THE EFFECTS OF CHILDHOOD MEANS-TESTED CASH TRANSFERS ON MORTALITY*

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We study the Mothers' Pension (MP) program—the first government-sponsored welfare program for poor families with children in the United States (1911-1935)—to estimate the impact of means-tested cash transfers on children's longevity and labor market outcomes. We collected individual-level administrative records of MP applicants' children, and matched them to death certificates and WWII enlistment records. The impact of the program is estimated by comparing children of mothers who received transfers with children of mothers who applied for transfers at the same time and in the same county and but were denied. Using the population of boys born between 1900 and 1925 in counties with information on rejected applicants (N~16,000), we find that boys of mothers who were accepted lived about a year longer. The effects were larger for the poorest boys and for boys in states with generous but targeted legislation.

I. Introduction

The Census Bureau reports that more than 20% of children were living in poverty in 2010 in the United States.[1] Recent literature documents that early-life exposure to disease, nutritional deprivation and other factors can have adverse long-term effects on education, labor market outcomes, and ultimately, mortality (Almond and Currie 2011). In the United States and

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

elsewhere, welfare programs—broadly defined as cash transfers to poor and disadvantaged families—were established primarily to help children of poor families. But, while parental income is one of the strongest predictors of educational attainment (Barrow and Schanzenbach, 2012; Reardon, forthcoming) and adulthood health (Case et al. 2002), it is not known whether welfare transfers improve poor children's lifetime outcomes.

We study the Mothers' Pension (MP) program - the first government sponsored welfare program for poor families with dependent children in the United States (1911-1935), to provide the first estimates of the long-term effects of cash transfers (welfare) on children's health and human capital over their lifetime. The Mothers' Pension (MP) programs, first established in Illinois in 1911, were the first government (state) sponsored welfare programs for families with dependent children in the United States.[2] Although the MP programs reached a small number or recipients, they were generous: on average the family income of recipients was increased by 12-25% as a result of the program and transfers lasted a median of 3 years. Eventually, the MP program served as the basis for the federal program Aid to Dependent Children (ADC, later Aid to Families with Dependent Children, and now TANF). The intent of MP programs was to improve the conditions of "young children that have become dependent through the loss or disability of the breadwinner" (Children's Bureau, 1933). We investigate whether the program succeeded.

One of the main challenges in evaluating whether cash transfers improve outcomes is identifying a plausible counterfactual: what would children's lives have been like in the absence of receiving transfers? We estimate this impact by comparing children of mothers who received transfers with children of mothers who applied for transfers in the same county and year but were denied. This strategy of comparing accepted and rejected applicants for program evaluation has been used successfully in the 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 in unobservable characteristics. We show 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 rejected applicants were more often deemed to be in less need of assistance. Under the assumption that the groups differ similarly on unobservable

[2] Many developed countries instituted similar programs at the same time (see Appendix Table 2) which shows that many countries chose insurance rather than transfer schemes.

characteristics, outcomes for boys of rejected mothers provide an upper bound for what could be expected of beneficiaries. We attempt to construct alternative counterfactuals comparison groups to examine the robustness of our findings.

The second challenge is to obtain data to successfully observe welfare receipt and long-term outcomes for a large sample of recipients (and plausible comparisons). We collected individual-level data from administrative records on thousands of children whose mothers applied for MP benefits between 1911 and 1935 in 14 states. We matched these records to death certificates to determine the children's eventual age at death, and to WWII enlistment records to assess their education and other outcomes in young adulthood. Our analysis uses the population of boys (N~16,000) born between 1900 and 1925 living in counties that collected information on rejected applicants. Most members of these cohorts have died by 2012,[3] so we can evaluate the impact of the program on mortality and on their lifetime outcomes.[4] Our econometric methods explicitly account for attrition and measurement error in matching administrative databases.

