is only observed in the 1940 census. The sample for Column 2 is limited to observations in the first census after a child was sent West. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

To investigate whether noise in the matching process could partly explain any of these results, we repeat the analysis after restricting the sample to matches in the top d% of initial linking scores, with d ranging from 100 to 30. Figure 7 displays the results, where (100 - d) is reported on the x-axis and the point estimate and 90% confidence interval for each of six main variables reflecting local economic opportunity from the regression specification with occupational income score as the dependent variable (as in Table 9 Column 1) shown on the y-axis. Overall, the patterns in the estimates are consistent with the full sample results, and the coefficients remain stable in sign and magnitude as confidence in match accuracy increases (and, correspondingly, sample

size decreases). One possible exception is the coefficient on the average local occupational income score among heads of household, where the coefficients increase and the 90% confidence interval nearly excludes 0 once the 40-50% lowest confidence matches are removed.

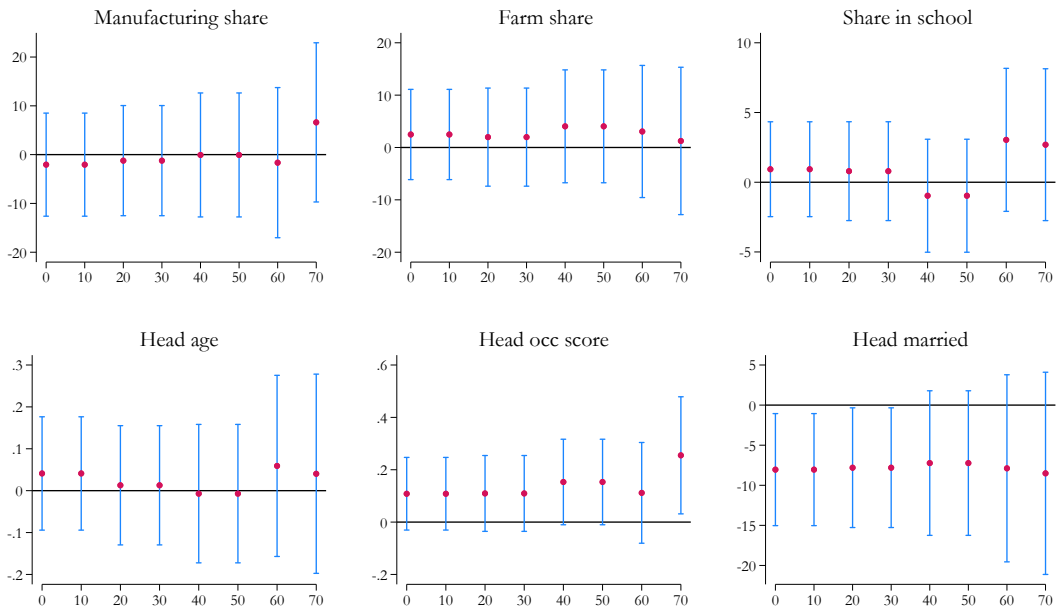


Figure 7: Coefficient plots for OLS regressions as in Table 9 Column 1 limiting the sample to only matches with a score in the top 100-d percentile as reported along the x-axes. Estimated coefficients and 90% confidence intervals shown on the y-axes. Estimates are stable in sign and magnitude as match confidence increases (and sample size decrease), with a possible slight increase in the coefficient on local income (occ score) that we investigate further in Table 11.

