The Long-Run Impact of Cash Transfers to Poor Families[†]

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Abstract

We estimate the long-run impact of cash transfers to poor families on children's longevity, educational attainment, nutritional status, and income in adulthood. To do so, we collected individual-level administrative records of applicants to the Mothers' Pension program—the first government-sponsored welfare program in the United States (1911–1935)—and matched them to census, WWII, and death records. Male children of accepted applicants lived one year longer than those of rejected mothers. They also obtained one-third more years of schooling, were less likely to be underweight, and had higher income in adulthood than children of rejected mothers.

More than 20 percent of children in the United States were living in poverty as recently as 2010.[1] A growing literature documents the adverse long-term effects of early-life exposure to disease, nutritional deprivation, and other factors associated with poverty on educational attainment, labor market outcomes, and ultimately, mortality (Almond and Currie 2011b). In the United States and elsewhere, welfare programs—broadly defined as cash transfers to poor families—were established primarily to help children. While parental income has been shown to be one of the strongest predictors of children's educational attainment (Barrow and Schanzenbach 2012; Reardon 2011) and health in adulthood (Case, Lubotsky, and Paxson 2002), it is still unknown whether these cash transfers provide lifelong benefits for children raised in poor families (Currie 1998).

There are multiple reasons why means-tested cash transfers could fail to help poor children: the amounts given may be insufficient; parents might not use the transfer in ways that benefit

their children, or might use the transfers inefficiently due to poor information (Dizon-Ross 2014). The program could also induce parental behavioral responses that are potentially detrimental to the child, altering their labor supply, fertility, or probability of remarriage. While a large literature considers parental responses to welfare receipt (Moffitt 1998), little is known about the overall impact of the transfers on the lifetime outcomes of the children of beneficiaries.

One of the main difficulties in evaluating whether cash transfers (or any public program) improve outcomes is identifying a plausible counterfactual: what would children's lives have been like in the absence of receiving transfers? The other difficulty lies in obtaining data on long-term outcomes for a large sample of recipients and plausible comparison groups. Survey datasets such as the National Longitudinal Survey of Youth (NLSY) and Panel Study of Income Dynamics (PSID) include only a small number of welfare recipients from recent cohorts and, moreover, suffer from substantial attrition.[2] Individual-level administrative records from the early years of the Aid to Dependent Children (ADC) program (1935–1962) have been lost or intentionally destroyed. Although records do exist for recipients of Aid to Families with Dependent Children (AFDC, the program that replaced ADC in 1962), these cohorts are too young for us to evaluate the impact of welfare participation on their longevity. Recent cohorts of recipients are also problematic because recipients tend to be eligible for many other transfers, such as Medicaid, housing assistance, and food stamps, thus making it difficult to evaluate the impact of cash transfers alone.[3]

To overcome these challenges, we collected administrative records from the precursor to the ADC program; the Mothers' Pension program (1911–1935), which was the first US government-sponsored welfare program for poor mothers with dependent children.[4] The intent of the MP program was to improve the conditions of "young children that have become dependent through the loss or disability of the breadwinner" (Abbott 1933, p. 1). The transfers generally represented 12–25 percent of family income, and typically lasted for three years. To look at the impact of these transfers, we track longevity and other outcomes of the children whose mothers applied to the program. These data include information on thousands of accepted and rejected applicants born between 1900 and 1925, most of whom had died by 2012. The identifying information in the application records allows us to link the children with other datasets to trace their lifetime outcomes.

For identification, we use as a comparison group *children of mothers who applied for transfers* and who were initially deemed eligible, but were denied upon further investigation. This strategy of comparing accepted and rejected applicants for program evaluation has been used successfully in studies of disability insurance (von Wachter, Song, and Manchester 2011; Bound 1989). Its validity depends on the extent to which accepted and rejected mothers and their children differ on unobservable characteristics. We document that rejected

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[2]We compute that in the NLSY and PSID surveys, 40 percent of children who received welfare are lost to follow-up after 20 years.
[3]We have been unable to find early AFDC records that contain identifying information that would allow us to match with data to measure long-term outcomes. Survey data for AFDC recipients exist for 1967 and later, but these do not include (and do not allow for) any long-term follow-up, or the construction of control groups.
[4]In 1935, the MP program was replaced by the federal Aid to Dependent Children (ADC, later Aid to Families with Dependent Children, and now Temporary Assistance to Needy Families (TANF)).

mothers were on average slightly better-off, based on observable characteristics at the time of application. We also match two subsamples of recipients to pre-application characteristics in the federal censuses (1900–1920) and in the 1915 Iowa State Census. Though the samples are smaller, we find that rejected applicants came from richer families: they had higher incomes, were more likely to own their homes, and conditional on homeownership, their homes were of greater value. These data are consistent with the information in our administrative records, which report that applicants were rejected most often because they were deemed to have sufficient support. Our findings are also in line with the few available historical accounts of these records. Finally, we directly investigate whether discrimination on the basis of race or nativity can explain our findings and conclude that they do not. Under the assumption that accepted and rejected applicants are otherwise similar, the outcomes for boys of rejected mothers provide a best-case scenario (upper bound) for what could be expected of beneficiaries in the absence of transfers.

Using data collected on over 16,000 boys from 11 states, who were born between 1900 and 1925, and whose mothers applied to the Mothers' Pension program, we find that receiving cash transfers increased longevity by about 1 year. This effect is greater for the poorest families in the sample: their longevity increased by 1.5 years of life. These results are very robust to alternative functional form specifications, alternative counterfactual comparisons (e.g., comparing eligible and ineligible families), and our treatment of attrition. Because income transfers were the *only* major public benefit that poor children were eligible for until 1950 (with the exception of public schooling), we can interpret our results as the effect of cash transfers alone.

To investigate potential mechanisms behind the positive effect on longevity, we match a subset of our records to WWII enlistment and 1940 census records. The results suggest that cash transfers reduced the probability of being underweight by half, increased educational attainment by 0.34 years, and increased income in early adulthood by 14 percent. Previous work has documented that all three measures (being underweight, income, and education) are independently associated with mortality (Flegal et al. 2005; Deaton and Paxson 2001; Cutler and Lleras-Muney 2008). A back of the envelope calculation based on estimates from these studies suggests that at least 75 percent of the observed increase in longevity can be explained by these three mechanisms.

Our analysis has some important limitations. We cannot examine outcomes for women because they typically changed their name upon marriage, making it extremely difficult to track long-term outcomes through sources that can be linked only with the consistent reporting of names. Nor can we study African-Americans because they are not well represented in our states or our data samples. Finally, though our results are based on larger samples with lower attrition than current panel surveys, there is still attrition in our sample. However, for a subset of our sample we were able to collect additional data, thereby significantly reducing attrition, and the results remain unchanged, thus suggesting that attrition is not influencing our results.

We conclude that cash transfers to poor families during the first part of the twentieth century ameliorated early life conditions enough to improve both medium- and long-term outcomes

of boys growing up in poverty. While conditions today differ significantly from those at the beginning of the twentieth century, which causes us to be cautious of drawing conclusions regarding the anticipated impact of cash transfers in the twenty-first century, it is still the case that the historical evidence constitutes the best available means to assess the impact of cash transfers across the life course. Moreover, three important similarities remain. First, both the MP program and current welfare programs target children in female-headed households—and we document that these children were, and continue to be, the poorest children in the population. Second, historical comparisons presented in the concluding section suggest that family income plays an important role in producing positive child outcomes, both today and at the beginning of the twentieth century, when the MP program operated. Finally, our short- and medium-term effects on education and health are consistent with contemporary evidence on the effect of poverty-reduction programs in the United States and in developing countries. Together these results suggest that targeted cash transfers are also likely to improve lifetime outcomes today. We return to the related literature and policy implications in the final section of the paper.

I. Mothers' Pension Programs: History and Characteristics

The MP program was a needs-based program, established on a state-by-state basis between 1911 and 1931. When it was replaced by ADC in 1935, 200,000 children were receiving MP benefits (Katz 1996). Several factors prompted the enactment of MP legislation. At the time, children of destitute parents were routinely sent to orphanages, and these children were thought to fare very poorly.[5] Moreover, among those who remained with their mothers, prominent judges of juvenile courts argued that maternal absence, due to full-time employment, was the main reason why many of these children became delinquent.[6] MP programs were seen as a cheaper and better alternative for children since income transfers would allow mothers to care for their children at home.[7] There was also a growing sense that poverty was not being adequately addressed by private charity. The spirit of the legislation is well captured in Colorado's law: "This act shall be liberally construed for the protection of the child, the home and the states and in the interest of public morals, and for the prevention of poverty and crime" (Lindsey 1913, p. 716).

States had complete discretion in establishing an MP program, setting eligibility criteria, and providing funding. Online Appendix Table S1 shows the details of the MP laws for all states with MP programs.[8] This information is available from various publications for years 1914,

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[5]The conditions in institutions for children were also often deplorable: "[T]he year before the Foundling Asylum was closed the death rate of foundling babies in the asylum was fifty-nine out of a hundred. After the Associated Charities put the babies into foster-homes, where they are given a mother's care, the death rate dropped to six out of a hundred" (Bullock 1915, pp. 92–93). The 1914 Kingsbury commission inspected 38 institutions for children in NYC and found 26 of them to be substandard "institutions in which beds were alive with vermin, in which antiquated methods of punishment prevailed and in which the children were given little else save religious instruction" (Hopkins 2011).

[6]Notable judges actively supportive of the legislation included Judge Portfield of Missouri, Judge Wilbur of LA, Judge Pinckney of Chicago, Judge Neely of Milwaukee, and Judge Lindser of Denver (Bullock 1915). Indeed, some claimed that MP laws lowered juvenile crime ("Mothers' Pensions Cut Juvenile Crime; Judge Neil Tells benefits of the Aid Illinois Gives to Poor Widows. Hopes to Extend System East Side Committee Appointed at Meeting to Push Fight for New York Law" *New York Times*, January 11, 1915).

[7]San Francisco gave institutions at most $11/month per child committed, compared with $6.25/month to MP widows. In general MP was about 1/3–2/3 the cost of boarding. The US Department of Labor, Children's Bureau (1922a) report cites additional numbers that suggest that ex post the cost of MP was indeed lower than that of institutionalization. The White House Conference on the Care of Dependent Children strongly recommended allowing poor children to stay at home, justifying this with the claim that the "best person to care for a child, save in exceptional cases, is its own mother" (*New York Times*, May 11, 1913).

1916, 1919, 1922, 1925, 1926, 1929, and 1934 (see online Appendix II for sources). Below we describe how the programs varied in terms of eligibility, generosity, duration, and conditions for receipt in 1922, the median application year in our data.

Eligibility

All states required the mother to be poor, though neither income nor property thresholds were specified. States also required the husband to be either missing or incapacitated (physically or mentally) and while poor widows were eligible everywhere, states varied with respect to their treatment of deserted or divorced women and women whose husbands were in prison or hospitalized. Citizenship was not required in most states; however even in those states that required citizenship, the intention to become a citizen was sufficient to qualify (US Department of Labor, Children's Bureau 1933). Evidence from Iowa suggests that by limiting eligibility to mothers with dependent children, the MP program succeeded in targeting the poorest children. Using data from the 1915 Iowa state census—the only individual survey of households that collected income prior to 1940—we find that boys under the age of 18 growing up in households without a married male (11 percent of all boys) were significantly poorer. They possessed half the income and were substantially more likely to be at the bottom of the income distribution than boys in households with a married male present (online Appendix Table S2).

Administration

Importantly, state MP laws only established guidelines; it was up to individual counties to create, fund, and administer their own programs. As a result, there was both substantial cross-state and within-state variation in program characteristics and implementation. For instance, many counties never implemented MP programs, despite the state law.[9] Moreover, in counties with laws, eligible families were underserved: the US Department of Labor, Children's Bureau (1926) estimated that only one-third of the targeted families received help.

Generosity

The state-legislated maximum monthly benefit for the first child varied across states, ranging from a low of $10 in Iowa, to a high of $35 in Ohio, with the total monthly amount increasing nonlinearly with the number of children in the family. In practice, generosity in benefit levels varied widely across counties within a state.[10] In our records, the average transfer ranges from $10 to $30 per month. To better understand the generosity of the benefits in real terms, we compare the monthly transfers to the average wages in manufacturing in the state (online Appendix Table S3): the average monthly MP transfer was between 17 percent and 20 percent of monthly manufacturing wages.[11] In a handful of counties, records of maternal income of MP recipients are available (online Appendix Table

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[8]Comparing the characteristics of the programs in our 11 states with the characteristics of MP programs in states for which we were unable to obtain individual records suggest that they are similar (online Appendix Table S1) and thus the MP programs examined here are representative of the existing programs.

[9]Detailed data by county on whether they had a program and whether records survive is available at http://individual.utoronto.ca/shari_eli/mp.html.

[10]For example, we calculate that across counties in Ohio in 1925, the level of benefits for a family of three ranged from a low of $3 per month to a high of $38 per month.

S3, column 4). Not surprisingly, maternal income was considerably lower than manufacturing wages, and relative to these lower levels, the MP transfers were more generous, representing 29–39 percent of maternal income.[12] Overall the evidence suggests that while MP transfers represented a substantial source of income for poor mothers, these additional cash transfers did not elevate them to the middle class. We cannot say definitively whether or how MP transfers may have crowded-out private transfers, but the historical evidence does not support strong crowd-out.[13]

Duration

In most states the transfers would be given until the pension was revoked. However, five states in our sample required reapplication at intervals ranging from three months to one year (online Appendix Table S1). In our records, the median duration in an MP program among recipients is three years.

