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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 US (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. Male children of accepted mothers received one-third more years of schooling, were less likely to be underweight, and had higher income in adulthood than children of rejected mothers.

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An online appendix is available at:
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I. Introduction

More than one in five US children was living in poverty as recently as 2010.[1] A growing literature documents that early-life exposure to disease, nutritional deprivation and other factors associated with poverty can have adverse long-term effects on education, labor market outcomes, and ultimately, mortality (Almond and Currie 2011). 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, forthcoming) and children's health in adulthood (Case et al. 2002), it is still not known whether cash transfers to poor families improve children's lifetime outcomes (Currie 1998).

We ask whether targeted cash transfers improve children's long-run outcomes, with a particular focus on longevity, by studying the Mothers' Pension (MP) program—the first government sponsored welfare program for poor families with dependent children in the United States (1911-1935). 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" (Children's Bureau, 1933). The transfers generally represented 12-25% of family income, and typically lasted for three years. In 1935, the MP program was replaced by the federal Aid to Dependent Children (ADC, later Aid to Families with Dependent Children, and now TANF).

One of the main challenges 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? Our main strategy is to use as a comparison group the children of mothers who applied for transfers but were denied. This strategy of comparing accepted and rejected applicants for program evaluation has been used successfully in studies of disability insurance (von Watcher et al. 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 mothers were on average slightly better-off based on observable characteristics at the time of application, consistent with the information in administrative records that applicants were most often rejected because they were deemed to have sufficient support. 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

[1] http://www.census.gov/prod/2011pubs/acsbr10-05.pdf. Accessed 8/30/2013.

what could be expected of beneficiaries in the absence of transfers. Thus our estimates are, if anything, likely to understate the benefits of the program. We also construct alternative counterfactual groups based on differences in eligibility across counties to examine the robustness of our findings.

The second challenge is to obtain data containing long-term outcomes for a large sample of recipients and plausible comparison groups. Existing survey data are insufficient for two reasons: they suffer from substantial attrition and they are too recent. For example, in the NLSY and PSID surveys, forty percent of children who received welfare are lost to follow-up after twenty years. Even without such high attrition, the data would not allow us to study longevity because the children of the NLSY and PSID are not sufficiently old. Administrative individual records from the early years of the 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 on longevity. Another important limitation of studying AFDC recipients is that they were also eligible for multiple in-kind transfers such as Medicaid, housing and food stamps, which makes it impossible to evaluate the impact of cash transfers alone.[2]

Instead, we use administrative records from the precursor to the ADC program—the Mothers' Pension program (1911-1935). These data have the advantage of (1) being available in large numbers and containing both accepted and rejected applicants; (2) containing identifying information that allows us to link the children with other datasets to trace their outcomes; and (3) including children who were born sufficiently long ago that we can measure their longevity.[3]

Using data on 16,000 children from eleven states who wereborn between 1900 and 1925 and whose mothers applied to the Mother's Pension program, we find that receiving cash transfers increased longevity by about one year. These effects are driven by the poorest families in the sample; for them, the longevity increases are larger (about 1.5 years of life). These results are very robust to alternative functional form specifications, alternative counterfactual comparisons (e.g. divorced women in some counties, but not others, were eligible for MP benefits), and our

[2] 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.
[3] We focus on the cohorts born 1900-1925, most of whom have died by 2012. See http://www.ssa.gov/OACT/NOTES/pdf_studies/study089.pdf

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 explore whether cash transfers in childhood are associated with improvements in education, income and nutritional status, since previous work has documented that all three have positive and independent effects on mortality. To do so, 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 0.4 years, and increased income by 14 percent during adulthood (ages 20-45).

Our analysis has some important limitations. We cannot examine outcomes for girls because women typically changed their name upon marriage, making it extremely difficult to track long-term outcomes. Nor can we study African-Americans because they are not well represented in our states or our samples. Finally, though our results are based on larger samples with lower attrition than current panel surveys, there is still some attrition in our sample. To confirm that attrition is not driving our results we collected additional data for the population from Ohio, thereby significantly reducing attrition for that state. The results remain unchanged when we reduce attrition in this way.

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, suggestion caution in drawing conclusions as to the anticipated impact of cash transfers in the twenty-first century, three important similarities remain and suggest the current relevance of the effects we document. First, both the MP program and current welfare programs target children in female-headed households. These children were, and continue to be, the poorest children. Second, historical comparisons presented in the last section of the paper reveal that income played 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 US and in developing countries. Altogether these results suggest that targeted cash transfers are also likely to improve lifetime outcomes today. Though we acknowledge our study's limitations, the historical evidence we employ are the only means presently available to assess the impact of cash transfers across the entire life course. We return to the related literature and policy implications in the final section of the paper.

II. 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. It lasted until it was replaced by ADC in 1935, at which time 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.[4] Moreover, among those who remained with their mothers, prominent judges of juvenile courts[5] 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[7] and better alternative for children since transfers would allow mothers to care for their children.[8] 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, declaring, "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."

States had complete discretion in establishing the program, setting eligibility criteria and providing funding. Moreover, the 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. Below we describe

[4] 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." 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." (Bullock 1915)
[5] 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).
[6] Indeed, some claimed that MP laws lowered juvenile crime (*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 is about 1/3-2/3 the cost of boarding. The 1922 Children's Bureau report cites additional numbers that suggest that ex-post the cost of MP was indeed lower than that of institutionalization.
[8] 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).

how the programs in the eleven states we study varied in terms of eligibility, generosity, duration and conditions for receipt, for 1922, the median application year in our data (Table 1).[9]

Eligibility. All states required the mother to be poor and for her husband to be either missing or incapacitated (physically or mentally), but neither income nor property thresholds were specified. All states made widows eligible, but varied with respect to deserted or divorced women or women whose husbands were in prison or hospitalized. Citizenship was not required in most states, but even when it was, the intention to become a citizen was sufficient to qualify. Evidence suggests that by limiting eligibility to mothers with dependent children, the MP program succeeded in targeting the poorest children. Examining data from the 1915 Iowa 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% of all boys) were significantly poorer. They had 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 (Table S5).

Generosity. The state-legislated maximum benefit for the first child varied across states from a low of $10.00 in Iowa, to a high of $35.00 in Ohio, with the total monthly amount increasing non-linearly with the number of children in the family. In practice, generosity in benefit levels varied widely within states, and across counties as well.[10]

Duration. In most states the transfers would be given until the pension was revoked, but five states required reapplication. The shortest re-application duration was in Montana and Oregon (3 months) and the longest was in Minnesota and Washington (1 year).

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 explicitly required that the mother be of "good morals." However, in the records that include information on reason for discontinuation (Table 2), there are very few instances in which a mother or child's failure to comply with conditions is listed as

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

[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.

the reason for discontinuation. We conclude that the MP programs we study should, in general, be considered as an unconditional cash transfer.

III. Data

a. MP records

We have attempted to collect all the MP records that survive. Our efforts have yielded approximately 80,000 individual records of MP recipients who applied between 1911 and 1935 in eleven 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 counties—but if a county has records, the universe of records is available. These data appear to be representative of the MP population at the national level based on a comparison of our data for 1930 with published statistics for the nation in 1931 (Table S2).[11]

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 transfers were discontinued. For a single county (Clay County, Minnesota) we have data from a detailed 1930 study based on home nurse visits to all 62 families in the MP program at that time.

b. Sample selection

For this analysis we keep only male children (under age 18) born 1900-1925, whose mothers applied to the program prior to 1930. By imposing these restrictions, we have a sample of individuals who are most likely deceased by 2012 and who did not apply as a result of the Great Depression. We drop individuals without a year of birth or year of application as well as those without a first or last name. We only keep counties where data on rejected applicants are available. We limit our analysis to males because matching females based on names is substantially more difficult since women often change their names upon marrying. Table S3

[11] 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.

shows the final sample: about 16,000 males in 60 counties from eleven states. Among those, 14 percent were rejected applicants, with the share rejected ranging from a low of 5 percent in Minnesota, to a high of 17 percent in Ohio, which is also the state for which we have the largest number of records, accounting for 34 percent of our sample.

The average transfer ranges from 10 to 30 dollars per month. To better understand the generosity of the benefits, we compare the monthly transfers to the average wages in manufacturing in the state (Table S4). The average monthly MP transfer was equal to 17 percent of weekly manufacturing wages and never exceeded 29 percent.[12] However, relative to maternal income, which was considerably lower, evidence suggests that MP transfers represented a greater share of income. In a subsample of counties in Illinois which collected information on maternal income, MP transfers represented about 29 percent of the median maternal monthly income of \$60.[13] Overall these comparisons suggest that MP transfers represented a substantial source of income for poor mothers, but did not move them into 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.[14]

c. 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 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. In a study of the completeness of the DMF, Hill and Rosenwaike (2001) conclude that reporting has increased over time and is nearly complete for older individuals (age 65 and older) since the early 1970s.[15] We matched individuals based on their first name, last name, middle name, as well as day, month and year of birth. Details of the matching procedure are in Appendix I.

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[12] 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.

[13] In real terms (1982 dollars), MP transfers were half the size of 2010 transfers, (Table S4). However in some states, like Iowa, the MP transfers were very close in real terms to current TANF benefit levels, whereas in others, like Connecticut, TANF transfers are much larger today. It is worth noting that the relative generosity of states during this period mirrors the generosity of the welfare transfers today.

[14] 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). If present, crowd-out would lead to a downward bias in the estimated effects of the transfer.

[15] 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, 4 percent to multiple records, and 48 percent have no match, providing us with information on age at death for 52 percent of our sample (Figure S1). Using life tables, 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.[16] However, given that these families are poor and existing evidence links poverty to shorter life expectancy, one would reasonably expect a smaller share of the MP applicants to be found in the DMF files.

d. Other state and county data

We include as controls all the time-varying characteristics of the MP laws described previously and listed in Table 1. 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.[17] For Ohio we were also able to obtain county-level expenditures for several years, including expenditures on total relief, outdoor relief, and children's homes (see Appendix II for details). These data allow us to rule out possible confounding and bias in the estimates if MP program characteristics (i.e., generosity and rejection rate) are influenced by other resources available for the poor in the county.