We expand on the evidence shown in Figure 7 by re-estimating the regression specification with orphan train rider adult occupational income score as the dependent variable, showing the estimates for the full set of of county characteristics across four models where the sample is restricted to the top 50% highest confidence matches, with the results presented in Table 11. The model in Column 1 is the main specification, as in Table 9 Column 1). Column 2 further limits the sample to children sent West at age 10 or younger, as in Table 10 Column 1. Columns 3 and 4 follow the same specifications except we add controls for a set of child covariates upon admission to the NYJA, including age sent West, ability to read/write, ability to read/write/calculate, orphan, Catholic, Jewish, born in NY, born abroad, intemperate parent, parent occupational score, admitted for misconduct, and admitted for economic opportunity. Sample size decreases because these controls are only observed for the subset of children sent West by the NYJA. The two coefficients with positive predictive value across several of the four specifications are the number of nearby colleges and the average occupational income score among local heads of household. The estimated effect of the number of colleges is higher for children

sent West at younger ages, by about 0.2 points on the index per college. The effect of local occupational income scores is higher for the NYJA subsample where we control for initial child characteristics (1.142 vs. 0.616 points), though there is no statistical relationship for children sent West by age 10 for the combined CAS and NYJA samples (Column 2).

Table 11: Adult Occupational Income Scores as a Function of County Characteristics, Top 50% of Matches

	Occupational Income Score			
	(1)	(2)	(3)	(4)
Urban	-0.972	0.0870	-3.981	-2.885
population share	(2.553)	(4.243)	(4.690)	(6.175)
Manufacturing	-0.0673	-9.347	3.535	3.436
industry share	(7.723)	(13.37)	(14.75)	(25.34)
Agricultural	4.043	-13.50	18.55	24.33
industry share	(6.553)	(13.47)	(14.40)	(28.04)
Share age 5–20	-0.968	-1.688	-7.544	-19.59*
in school	(2.462)	(4.136)	(7.533)	(10.46)
Log patents	0.539**	0.284	0.498	0.254
per capita	(0.219)	(0.441)	(0.514)	(0.949)
Log	-0.129	-0.266	-0.0117	0.0722
population	(0.172)	(0.303)	(0.308)	(0.500)
Number of	0.0217	0.191	0.248	0.432**
colleges	(0.0867)	(0.142)	(0.158)	(0.192)
County head	-0.00706	-0.119	-0.211	-0.462
age	(0.100)	(0.194)	(0.230)	(0.391)
County head	0.153	-0.0889	0.506	0.849
occupation score	(0.0993)	(0.164)	(0.329)	(0.548)
County family	0.191	-1.800	1.518	0.913
size	(0.691)	(1.336)	(1.681)	(2.655)
County head	-7.231	13.06	-29.64*	-12.00
married	(5.481)	(10.50)	(16.16)	(27.71)
West by Age 10		X		X
Child Controls			X	X
F-stat	1.140	1.097	1.536	3.789
p-value	0.325	0.361	0.113	0.000
N	7071	2039	2390	674
R^2	0.069	0.068	0.138	0.168

NOTE: Each column reports results from regressions with orphan train rider adult occupational income score as the dependent variable. All models include a constant, a quadratic in age, and census year fixed effects. "County" variables are local averages in the first census year after a child was sent West. Child controls are age sent West, ability to read/write, ability to read/write/calculate, orphan, Catholic, Jewish, born in NY, born abroad, intemperate parent, parent occupational score, admitted for misconduct, and admitted for economic opportunity. Standard errors are clustered by child and reported in parentheses. Samples are restricted to observations at age 18 or older among matches in the top 50% of confidence scores, with Columns 2 and 4 further restricted to children sent West by age 10. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

7 Foster Placement Effects

We next consider the effect of the specific households where orphan train riders were initially placed. In much of the previous literature, children who change communities move with their families (see, for example, Chetty et al. (2016) and Kawano et al. (2024) on military families). With the orphan train movement, children were assigned a new location and a new household, allowing us to analyze county and household assignment separately. Oreopoulos (2003) hypothesizes that neighborhood quality matters less than family situation for explaining economic outcomes in adulthood, and this becomes testable in a clean manner when both are reassigned simultaneously.