Additional Requirements or Conditions

While most states required the mother to stay at home, Illinois, Minnesota, Montana, Ohio, Oregon, and Wisconsin allowed counties to require or regulate maternal work. Many laws also explicitly required that the mother be of "good morals." However, in the records that include information on reason for discontinuation (Table 1), there are very few instances in which a mother or child's failure to comply with these conditions is listed as the reason for discontinuation. The most common cause of discontinuance was loss of eligibility due to remarriage. We conclude that the MP program should be viewed as an unconditional cash transfer, rather than as a transfer that was contingent on specified actions by the recipient.

II. Data

A. MP Records

We have attempted to collect all the MP records that survive containing dates of birth and full names. Our efforts have yielded approximately 80,000 child recipients whose mothers applied to the MP program between 1911 and 1935 in 11 states: Idaho, Illinois, Iowa, Minnesota, Montana, North Dakota, Ohio, Oklahoma, Oregon, Washington, and Wisconsin. These data include the full universe of families who received MP benefits in the county, state, and year. For some states, we have the full universe of counties that provided MP benefits, while for others we have only a subset of these counties; but if a county has records, the universe of records is available.

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[11]Alternatively in 1919, MP transfers ranged from 8 to 22 percent of the total household income in urban two-parent households or 20 to 60 percent of a farm laborer's income.

[12]In the subsample of counties in Illinois that collected information on maternal income, MP transfers represented about 29 percent of the median maternal monthly income of $60. In Hamilton County (Ohio) in 1914, detailed investigation of MP mothers found that their weekly earnings averaged $4.63 and the average pension per month was $23.28 (Bullock 1915). A 1926 study by the Department of Welfare in PA found that among 2,404 families receiving grants, MP transfers accounted for 39 percent of total income (Lundberg 1928).

[13]For example, in Pennsylvania, the 1926 survey of families receiving MP pensions showed that 11 percent of families were receiving additional aid from private charity (Lundberg 1928). In Hamilton County in 1914, 70 percent of recipients were also receiving charity from other sources (Bullock 1915). If present, crowd-out would lead to a downward bias in the estimated effects of the transfer.

These data appear to be representative of the MP population in the states on which we focus based on a comparison of our data for 1930 with published statistics for ten of these states in 1931 (online Appendix Table S4a and Figure S5).[14] For a few counties we also compared the average grants in our data with published county-level averages (online Appendix Table S4b) and verified their similarity.[15]

From the MP records, we observe each mother's first and last name, the county or town of her residence, the full names of her children, their dates of birth, the reason for her application (widowed, abandoned, etc.), and whether the application was accepted or rejected. If the application is accepted, we observe the monthly amount of the pension, and dates of receipt. For some counties we have additional information, such as the reason why an applicant was rejected or the reason why transfers were discontinued. For a single county (Clay County, MN) we have data from a detailed 1930 study based on nurse visits to the homes of all 62 families in the MP program at that time.

B. Mortality Data and Matching

Each male child of every MP applicant was matched to records from the Social Security Death Master File (DMF). The DMF contains the name, date of birth, date of death, and Social Security number for 88 million individuals whose deaths were reported to the SSA from 1965 until 2012. We matched individuals based on their first, middle, and last name, as well as their day, month, and year of birth. Details of the matching procedure are in online Appendix I.

Not all individuals who died can be found because individual death records are only systematically available for the population after the mid-1970s (Hill and Rosenwaike 2001). [16] Individuals who died before the mid-1970s may be in the database but the records are incomplete. Based on cohort life tables, we calculate that 72 percent, 48 percent, and 28 percent among the 1900, 1910, and 1920 cohorts are likely to have died by 1975. Also, we can only follow individuals up to 2012. The fraction of those surviving past 2012 is 0 for both 1900 and 1910 cohorts but it rises to 5.3 percent for the 1920 cohort, and to 31.5 percent for the 1930 cohort. We compare the predicted share of missing matches by cohort assuming the matches are missing only because of deaths prior to 1975 or after 2012 (online Appendix Figure S1a), with the actual share of missing matches in our data (online Appendix Figure S1b). Both show a very similar U-shaped pattern, leading us to conclude that the missing data pattern by cohort in our sample is consistent with mortality-driven attrition. However, we limit attention to cohorts born before 1925 to minimize the share of individuals that are still alive.

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[14]Published statistics by state are available in 1921 for selected states, and in 1928, but the most detailed and comprehensive statistics are available for 1931. Across nine of the states where we can compare average 1931 program characteristics in our sample and average 1931 program characteristics in published figures for the state, a regression of state average on sample average yields β = 0.605 and p = 0.03 for the mean monthly grant, and β = 0.819 and p = 0.186 for the mean family size. Our sample states are somewhat less generous than the US average ($20.12 versus $31.97) owing to our exclusion of four particularly generous states (Massachusetts, New York, Rhode Island, and Connecticut). Average family size is also slightly smaller in our sample states (2.62) than in the United States (2.71). See online Appendix Table S4a.

[15]There are very few published statistics by county for the counties. In online Appendix Table S2b we provide all of the evidence by county we were able to collect.

[16]By the early 1970s, the authors conclude that 95 percent of deaths of persons 65 years of age and older and 75 percent of deaths of those ages 25–64 were included in the DMF.

We were able to match 48 percent of our sample to a unique SSA death record. Four percent were linked to multiple records. Therefore, we have information on age at death for 52 percent of our sample and 48 percent had no match. Using life tables and the age at which we observe children alive and in the MP program, we computed the number of individuals who would be expected to die prior to the existence of comprehensive DMF data (around 1975). These calculations suggest that about 32 percent of those in the MP records should have died prior to the DMF; therefore we find at least one match in the DMF for more than 77 percent of the individuals whose death records should be in the DMF, assuming the MP applicants are a representative sample.[17] However, given that these families are poor and existing evidence links poverty to shorter life expectancy, one would reasonably expect deaths before entry into the DMF to be higher than 32 percent, so our match rate likely exceeds the 77 percent figure. We conclude that the amount of attrition is reasonable and we use different methods to assess its influence on our results.

C. Other State and County Data

We include as controls all the time-varying characteristics of the MP laws described previously (and listed in online Appendix Table S1). We also include state-level, time-varying characteristics that we believe might have affected the existence or generosity of the program: the ratio of state manufacturing earnings to national manufacturing earnings, laws governing school attendance, and expenditures on social programs, education and charitable institutions, hospitals, and prisons.[18] For Ohio in particular, we were able to obtain county-level expenditures for several years, including expenditures on total relief, outdoor relief, and children's homes (see online Appendix II for details). These data allow us to rule out possible confounding factors and bias in the estimates (i.e., if MP program characteristics such as generosity and rejection rate are influenced by other resources available for the poor in the county).

D. Sample Selection

To maximize the quality of the matched data we made several sample restrictions. We dropped individuals without a year of birth or year of application as well as those without a first or last name. Our work and the results from the existing literature suggest that matching rates are substantially lower in the absence of this key information. For the same reasons, we did not collect county records that failed to include this information. As noted previously, we limit our analysis to males because women often change their names upon marrying and thus are substantially harder to match. We also restrict attention to cohorts born between 1900–1925 to maximize the likelihood that we find individuals in the mortality records and the likelihood that individuals have died by 2012.

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[17]For comparison, we computed follow-up rates for the two datasets that have been used for evaluating the effects of welfare on children's outcomes: the National Longitudinal Study of Youth 1979 (NLSY) and the Panel Study of Income Dynamics (PSID). We kept only male children whose mother was receiving welfare when they were first interviewed and used the latest wave of the survey to see how many had died and what the follow-up rate is in these prospective samples. There are about 1,400 boys in the NLSY and 1,066 in the PSID (born between 1951 and 1968 in the case of the PSID) whose mothers received welfare during their childhood, and within 20 years about 40 percent are lost to follow-up, and none are known to have died. Thus, these samples are substantially smaller and suffer from much larger attrition than our data.

[18]These state-level variables were available for several of the years and we interpolated in between cross sections.

We made some additional sample restrictions for data quality reasons. We dropped a few individuals older than 19 or born after the mother applied because they are very rare and information on older children or new children does not appear to have been systematically collected in the records.[19] We also drop individuals whose mothers applied to the program after 1930 as we were not able to collect these records systematically since many programs were defunded during the Great Depression.[20]

Finally, to maximize the internal validity of the study, we exclude counties without information on rejected applicants. Online Appendix Table S5 shows the details of our sample selection and how it affects our sample size. Our final sample includes approximately 16,000 males in 75 counties from 11 states and appears to be representative of the states and counties. We present estimates of the extent to which county characteristics (based on the 1910 census and including socioeconomic index, share old, young, white, foreign born, literate, in manufacturing, and in agriculture) predict inclusion in our final sample (online Appendix Table S6). While none of the characteristics we examine is a statistically significant predictor of inclusion in our sample, the included counties had higher fractions literate and immigrant. We conclude that our final analysis sample consisting of 75 counties is generally representative of the nation at the time.

Among the 16,000 in our final sample, 14 percent were rejected applicants. In particular, the share of rejected applicants ranges from a low of 5 percent in Minnesota to a high of 17 percent in Ohio, the state from which most individuals in our sample originate (34 percent of individuals in our sample come from Ohio). The variation in rejection rates across counties is likely due to the fact that in some areas applicants were summarily rejected without filing a formal application, which led to a lower formal rejection rate. A study by Abbott and Breckinridge (1921, p. 72) of MP programs in Illinois during the 1910s states the following regarding the MP application process:

> *A woman who is found, upon preliminary questioning, to be plainly ineligible to [receive] a pension is not allowed to file her application. If she is destitute and ineligible for a pension, she is told that she must apply to some relief agency and is told where to go. If it is not clear that an applicant is ineligible, the application is filed, the court officer investigates and the committee, on the basis of this investigation, recommends that the application be granted or dismissed.*

We investigate the comparability of accepted and rejected applicants in our records further in the sections that follow. But it is worth emphasizing that many applicants who applied but were immediately turned down are not in our records, only those who passed a preliminary evaluation are. This further supports our use of the rejected as a control as it underscores the similarities across the two groups. We are not claiming there was no discrimination against some groups in the MP program; indeed as we discussed the laws themselves often explicitly excluded nonnatives, abandoned or divorced women, working women, and those

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[19]In many counties only eligible children are listed.

[20]Many county programs ended or shrank due to lack of funds (Abbott 1934). In the counties that maintained their programs and for which we have data, many married women with unemployed husbands were allowed to receive funds suggesting large changes in the composition of families in the MP program. Finally, very large relief programs were in place between 1933 and 1940, and many of these were substantially more generous than the MP program.

deemed of low morals. This is why we cannot (and do not) compare categorically eligible versus ineligible groups, but rather compare individuals who were both deemed eligible to apply. Nevertheless we return to the issue of comparability and discrimination, and attempt to address these issues with additional data.

III. Empirical Strategy and identification of the Effects of Transfers

A. Basic Empirical Model

We start by estimating an accelerated failure time (AFT) hazard model of the functional form

$$\log(\text{Age at death})_{ifts} = \theta_0 + \theta_1 MP_f + \theta_2 \mathbf{X}_{if} + \theta_3 \mathbf{Z}_{st} + \boldsymbol{\theta}_c + \boldsymbol{\theta}_t + \varepsilon_{if},$$

where the dependent variable is the natural log of the age at death for a given individual i in family f born in year t living in county c (state s), MP_f is defined as an indicator for whether the child's family received MP benefits, and $\mathbf{X}$ is a vector of relevant family characteristics (marital status, number of siblings, etc.), and child characteristics (year of birth and age at application). We also control for county-level characteristics in 1910, and state characteristics in the year of application ($\mathbf{Z}_{st}$). In our preferred specification we also control for county fixed effects ($\boldsymbol{\theta}_c$) and cohort fixed effects ($\boldsymbol{\theta}_t$). Thus, the effect of the program θ_1 is identified by comparing the average age at death of accepted boys to rejected boys within county and year of birth, conditional on other observables. Standard errors are clustered at the county-level.

B. Model to Address Attrition and Multiple Matches

This baseline specification provides a convenient summary measure of the total effect of the program on longevity. But it does not allow us to easily deal with attrition and places strong restrictions on the shape of the hazard rate. Therefore we also estimate the effect of cash transfers on outcomes using the following logit model:

$$P(\text{survived to age a} = 1)_{iftcs} = f(\theta_0 + \theta_1 MP_f + \theta_2 \mathbf{X}_{if} + \theta_3 \mathbf{Z}_{st} + \boldsymbol{\theta}_c + \boldsymbol{\theta}_t + \varepsilon_{if}),$$

where P is probability of surviving past age a for a given individual with all other covariates defined as before. We can estimate this model for all ages. And to investigate the role of attrition, we can assume that all those without a match in the DMF were deceased and set the binary indicator for survival used in equation (2) to zero for these unmatched observations. We can then compare survival regression results for the "matched sample" (where only those with unique ages at death are included) with results from the " full-sample" where we impute a zero to those without an age at death in all our estimations. If the missing data is entirely explained by early mortality as suggested by the life tables, then the full-sample estimates will be correct. In addition to missing matches (attrition), we have multiple matches for 4 percent of our sample. We use the estimation procedures developed by Bugni, Honoré, and Lleras-Muney (2014) to account for multiple matches in the logit estimation.[21]

C. Evaluation of an identification Strategy Based on Rejected Applicants

For identification of causal effects, we use rejected applicants as the counter-factual, a strategy that has been used by others to estimate program impacts (e.g., Bound 1989; von Wachter et al. 2011). The rationale for using rejected applicants is that they are likely similar to recipients on observable and unobservable characteristics. Not only are they likely to face similar economic conditions at the time of application, but they are also likely to share the same level of (unobserved) factors such as "motivation" and knowledge of the MP program. [22] Moreover, as explained above, they likely appeared eligible upon first examination.