IV. Empirical Strategy and identification of the effects on transfers

a. Basic empirical model

We estimate the effect of cash transfers on outcomes using the following logit model:

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[16] For comparison, we computed follow-up rates for the 2 data sets that have been used for evaluating the effects of welfare on children's outcomes: the National Longitudinal Study of Youth 1979 (NLSY hereafter) and the Panel Study of Income Dynamics (PSID hereafter). 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 1400 boys in the NLSY and 1066 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.

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

$$P(\textit{survived to age } a{=}1)_{iftcs} = f(\theta_0 + \theta_1 MP_f + \theta_2 X_{if} + \theta_3 Z_{st} + \theta_c + \theta_t + \varepsilon_{if}) \quad (1)$$

where the outcome is the probability of surviving past age *a* for 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 *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 can also control for county-level characteristics in 1910, and state characteristics in the year of application (Z_{st}). In our preferred specification we include county fixed effects (θ_c) and cohort fixed effects (θ_t). Thus, the effect of the program θ_1 is identified by comparing the average survival of accepted boys to rejected boys within county and year of birth, conditional on other observables. Because we have multiple children per family, standard errors are clustered at the family level. We also estimate an Accelerated Failure Time (AFT) hazard model of the type:

$$\log(\textit{Age at death})_{ifts} = \theta_0 + \theta_1 MP_f + \theta_2 X_{if} + \theta_3 Z_{st} + \theta_c + \theta_t + \varepsilon_{if} \quad (2)$$

where the dependent variable is the natural log of the age at death for a given individual, and all other explanatory variables are defined as in Equation (1).

Using matched administrative data poses three challenges. The first is attrition. Based on evidence from life tables, it is reasonable to assume that those without a match in the DMF were deceased by 1975 (the start of comprehensive reporting in the DMF) and to include them in our estimation sample for survival (but not the AFT hazard models because we cannot observe age at death for this group). We can then compare survival regression results using the full sample to results when using only individuals matched in the DMF.

In addition to missing matches (i.e., attrition), we have multiple matches for a few records (4 percent of matches have multiple possible matches and thus outcomes), as well as possible measurement error in matching (i.e., even if we find a unique match, we are not certain that it is the correct match.) We developed estimation procedures to account for both multiple matches and measurement error in estimating (1) and (2).[18] We also present our results using only the unique matches, as is typically done in the literature, for comparison.

b. Identification strategy: Rejected applicants

[18] Programs (in STATA) and documentation available at http://www.econ.ucla.edu/alleras/research/data.html.

For identification of causal effects, we use rejected applicants as the counterfactual. This strategy 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.[19] To assess the comparability of these groups, we investigate reasons for rejection, and we compare the observed characteristics of accepted and rejected applicants.

Table 2 reports the distribution of rejection reasons for those for whom this information was reported (about 60 percent of rejected applicants). The most common reason for rejection was insufficient need (35 percent), in which case the rejected applicants represent an upper bound on what could be expected of the beneficiaries in the absence of pension receipt. Marriage or remarriage is a common reason for rejection, while ineligibility due to insufficient length of residency and non-citizenship appear very uncommon. Case studies that examined reasons for rejection from specific counties are consistent with the statistics we report. For instance in Chicago during 1911-1927, it was reported that sufficient means accounted for half of all rejected and most cancelled pensions (Goodwin 1992). In Clay County, Minnesota, where we have detailed information for all families, the most commonly reported reason for discontinuation of a pension was that the family was judged capable of self-support.

On observables, accepted and rejected applicants look similar but not identical. On average, rejected applicants were slightly older and had somewhat smaller families, and the average age of the children in the family was higher, particularly the age of the youngest child (Table 3). Marital status of the mothers also differed. Among the accepted, there are more children of widows and fewer children of mothers with unknown/unreported marital status. Interestingly, among accepted children, the 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 2006 and references therein). When we examine which family characteristics are associated with acceptance and, conditional on acceptance, generosity of transfer received in a regression framework, the same patterns

[19] 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.

emerged (Table 4). Large families with younger children were more likely to be accepted, were given more money and (for those for whom we observe the duration) were helped for longer periods. Illiterate mothers (as proxied by missing DOB of the child) were also helped more.

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 observable characteristics of the family using the 1915 Iowa census data. 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).[20] With these coefficient estimates (Table S6), we then predict average income (in 1915) for accepted and rejected applicants based on their observable characteristics. We find that on average, rejected mothers have predicted income that is 35 percent higher than that of accepted mothers, consistent with the reasons for rejection.[21]

A related concern with this strategy of comparing accepted and rejected applicants is evidence of racial discrimination in the MP program. 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. Only in Ohio and Pennsylvania did black mothers appear to receive MP benefits at expected rates (Children's Bureau 1933). Without information on race in the applications, this raises the possibility that blacks may be disproportionately represented among the rejected applicants, biasing our estimates. We present evidence from multiple sources that this is not likely the case in our sample. First, as shown above, accepted applicants appear to be worse off on observables than the rejected, which is inconsistent with blacks being disproportionately rejected. Second, we observe the race of accepted and rejected applicants who are linked with their 1940 census or WWII enlistment records. Third, we present results in which we drop from the sample all counties with a disproportionate share of black residents. Fourth, we present results for Ohio only, one of just two states in which blacks received MP benefits at appropriate rates according

[20] We use coefficient estimates from a Tobit, also presented in Table S6, because of zero reported income and the need to avoid predictions of negative income. The table shows that the main unobserved determinants of poverty aside from location are whether or not the household owned property, the presence of ill or old individuals, and race.

[21] The county characteristics, as measured in 1910, are roughly similar for the accepted and rejected applicants (Table 3, Panel B). Since we restrict our sample to counties with both accepted and rejected applicants, the mean differences reported here reflect underlying differences in the shares of accepted and rejected across counties. Accepted applicants are more likely to be in urban counties, implying the acceptance rate was higher in urban counties, but otherwise the characteristics are similar.

to the Children's Bureau 1933 report. All four exercises suggest that blacks do not constitute a large share of rejected applicants and therefore are not biasing our results.[22]

c. Differential attrition and matching to mortality records

Next we investigate the possibility of differential attrition and matching to the mortality records based on observable characteristics of the mother and child. Table 3 presents summary statistics by accepted/rejected status for the entire sample, the sample with a match to mortality data, and for the sub-sample with unique matches. The characteristics of individuals we find are very similar across samples of rejected or accepted applicants. Thus, conditional on acceptance/rejection, there appears to be no systematic difference in the characteristics of those with a match in the mortality records and those without a match. However, the accepted are eight percent more likely to be found in the mortality records (Panel A of Table 5), and this is true with or without controlling for covariates. This can be explained by either lower mortality rates among the accepted, or identical mortality but rejected children being harder to find in the mortality records. Later, we present evidence in favor of the lower mortality explanation.

V. Mortality results

a. Preliminary visual evidence

Accepted boys lived on average to age 73.3, about 0.8 of a year longer than rejected boys (Table 3). We estimate the density of the age at death for accepted and rejected applicants, using all matches (Figure 1a), and using unique matches only (Figure 1b). Both Figures show that the distribution of the age at death of accepted applicants is shifted to the right of the distribution of rejected applicants, suggesting that accepted applicants lived longer. 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.

b. Main Results

We start by presenting estimates of the effect of the MP program on the probability of survival past age 60, 70 and 80, using all matches and assuming those without a match died prior to 1975 (Panel A, Table 6). Column 1 includes only state and cohort dummies, column 2 adds all

[22] The most likely explanation is that the discrimination was such that black women did not bother even applying to the program, knowing that they would be rejected.

individual controls, county characteristics in 1910 and state characteristics at the time of application. Column 3 adds county fixed effects. The results are not very sensitive to the covariates we include. The implied marginal effects suggest statistically significant increases in the probability of survival past age 70 (10-20 percent), and the probability of survival past age 80 (9-15 percent).

We perform a number of robustness checks. These include using the date of birth from the death certificate instead of the one from the MP records, which differ in less than 10% of our sample (column 4),[23] dropping those without age at death instead of assuming they died (column 5), or using unique matches only (column 6). In the last column, we drop counties with a share of blacks in the top quarter for the state (never higher than five percent). The effects remain positive and are somewhat larger for surviving past age 60. Next we abandon the arbitrarily chosen cutoffs of ages 60, 70 and 80 and estimate our survival model using ML and the fully saturated specification for each age at death between 58 and 88, which correspond to the 10th and 90th percentiles of the distribution of the age at death. Figure 2 shows the marginal effects as a percentage of the survival rate of rejected applicants, computed using coefficients from estimation with and without imputing the missing observations as 0s. All coefficients are positive and significant after age 67, regardless of whether we impute missing values as 0.

In panel B of Table 6 we present estimates for longevity from the AFT hazard model. This analysis is based on all matched records, dropping those with no match (i.e., missing date of death). Again 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 0.7 to 1.4 years of life depending on the specification and sample.

We also present estimates that take advantage of our ability to predict family income using the 1915 Iowa census to further limit our comparison to accepted and rejected applicants who appear most similar in terms of resources (income) available to them. To do so, 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.

[23] If the date of birth in the MP and DMF records differ, it is sometimes the case that the DOB occurs later in the MP records. We believe this is likely due to mothers underreporting the age of their children in an effort to increase length of eligibility, in addition to just errors in recording. Reasons for discontinuation from Clay County, Minnesota support this.