We first describe how we identify assigned foster households in the decennial censuses. Both the NYJA and the CAS paper records list the person with whom each child was placed. After digitizing and cleaning the records, we perform census matching for each foster parent, searching for them in the first census after the child was sent West. Matches receive higher scores for exact accordance on first name, last name, and last initial, with additional weight given to rare surnames and individuals living in the county where child was sent. Additional details of the matching process are given in the Appendix, and the number of matches by sending agency and census year is listed in Table 12. Overall, we obtain a candidate foster household match for 40% of the male children for whom we also found a matching census record. Match counts are higher in the earlier decades, particularly for the CAS, whose sending records are only available into the early 20th century. The higher foster match rate for the NYJA is likely due in part to those records being slightly more consistent with listing the full names of assigned fosters and also in part to the narrower range of geographic destinations.

Table 12: Census Matching Results for Foster Households

		Foster Matches by Census after West							
Group	Males in Sample	1860	1870	1880	1900	1910	1920	Unique Matched	Match Rate
NYJA sent West	1,334	18	448	309	223	23	3	1,024	0.768
CAS sent West	2,003	143	699	171	46	0	0	1,059	0.528
Total	5,203	161	1,147	480	269	23	3	2,083	0.400

In Section 4 we showed that destination county characteristics were unrelated to the characteristics of the orphan train riders. Now, for the NYJA subsample for which we have detailed child characteristics from the admission records, we ask whether child characteristics correlate with the characteristics of their assigned foster households. The model we estimate is:

$$FosterCharacteristic_i = \beta_0 + \mathbf{ChildCharacteristics}\beta + \gamma_t + \epsilon_i \tag{3}$$

where each of five separate foster household characteristics (occupational income score, household size, age, literacy, and agricultural status) are regressed on a vector of NYJA child characteristics and a set of census year fixed effects. Table 13 summarizes the regression results. While there is no systematically strong statistical relationship between child and foster traits, assignment is also not entirely random. Children who are older when sent West are more likely to go to smaller households and weakly more likely to go to a foster parents with a higher occupational income score and less likely to go to a farm household. Literate children are more likely to match with literate fosters. Relative to children born in the United States outside of New York City, children born in New York or abroad are likely to be assigned to older, less literate households with lower occupational income scores.

Table 13: Foster Characteristics and NYJA Child Characteristics

	(1) Foster occ score	(2) Foster HH size	(3) Foster age	(4) Foster literate	(5) Foster on farm
Age sent West	0.37*	-0.078**	-0.066	-0.0015	-0.015*
	(0.19)	(0.039)	(0.21)	(0.0037)	(0.0082)
Can read & write	0.075	0.28	1.48	0.055**	0.063
	(1.04)	(0.21)	(1.33)	(0.025)	(0.050)
Can read, write,	-0.58	0.20	-0.017	-0.019	0.029
& calculate	(0.84)	(0.19)	(1.19)	(0.019)	(0.043)
Orphan	0.019	-0.20	-0.020	0.013	0.017
	(0.93)	(0.19)	(1.21)	(0.019)	(0.044)
Catholic	0.070	-0.040	-0.54	-0.0096	0.026
	(0.83)	(0.19)	(1.17)	(0.021)	(0.043)
Jewish	1.41	0.095	-6.93***	-0.026	0.037
	(2.90)	(0.48)	(2.33)	(0.066)	(0.13)
Born in NYC	-3.30	0.056	3.41**	-0.046***	0.013
	(2.23)	(0.31)	(1.64)	(0.012)	(0.077)
Born abroad	-5.16**	-0.14	5.06**	-0.065***	0.026
	(2.41)	(0.36)	(2.06)	(0.021)	(0.087)
≥ 1 parent	-0.38	0.085	0.36	0.0039	0.031
intemperate	(0.74)	(0.19)	(1.11)	(0.020)	(0.039)
Parent occupational	0.014	0.0096	0.035	-0.0020*	-0.0023
income score	(0.041)	(0.0093)	(0.057)	(0.0011)	(0.0020)
Admitted for crime	-1.22*	0.21	-1.91*	0.0055	0.018
or misconduct	(0.74)	(0.17)	(1.05)	(0.018)	(0.039)
Admitted for econ-	0.42	0.23	0.19	-0.017	0.061
omic opportunity	(1.11)	(0.21)	(1.30)	(0.025)	(0.046)
N	733	831	831	831	831