We investigate here the validity of using the rejected as a counterfactual through a systematic comparison of the two groups. First, we compare the characteristics of accepted and rejected boys based on characteristics in the administrative records. Second, we match a subset of the data to census records in years prior to the date of application. These census manuscripts contain measures of income, homeownership, education, and occupation, which we use to better assess and compare the accepted to the rejected. Third, we examine the reasons for rejection and discontinuance among the two groups.

On observables, accepted and rejected applicants look similar but not identical. On average, rejected applicants were slightly older and came from slightly smaller families. For rejected applicants, the average age of the children in the family was higher, particularly the age of the youngest child (see Table 2A for a comparison of means and online Appendix Figure S2 for a comparison of distributions). That courts rejected families with children of, or close to, working age as well as families with only a single child is consistent with qualitative evidence on the rejection of families because they were considered in less need of support. Widowhood (the omitted category) was also more predictive of both acceptance and duration most likely because it was considered more permanent than paternal imprisonment or hospitalization, the two most common other sources of eligibility, and also because widowed women were generally poorer (online Appendix Table S2).

Interestingly, among accepted children, the exact date of birth of the child is more likely to be missing. We speculate that this could be a potential marker for illiteracy, given that heaping (rounding) in reports of age is correlated with illiteracy (see A'Hearn and Baten 2009 and references therein).[23] We test whether these characteristics jointly predict MP receipt, and conditional on receipt, duration or generosity of transfer by regressing an indicator for accepted status on child and family characteristics (Table 1) and find the same patterns.

To assess whether these differences in family characteristics correlate with differences in family income, we estimate the income of accepted and rejected MP applicants based on

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[21]The details of the maximum likelihood estimator are in the online Appendix. Programs (in STATA) and documentation available at http://www.econ.ucla.edu/alleras/research/programs.html.
[22]Others (Dale and Krueger 2002) have justified using rejected applicants as the counterfactual (in the context of college admission) by arguing that rejected applicants apply because they have good reason to believe, based on observables and unobservables, that they should be accepted.
[23]In Cook County, for instance, where "the court is very reluctant to pension any child of working age." Finally, single children are more likely to be rejected, also consistent with reports from Abbott (1934) and Goodwin (1992). In Cook County and Philadelphia women with only one child were systematically rejected (Abbott and Breckenridge 1921).

observable characteristics of the family using the 1915 Iowa census data (prior to the 1940 federal census, the only large-scale survey with information on both income and family characteristics was the 1915 Iowa state census). Specifically, we regress family income on the family characteristics we observe in the MP records (family size, age of all siblings, maternal marital status, length of family name). With these coefficient estimates, we then predict average income (in 1915) for accepted and rejected applicants based on their observable characteristics. We predict that on average accepted applicants had 7–9 percent lower family incomes than rejected applicants. Online Appendix Figure S2d shows the entire distribution of predicted family income. We find that children from accepted families are more likely to have predicted income (based on their characteristics) at or below zero and slightly lower predicted incomes when they are positive. Overall, the evidence shows there are in general small differences between the accepted and the rejected. The few statistically significant differences suggest that the accepted came from slightly worse-off families.

Our second exercise comparing the socioeconomic status of accepted and rejected children, involves matching two subsamples to census manuscripts in the years prior to MP application, allowing us to compare accepted and rejected applicants on pretreatment characteristics in the study including actual, not just predicted income. However, it should be noted that these measures are taken prior to application (when fathers are present) and not at the time of application when fathers are no longer present. As such, they are imperfect measures of family circumstances at the time of application. Despite this, the results of this comparison are still meaningful and suggest that on average, accepted boys come from poorer families.

We first matched Ohio boys to the 1900–1920 federal censuses.[24] We were able to match 822 boys from 358 families and found accepted and rejected boys at similar rates: 719 accepted and 99 rejected. We focus on Ohio because it is one of the largest states in our sample (39 percent of the sample) and because we were able to match a larger share to their death records. On average, accepted applicants were less likely to be native-born (87 percent versus 88 percent), less likely to own their home (41 percent versus 54 percent), and had slightly lower incomes imputed based on the occupation of the father ($517 versus $531), though none of the differences reaches statistical significance (Table 2B, panel A). We also present the distribution of imputed income by accepted status in Online Appendix Figure S4 where one observes that the distributions are very similar. These results confirm earlier findings that suggest that the accepted were in fact slightly worse off than the rejected in terms of resources.

We also match MP boys from Iowa to the Iowa 1915 state census which contains measures of family income, homeownership, home value, and paternal educational attainment (panel B of Table 2B). We were able to match 447 children from 257 families. Of those matched, 47 children were from rejected families. Not only is the sample relatively small, but we were unable to find accepted children at the same rate as rejected children, leading to potential

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[24]Iowa was chosen because its 1915 state census has detailed income data. We only matched Ohio boys to previous censuses because they constitute the largest subsample in our data. The lower match rate for the Ohio observations was the result of a linkage performed mechanically rather than by hand. The Iowa records were linked manually.

selection bias that we believe leads to positive selection of the accepted into this subsample. [25] Given this, these results should be interpreted with caution.

Even with these caveats, we still find that compared to rejected applicants, accepted applicants came from poorer families. In particular, accepted families had about 5 percent lower levels of income and substantially lower rates of homeownership (15 percent versus 38 percent). Conditional on homeownership, the homes of the accepted were worth less than half of the value of the homes of the rejected and there is no difference across accepted and rejected in the share of the home value that is mortgaged. We also compare the distributions of income and home value for the accepted and rejected (Online Appendix Figure S4) which appear very similar.

The only pretreatment characteristic on which the rejected appear worse off than the accepted is father's education. Finding that the accepted are better off on 1 characteristic out of 11 examined might be expected, especially given the small sample size. Upon further inspection, this finding is driven by a handful of rejected fathers with extremely low levels of schooling (Online Appendix Figure S4). A comparison of median schooling shows that accepted and rejected are equivalent (median schooling is 8 for both). Moreover, a number of families are missing paternal education but not income and the missing observations are nonrandomly distributed.[26] It is likely that were we not missing paternal education for some of our sample, the difference in average schooling across accepted and rejected would be smaller. In sum, based on our comparison of 11 pretreatment characteristics all of which except for 1 show that the accepted boys came from poorer families, we conclude that the accepted were poorer than the rejected.

Our third piece of evidence comes from the analysis of administrative records showing reasons for rejection or reasons for discontinuance among those accepted. The most common reason for rejection was insufficient need (35 percent). Marriage or remarriage is a common reason for rejection (8 percent), whereas ineligibility due to insufficient length of residency and noncitizenship appear to be very uncommon (Table 3). In Clay County, MN, where we have detailed information for all families, the most commonly reported reason for discontinuation of a pension was that the family was judged to be capable of self-support. Abbott and Beckenridge (1921) also report that in Cook County 60 percent (293 out of 532) of the rejected applicants were denied because of sufficient funds.

All three exercises support our assumption that differences between the groups are on average small and insignificant, or that the accepted are somewhat poorer. Given these results we proceed to look at whether the program impacted outcomes, under the assumption

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[25]For example, if we compare the predicted income of accepted and rejected children (based on observable characteristics of the family), there is no difference in predicted income of the accepted and rejected children whom we find in the 1915 Iowa state census, but among those whom we do not find, the accepted have lower predicted income than the rejected. In other words, the accepted who are missing from the sample are worse off on observables. Had we been able to include the accepted at rates equal to the rejected, we would likely find the accepted to be even more disadvantaged.

[26]Among rejected families, the average income of those with missing paternal education is relatively high at $1,197, suggesting that the (missing) education levels are likely high among the missing rejected. In contrast, among the accepted missing paternal education, the average income is relatively low at $533, consistent with low levels of (missing) paternal education among the accepted.

that mean comparisons between these groups will yield a lower bound on the effect of the program.

IV. Mortality Results

A. Preliminary Evidence

Accepted boys lived on average to age 72.4, nearly one year longer than rejected boys (Table 2A). Examining the full distribution of longevity, we observe that the distribution of the age at death of accepted applicants is shifted to the right of the distribution of rejected applicants (Figure 1). The distributions are statistically different at the five percent level. The largest differences are observed between ages 60 and 80, where the distributions are the densest.[27]

B. Main Results

Panel A of Table 4 shows the estimates for longevity estimated using the AFT hazard model in (1) based on the sample of individuals with a unique age at death. In column 1, we include only state and cohort dummies. In column 2, we add all individual controls, county characteristics in 1910 and state characteristics at the time of application. In column 3, we add county fixed effects. The results are not very sensitive to the inclusion of covariates. In column 4 we use the date of birth from the death certificate instead of the date on the MP application. The coefficient on acceptance is positive in all specifications and the implied effects are large: acceptance increased life expectancy by about a year, relative to a mean of 72.5, among the rejected. The estimates range from 1.1 to 1.3 years of life depending on the specification and sample.

To assess the role of attrition in this finding, we present estimates of the effect of the MP program on the probability of survival past ages 60, 70, and 80 (panel B of Table 4), by first assuming those without a match died prior to age 60 (columns 1–4), and then by dropping all unmatched records (column 5). We find statistically significant increases in the probability of survival past age 70 (ranging from 10 to 20 percent), and the probability of survival past age 80 (of about 9 to 15 percent). The results for survival past 60 are entirely driven by our assumption regarding attrition, whereas the results for ages 70 and 80 are not. These results are very similar if we use the date of birth from the death certificate instead of the one from the MP records (column 4), or limit the analysis to unique matches only (column 5).[28]

Next, we abandon the arbitrarily chosen cut-offs of ages 60, 70, and 80 and estimate our survival model using the fully saturated specification for each age at death between 58 and 88, which correspond to the tenth and ninetieth percentiles of the distribution of the age at

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[27]The age-at-death distribution for the rejected applicants in Figure 1, panel A, and in the following figures, is bi-modal with a pronounced peak around age 65, followed by a second peak closer to the peak of the distribution for the accepted applicants. It is possible that this is no more than the result of estimating a continuous distribution with a relatively small number of observations. Behncke (2012, p. 282), however, shows that retirement "worsens self-assessed health and an underlying health stock." If, as we expect, MP recipients are healthier than rejects, we might see more mass in the age-at-death distribution centered around retirement for rejects than for recipients, as the latter have a larger health stock to draw down before their mortality around retirement can be affected.

[28]These dates differ in less than 10 percent of our sample. However when the date of birth in the MP and DMF records differ, the DOB occurs somewhat later in the MP records, consistent with mothers underreporting the age of their children to increase length of eligibility. Reasons for discontinuation from Clay County, MN support this.

death. Figure 2 shows the marginal effects as a percentage of the survival rate of rejected applicants, computed using coefficients from estimation with (top panel) and without (bottom panel) imputing the missing observations as zeros. All coefficients are positive and significant after age 67, regardless of whether we impute missing values as 0 or not. Joint tests of statistical significance show that we cannot reject the null that all coefficients are positive.

C. Heterogeneity by Income and Urban Residence

In this section we explore heterogeneous effects of the MP program by family income, age of the child, and urban residence. Though we do not observe family income, we are able to predict family income for observations in all the states in our data based on observable demographic characteristics of the families and the estimated relationship between those characteristics and family income in the 1915 Iowa census.[29] After predicting income, we split the sample into low income (below median predicted income) and high income (above median predicted income) and compare accepted and rejected within these two broad income groups. In so doing, we further limit our comparison to accepted and rejected applicants who appear most similar in terms of resources (income) available to them. We also confirm that when we stratify the sample in this way, that the accepted and rejected within these subsamples are still similar on observables (Online Appendix Table S7c).

The effect of the MP program appears to be larger among the poorest in the sample (Figure 3).[30] For those predicted to have income below the median, acceptance increases longevity by 1.44 years, which is 15 percent higher than the effect for those above the median income, though the difference across the two subgroups is not statistically significant (Table 5).[31] Also note that the average age at death is higher for the sample with higher predicted family income in childhood, which suggests that our predicted income is indeed correctly classifying individuals into income categories, since family income is a well-known predictor of mortality.

Existing work on the importance of conditions in early childhood in determining later long-term outcomes (Almond and Currie 2011a) suggests that the effects of the MP program might decrease in the child's age. We split our sample into 3 groups by age of the child at the time of application: children younger than 5, children aged 5–9, and children aged 10–14. We find slightly larger, though not statistically significantly so, effects for younger children: children under 10 are 17–18 percent more likely to survive past age 70 than their rejected counterparts, relative to 11 percent for children aged 10–14.

A criticism of the MP program was its reliance on counties as the main administrative unit. This resulted in wide variance across counties in the implementation of the program. In particular, the historical record suggests that rural and urban counties implemented the programs differently.[32] When we split the sample into counties above and below the median

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[29]Number of children, age and gender of children, marital status of mother are the characteristics we can use to predict family income.
[30]Figure 3 presents unconditional age-at-death distributions: simply splitting the sample and showing the distributions.
[31]Table 5 presents conditional effects: splitting the sample and estimating the same regression as in Table 4.

share urban, we find no significant differences in the magnitudes: the effects are positive and similar in magnitude in both samples (Figure 3 and Table 5).