The effect of the MP program appears to be larger among those poorest in the sample (Figure 3). Table 7 presents the estimates separately by predicted income, for survival (Panel A) and age at death (Panel B). Two features are worth noting. First, the average survival rates and age at death are always larger for the sample that is predicted to have larger incomes, which suggests that our predicted income is indeed correctly classifying individuals into income categories, since income is a well-known predictor of mortality. Second, the results confirm that the poorest individuals in the sample benefitted substantially more than those coming from relatively richer households. For the poorest boys, the gain in longevity is about 1.5 years of life.

c. Additional robustness checks

We conduct several additional robustness checks for the probability of survival past 70, (Table S7). The results are robust to using a standard logit models (Panel A) or to using the logit maximum likelihood model we develop in Bungi, Honore and Lleras-Muney (2013) to properly account for multiple matches and measurement error (Panel B) ; to using only unique matches, using the highest quality matches or using all matches; to allowing for measurement error; to matching accepted and rejected on propensity scores; and to dropping individuals with three or more matches from the sample. Although the coefficients differ in magnitude when we change the sample, the marginal effects remain similar across all specifications.

In addition, we find that the estimated positive effects of the programs are not a function of the number of rejected applicants or their share. If characteristics of the control group vary with the rejection rate (i.e., counties who reject few (many) families apply a looser (stricter) standard for acceptance), we can interpret this as further evidence that the results are not driven by different characteristics of the treatment (accepted) and control (rejected) groups.

d. Results for Ohio

We present separate estimates for the state of Ohio (34 percent of our sample) as a final robustness check for three reasons. First, as the 1931 Children's Bureau study reported, there was no evidence of discrimination against black women in Ohio's MP program. If the estimates are unchanged when we limit the sample to the state of Ohio, this would suggest that discrimination against blacks in other MP programs is not driving the results.

Second, for Ohio we can include data on county expenditures on social programs at the time of application, eliminating a potential source of bias in our estimates if counties are more likely to reject applicants when there are other sources of support in the community. The results, presented in Panel C of Table S7 with and without controls show that the estimated effects are essentially unchanged, though perhaps slightly larger, for the Ohio sample. Thus discrimination against black mothers and potential correlation with other social programs are not biasing our results. Finally, Ohio maintains death records going back to 1958, allowing us to match Ohio boys to earlier death records. We also manually searched for unmatched children of accepted and rejected Ohio MP applicants on Ancestry.com, a database that includes deaths from WWII and other wars, as well as other death records (e.g., cemetery listings). When we do this, we increase our match rate to 60 percent. (Figure S1, Panel C). There are three notable results from this exercise. First, the estimates are unchanged when we extend our records in this way. This is visible in Figure 4, which depicts the densities of the age at death, and in Table S7, Panel C which reports the estimates for surviving past 70. Second, we continue to find death records for accepted applicants at higher rates, suggesting that the higher match rate does indeed correspond to a real mortality effect (Table 5, Panel B). Third, the age at death among rejected applicants whom we did not find in the DMF records is 66.3, compared to age at death among accepted applicants of 67.2 (a difference of nearly one year of life). Since this is exactly what we estimate using the full data, this suggests that our inability to link the MP applicants to death records prior to the mid-1970s is in fact related to the higher mortality of the rejected applicants.

About 60 percent of the newly found death records show deaths prior to age 70, but only about 30 percent died before age 60. We conclude that the assumption that the missing are dead is reasonable for survival past 70, but not for survival at younger ages, which might explain why our results for survival past age 60 are small and sometimes insignificant.

e. 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 Appendix III for details).[24] The two alternative counterfactual groups are orphans and children of ineligible mothers (e.g., single or divorced mothers in states that excluded these groups).

[24] 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.

Orphans are identified as children living in institutions in the census. Since MP programs were developed in large part to prevent institutionalization of children by allowing them to remain home, orphans represent an appropriate counterfactual in this context. We find that the orphans are very similar to the rejected applicants in terms of longevity, with both living shorter lives than accepted applicants (Panel A, Figure 5).

The second counterfactual group is comprised of children of single or divorced women drawn from the census, in states where these women were not eligible for the MP program. We compare this group of ineligible women to accepted MP children whose fathers are disabled or institutionalized (but not widows) because on observables these children appear more similar. Children of accepted women lived longer than this alternative control group of ineligible children of single and divorced women (Figure 6 Panel B).[25]

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

We explore potential mediating factors to understand the ways in which income transfers in childhood improved longevity. Previous work has shown a significant relationship between education, income and being underweight with mortality. More specifically, an increase in schooling of 0.25 years is associated with a 0.15 year increase in longevity in OLS regressions (Cutler and Lleras-Muney 2008); those with income in the top 5 percent of the distribution live 25 percent longer than those in the bottom 5 percent (Deaton 2002); and being underweight in adulthood is associated with a relative risk of mortality that ranges from 1.38 to 2.3 (Flegal et al. 2005). In this section we estimate the effect of the MP program on these medium-term adulthood outcomes by linking the MP sample with WWII enlistment records and 1940 census records.

a. WWII enlistment records

We can match the MP records with WWII enlistment records for individuals who enlisted in the Army during 1938-1946.[26] For all enlistees we observe education, marital status, and two health measures (weight and height), which are markers of nutritional deprivation in childhood. Height, in particular, has been linked with childhood nutrition, as well as adult cognitive ability and labor market outcomes (Case and Paxson 2008).

[25] If we include MP widows, the results are even larger.
[26] Enlistment records are available for 9 million (of the 16.5 million) individuals who served in WWII.

The WWII results should be viewed as suggestive rather than definitive for two reasons. First, our match rate is quite low—lower than our match rate for mortality (Table 5, Panel C). This is because WWII records do not contain date of birth, though they contain state of birth, which we add to our matching criteria. Second, the WWII records are not a complete or random subset of the male population because of induction rules and exemptions. Table S8 shows that our matched sample is younger—this is to be expected because males aged 18-25 in 1942 served at much higher rates than older men (Hogan 1981) and they are also likely otherwise healthier given the mental and physical requirements of enlistment. Consistent with this positive selection into the WWII records, we find accepted applicants at higher rates than rejected applicants (Table 5, Panel C).

Evidence of the effects on educational attainment from WWII records is presented in Figure 6 and Table 8. The education outcome is censored below at 8 years of schooling (the minimum), and is also censored for those still in school. We find that children of accepted families are 20 percent more likely to have more than 8 years of school (Table 8, 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. However, when we include the full set of controls, the point estimates generally remain similar, but are no longer precisely estimated, which is not surprising given the small sample size for this analysis.

MP receipt also significantly reduces the probability of being underweight (Figure 8). Estimates in Table 8 imply a statistically significant 50 percent reduction, with similar results with and without controls. The estimates for height, weight and BMI (measured continuously) are positive but not significant. Our results showing the greatest impact of MP receipt at the lower tail of the distribution of weight suggest that the effects of cash transfers may be greatest for the most disadvantaged. The detailed records from Clay County nurse visits noted malnutrition as one of the most commonly observed health problems among families in the MP program. These results suggest that the transfers helped families improve the nutrition of their children. Finally, in the last row of Table 8 we show that, in WWII records, accepted applicants are more likely to be black than rejected applicants. Although this difference is not statistically significant it again suggests that black children are not over-represented among rejected applicants.

b. 1940 Census Records

We present the distribution of longevity, education, income and log(income) for accepted and rejected MP applicants matched to 1940 census data in Figure 8. The distributions of income for accepted and rejected applicants are significantly different, with the accepted applicants less likely to be found in the bottom half of the distribution of income and more likely to be found in the top half of the income distribution (Figure 7). The results from regression analyses presented in Table 9 suggest that MP recipients have incomes nearly 14 percent higher than their rejected counterparts in 1940, when they are young adults (Panel A, Table 9). The results with respect to years of schooling are similar to those from WWII records (0.4 more years of school). We also examine whether rejected applicants are more likely to be black using the 1940 census records which contain race. Again, we find no large or significant relationship between race and rejection in these records.

VII. Interpretation and policy relevance

This is the first study to document that cash transfers to mothers of poor children substantially increase children's longevity. Our additional findings suggest that underlying nutrition, educational attainment and income in adulthood are all likely mediating factors. 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 type of family. In fact, the income gap between children in two-parent versus single-mother families has only grown over time (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 (Table S10). The relationship between parental income and child human capital in 2010 is remarkably similar to that found in 1915.[27] 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 US has found

[27] 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.

positive effects of food stamps on pregnancy outcomes (Almond, Hoynes and Schanzenbach 2011) and adult obesity (Hoynes, Schanzenbach and Almond, 2012), as well as positive effects of cash transfers through the tax code in the US and Canada on child cognitive achievement and health (Dahl and Lochner 2012; Milligan and Stabile, 2008).[28] 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 check-ups, 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 2012), 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 benefits over the lifetime of the recipients.

Recent theoretical and empirical work (Cunha and Heckman 2007 and 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 de-worming campaign in the American South on children's educational outcomes and their adult income; these estimates are consistent with those from randomized de-worming campaigns in Kenya (Miguel and Kremer 2004). Even earlier, prenatal conditions have long-term consequences for children's health and on socio-economic outcomes (Barker 1995; Almond 2006).

None of these studies address whether cash transfers effectively or efficiently alleviate these adverse early-life shocks and improve lifetime outcomes. Current aid to poor women takes the form of in-kind and cash transfers, with the US 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

[28] 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.

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.

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Table 1: Mothers' Pension Program Characteristics in 1922

States with MP Records Collected

	Eligibility					Requirements			Benefits		
State (year MP enacted)	Deserted or divorced	Husband in institution	Child age elig.	Resid-ency	Citizen	Prop limit	Work	Re-apply?	1st child	add'l child	Max
Connecticut (1919)	No	no	16	No	No				**	**	No
Idaho (1913)	No	yes	15	Yes	No				10	5	No
Illinois[a] (1911)	No	yes	14	Yes	Yes	yes	yes		25	15	60
Iowa (1913)	No	yes	16	No	No				12	12	No
Minnesota (1913)	Yes	yes	16	Yes	Yes	yes	yes	yes	20	15	No
North Dakota (1915)	No	no	14	Yes	No				15	15	No
Ohio (1913)	Yes	yes	16	Yes	No		yes	yes	35	10	No
Oklahoma (1915)	No	yes	14	Yes	No				10	5	No
Oregon (1913)	No	yes	16	Yes	Yes	yes	yes	yes	15	10	60
Washington (1913)	No	no	15	Yes	No			yes	15	5	No
Wisconsin (1913)	Yes	yes	14	Yes	No	yes	yes		15	10	No

Source: Children's Bureau (1922c).