All models include Census year fixed effects. Heteroskedasticity robust standard errors in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

We next estimate the relationship between foster traits and child occupational income scores in adulthood using the pooled CAS and NYJA samples. An important caveat is that the selection on observable child traits documented above raises concerns about potential selection on unobservable traits as well. While it seems a-priori difficult for potential foster parents to observe subtle child characteristics correlated with long-term outcomes during the short matching period between children and foster households, anecdotes such as the story of a child biting an unappealing host father suggest that some signals may have been observable. We examine the degree to which these unobservables might bias results by controlling for observable traits and verifying that the effect of foster characteristics is not attenuated. We estimate the following specification

$$\ln(occscore_{it}) = \beta_0 + \beta_1 \ln(occscore)_{ci} + X_i\beta + \gamma_t + \epsilon_i \tag{4}$$

Following standard practice in the literature on intergenerational mobility we regress log child occupational income score in adulthood on log foster occupational income score to estimate an elasticity, controlling for a quadratic in age, a set of additional foster household characteristics, and census year fixed effects, with standard errors clustered by individual child. We report four sets of incremental models: a baseline model only including log foster occupational score plus age and census controls (Columns 1 and 5), a second model adding controls for additional foster household characteristics (Columns 2 and 6), and a third also adding a set of controls for child characteristics when admitted to the NYJA (by necessity, restricted to the subset of NYJA orphan train riders) (Columns 3 and 7). Finally, columns 4 and 8 include controls for both characteristics of the child and the county to which they were sent. Given the hypothesis that age at relocation mediates the effect of moving, we re-estimate the same three models restricting the sample to children who were age 10 or younger when sent West, which are the coefficients reported in Columns 5-8. Based upon the evidence of potential noise in lower quality matches discussed in Section 6, for these models we limit the sample to orphan train rider matches among the top 50% in confidence.

In line with the previous literature, we find that the magnitude of the estimated coefficient of intergenerational transmission is substantially larger for children placed out below 10 years of age, with values for younger children in Panel A ranging from 0.208 to 0.240 compared to estimates of 0.0602 to 0.0703 for all orphan train riders. The estimates for the NYJA sample in Panel B are more similar across ages, again with point estimates of about 0.15 to 0.2. As a point of comparison, estimates in the literature on intergenerational mobility for the early 20th century are in the neighborhood of around 0.35-0.40 for biological parents (Olivetti and Paserman 2015).

The occupational income score of the head of the foster household is, by far, the most predictive variable of the child's long-term occupational income. Additional foster household characteristic explain little further

variation in child outcomes, with only the number of other boys in the foster household showing some signs of significance among younger orphan train riders. Conversely, adding controls for child characteristics improves model fit (columns 2 vs. 3 and 6 vs. 7), but does not significantly attenuate the effect of the foster occupation score. Similarly, controlling for county level characteristics (columns 4 and 8) does little to modify the qualitative impact of foster occupational score. Only in columns 7 and 8 of Panel B, where the sample size falls to 287 observations, does this variable lose significance, largely due to imprecise estimation in this small sample.