D. Discrimination and the Composition of Rejected Applicants

If rejected mothers were subject to discrimination on the basis of unobservable characteristics that are negatively correlated with child outcomes, this could threaten our identification strategy. We consider the two most likely sources of discrimination: race and nativity. With respect to discrimination on the basis of race, in a 1931 survey and analysis of the MP program, the US Department of Labor determined that 96 percent of MP recipients were white despite the fact that black mothers were at least as likely to be in need, consistent with other accounts (Goodwin 1992 and citations therein, Ward 2005). To address this, we link a subset of the MP records to WWII enlistment data and 1940 census data that contain race (Tables 6 and 7). This is necessary because MP records do not report the race of applicants. We find that blacks are not more likely to have been rejected. We also present results for Ohio where the Department of Labor reported a lack of discrimination against black mothers in its 1931 report. The results are unchanged when we limit the analysis to the Ohio sample (Online Appendix Table S7a).

Similarly, discrimination against immigrants could result in their disproportionate representation among rejected applicants, threatening identification and biasing our results in favor of estimating positive effects of the MP program. The historical record on this, however, is more mixed with some reports of discrimination against mothers on the basis of nativity and other reports of no discrimination.[33] We present two pieces of evidence suggesting that discrimination on the basis of nativity is not biasing our results. First, in the subsample in which we matched individuals to pre-application censuses, we find that in fact immigrants were not less likely to receive pensions (Table 2B). We also split the sample by fraction foreign born in the county and find similar effects of MP receipt on age at death in samples with a high and low fraction of immigrants (Table 5). As a final effort to control for underlying differences between the accepted and rejects, we match accepted and rejected on propensity scores. The results are unchanged (Online Appendix Table S7a).

We consider additional sources of differences between accepted and rejected mothers: age of the child (older children more likely to be rejected) and number of children (very small and very large families more likely to be rejected). When we drop individuals over the age of 14 (the maximum age in most states) or over age 10, our results are unaffected (Table 5).[34] Likewise, if we drop boys from extremely small or large families, we obtain similar

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[32]Rural counties, in particular, were criticized at the time; many were underfunded (Abbott 1933), they did not have bureaucracies in place to administer the programs, and lacked records. Some of the rural programs were also seen as applying laws loosely and providing no proper supervision of recipients. A few studies on the other hand suggest that rural counties were more generous to unwed deserted or divorced women, and to immigrants. See Leff (1973); Ladd-Taylor (1994); Skocpol (1992); Goodwin (1991); Davis (1930); Carstens (1913); Lundberg (1921); Abbott (1917); Abbott and Breckinridge (1921); and Mudgett (1924) for examples.

[33]For example in Cook County, Minneapolis and St. Paul (Mudgett 1924), immigrants accounted for about half of the recipients; Goodwin reports that "German, Italian, Irish and Polish families received pensions in numbers greater than their representation during the 1920s." However, there is some evidence that (more recent) immigrants from southern Europe and eastern Europe were more likely to be discriminated against than other immigrants: a US Department of Labor, Children's Bureau (1922a, b) report states than Italian and Czechoslovakian families were granted lower allowances than other groups. Ward (2005) states that Mexicans, Italians, and Czechoslovakians were less likely to be helped in the city of Chicago.

[34]Among these children, we find no significant differences in the effects of the program.

estimates. Finally, we reestimate the model dropping individuals born after 1920, since the life table calculations suggest a nontrivial portion of them might still be alive in 2012. This makes no difference. The estimates are very similar when we look at the early cohorts (1900–1910) or later cohorts (1911–1920).

E. Aggregate Results

We estimate models with data aggregated at the level of the county, year of application, and year of birth. In these models, we regress the fraction surviving past age 70 (or the average age at death) on the fraction accepted. Whether we use weights or include controls, we find positive effects. These effects (Online Appendix Table S7b) are similar in magnitude to those we estimate using the individual data: between a 13 and a 25 percent increase in survival past age 70 or about 1. 2–1.9 additional years of life. These results rule out the possibility that counties with high rejection rates are driving the results.

F. Results for Ohio

We present separate estimates for the state of Ohio—the state from which 34 percent of individuals in our sample originate—for several reasons. First, Ohio and Pennsylvania were the only two states where black mothers appear to have received MP benefits at expected rates given their share in the population (as identified by the Children's Bureau in their 1933 report), which makes it unlikely that discrimination against blacks is driving our results. Second, the same Children's Bureau report (1933) indicates that applicants from Ohio were not required to be US citizens, which suggests that Ohio lawmakers were not advocating the exclusion of immigrants from the MP program. Lastly, we collected county-level expenditures on social programs over time from the Ohio General Statistics (available for 1915–1922) to control for other sources of support at the time of application, so as to rule out the possibility that higher rejection rates maybe correlated with a greater safety net, potentially biasing our estimates.

Figure 4, which depicts the densities of the age at death, shows that in Ohio, rejected boys lived shorter lives than accepted boys. The estimates in Online Appendix Table S7a, with and without controls, show that the longevity effects are essentially unchanged, though perhaps slightly larger for the Ohio sample. This suggests that neither discrimination against blacks and nonnative mothers nor the availability of other social programs are biasing the results.

G. Attrition and Multiple Matches

We are more likely to match accepted applicants to their death certificates (55 percent) than we are to match rejected applicants (49 percent). This 10 percent differential matching rate (Table 2A) is *not* eliminated when we control for observable characteristics (Figure 5). This difference in the match rate is consistent with the MP program improving health and lowering mortality at all ages. Alternatively, if rejected boys have a lower match rate for other reasons, this could bias our estimates.

We present several pieces of evidence that support differential attrition due to mortality rather than other factors. Based on the life tables for the 1910 birth cohort in the United

States, we compute that a 10 percent reduction in adult mortality from age 40 onward for the 1910 cohort is equivalent to an increase in longevity of approximately 0.9 years. Thus, the magnitude of the attrition is consistent with the magnitude of the mortality declines we estimate. In addition, when we matched applicants to retrospective pre-application records (as in Table 2B), we find that the match rates of both the accepted and rejected applicants are the same, which rules out the concern that differences in the names across the two groups cause the differential matching. Lastly, when we find more than one possible match, we do so at identical rates across accepted and rejected applicants (Figure 5), which is consistent with mortality, rather than other factors, affecting attrition rates.

For the state of Ohio we collected additional data on deaths prior to 1975, by matching MP records with the state death records that date back to 1958, and by manually searching for death certificates for unmatched children of Ohio MP applicants on Ancestry.com. In so doing, we increased our match rate to 60 percent.[35] When we add these additional matches (Online Appendix Table S7a, panel D), our results remain unchanged. Among the newly matched, the mortality differential between accepted and rejected is again about one year (66.3 versus 67.2 age at death), which is exactly what we estimate using the original data. For this newly matched sample, we continue to find death records for accepted applicants at higher rates. Even when we push mortality comparisons back to 1958, the rejected die younger than the accepted, which suggests that our inability to link rejected MP applicants to death records prior to the mid-1970s is in fact related to their higher mortality.

This newly matched sample also sheds light on why our results are small and imprecise when the dependent variable is the probability of survival to age 60. Approximately 60 percent of the newly found death records show that children of MP recipients did not survive to age 70, but only about 30 percent of children died before age 60. We conclude that the assumption that the missing are dead is reasonable for specifications in which the dependent variable is the probability of survival past 70 but not when it is survival to ages younger than 70.

Finally, we consider cases of multiple matches which represent 4 percent of our sample. The results are similar when we estimate standard logit models using unique matches, or when we use the maximum likelihood methods developed in Bugni, Honoré, and Lleras-Muney (2014) to include multiple matches. The results are also not sensitive to the exclusion of observations with more than three matches, or choosing the highest quality match for those with multiple matches (online Appendix II). Although the coefficients differ in magnitude when we change the sample, the marginal effects remain similar across all specifications (Online Appendix Table S7a).

H. Alternative Counterfactuals From the 1900–1930 Censuses

For comparison, we constructed two alternative "control" groups from the 1900, 1910, 1920, and 1930 censuses and matched them with their death records in the DMF (see online

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[35]The Ohio data on Ancestry.com includes deaths from WWII and other wars, as well as other death records (e.g., cemetery listings). Another way to increase the rate at which we find matches is to loosen the matching criteria. This results in a substantially higher number of matches, but also increases the number of individuals with multiple matches and therefore measurement error so it is not our preferred method of addressing this issue. Nevertheless the results using this data are very similar.

Appendix III for details).[36] The first alternative counterfactual group is orphans, who are identified as children living in institutions in the census. Since MP programs were developed in large part to prevent the institutionalization of children in orphanages and instead allow children to remain at home with their mothers, orphans represent an appropriate historical counterfactual. We find that the orphans are very similar to the rejected applicants with regard to longevity, with both living shorter lives than accepted applicants (Figure 6, panel A).

The second counterfactual group is comprised of children of single or divorced women, who were drawn from the census records in states where these women were not eligible for the MP program. We compare children of this group of ineligible women to accepted MP children whose fathers were disabled or institutionalized (but not to children of widows) because, on observables, these children appear similar.[37] We find that children of accepted women lived longer than this alternative control group of ineligible children of single and divorced women (Figure 6, panel B).

V. Results for Educational Attainment, Health, and Income in the Medium-Term

To understand the ways in which income transfers in childhood improved longevity, we explore potential mechanisms. Previous work has shown education, income, and weight (being underweight, in particular) are strongly associated with mortality (Flegal et al. 2005; Deaton and Paxson 2001; Cutler and Lleras-Muney 2008). In this section we estimate the effect of the MP program on these possible medium-term (adulthood) outcomes by linking the MP sample to 1940 census and WWII enlistment records.

A. 1940 Census Records

Matching MP applicants to 1940 census data allows us to examine the impact of MP receipt on educational attainment and income during young adulthood. Rejected boys are much more likely to have not started high school and accepted boys are more likely to have graduated high school (Figure 7, panel A). Results from regression analysis with full sets of controls shows that MP receipt results in between 0.3 and 0.4 more years of schooling, though we lose precision when we add the full set of controls.

With respect to income, we observe that the distribution of log income is shifted to the right for accepted boys (Figure 7, panel B). Results from a regression with full controls show that MP recipients on average have incomes that are 14 percent higher than their rejected counterparts in 1940 (Table 6). Online Appendix Figure S5 shows that MP receipt is associated with a positive increase in the chance of being in the twenty-fifth, fiftieth, or seventy-fifth percentile of the distribution in the sample, but most of these estimates are not statistically significant.

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[36]For this analysis we also include data on MP recipients in Colorado and Connecticut. These two states were excluded from the previous analysis because we do not have information on rejected applicants for these two states.
[37]If we include MP widows, the results are even larger.

B. World War II Enlistment Records

We can match individuals in the MP records to their WWII enlistment records for the cohort that enlisted in the Army during 1938–1946.[38] For all enlistees, we observe educational attainment as well as two anthropometric measures (weight and height), which are markers of nutritional deprivation in childhood. Adult height, in particular, has been linked with childhood nutrition, as well as adult cognitive ability and labor market outcomes (Case and Paxson 2008).

The WWII results should be viewed as suggestive, rather than definitive, for two reasons. First, our match rate is low (17.2 percent of males overall, 32.9 percent for those born 1919–1925), lower than our match rate for mortality. This is because WWII records do not contain exact date-of-birth. They do however contain state-of-birth, which we add to our matching criteria. Second, the WWII records are a selected subset of the male population because of induction rules and exemptions. As a result, our matched sample is younger given that males aged 18–25 in 1942 served at much higher rates than older men (Hogan 1981), healthier given the mental and physical requirements for enlistment, and more highly educated given minimum schooling requirements for enlistment of 8 years.[39]

We estimate the effect of MP transfers on the fraction with more than eight years of school (Figure 7), or using a censored regression for years of schooling (Table 7). We find that children of accepted families are 20 percent more likely to have more than eight years of school (Table 7, column 1 without controls and column 2 with controls). When we estimate a censored model that accounts for the two sources of censoring, we find that MP recipients complete a third of a year more school than rejected applicants, and the effect is marginally significant. When we include the full set of controls, the point estimates remain similar, but are no longer precisely estimated. These results are very similar to the results from the 1940 census records (Table 6).

MP receipt also significantly reduces the probability of being underweight (Figure 8). Recall that in the detailed MP records from Clay County, MN, nurses noted malnutrition as one of the most commonly observed health problems during visits to families in the MP program. Estimates in Table 7 imply a statistically significant 50 percent reduction in the probability of being underweight, with similar results but less precision with the full set of controls. The estimates for height, weight and BMI (measured continuously) are also positive and significant for weight and BMI. Our results showing greater impact of MP receipt at the lower tail of the distribution of weight are consistent with our earlier finding that the effects of cash transfers on mortality were greatest among the most disadvantaged families. We conclude that the transfers helped families improve the nutrition of their children, particularly for those at greatest risk of malnutrition.

[38]Enlistment records are available for 9 million (of the 16.5 million) individuals who served in WWII.
[39]Consistent with this positive selection into the WWII records, we find accepted applicants at higher rates than rejected applicants. Results available upon request.