We find that receiving cash transfers increases longevity by about a year. We also find that poorer children benefited substantially more than others, as did children in states with targeted but generous programs. These results are very robust to alternative specifications. 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 these as the effect of cash transfers alone. In kind transfers, such as Food Stamps and Medicaid, constitute a much larger share of total transfers for welfare recipients today (Currie and Gahvari 2008).

In young adulthood, acceptance into the MP program increased the fraction of boys having completed more than primary school. It also reduced the probability of being underweight between ages 18 and 45 by 35 percent, consistent with the mortality results, and with contemporaneous reports that malnutrition was common among MP recipients. The effects for labor market outcomes are ambiguous, showing insignificant but large effects in the likelihood of

[3] http://www.ssa.gov/OACT/NOTES/pdf_studies/study089.pdf

[4] In contrast, most of the individual records from the early years of the ADC program (which replaced the MP programs in 1935) have been lost. In many cases this occurs because the cases are part of department of Social Services records, which have very short retention schedules in most states. In Kentucky for example, the retention schedule is only 5 years after a case is closed. As a result ADC records from the 30s and 40s have mostly been destroyed.

a white collar occupation, but also large and insignificant effects on the log of occupational score—our proxy for labor market earnings.

We draw several conclusions from these results. The first is that poverty in childhood negatively affects important long-term outcomes, such as educational attainment and mortality. Second, cash transfers to the poor can substantially ameliorate early conditions enough to improve both medium- and long-term outcomes. However some medium-term effects are ambiguous, particularly labor market outcomes. Last, there is substantial heterogeneity in the effects based on program features and population characteristics, suggesting that legislation can be designed to maximize the effectiveness of the program.

II. Related Literature

Research on the effect of targeted income transfers on long-term outcomes is very limited (Currie, 1998). However, there are a number of other literatures within economics related to this research. These include: (1) theoretical and empirical research on the impact of early conditions on long-term outcomes, (2) estimates of the relationship between parental income and child outcomes, (3) the incentive effects of means-tested cash transfers, and (4) research estimating the short-, medium- and long-term effects of various public programs. We discuss each in turn.

Recent theoretical and empirical work on the development of human capital emphasizes the importance of conditions in early childhood in determining long-term outcomes. More specifically, the concepts of "dynamic complementarity" and "sensitive windows of development" developed by Cunha and Heckman (2007) and Heckman (2007) imply that investment in a child's human capital have a greater return if they occur early in life. Recent empirical work largely supports this idea. For example, Almond (2006) estimates large effects of *in utero* exposure to influenza on long-term outcomes, such as education and disability. Bleakley (2007) estimates large effects of a public health de-worming campaign in the American South on educational outcomes and adult income. Recent evidence from randomized trials with primates shows that deprivation in early life has large effects on long-term health (Conti et al. 2012). These studies and many others (see Almond and Currie 2011 for a recent review) suggest that there are very large returns over the long-run to improving *in utero* and/or childhood circumstances.

These findings raise the obvious question of whether income—and more specifically, a policy of cash transfers to the poor—can effectively ameliorate adverse life conditions to improve long-term outcomes. Indeed, parental income is one of the strongest predictors of educational attainment (Barrow and Schanzenbach, 2012; Reardon, forthcoming), child and adult health (Case et al. 2002). However, income is positively correlated with many other characteristics that are also positively correlated with child outcomes, such as parental education and marital status. It is not clear ex-ante clear that (exogenous) increases in parental income would improve child outcomes. On the one hand, increased income could increase expenditures on children, and/or allow parents to spend more time with children—the two main determinants of children's human capital (Becker 1993). However, parents who receive the transfers need not purchase goods and services that benefit children. This is in fact an important argument in favor of transfers in kind (such as housing and health insurance), which today constitute a large share of total transfers in the US (Currie 2007). Even if parents increase their spending on children as a result of the transfers, these expenditures might not translate into better outcomes in the long-run. For example, Jensen and Miller (2008) show that in poor settings, when income increases, the quality of diet can actually fall. Although not directly comparable, the evidence from *conditional* cash transfers (which typically condition cash transfers on children's enrollment and health care use) suggests that conditional cash transfers increase short-term enrollment and access to health. The long-term effects of these programs are yet to be studied, and the effect of the conditionality is not known (Fiszbein and Schady, 2012).