Table 14: Child occupational income scores as a function of foster characteristics

	All Matches				Age West ≤ 10			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A: Full Sample								
Ln(foster occ score)	0.0641	0.0602		0.0703	0.208***	0.240***		0.236***
	(0.0467)	(0.0642)		(0.0643)	(0.0660)	(0.0576)		(0.0777)
Foster HH size		-0.00795		-0.00662		-0.00882		-0.00835
		(0.00905)		(0.00867)		(0.0182)		(0.0187)
Foster N boys		0.0120		0.0134		0.0867***		0.0768**
		(0.0190)		(0.0193)		(0.0309)		(0.0316)
Foster on farm		-0.00774		-0.00246		0.0439		0.0608
		(0.0571)		(0.0497)		(0.0794)		(0.0830)
Foster age		0.000928		0.000408		0.000895		0.000115
		(0.00122)		(0.00110)		(0.00176)		(0.00186)
Foster literate		-0.0114		-0.00333		0.00522		-0.00127
		(0.0360)		(0.0261)		(0.0511)		(0.0441)
County Controls				X				X
F-stat	1.886	0.227		0.510	9.905	2.588		3.880
p-value	0.170	0.951		0.513	0.00206	0.0290		0.0014
N	1625	1625		1625	488	488		488
R^2	0.139	0.140		0.176	0.150	0.176		0.204
Panel B: NYJA Sample								
Ln(foster occ score)	0.0915*	0.155**	0.205***	0.219***	0.192**	0.187**	0.138	0.110
	(0.0503)	(0.0623)	(0.0781)	(0.0698)	(0.0796)	(0.0816)	(0.120)	(0.144)
Foster HH size		-0.00341	0.00000825	-0.000777		-0.00977	0.00873	0.00694
		(0.00970)	(0.00869)	(0.00862)		(0.0208)	(0.0221)	(0.0179)
Foster N boys		0.00816	-0.000877	0.00561		0.0861**	0.0341	0.0350
		(0.0195)	(0.0189)	(0.0194)		(0.0358)	(0.0391)	(0.0340)
Foster on farm		0.0695	0.0424	0.0239		0.00616	-0.101	-0.0836
		(0.0691)	(0.0828)	(0.0626)		(0.128)	(0.118)	(0.141)
Foster age		0.000291	-0.000635	-0.000968		0.000779	0.000405	-0.00219
		(0.00132)	(0.00133)	(0.00125)		(0.00184)	(0.00245)	(0.00238)
Foster literate		0.0371	0.0232	0.0153		0.0268	0.0391	-0.00749
		(0.0280)	(0.0260)	(0.0249)		(0.0560)	(0.0473)	(0.0353)
County Controls				X				X
Child Controls			X	X			X	X
F-stat	3.310	1.723	2.373	3.676	5.836	4.181	1.496	1.115
p-value	0.0698	0.115	0.0298	0.0016	0.0177	0.000948	0.192	0.3458
N	1268	1268	1101	1101	372	372	286	287
R^2	0.162	0.166	0.205	0.240	0.197	0.222	0.368	0.398

Outcome for every specification is log child occupational score after age 18. Panel A includes both CAS and NYJA children, while Panel B is restricted to NYJA children only. All specifications control for age, age-squared, and Census year fixed effects. Child controls are age sent West, ability to read/write, ability to read/write/calculate, orphan, Catholic, Jewish, born in NY, born abroad, intemperate parent, parent occupational score, admitted for misconduct, and admitted for economic opportunity. County controls are the set listed in Table 11. F-statistics test the joint hypothesis that all foster characteristics are zero. All samples are restricted to foster matches with scores above 19 and child matches with scores above 38. Standard errors clustered by individual child in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

8 Conclusion

The orphan train movement represented one of the largest child welfare experiments in American history, relocating approximately 200,000 children from urban institutions to primarily rural households across the country between 1854 and 1929. Our analysis of this natural experiment provides new evidence on the relative importance of geography versus family environment in determining children's life outcomes.

Although the primary state objective of the program was to improve children's prospects, we find no evidence that being sent West impacted adult economic outcomes relative to remaining in New York institutions. Point estimates suggest small negative effects on occupational income status, though these are not statistically significant and likely correlate with a higher propensity to be found farming.