C. Magnitudes

We find that receipt of MP transfers resulted in a significant 50 percent decrease in under-nutrition, a 13 percent increase in income, and an increase of 0.4 years of school among young adults. Would these effects result in the longevity gains that we estimate? Being underweight in adulthood is associated with a relative risk of mortality that ranges from 1.38 to 2.3 (Flegal et al. 2005). This is a large effect but since only 10 percent of our sample is underweight, the expected increase in longevity through this channel would be small. However, income and education likely play a larger role in explaining our mortality results. Based on Deaton and Paxson's (2001) estimate of the long-term elasticity of mortality with respect to income (−0.3 to −0.6), a 30 percent increase in income would lower mortality by at least 10 percent, increasing longevity by 0.9 years.[40] Cutler and Lleras-Muney (2008) report that an increase in schooling of 0.25 years is associated with a 0.15 year increase in longevity in OLS regressions. We conclude that the estimated effects of the MP transfer on education and income would imply at least one additional year of longevity, which is consistent with our estimated effects on longevity (1.1–1.4 years). These two mechanisms alone explain 75–95 percent of the increase in life expectancy associated with MP transfers.

Our mortality results are also consistent with the estimated short-run effects of other cash programs on mortality. Conditional cash transfers in Mexico, which are estimated to account for about 30 percent of pre-transfer income, decrease short-run mortality by about 4 percent for the elderly, and by about 17 percent among infants, with effects up to 30 percent for the poorest families (Barham 2011; Barham and Rowberry 2013). A decrease in mortality of 5 (10) percent throughout the lifetime would increase life expectancy for the 1910 cohort by about 0.5 (1) years. Thus our results are consistent with a 10 percent decline in adult mortality resulting from a 20–30 percent increase in childhood income.

The estimated effects of income may be underestimated. In a period of high infectious disease, such as the early twentieth century, improvements in nutrition are likely to lower the spread of disease; if this is the case, the rejected children might have benefited from the transfers leading us to understate the total (i.e., to society) benefits of the transfers. Indeed Fishback, Haines, and Kantor (2007) show relief monies during the 1930s lowered infant mortality for all children. This seems unlikely in our case because the MP program covered a very small share of the poor, so spillovers would be minimal.

We could also be overestimating the increase in income associated with the transfers. For instance, if rejected families get other relief, then we overestimate the increase in income and understate the effects of income. The availability of non-MP benefits and their provision to rejected MP applicants would narrow the "true" gap between resources available in rejected and accepted households. If we observe an effect size of X per unit of MP but each unit of MP provided resulted in a resource differential of only $\alpha X (\alpha < 1)$, then the "true" effect of MP in the absence of other benefits available to rejected families would, to a first approximation, be X/α. There is scant evidence on this, but for example Abbott and

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[40]We calculate that a 10 percent reduction in adult mortality from age 40 onward for the 1910 cohort is equivalent to an increase in longevity of about 0.9 years. For this calculation it is not clear how to average the 30 percent increase in family income during childhood and the 13 percent increase in adulthood. We used 30 percent increase in income and the −0.3 lower elasticity

Beckenridge (1921) report that families who were dropped from the MP program in Cook County in 1913 subsequently received charity in a smaller amount.[41]

A final interpretation question relates to whether our estimates apply to the poor in the United States today. On the one hand our effects are likely to be larger than in the current United States because life expectancy rises much faster with income at lower levels of income (Preston 1975). On the other hand, the effect of education and income on mortality today may be larger due to the availability of new technologies. However because our short-term estimates are comparable to those found in contemporary studies, it is reasonable to predict a similar impact over the longer term.

VI. Interpretation and Policy Relevance

This is the first study to document that cash transfers to mothers of poor children substantially increase children's longevity. Additionally, we find that underlying nutrition, educational attainment, and income in adulthood are all likely mediating factors. Our results rely crucially on our comparison of families that were deemed eligible and applied but were rejected upon investigation. The evidence from our records and from the subsidiary data we collected shows that on average rejected children were from slightly better-off families in terms of resources, and thus our findings represent lower bounds on the effects of transfers. The implied elasticity of adult mortality rates with respect to family income in childhood is roughly −0.33 to −0.5.

While conditions today differ significantly from those at the beginning of the twentieth century, three important similarities remain. Then and now, women raising children alone (whether divorced, unmarried, widowed, abandoned, etc.) represent the most impoverished families. In fact, the income gap between children in two-parent versus single-mother families has only grown over time (Online Appendix Table S9). Secondly, the relationship between parental income and the development of child human capital is similar in these two periods. Using census data from 1915, 1940, 1960, 1980, and 2010, we estimate the relationship between log(real family income) and child grade in school for all children ages 7–14 (Online Appendix Table S10). The relationship between parental income and this measure of child human capital in 2010 is remarkably similar to that found in 1915.[42] On the other hand, today poor women with children are eligible for other programs, thus it is possible cash transfers today would be differently spent than cash transfers in the past.

Finally, our estimated short and medium-term effects are consistent with estimates of the impact of contemporary anti-poverty programs on short- and medium-term outcomes. Recent work in the United States has found positive effects of food stamps on pregnancy outcomes (Almond, Hoynes, and Schanzenbach 2011) and adult obesity (Hoynes,

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[41]In work on New Deal relief programs, Stoian and Fishback (2010) and Wallis and Benjamin (1981) show how these programs generally aimed to supplement family income up to some target level, rather than at the full amount specified by the program guidelines. This was said at the time to have been the same procedure that relief programs in the 1920s had used. If so, the actual amounts paid out in MP benefits may have been less than the stated maximum for each state's program, and we have underestimated the per dollar effect of MP benefits by focusing on the stated maximum benefit payable in each state.

[42]Consistent with this, Dow and Rehkopf (2010) estimate that the relationship between income and mortality was high at the beginning of the twentieth century, subsequently declined over the course of the middle of the century, but has risen steadily since then.

Schanzenbach, and Almond 2012), as well as positive effects of cash transfers through the tax code in the United States and Canada on child cognitive achievement and health (Dahl and Lochner 2012; Milligan and Stabile 2008).[43] Likewise, in developing countries, there have been numerous evaluations (often based on randomized controlled trials) of conditional cash transfer (CCT) programs, which require participants to enroll their children in school, get regular checkups, etc. as a condition of receipt. These CCTs are estimated to have significant short-run effects on such outcomes as infant mortality and school enrollment (Barham 2011; Barham and Rowberry 2013), but there is still uncertainty about their long-term effects on learning, total years of education, wages, or anthropometric outcomes. Our results suggest that the short- and medium-term improvements observed in these contemporary programs are likely to generate large longevity gains for the recipients.

Recent theoretical and empirical work (Cunha and Heckman 2007; Heckman 2007) on the development of human capital emphasizes the importance of conditions in early childhood in determining long-term outcomes. Evidence from a randomized trial with primates shows that deprivation in early life has large effects on long-term health (Conti et al. 2012). Bleakley (2007) estimates large effects of a public health deworming campaign in the American South on children's educational outcomes and their adult income. Even earlier, prenatal conditions have long-term consequences for children's health and on socioeconomic outcomes (Barker 1995; Almond 2006).

None of these studies addresses whether cash transfers can alleviate adverse early-life shocks and improve lifetime outcomes. Current aid to poor women takes the form of in-kind and cash transfers, with the United States generally favoring in-kind transfers. Proponents of in-kind transfers argue that cash transfers may not encourage consumption of goods and services that benefit children (Currie and Gahvari 2008). In addition, welfare receipt can be stigmatizing and can create incentives for parents to modify their behavior in order to remain eligible for program benefits by, for example, remaining unmarried or out of the labor force, or by having more children (Moffitt 1992; Kearney 2004). On the other hand, cash transfers have the advantage of being less costly to deliver and of not constraining recipient consumption choices, allowing families to respond to unforeseen shocks as necessary. Overall, our findings suggest the net effect of cash transfers on longevity is positive. Whether cash transfers are more or less cost-effective than in-kind transfers or conditional cash transfers is an important question for future research.

Acknowledgments

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[43]Akee et al. (2010) also find that a government cash transfer to families on American Indian reservations are associated with improved medium-term outcomes including educational attainment and criminal activity.

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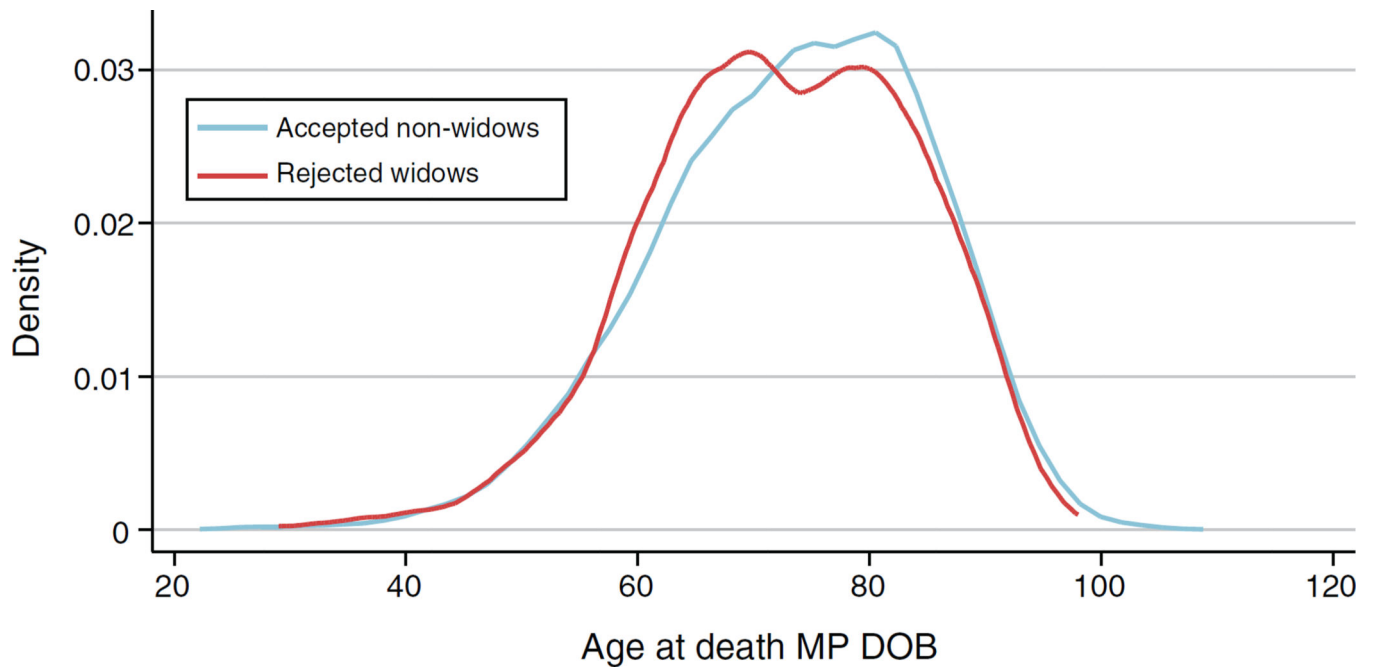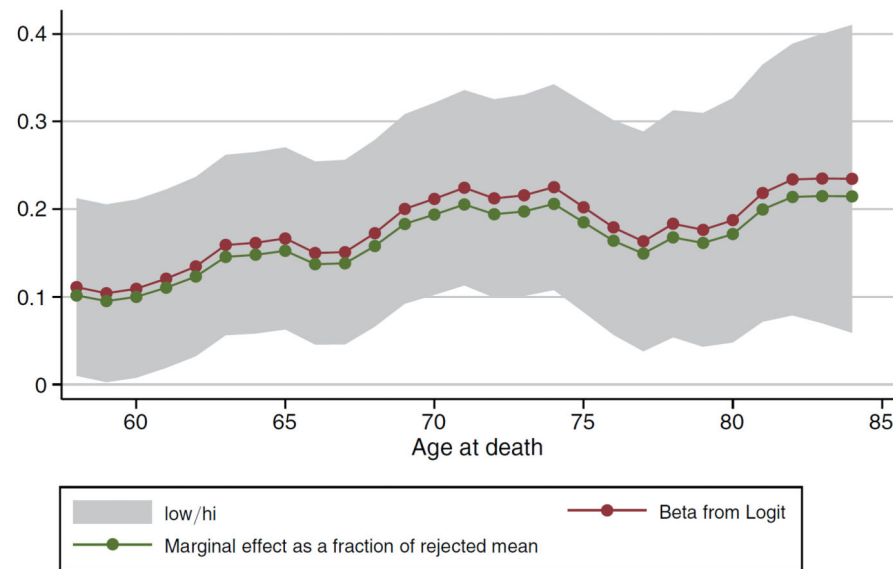**Figure 1.**
Distribution of Age at Death

Notes: Figure based on matched sample of boys of accepted and rejected applicants. We reject the null that the distributions are the same using a Wilcoxon rank-sum test. This figure includes only those with unique matches to age certificates. Deaths below 20 dropped.