In their review of the empirical evidence on the impact of income (not welfare, per se) on child outcomes, Black and Devereux (2010) conclude that "there is still little compelling work on the direct causal role of parents' income on children's outcomes." Two studies find that increasing parental income through the tax code moderately improves short term education measures. Milligan and Stabile (2008) find that an additional $1000 in the Canadian child benefit increased the math and English test scores of 4-6 year old children, but by less than ten percent of a standard deviation. Dahl and Lochner (2012) find that an additional $1000 provided through

the Earned Income Tax Credit (EITC) increases child test scores by 6 percent of a standard deviation.[5]

Unlike these tax benefit programs, welfare transfers can create incentives for parents to modify their behaviors in order to remain eligible by, for example, remaining unmarried or out of the labor force (Moffit, 1992). These behavioral responses might have additional adverse (or positive) consequences on children, and they might strengthen or dampen the effects of the transfers themselves. For instance the effect of maternal employment during childhood (which welfare policies can affect) is greatly debated.

Welfare transfers can also be seen as "stigmatizing" and therefore potentially damaging. It could also be the case that children whose mothers receive transfers become better informed and comfortable with the welfare system, and thus more likely to depend on it as adults (Levine and Zimmerman, 2000). Finally, transfers might not make a difference if they are not sufficiently large (relative to needs), or if they crowd-out other public or private transfers. It is possible that total incomes might remain unchanged if family members, charitable institutions or other government programs lower their transfers as a result of the MP program.

This work also relates to existing research on the extent to which program effects either fade or increase over time and thus how we can evaluate them. In the short-run, the evidence suggests that welfare transfers have positive effects though there is large heterogeneity (Currie 1998). However, short- or medium-term effects need not match long-term impacts. Short-term effects might fade over time—for example many of the cognitive gains from Head Start were found to fade fairly quickly (Bitler, Domina and Hoynes, 2012, Lee and Loeb 1995). On the other hand, any returns to childhood investments might be delayed or not visible until much later, as is the case for in utero nutrition (Black, Devereaux and Salvanes 2007). Likewise, Chetty et al. (2010) find that long-term returns to small class sizes in early childhood far exceed the medium-term return, and Heckman et al (2010) find that early preschool programs appear to have large returns in adulthood despite evidence of minimal effects on schooling in the medium-term.

III. Mothers' Pension Programs: History and characteristics

[5] The EITC is not a pure income transfer, but rather a conditional one and previous work has found that it increases labor force participation among mothers.

The MP program was a needs-based program, established on a state-by-state basis between 1911 and 1931. It lasted until 1935, at which time 200,000 children were receiving MP benefits (Katz 1996).[6] Several factors prompted the enactment of the legislation. At the time, children of destitute parents were routinely sent to orphanages, and these children tended to fair very poorly.[7] MP programs were seen as a cheaper[8] and better alternative for children since it would allow mothers to care for their children.[9] Prominent judges of juvenile courts[10] argued that maternal absence was the main reason why many children became delinquent.[11] Preventing truancy and improving the welfare of mothers and children were also broad priorities in the progressive era. There also was a growing sense that poverty was not sufficiently well 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."

In addition to been poor, all laws conditioned eligibility on the mother's ability to properly care for her child. But program laws and implementation varied substantially. States had complete discretion in establishing the program, setting eligibility criteria and providing funding. For example, by 1931, four states had still not established an MP program.[12] MP laws only established guidelines—it was up to individual counties to create, fund and administer the program. As a result there is substantial within in-state variation in how policies were implemented. Below we describe how the programs in our 14 states varied in terms of

[6] The MP program was eventually replaced by the federal program Aid to Dependent Children, in Title IV of the Social Security Act of 1935.

[7] The conditions in institutions were also often questionable for children "the 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".