Among children sent West, substantial variation in county-level economic opportunities also had no detectable effects on adult success. Counties with greater urbanization, industrialization, educational infrastructure, or innovation showed no advantage in promoting upward mobility for relocated children. These null results persist across multiple specifications and robustness checks, suggesting that the challenge of identifying exogenous place effects versus potential selection in location choice previously identified in the literature (Oreopoulos 2003) is valid, and corroborating recent findings that child relocation programs have marginal effects on later income and employment (Ludwig et al. 2013; Harding et al. 2023).

Within the household, foster household characteristics are significant predictors of children's later economic outcomes. The occupational status of foster heads of household predicts children's adult occupational status with an intergenerational elasticity of roughly 0.2, depending on the specification. This finding aligns with a substantial literature emphasizing the central role of family environments in child development and suggests that household factors matter even when households are reassigned quasi-randomly in childhood. The contrast between our null place effects and positive family effects suggests that policy interventions should focus on household quality rather than geographic placement strategies.

For modern foster care policy, our findings suggest that placement decisions should prioritize foster family characteristics over geographic factors. While the historical context limits direct policy applications, the results support focusing resources on foster family selection, training, and support rather than geographic placement strategies. The orphan train movement's philosophy that "country air" and rural environments would inherently benefit urban children finds little support in our data.

Several important limitations warrant note. Our analysis is restricted to males due to difficulties linking females across censuses after marriage-related name changes. The historical setting, with its different transportation costs, communication technologies, and economic structures, may limit generalizability to modern contexts. Additionally, our measures of adult outcomes, while comprehensive for the historical

period, cannot capture important dimensions of well-being that previous work has identified as impacted by neighborhood effect, such as physical or mental health.

The orphan train movement also differed from modern foster care in several key ways that may affect the interpretation of our results. Children were permanently separated from their birth families with little prospect of reunion, unlike modern foster care which often aims for family reunification. The legal frameworks governing child welfare were much weaker, with limited oversight of foster placements. Rural households in the late 19th century may have viewed orphan train children primarily as sources of labor rather than family members, potentially limiting the development of strong family bonds that modern research suggests are crucial for child outcomes.

Our findings also speak to broader questions about the determinants of intergenerational mobility in American history. The period we study, spanning the late 19th and early 20th centuries, was characterized by rapid economic transformation, geographic expansion, and social change. That place effects appear modest even in this context of dramatic regional variation suggests that family-level factors may be consistently important determinants of children's life chances across different historical periods.

Future research could extend this analysis in several directions. Examining outcomes for female orphan train riders through alternative linking strategies could provide a more complete picture of the program's effects. Investigating longer-term intergenerational effects by tracking the children and grandchildren of orphan train riders could reveal whether place effects manifest over longer time horizons. Exploring heterogeneous effects across different types of children, foster households, and destination characteristics could identify conditions under which place effects might be more important.

Additionally, obtaining records from other sending agencies, particularly the New York Foundling Hospital, could provide tighter, causal estimates of the role of foster household characteristics. The children sent West by this agency tended to be sent at much younger ages, including in infancy, and unlike the CAS and NYJA children, who were matched upon arrival, these children matched with foster families arbitrarily ahead of time based on superficial traits such as hair color.

The orphan train movement ultimately reflected the optimism of Progressive Era reformers who believed that environmental change could transform children's lives. While our analysis suggests this optimism was misplaced with respect to geographic factors, the movement's emphasis on removing children from harmful environments and placing them with caring families contained important insights. The variation in outcomes based on foster family characteristics suggests that the movement's core insight that changing children's environment can be beneficial remains valid, albeit not because of the mechanism hypothesized by the participating agencies. The lesson from this historical experiment is thus not that environment doesn't matter for children's outcomes, but rather that family-level environments appear to matter more than geographic

ones. Large-scale interventions that rely primarily on changing children's physical environment, without attention to the quality of their immediate caregiving relationships, are unlikely to be transformative.