*Colorado's law did not determine any minimum or maximum amounts but specified that amounts should be deemed "sufficient".

**Connecticut's law had a more complicated schedule of payments depending on the family characteristics.

a. larger amounts were given in counties with populations with more than 300,000. For smaller areas the amounts were 15/10

"Work" consists of laws that specified that maternal employment could be demanded and constrained in terms of the number of days/hours at work.

"Citizenship" consists or requiring either citizenship or the intention of becoming one.

Table 2: Reasons for Rejection
Distribution in all Records and in Estimation Sample

	A. All records		**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
N observations with data	*3,738*	*13,794*	*660*	*4,692*

Panel A includes information from any record with non-missing values in original MP data. Panel B includes information only from those records with non-missing values, from boys born between 1900-1925 who applied before 1930 and in counties with records from rejected applicants

Table 3: Summary Statistics for Estimation Sample
Boys ages 0-18, Born 1900-1925 in Counties with Rejected Applicants Only

	Full sample		Sample matched to Death certificate		Sample with unique matches	
	Rejected	Accepted	Rejected	Accepted	Rejected	Accepted
Age at death	72.58	73.34	72.58	73.34	72.53	73.34
A. Individual characteristics						
Year of application	1920.8	1921.7	1921.0	1921.9	1921.0	1921.9
YOB of child	1912.1	1913.4	1912.2	1913.6	1912.2	1913.6
Child age (years)	8.72	8.22	8.84	8.3	8.84	8.31
Number of kids in family	3.6	3.77	3.52	3.74	3.54	3.73
Age of oldest kid in family	11.86	11.48	11.82	11.48	11.85	11.47
Age of youngest kid in family	5.59	4.81	5.65	4.84	5.63	4.85
Length of family name	6.39	6.45	6.33	6.35	6.35	6.36
Widow	0.51	0.54	0.53	0.55	0.53	0.55
Divorced	0.04	0.03	0.03	0.03	0.03	0.03
Husband abandoned/prison/hospital	0.18	0.19	0.18	0.19	0.18	0.19
Mother's marital status unknown	0.27	0.25	0.26	0.23	0.26	0.23
Missing age at death	0.54	0.49	0	0	0	0
Day or month of birth missing	0.02	0.03	0.01	0.03	0.01	0.02
Number of matches	0.48	0.54	1.04	1.06	1	1
Quality of merge with DMF file	1.19	1.18	1.19	1.18	1.18	1.17
B. 1910 county characteristics						
Duncan socio-economic Index	24.21	24.43	24.31	24.71	24.33	24.94
SD of SEI	20.85	20.52	20.79	20.55	20.81	20.72
% living in urban areas	0.34	0.4	0.36	0.42	0.36	0.42
% females that are widowed	0.06	0.06	0.06	0.06	0.06	0.06
% under 16 living with only mom	0.06	0.06	0.06	0.05	0.06	0.06
% workers with SEI<20th %	0.11	0.11	0.11	0.1	0.11	0.1
% women 16+ in the labor force	0.18	0.18	0.18	0.18	0.18	0.18
% children 10-15 working	0.07	0.07	0.07	0.07	0.07	0.07
Average value of farm land	84.46	92.82	81.48	94.11	81.49	95.15
Duration of transfers in years	3.09	4.13	3.2	4.26	3.2	4.27
Monthly amount - 1982 dollars	0	311.21	0	311.5	0	311.38
Number of children	2219	14070	1,028	7,217	995	6,914
Number of families	1,353	8,131	640	4,229	616	4031
Number of counties	75	75	75	65	75	64
Number of observations	2,264	14,514	1,073	7,661	995	6,914

Table 4: Determinants of Acceptance and Generosity of Transfers

Dependent variable:	Accepted =1	Log(monthly amount)	Log(duration in years)	Log (lifetime transfer)
Model:	Logit	OLS	OLS	OLS
Child age (years)	-0.00218	0.00830**	-0.0160	-0.0173
	[0.003]	[0.003]	[0.011]	[0.012]
Number of kids in family (imputed)	0.00348	0.111***	0.0407***	0.132***
	[0.003]	[0.005]	[0.011]	[0.013]
Age of oldest kid in family record	0.000344	0.000641	0.00997*	0.0178***
	[0.002]	[0.002]	[0.006]	[0.006]
Age of youngest kid in family record	-0.0053**	-0.0251***	-0.0327***	-0.0551***
	[0.002]	[0.002]	[0.005]	[0.006]
Length of family name	0.00132	-0.000570	0.00365	0.00328
	[0.002]	[0.003]	[0.008]	[0.009]
(Widowed is the excluded category)				
Divorced	-0.0589**	0.000729	-0.106	-0.100
	[0.025]	[0.022]	[0.066]	[0.073]
Husband abandoned, in prison/hospital	-0.00990	0.00104	-0.116***	-0.0922**
	[0.011]	[0.013]	[0.039]	[0.043]
Mother's marital status unknown	-0.0962**	-0.0303	-0.000755	-0.00595
	[0.040]	[0.040]	[0.105]	[0.120]
Day or month of birth missing	-0.0185	0.0594*	0.415**	0.519***
	[0.030]	[0.035]	[0.185]	[0.199]
County and cohort FE?	yes	yes	yes	yes
State characteristics (year of application)	yes	yes	yes	yes
Mean of Y	0.864	5.511	1.306	6.647
Number of individuals	16,288	13,963	6,868	6,806

* p<0.10, ** p<0.05. 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 (whether citizenship is required, whether there is a residency period in county required, the maximum legislated amount for the fits child and the legislated amount for each additional child).

Table 5: Differential Attrition and Matching of Males in MP Records

	Coefficient on Accepted =1 from Logit specification				
Panel A: MP matched to DMF					
Missing match=1	-0.200**	-0.174**	-0.176**	-0.130**	-0.110**
(M=0.527, N=16,288)	[0.051]	[0.052]	[0.052]	[0.053]	[0.055]
More than one match=1					
(M=0.0718, N=8,247)	0.261	0.271	0.0687	0.0851	0.0808
	[0.192]	[0.193]	[0.205]	[0.205]	[0.216]
Panel B: Ohio MP matched to additional death records					
Missing match=1	-0.206**	-0.198**	-0.200**	-0.126	-0.160*
(M=0.205 N=5,469)	[0.088]	[0.089]	[0.089]	[0.093]	[0.095]
More than one match=1					
(M=0.578, N=3,495)	-0.167	-0.168	-0.179	-0.205*	-0.243**
	[0.107]	[0.109]	[0.109]	[0.112]	[0.115]
Panel C: MP matched to WWII records					
Missing match=1	-0.212**	-0.193**	-0.199**	-0.231**	-0.149**
(M=0.848, N=16,288)	[0.068]	[0.069]	[0.069]	[0.071]	[0.075]
More than one match=1 (if					
matched) (M=0.139, N=2,895)	0.144	0.147	0.152	0.224	0.232
	[0.177]	[0.178]	[0.178]	[0.183]	[0.205]
Individual Characteristics		x	x	x	x
Match Quality			x	x	x
State-year & 1910 county controls				x	x
County and cohort dummies					x

* p<0.10, ** p<0.05. Individual controls include child age at application, age of oldest and youngest in family, number of siblings, number of letters in name, year of application, and dummies for the marital status of the mother. Match controls include a dummy for whether date of birth is incomplete. County controls for 1910 include all characteristics listed in Panel B of Table 4. 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 (whether citizenship is required, whether there is a residency period in county required, the maximum legislated amount for the fits child and the legislated amount for each additional child).

Table 6: Cash Transfers and Long-Term Mortality

	1	2	3	4	5	6	7
	Main results (DOB from MP records)			DOB From DMF	drop missing DOD	unique matches only	counties with few blacks
Panel A: Effects on survival, Logistic model							
P(survived to 60)	0.192***	0.111**	0.0984*	0.0912*	0.0440	0.0558	0.112*
	[0.047]	[0.049]	[0.051]	[0.051]	[0.118]	[0.118]	[0.058]
N	16,289	16,288	16,288	16,288	8,244	7,908	11,694
% effect	11%	6%	6%	5%	1%	1%	7%
P(survived to 70)	0.263***	0.199***	0.201***	0.185***	0.252***	0.266***	0.242***
	[0.052]	[0.053]	[0.055]	[0.055]	[0.075]	[0.075]	[0.063]
N	16,289	16,288	16,288	16,288	8,244	7,908	11,694
% effect	19%	14%	14%	13%	10%	11%	18%
P(survived to 80)	0.229***	0.182***	0.170**	0.171**	0.146*	0.152*	0.206**
	[0.066]	[0.067]	[0.070]	[0.071]	[0.080]	[0.080]	[0.080]
N	16,289	16,288	16,288	16,288	8,244	7,908	11,694
% effect	20%	16%	15%	15%	10%	11%	18%
Panel B: Effects on longevity, AFT model							
Log(age at death)	0.0101	0.0089	0.0117*	0.0100*		0.0177***	0.0139*
	[0.007]	[0.007]	[0.007]	[0.006]		[0.006]	[0.008]
effect (yrs)	1.01	1.01	1.01	1.01		1.02	
N	8,255	8,254	8,254	8,251		7,908	5,820
State FE	Y						
Cohort FE	Y	Y	Y	Y		Y	Y
State charac.		Y	Y	Y		Y	Y
County 1910 char.		Y					
County FE			Y	Y		Y	Y
Individual controls		Y	Y	Y		Y	Y

* p<0.10, ** p<0.05. % effects computed relative to the average for rejected boys. Individual controls include child age at application, age of oldest and youngest in family, 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 Panel B of Table 4. 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).