Panel A. Probability of surviving past given age (full sample, missing age at death assumed dead)

Panel B. Probability of surviving past given age (matched sample; unique matches only)
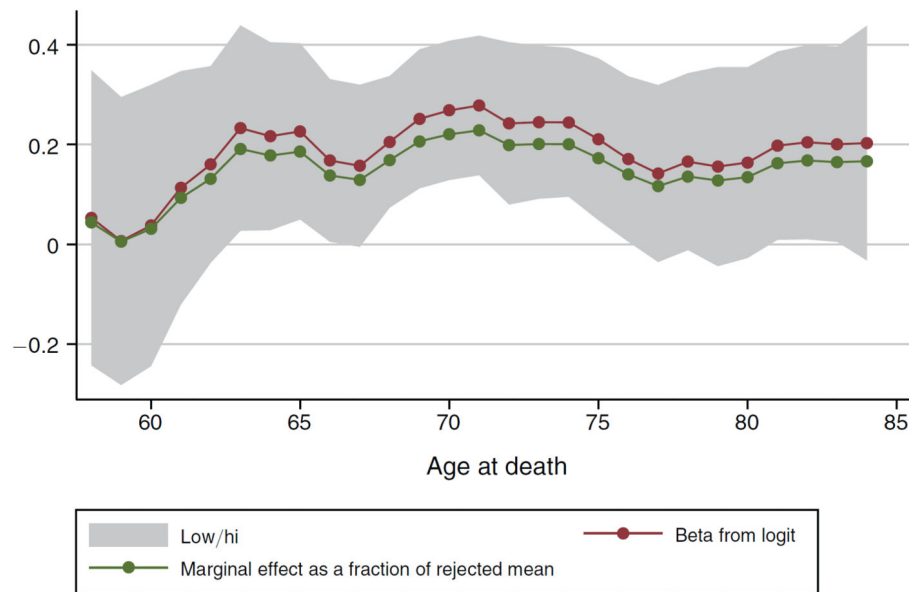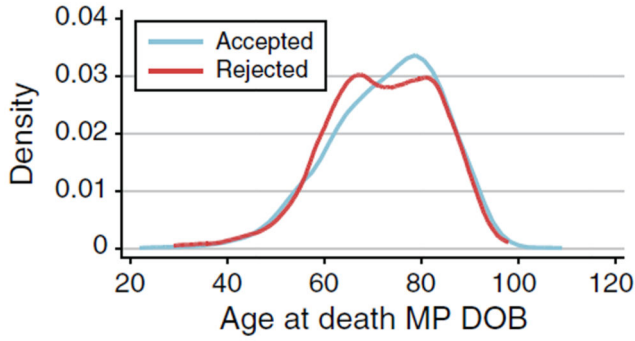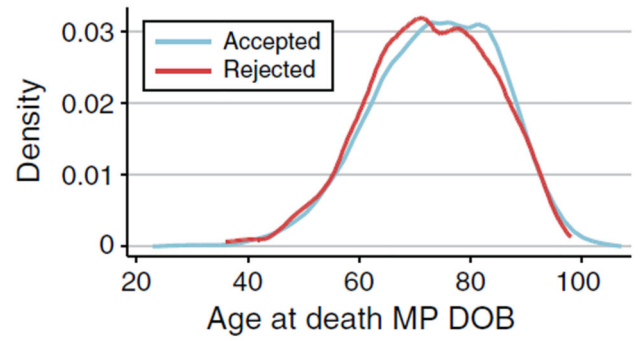

Figure 2.
Effects by Age

Notes: The coefficients are jointly significantly different from zero ($p > 0.001$). All specifications use the full set of controls as in column 4 of Table 4.
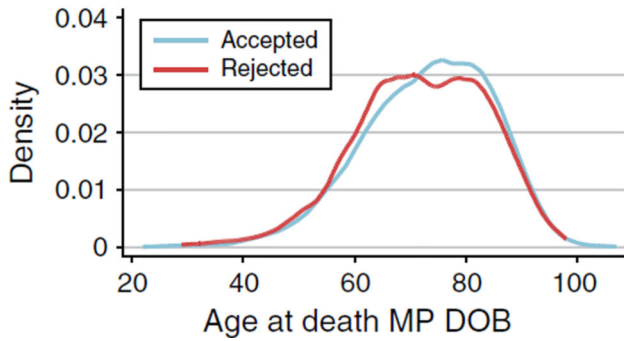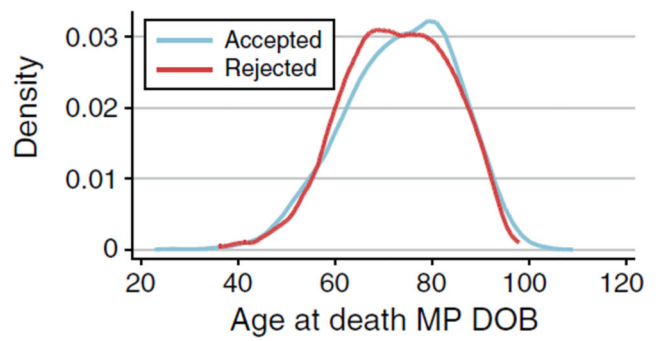
Figure 3.
Estimates by Predicted Family Income and Urbanicity (*Unique Matches Only*)
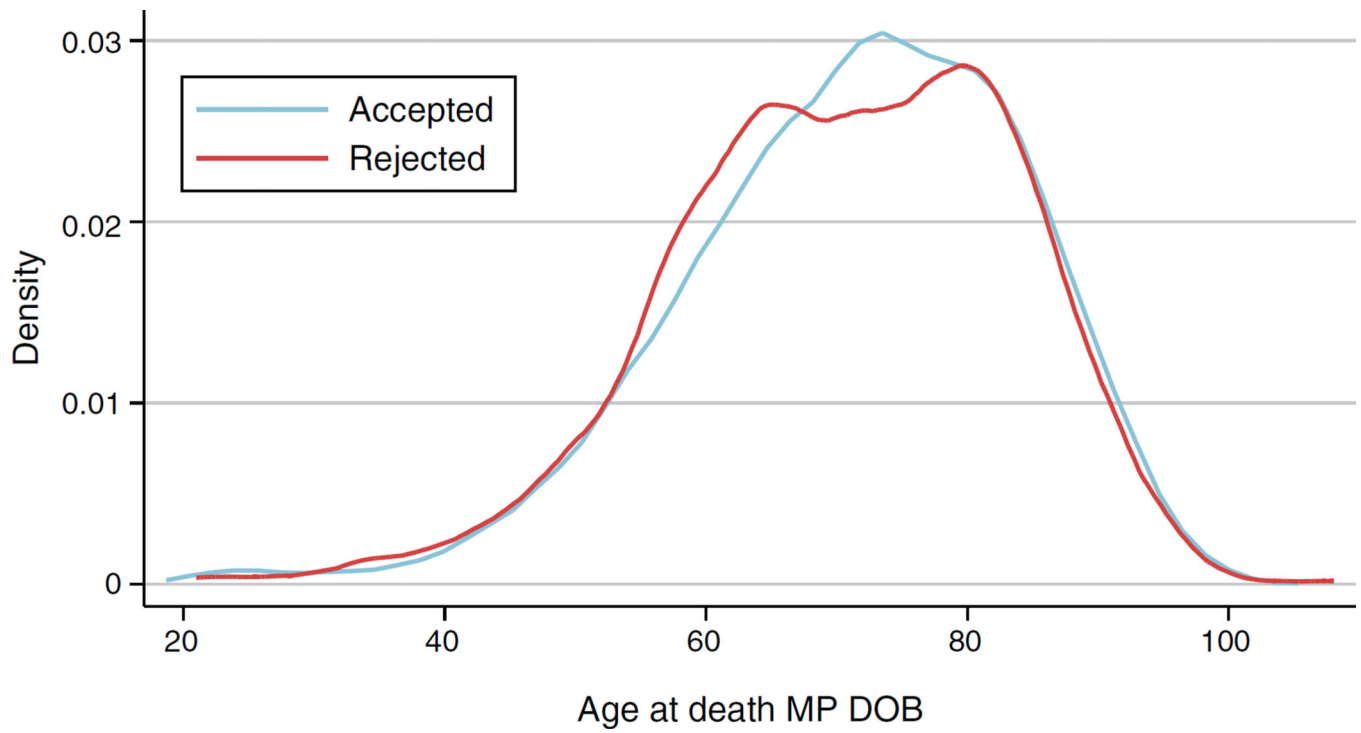
Figure 4.
Ohio, Matching to Additional Death Records
Notes: Records matched first to Death Mortality Files (DMF) and then to Ohio state death records that go back to 1958. Remaining unmatched records were then manually imputed by searching individual records in Ancestry.com. Unique matches only.

Panel A. Does acceptance predict missing age at death?
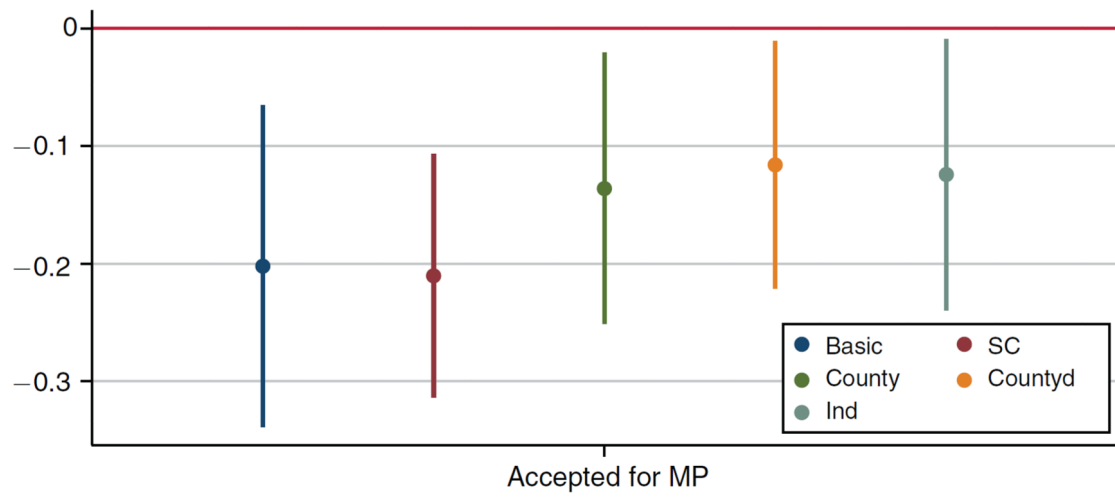
Panel B. Does acceptance predict multiple matches?
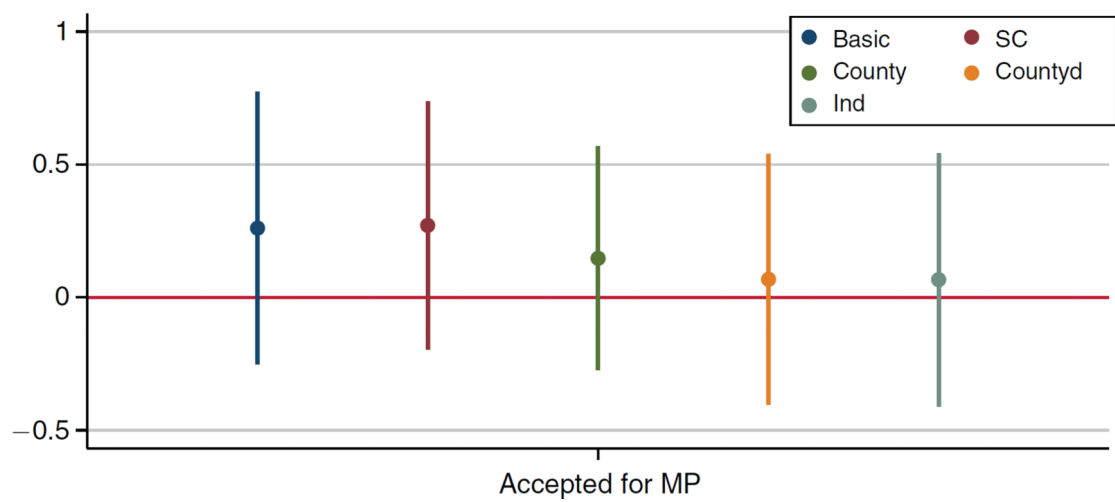
Figure 5.
Differential Attrition and Matching
Notes: coefficient from logit on Accepted = 1. Graphs display coefficients and 95 percent confidence intervals.