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A Details on Matching Criteria

Here we provide details on the match scoring criteria used to evaluate candidate matches for each search group. In the matching algorithm, matches can receive positive points for matching exactly on certain criteria and can be penalized points for failing to match as well. Although the points/penalty system is rank equivalent to having zero penalties and increasing the match points by the penalty amount, we use use the penalty system as a matter of preference. The dtalink program can estimate weights automatically, but in practice we found that for the specific parameters of our matching problem user-suggested weights, while less systematic, consistently yielded better matches when reviewer by a human. Rare last names are classified as those outside of the top 1,000 surnames in a given census year.

A.1 NYJA children sent West

We block on first initial, last initial, and age being with plus or minus five years (inclusive) of the NYJA child's age in the census year, calculated based on reported birth year or reported age upon admission to the NYJA, and we only search for matches in census years after the reported year when the child was sent West.

Table 15: Scoring and Penalty Scheme

Variable	**Points**	**Penalty**
Middle initial	2	0
Exact first name	4	-2
Exact last name	6	-1
Exact rare first name	4	0
Exact rare last name	5	0
Soundex first name	5	-5
Soundex last name	5	-4
Gender	1	-13
Exact foster last name	10	0
Soundex foster last name	3	0
Exact age	1	0
Age within 1 year	2	-1
Listed as boarder	5	0
Birthplace match	3	0
From New York	5	0
Last name different from head	5	0
In destination state	3	-1
In destination county	3	0
Maximum possible	72	—
Cutoff	36	—

A.2 NYJA children not sent West

We block on first initial, last initial, and age being with plus or minus five years (inclusive) of the NYJA child's age in the census year, calculated based on reported birth year or reported age upon admission to the NYJA, and we only search for matches in census years after the reported year when the child was admitted to the NYJA.

Table 16: Scoring and Penalty Scheme

Variable	**Points**	**Penalty**
Middle initial	2	0
Exact first name	4	-2
Exact last name	6	-1
Exact rare first name	4	0
Exact rare last name	5	0
Soundex first name	4	-5
Soundex last name	5	-4
Gender	1	-13
Head first name same as father's	4	0
Head first name sounds like father's	3	0
Head first name same as mother's	4	0
Head first name sounds like mother's	3	0
Last name same as head's	10	0
Last name sounds like head's	8	0
Exact age	1	0
Age within 1 year	2	-1
Birthplace match	3	0
From New York	5	0
In New York	3	-1
Maximum possible	77	—
Cutoff	36	—

A.3 CAS children sent West

We block on first initial, last initial, and age being with plus or minus five years (inclusive) of the NYJA child's age in the census year, calculated based on reported birth year or reported age upon admission to the NYJA, and we only search for matches in census years after the reported year when the child was sent West.

Table 17: Scoring and Penalty Scheme

Variable	**Points**	**Penalty**
Middle initial	2	0
Exact first name	4	-2
Exact last name	6	-1
Exact rare first name	4	0
Exact rare last name	5	0
Soundex first name	5	-5
Soundex last name	5	-4
Gender	1	-13
Exact foster last name	10	0
Soundex foster last name	3	0
Exact age	1	0
Age within 1 year	2	-1
Listed as boarder	5	0
Birthplace match	3	0
From New York	5	0
Last name different from head	5	0
In destination state	3	-1
In destination county	3	0
Maximum possible	72	—
Cutoff	36	—

A.4 Foster parents for NYJA and CAS children sent West

For assigned foster parents, we block on being listed in the census as the head of the household, being in the state where the child was placed, and having the same first initial of their last name as the reported foster parent.

Table 18: Scoring and Penalty Scheme

Variable	**Points**	**Penalty**
Exact first name	3	0
Exact last name	10	0
First name initial	3	0
Exact rare last name	6	0
In destination county	3	0
Maximum possible	25	—
Cutoff	18	—