Table 7: Effects of Cash Transfers by Initial (Predicted) Family Income

	Poorest	Richest
Panel A: Effects on survival, Logistic model		
P(survived to 60)	0.109*	0.0563
	[0.061]	[0.097]
N	11,982	4,557
Mean for rejected	0.397	0.463
% effect	6.57%	3.02%
P(survived to 70)	0.212***	0.107
	[0.066]	[0.101]
N	11,982	4,557
Mean for rejected	0.267	0.326
% effect	15.54%	7.21%
P(survived to 80)	0.194**	0.105
	[0.085]	[0.127]
N	11,982	4,557
Mean for rejected	0.132	0.178
% effect	16.84%	8.63%
Panel B: Effects on longevity, AFT model		
Log(age at death)	0.0198**	-0.0091
	[0.008]	[0.012]
N	6,080	2,425
Mean for rejected	71.88	73.95
effect (years)	1.423224	-0.67295

* p<0.10, ** p<0.05. Both panels estimates models like those in column 3 of Table 7. Panel A uses the MLE procedure we developed, using all matches, imputing missing observations as dead and controlling for all observables. Panel B estimates models using the GMM method we developed, does not include individuals with missing age at death and include all controls in the estimation.

Table 8: The MP Program and Medium-Term Outcomes:
Education and Health from WWII Records
Unique Matches, OLS or Logit Coefficients Reported

Dependent variable	No Controls	All controls	N	Mean rejected
A. Education				
Has exactly 8 years of school	-0.0677**	-0.0355	2,446	0.330
	[0.030]	[0.032]		
Education -- left and right	0.348*	0.238		
censored	[0.196]	[0.209]		10.38
B. Anthropometrics				
Height (cms)	1.346	1.142	1844	174.5
	[1.067]	[1.229]		
Weight (pounds)	3.879*	3.417	1706	144.7
	[2.157]	[2.330]		
BMI	0.537*	0.464	1706	22.06
	[0.299]	[0.355]		
Underweight	-0.690**	-0.638*	1706	0.09
	[0.272]	[0.336]		
Obese	0.416	0.998	1706	0.03
	[0.474]	[0.612]		
C. Race				
Black=1	0.282	0.0284	1691	0.038
	[0.352]	[0.381]		

* p<0.10, ** p<0.05. 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 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 fits child and the legislated amount for each additional child).

Table 9: The MP program and Medium-Term Outcomes: Education and Income from 1940 Census Records Coefficient on Accepted reported. OLS models

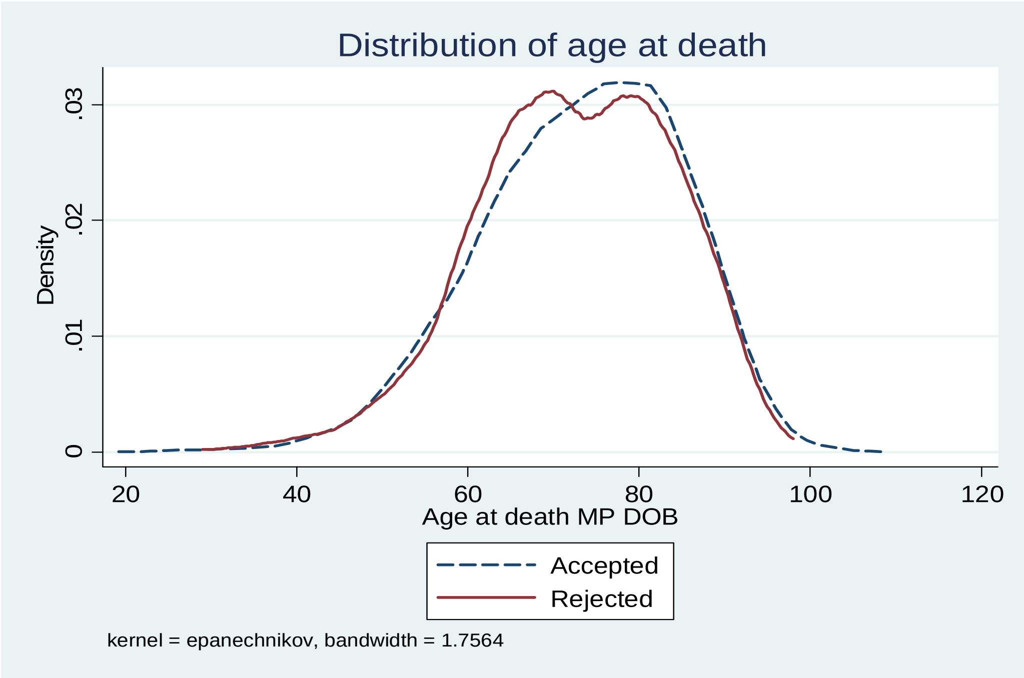
Dependent variable	No Controls	All controls	N	Mean rejected
A. Income and Education				
Annual Income in 1939	76.76	89.50*	1,960	666.2
	[50.053]	[48.461]		
Years of schooling	0.464**	0.368*	2,058	9.363
	[0.181]	[0.197]		
B. Race				
Black =1	0.00440	0.00319	2099	0.008
	[0.006]	[0.007]		
C. Effect on age at death				
Log age at death	0.0281**	0.0357***	2,099	69.34
	[0.012]	[0.013]		

* p<0.10, ** p<0.05. Subsample of MP applicants matched to the preliminary release of the 1940 census. Estimation sample restricted to 1900-1925 cohorts in states and counties with rejected applicants. All models are estimated using county and cohort fixed effects and include state 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 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 fits child and the legislated amount for each additional child).

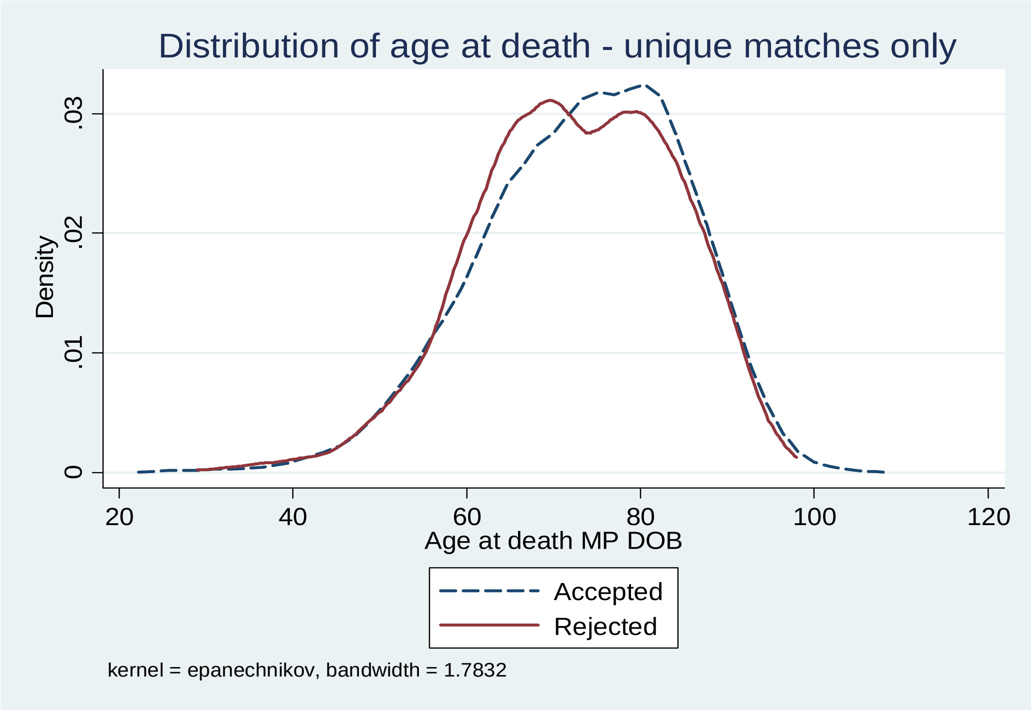
Fig. 1: Distribution of Age at Death.