Panel A. Age at death: MP boys and census orphans
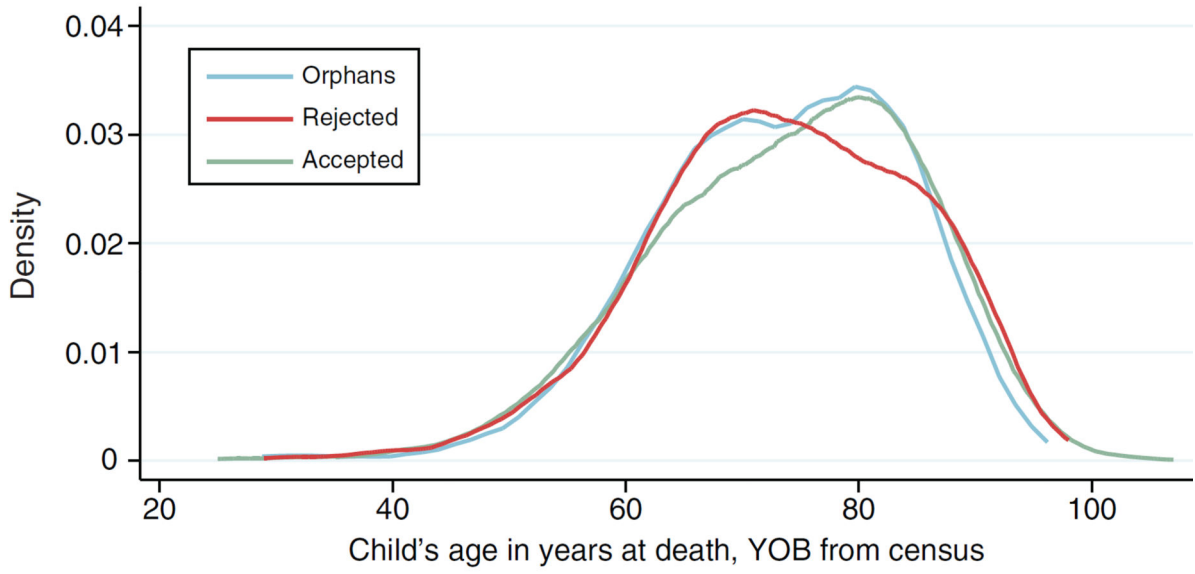
Panel B. Age at death: Children of poor non-widows
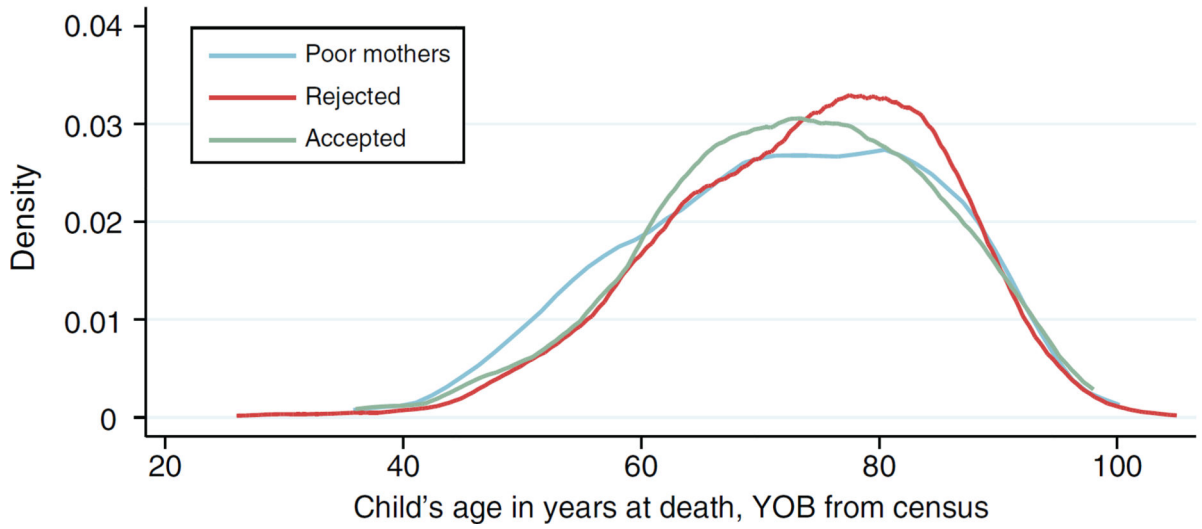
Figure 6.
Alternative Counterfactuals

Notes: Panel A: Unique matches only. Orphans defined as children living in institutions.
Panel B: From ln states where divorced/abandoned mothers are ineligible: unique matches
only. We exclude CO, MN, OH, and WI.

Panel A. Distribution of log income 1940 census

Kernel = epanechnikov, bandwidth = 0.1668
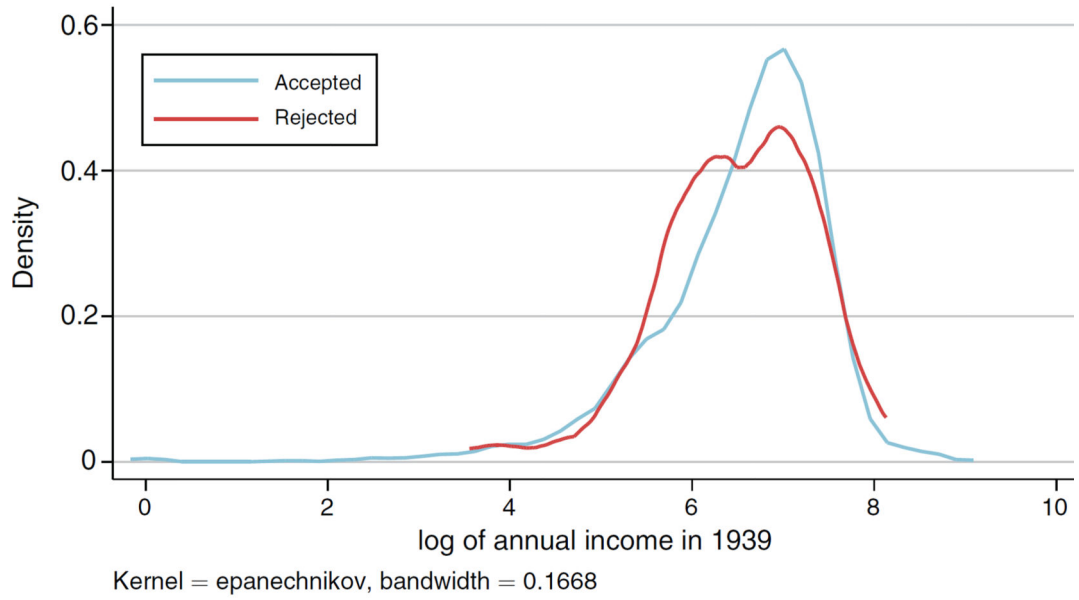
Panel B. Education distribution in 1940 census

Figure 7.
Effect of MP on Outcomes from 1949 Census Records

Panel A. Education distribution in WWII records

Panel B. Distribution of BMI in WWII records

Figure 8.
Effect of MP on Outcomes from WWII Records
Note: Panel B: Graph from unique matches only.

Table 1

Determinants of Acceptance and Generosity of Transfers

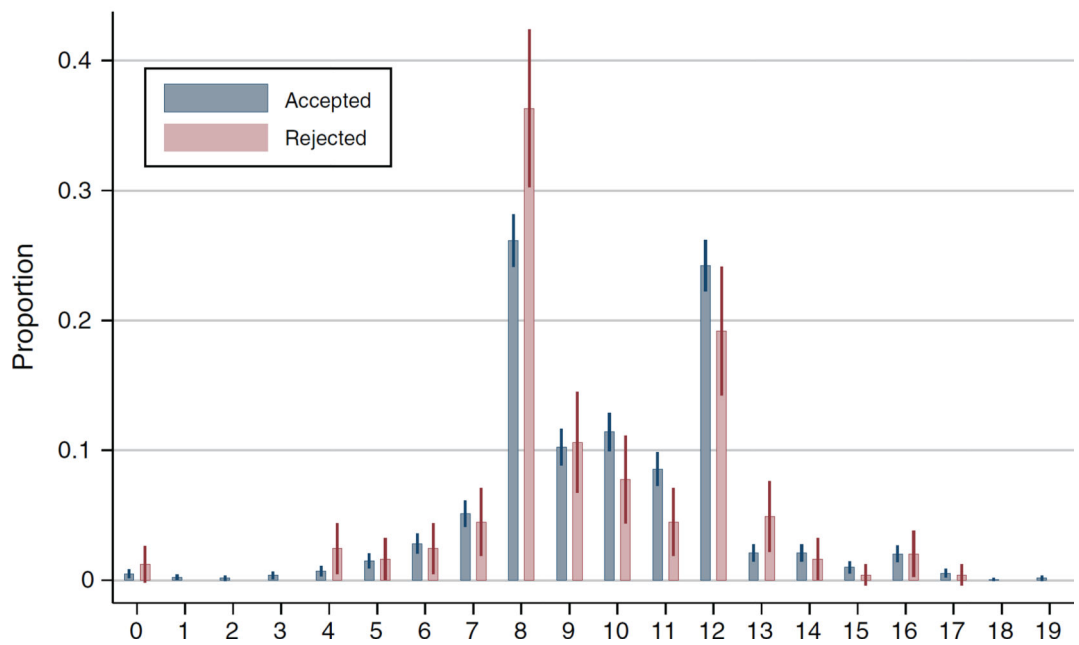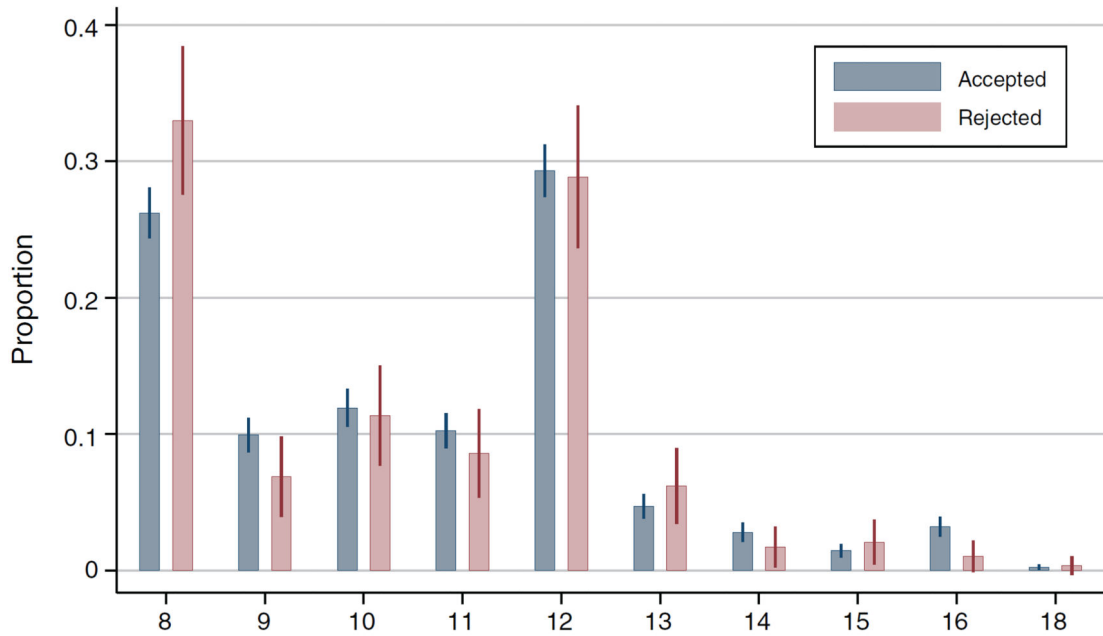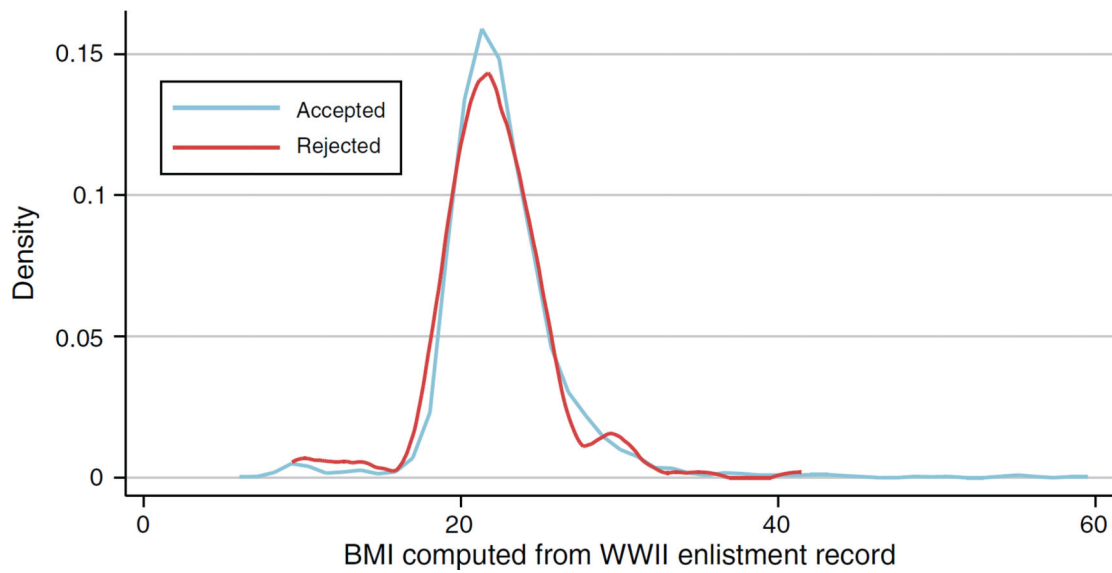|	Full sample			Matched sample		
	Accepted	ln(amount)	ln(duration)	Accepted	ln(amount)	ln(duration)
Child age (years)	−0.00324 [0.005]	0.00841 [0.008]	−0.0146 [0.011]	−0.00277 [0.004]	0.00177 [0.007]	−0.0139 [0.017]
Length of family name	0.00176 [0.002]	−0.000433 [0.003]	0.00455 [0.008]	−0.000319 [0.003]	0.00240 [0.003]	0.00779 [0.010]
Family size = 2	0.0632 *** [0.012]	0.265 *** [0.025]	0.268 *** [0.063]	0.0349 ** [0.017]	0.273 *** [0.033]	0.331 *** [0.061]
Family size = 3	0.0732 *** [0.014]	0.479 *** [0.034]	0.413 *** [0.064]	0.0511 *** [0.019]	0.491 *** [0.042]	0.511 *** [0.062]
Family size = 4	0.0887 *** [0.020]	0.605 *** [0.046]	0.436 *** [0.063]	0.0624 *** [0.024]	0.624 *** [0.055]	0.545 *** [0.061]
Family size = 5	0.0930 *** [0.026]	0.748 *** [0.052]	0.524 *** [0.083]	0.0570 * [0.032]	0.755 *** [0.061]	0.578 *** [0.092]
Family size = 6	0.0917 *** [0.022]	0.804 *** [0.064]	0.542 *** [0.108]	0.0626 ** [0.030]	0.807 *** [0.078]	0.588 *** [0.120]
Family size = 7	0.102 *** [0.031]	0.946 *** [0.071]	0.478 *** [0.125]	0.0654 * [0.038]	0.922 *** [0.093]	0.573 *** [0.120]
Family size = 8 or more	0.0587 * [0.034]	0.977 *** [0.069]	0.516 *** [0.126]	0.0704 [0.046]	0.975 *** [0.077]	0.586 *** [0.125]
Age of oldest kid in family	−0.00177 [0.002]	−0.00864 *** [0.003]	0.000259 [0.007]	−0.00129 [0.002]	−0.00547 * [0.003]	−0.00665 [0.006]
Age of youngest kid in family	−0.00247 [0.002]	−0.0132 * [0.003]	−0.0207 * [0.007]	−0.00258 [0.002]	−0.0133 * [0.003]	−0.0178 ** [0.007]
Divorced	−0.0566 * [0.032]	0.000120 [0.021]	−0.100 * [0.036]	−0.0415 [0.034]	0.00865 [0.028]	0.0148 [0.030]
Husband abandoned/prison/hospital	−0.00639 [0.009]	0.000555 [0.015]	−0.116 * [0.026]	−0.00448 [0.010]	−0.00651 [0.017]	−0.0815 * [0.026]
Mother's marital status unknown	−0.0938 [0.061]	−0.0365 [0.032]	0.00975 [0.086]	−0.116 [0.076]	−0.0333 [0.038]	0.0764 [0.065]
Day or month of birth missing	−0.0210 [0.033]	0.0488 [0.038]	0.393 * [0.197]	0.0399 [0.039]	0.0433 [0.065]	0.356 * [0.211]
Observations	16,068	13,787	6,868	7,859	6,820	3,677
Mean	0.865	5.509	1.306	0.875	5.508	1.340

Note: Robust standard errors in brackets.