Boys of Accepted and Rejected Applicants

a. All matches

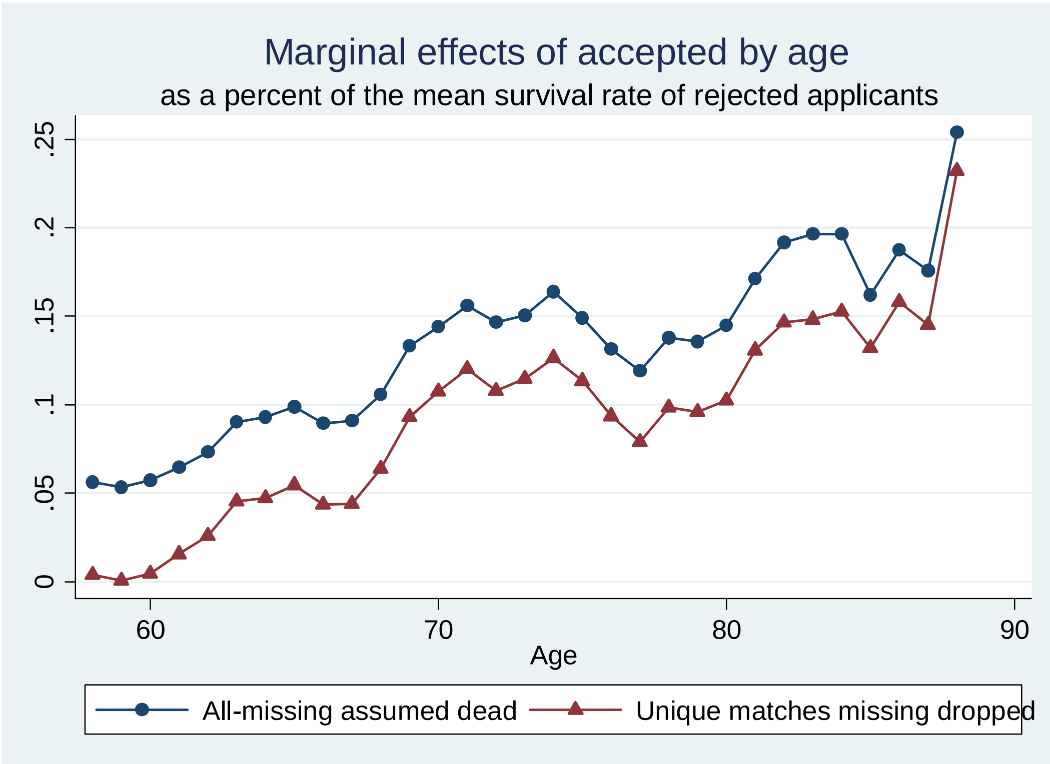


b. Unique matches



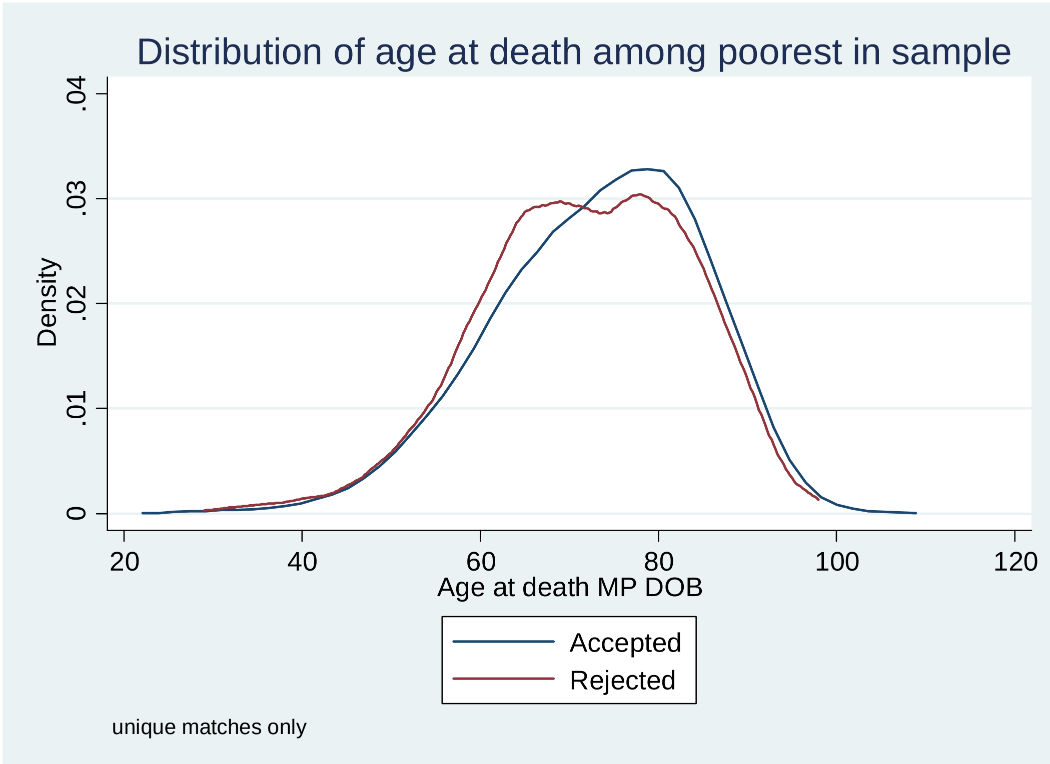
We reject the null that the distributions are the same using a Wilcoxon rank-sum test (unique matches).

Fig. 2: Effects by Age



Each dot represents the marginal effect of "Accepted = 1" as a percent of the survival rate to a given age. Coefficients for surviving past ages 63 are all significant at the 10 percent or higher for both sets of estimates.

Fig. 3: Estimates by predicted family income



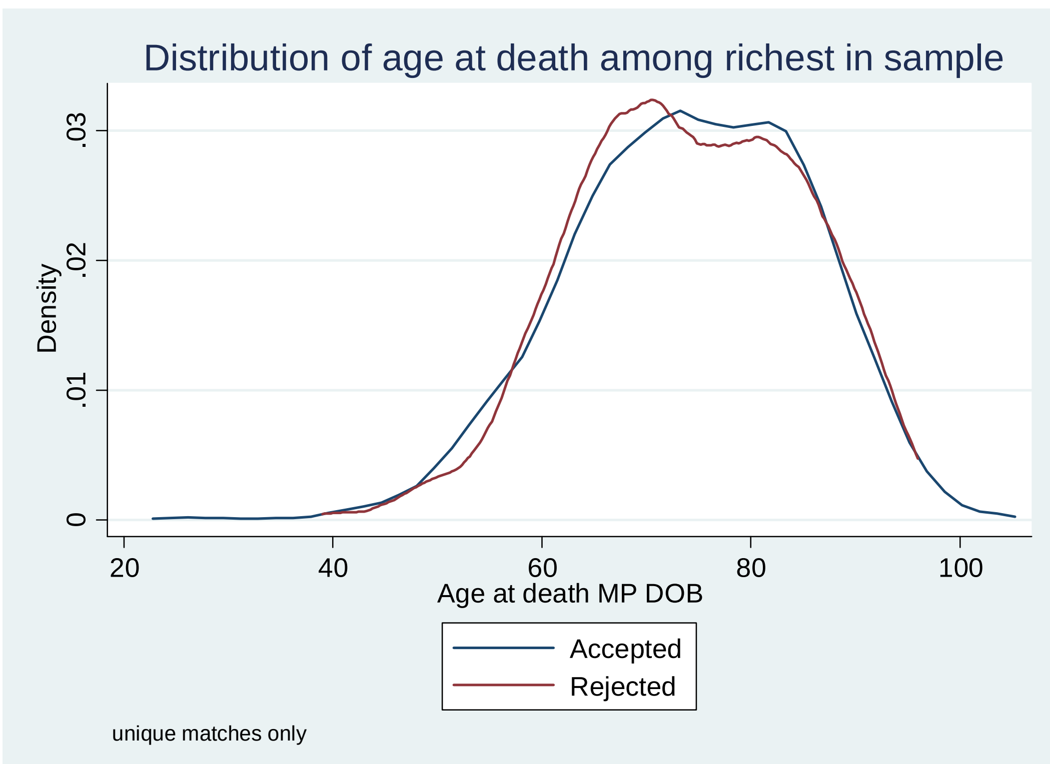
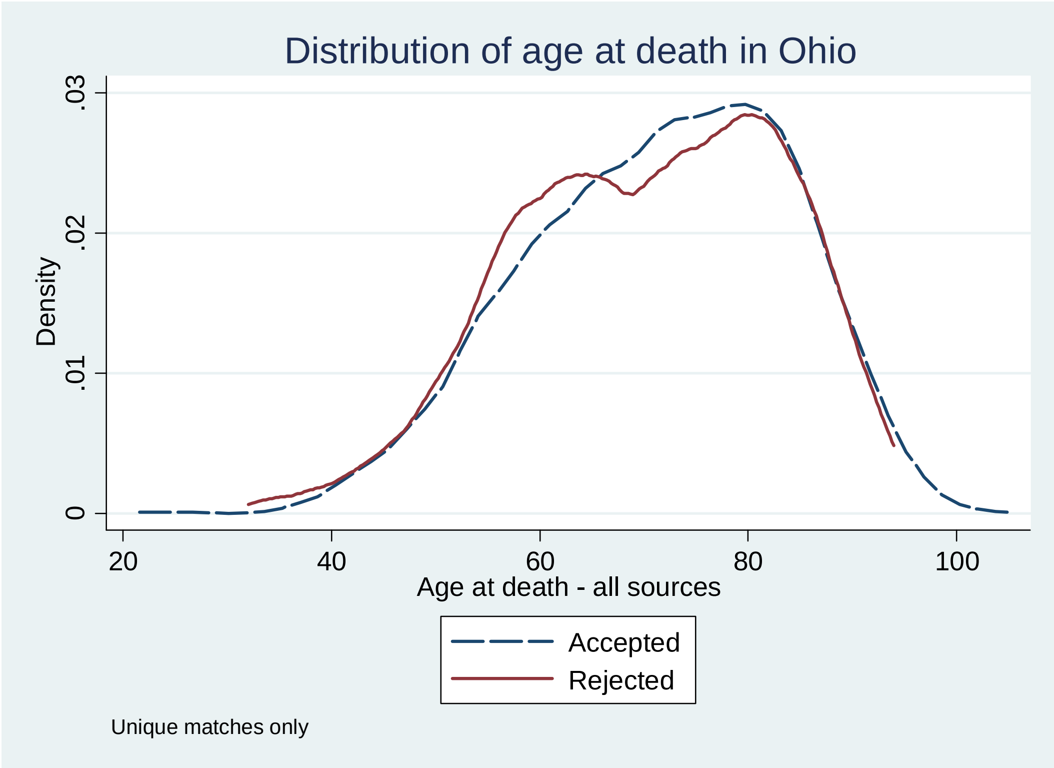


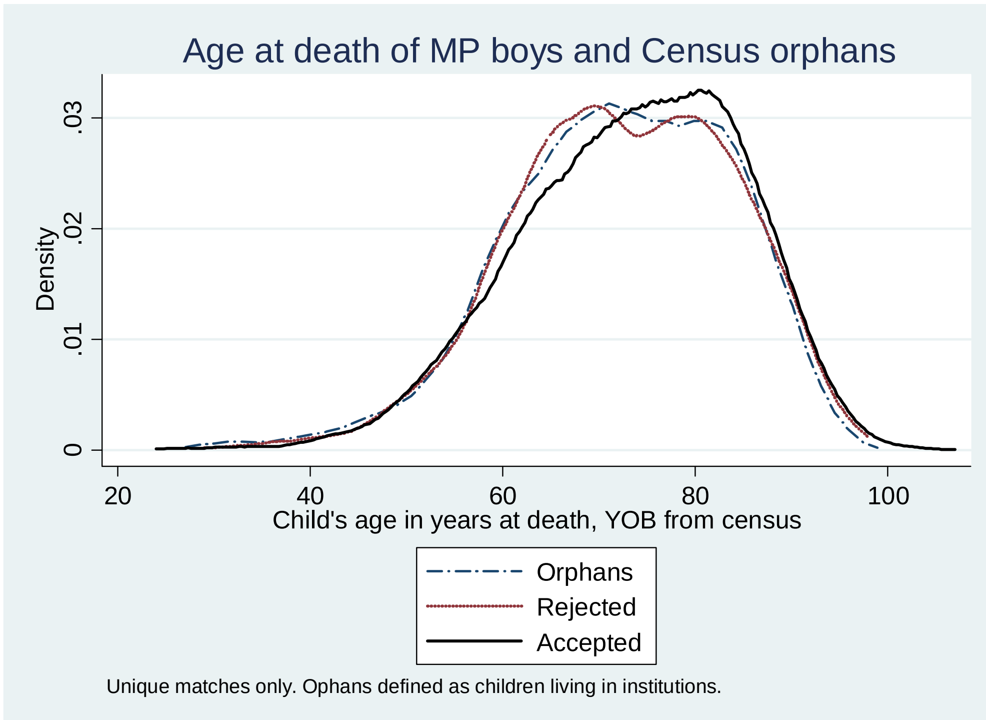
Fig. 4: Ohio, matching to additional death records.



Records matched first to Death Mortality Files (DMF). Unmatched records were then manually imputed by searching individual records in Ancestry.com. Unmatched records were then matched to Ohio and Illinois state death records.

Fig. 5: Alternative counterfactuals

Panel A: Orphans from the 1900-1930 Census



Panel B: States where single and divorced are ineligible

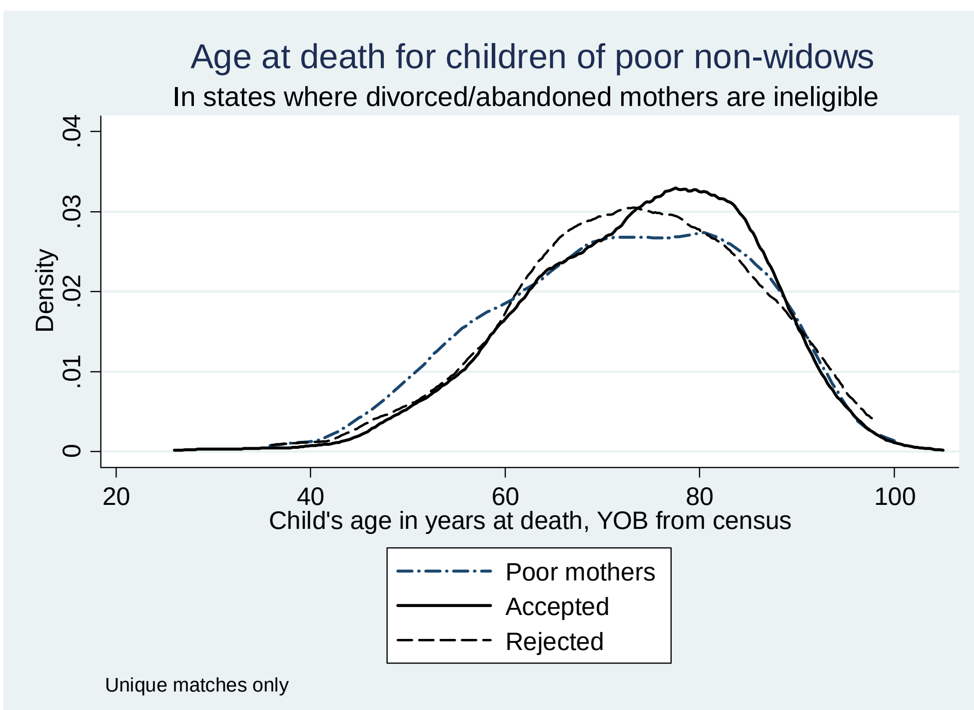
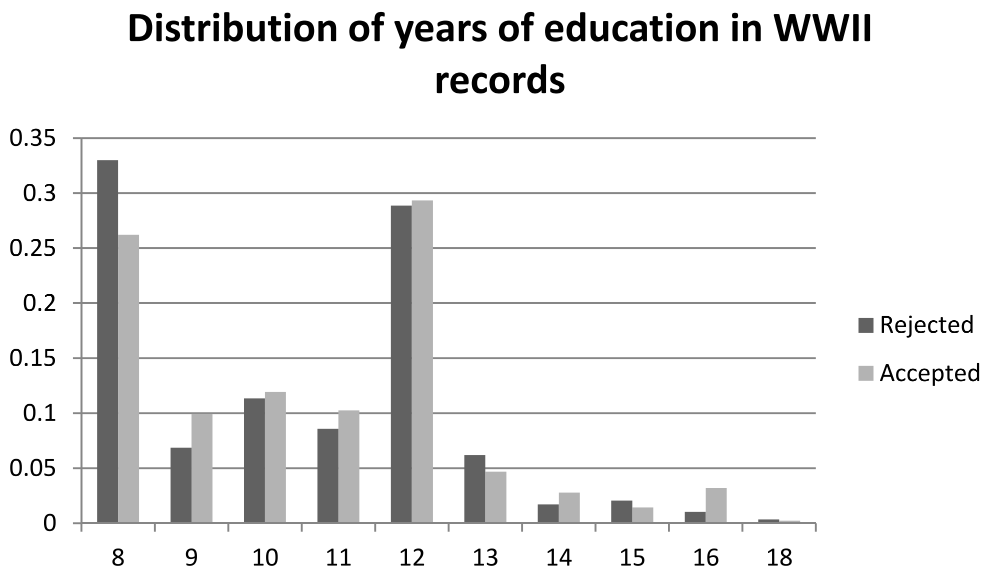