***
 significant at the 1 percent level.

**
 significant at the 5 percent level.

*
 significant at the 10 percent level.

Table 2

A—Summary Statistics for Estimation Samples

	Full sample		Sample matched to unique age at death	
	Mean rejected	Difference (accepted–rejected)	Mean rejected	Difference (accepted–rejected)
Panel A. Individual characteristics				
Year of application	1,920.81	0.87 [0.694]	1,921.00	0.91 [0.779]
Year of birth of child	1,912.05	1.397** [0.693]	1,912.16	1.43* [0.766]
Child age (years)	8.74	−0.508*** [0.124]	8.827	−0.519*** [0.155]
Day or month of birth missing	0.02	0.014 [0.011]	0.007	0.011* [0.006]
Number of children in family	3.598	0.171 [0.133]	3.538	0.193 [0.157]
Age of oldest child in family	11.868	−0.38 [0.246]	11.861	−0.395 [0.261]
Age of youngest child in family	5.623	−0.799*** [0.170]	5.63	−0.775*** [0.187]
Length of family name	6.385	0.06 [0.054]	6.345	0.015 [0.078]
Widow	0.512	0.023 [0.041]	0.532	0.017 [0.045]
Divorced	0.034	−0.005 [0.011]	0.03	0.001 [0.014]
Husband abandoned/prison/hospital	0.178	0.007 [0.024]	0.171	0.017 [0.025]
Mother's marital status unknown	0.277	−0.026 [0.048]	0.268	−0.035 [0.056]
Predicted family income	412.528	−28.335** [13.886]	423.331	−36.073 [13.515]
Panel B. Age at death and matching				
Age at death			72.44	0.996* [0.519]
log age at death			4.269	0.013* [0.008]
Number of matches	0.487	0.061*** [0.018]		
Quality of merge with DMF file	1.186	−0.006	1.183	−0.017 [0.012]
Observations		*N*=16,069		*N*=7,860
Panel C. Detailed sample sizes				
Children	2,177	13,892	983	6,877
Families	1,346	8,104	608	4,067
Counties	75	75	75	64

B—Additional Retrospective Data on MP Applicants

	Mean rejected	Difference (accepted–rejected)	Observations
Panel A. Ohio			
Probability found	0.105	0.007 [0.021]	7,456
Share native-born	0.880	−0.012 [0.061]	822
Share homeowner	0.549	−0.143 [0.089]	821
Imputed income (based on occupation)	531	−14.04 [35.61]	811
Panel B. Iowa			
Found in sample	0.644	−0.103* [0.0293]	812
Family income	721.5	−33.51 [35.53]	447
ln(income + 1)	6.375	−0.151	447
No income	0	0.0325 [0.0188]	447

B—Additional Retrospective Data on MP Applicants

	Mean rejected	Difference (accepted–rejected)	Observations
Homeowner	0.383	−0.233* [0.0686]	447
Home value (conditional on ownership)	5,292	−2,776*	78
Debt (mortgage/home value)	0.271	−0.00789 [0.147]	76
Paternal education	6.595	1.342* [0.437]	442
Literate	1	−0.0196** [0.00339]	456

Notes: Includes boys ages 0–18, born 1900–1925 in counties with rejected applicants only. We estimate a regression of each characteristic on a dummy for accepted status, clustering the standard errors at the county level. The coefficient and standard error for the constant is reported under "rejected" for each sample.

Table 3

Reasons for Rejection Distribution in All Records and in Estimation Sample (*Percent*)

	Panel A. All records		Panel B. Boys in sample	
	Reason MP denied	**Reason MP ended**	**Reason MP denied**	**Reason MP ended**
Other means	35.26	17.38	37.42	20.01
Ineligible				
Ineligible, reason unspecified	29.53	43.13	19.24	39.97
Married or husband returns	7.95	27.25	6.97	25.34
Moved from county	3.58		6.52	
No children eligible	2.03		2.12	
Doesn't meet residency requirement	1.36		2.73	
Not a citizen	0.32		0.76	
Other reasons				
Withdrew	8.29		3.64	
Application incomplete	4.09		6.97	
Immoral/unfit	3.80	3.22	4.24	4.39
Not dependent for long enough	1.93		5.61	
Mother lied in application	0.51		1.36	
Child delinquent		1.62		
Divorced	0.70		1.06	
Mother died/hospitalized/in prison	0.64	4.23	1.36	4.11
Observations with data	3,738	13,794	660	4,692

Table 4

Cash Transfers and Long-Term Mortality

	(1)	(2)	(3)	(4)	(5)
Panel A. log age at death					
Accepted	0.0157 ** [0.006]	0.0158 ** [0.007]	0.0182 ** [0.007]	0.0167 ** [0.007]	
Mean of rejected	72.44	72.44	72.44	72.44	
Effect in years	1.14	1.16	1.32	1.2	
Observations	7,860	7,860	7,860	7,860	
Panel B. Survival estimation P(survived past 60) Accepted	0.193 *** [0.048]	0.121 ** [0.049]	0.108 ** [0.052]	0.103 ** [0.052]	0.0377 [0.144]
Mean of rejected	0.421	0.421	0.421	0.421	0.880
Percent effect	11	7	6	6	0
P(survived past 70) Accepted Mean of rejected	0.265 *** [0.052] 0.287	0.205 *** [0.053] 0.287	0.211 *** [0.056] 0.287	0.199 *** [0.056] 0.287	0.267 *** [0.071] 0.596
Percent effect	19	15	15	14	11
P(survived past age 80) Accepted	0.239 *** [0.067]	0.195 *** [0.068]	0.187 *** [0.071]	0.192 *** [0.071]	0.164 * [0.097]
Mean of rejected	0.146	0.146	0.146	0.146	0.305
Percent effect	20	17	16	16	11
Observations	16,069	16,069	16,069	16,069	7,858
State fixed effects	X				
Cohort fixed effects	X	X	X	X	X
Individual controls		X	X	X	X
State characteristics		X	X	X	X
County 1910 characteristics		X			
County fixed effects			X	X	X

Notes: Standard errors (in brackets) are clustered at the county level. For panel B, logit coefficients reported. Percent effects computed relative to the average for rejected boys. Individual controls include child age at application, age of oldest and youngest in family, dummies for the number of siblings, number of letters in name, a dummy for whether date of birth is incomplete, year of application, and dummies for the marital status of the mother. County controls for 1910 include all characteristics listed in Table S6. State characteristics at the time of application include manufacturing wages, education/ labor laws (age must enter school, age can obtain a work permit, and whether a continuation school law is in place), state expenditures in logs (education, charity, and total expenditures on social programs), and state laws concerning MP transfers (work required, reapplication required, the maximum legislated amount for the first child, and the legislated amount for each additional child).

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significant at the 1 percent level.

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significant at the 5 percent level.

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significant at the 10 percent level.

Table 5

Heterogeneity and Robustness Checks

Stratified sample	coefficient on accepted	N	Mean of rejected applicants	Effect of acceptance in years	p-value for test of equality across groups
Panel A. Subgroup analysis					
Income above median	0.0171 * [0.009]	3,944	72.78	1.24	
Income below median	0.0200 * [0.012]	3,915	72.02	1.44	0.84
Fraction urban above median	0.0225 * [0.012]	4,282	72.04	1.62	
Fraction urban below median	0.0162 ** [0.007]	3,577	72.87	1.18	0.64
Fraction foreign born above median	0.0176 ** [0.008]	3,729	72.98	1.28	
Fraction foreign born below median	0.0185 [0.012]	4,130	72.02	1.33	0.95
Panel B. Additional robustness checks					
Full sample	0.0182 ** [0.007]	7,860	72.44	1.32	
Age <= 14	0.0201 *** [0.006]	7,408	72.32	1.45	0.25
Age <= 10	0.0173 ** [0.008]	5,202	72.32	1.25	0.86
Born 1900–1920	0.0219 ** [0.008]	6,798	72.32	1.53	0.12
Born 1900–1910	0.0211 ** [0.010]	2,524	72.32	1.53	0.69
Born 1911–1920	0.0189 [0.012]	4,274	72.32	1.37	0.92
Family size between 3 and 7	0.0244 ** [0.009]	5,178	72.32	1.76	0.22

Notes: Table shows effect of "accepted" on log (age at death) for selected subsamples. Standard errors (in brackets) are clustered at the county level. Effects computed relative to the average for rejected boys. All coefficients are estimated from separate regressions, with standard errors clustered at the county level and include all controls: individual characteristics (age at application, age of oldest and youngest in family, dummies for number of siblings, number of letters in name, a dummy for whether date of birth is incomplete, year of application, and dummies for the marital status of the mother); state characteristics at the time of application include manufacturing wages, education/labor laws (age must enter school, age can obtain a work permit, age can enter school, age can obtain a work permit, and whether a continuation school law is in place), state expenditures in logs (education, charity, and total expenditures on social programs), and state laws concerning MP transfers (work required, reapplication required, the maximum legislated amount for the first child, and the legislated amount for each additional child); county dummies and cohort dummies.

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significant at the 1 percent level.

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significant at the 5 percent level.

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significant at the 10 percent level.

Table 6

The MP Program and Medium-Term Outcomes from the 1940 Census

	No controls	All controls	Observations	Mean rejected	Percent effect
Annual income in 1939	76.06 [54.222]	90.93 ** [35.976]	1,960	666.2	13
Years of schooling	0.431 * [0.230]	0.316 [0.262]	2,058	9.363	4
Black = 1	0.00440 [0.007]	0.00607 [0.007]	2,099	0.008	40

Notes: Standard errors (in brackets) are clustered at the county level. Effects computed relative to the average for rejected boys. All coefficients are estimated from separate regressions, with standard errors clustered at the county level and include all controls: individual characteristics (age at application, age of oldest and youngest in family, dummies for number of siblings, number of letters in name, a dummy for whether date of birth is incomplete, year of application, and dummies for the marital status of the mother); state characteristics at the time of application include manufacturing wages, education/labor laws (age must enter school, age can obtain a work permit, and whether a continuation school law is in place), state expenditures in logs (education, charity, and total expenditures on social programs), and state laws concerning MP transfers (work required, reapplication required, the maximum legislated amount for the first child, and the legislated amount for each additional child); county dummies and cohort dummies.

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significant at the 1 percent level.

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significant at the 5 percent level.

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significant at the 10 percent level.

Table 7

The MP Program and Medium-Term Outcomes from WWII Records

	Models	No controls	All controls	Observations	Mean rejected	Percent effect
Panel A. Education						
Has exactly eight years of school	Logit	−0.326 ** [0.137]	−0.206 [0.170]	2,446	0.33	20
Education: left and right censored	Censored regression	0.348 * [0.197]	0.249 [0.201]	2,446	10.38	2
Panel B. Anthropometrics Height (cms)	OLS	1.346 [0.827]	1.142 [1.248]	1,844	174.5	1
Weight (pounds)	OLS	3.879 * [1.955]	3.417 * [1.984]	1,817	144.7	2
BMI	OLS	0.537 *** [0.215]	0.464 * [0.239]	1,706	22.06	2
Underweight	Logit	−0.690 ** [0.298]	−0.638 [0.411]	1,706	0.09	58
Obese	Logit	0.416 [0.496]	0.998 [0.751]	1,706	0.03	98
Panel C. Race Black = 1	Logit	0.282 [0.289]	0.0284 [0.274]	1,691	0.038	3

Notes: Standard errors (in brackets) are clustered at the county level. Model in column 2 is estimated using county and cohort fixed effects and include individual characteristics at the time of application. State characteristics at the time of application include manufacturing wages, education/labor laws (age must enter school, age can obtain a work permit, age can obtain a continuation school law is in place), state expenditures in logs (education, charity and total expenditures on social programs), and state laws concerning MP transfers (work required, reapplication required, the maximum legislated amount for the first child, and the legislated amount for each additional child).

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significant at the 1 percent level.

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significant at the 5 percent level.

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significant at the 10 percent level