Fig. 6: Effect of MP on outcomes from WWII records



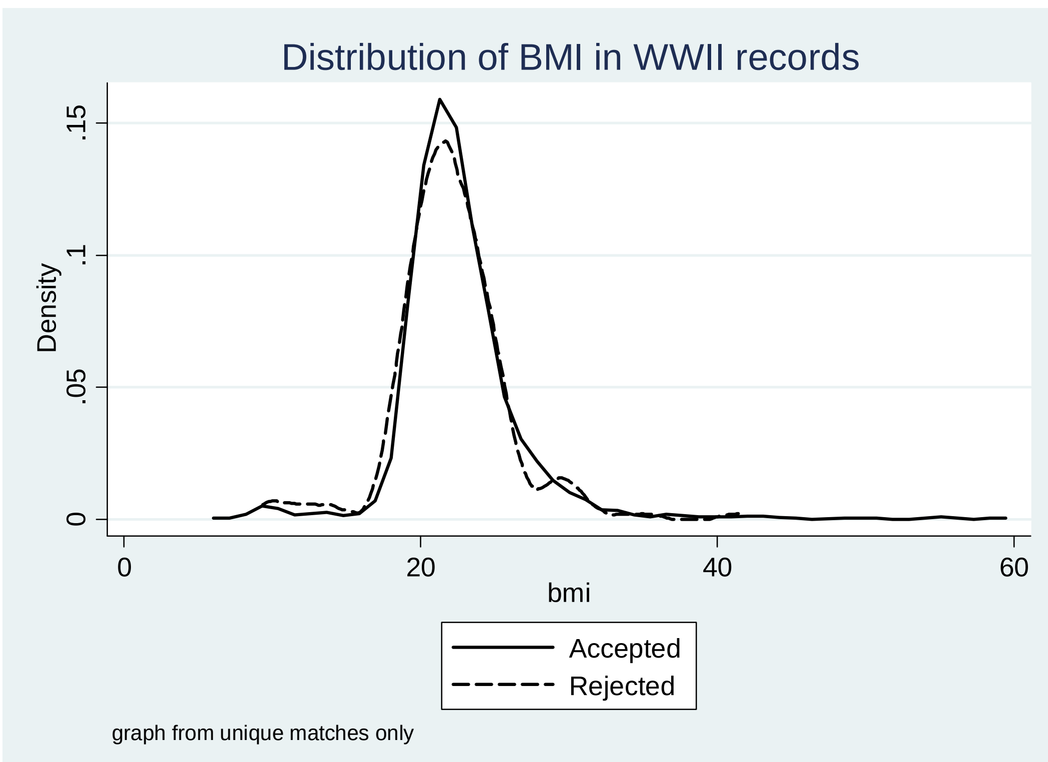
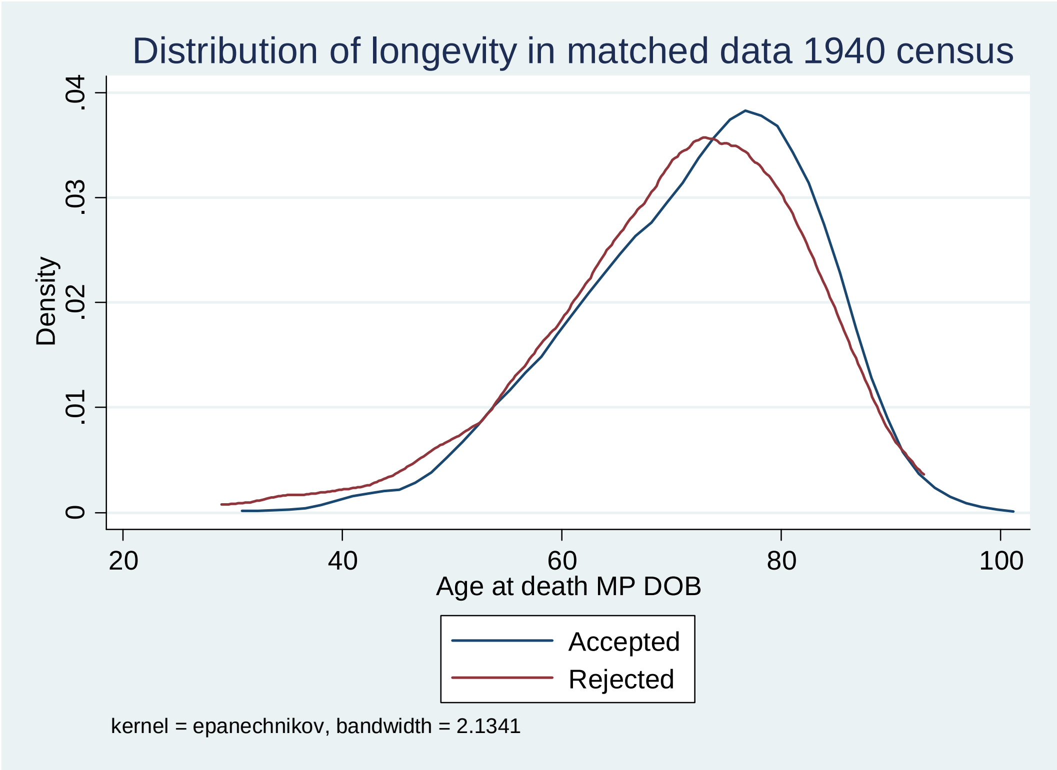
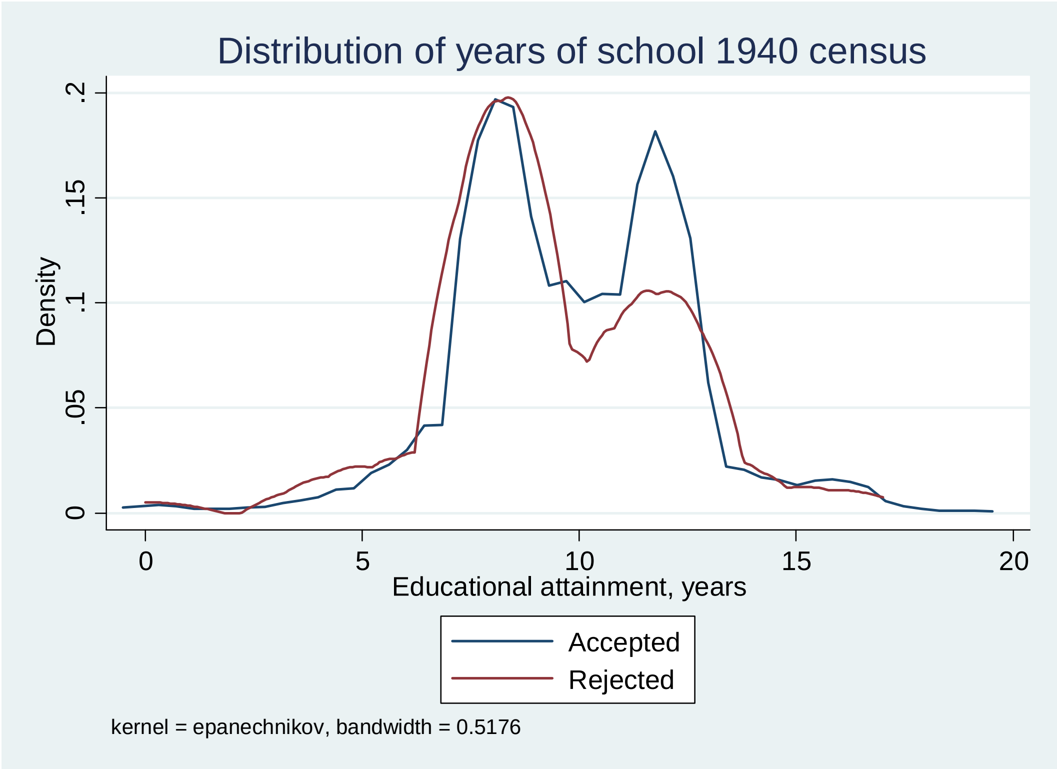
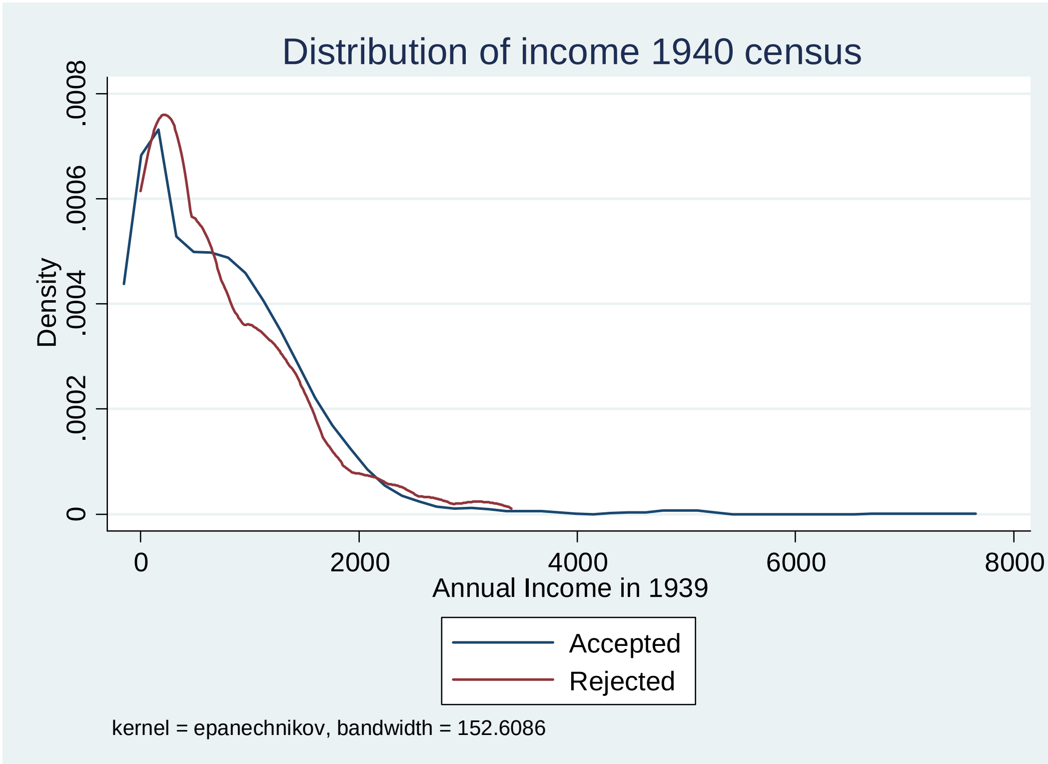
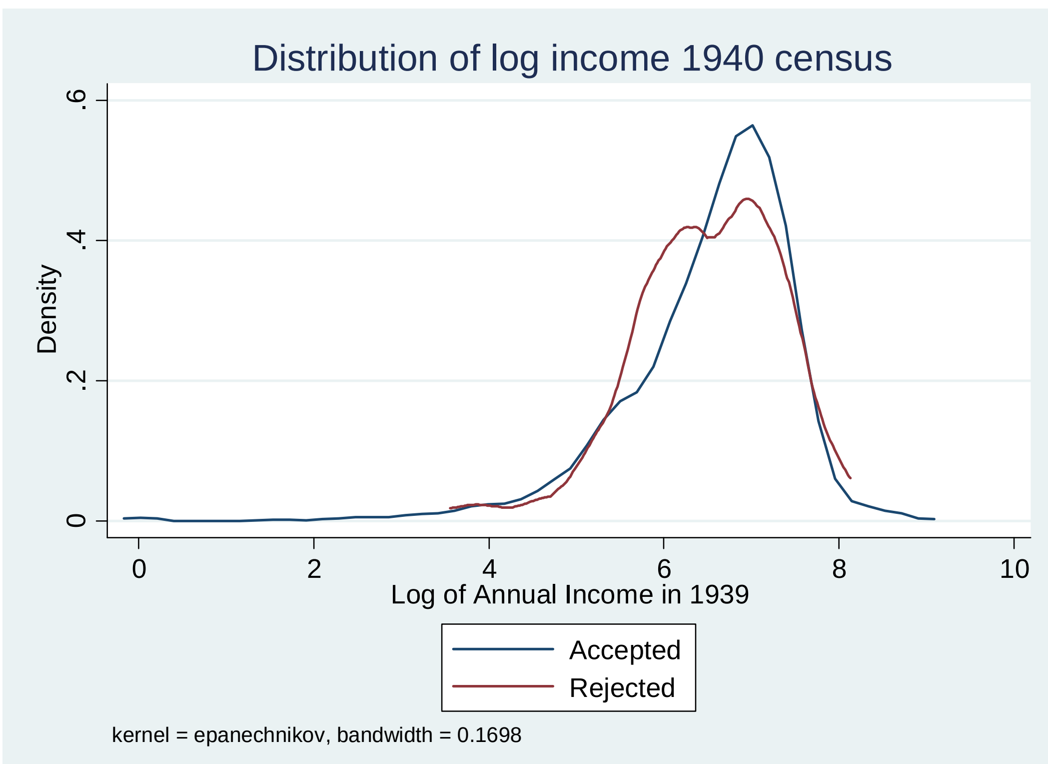


Fig. 7: Effect of MP on outcomes for 1940 Census Sample






Distribution of income 1940 census
Density
0
.0002
.0004
.0006
.0008
0
2000
4000
6000
8000
Annual Income in 1939
Accepted
Rejected
kernel = epanechnikov, bandwidth = 152.6086


Distribution of log income 1940 census
Density
0
.2
.4
.6
0
2
4
6
8
10
Log of Annual Income in 1939
Accepted
Rejected
kernel = epanechnikov, bandwidth = 0.1698