[8] San Francisco used to give institutions at most 11/month per child committed, widows MP by comparison was 6.25 a month. In general MP is about 1/3-2/3 of the cost of boarding. The 1922 Children's bureau report cites additional number that suggest that ex-post the cost of MP was indeed lower than that of institutionalization.

[9] White House Conference on the Care of Dependent Children came out with a strong recommendation to allow poor children to stay at home. "best person to care for a child, save in exceptional cases, is its own mother" (NYT may 11, 1913).

[10] Among the most notable judges that were actively supportive of the legislation were Judge Portfield of Missouri, Judge Wilbur of LA, Judge Pinckney of Chicago, Judge Neely of Milwaukee and judge Lindser of Denver (bullock 1915).

[11] Indeed some claimed that MP laws were responsible for a decline in juvenile crime (NYT January 11, 1915)

[12] These are Georgia, South Carolina, Alabama, and New Mexico.

eligibility, generosity, duration and conditions for receipt, for 1922, the median application year in our data (Table 1).[13]

Eligibility. All laws required the mother to be poor, but specific income thresholds were never specified. Some states limited the amount of property eligible applicants could own, though in most cases the law did not specify property limits. All states made widows eligible, but only four made deserted or divorced women eligible. Women whose husband was in prison or hospitalized were generally eligible, except in a few states. In practice (perhaps due to lack of funds) many counties deemed women with a single child ineligible (this was reported to be the case in Wisconsin and Pennsylvania). States varyingly guaranteed eligibility for children through age 14, 15, 16 or 18. Mothers typically became ineligible if they moved from the county or remarried, or if their husband returned, or if they otherwise found an alternative source of support. Most states required residency in the county or state. Citizenship was not required in most states, but even when it was, the intention to become a citizen was sufficient to qualify.

Generosity. MP programs were generally funded from local taxes, but in many states, the state was supposed to provide matching grants. As a result, the generosity of the transfers also varied. Most states' laws aimed to provide benefits that were deemed to be sufficient to allow for the mother to stay home and care for her children (Children's Bureau 1933), with the amount of the monthly pension increasing with the number of children in a non-linear fashion. The legislated maximum benefit for the first child varied across states from a low of 10 dollars in Iowa, to a high of 35 dollars in Ohio. In practice, generosity varied widely within states, as grants were often made after investigation of the family and based on standard budgets that computed the cost of children and other needs. For example in Ohio in 1925, the level of benefits for a family of 3 ranged from a low of $3 per month to a high of $38 per month.[14]

Duration. States in our sample varied in terms of the default duration of the transfers. In most states the transfers would be given until the pension was revoked but five states required

[13] Comparing the characteristics of the programs in our 14 states with the characteristics of MP programs in states for which we were unable to obtain individual records suggest that they are similar (Appendix Table 1) and thus the MP programs examined here are representative of the existing programs and the results generalizable.
[14] Author's computation using the MP records collected for Ohio. See Data section for details.

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. Some states had additional requirements for eligibility, beyond poverty. Pennsylvania required children to be in school.[15] 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. Minnesota also required mothers to speak English. Many laws explicitly required that the mother be of "good morals," for instance in Pennsylvania and Ohio women were not allowed to have unrelated men as boarders.

IV. 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 between 1911 and 1935 in 14 states: Colorado, Connecticut, Idaho, Illinois, Iowa, Minnesota, Montana, North Dakota, Ohio, Oklahoma, Oregon, Pennsylvania, Washington and Wisconsin.[16] 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. Appendix Table 1 shows how our data for 1930 compare to the published statistics for 1931.[17] Except for Colorado and Connecticut, for which very few counties are available, the average monthly grants are similar.[18]

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

[15] It is unclear whether these requirements were enforced always. But the 1934-24 study of counties conducted by the Children's Bureau suggested that at least in most counties studied schools were indeed contacted.
[16] We attempted to locate data for all states. See Appendix Table III for details on the status of data collection. Many records do not survive. For example Michigan records for Detroit and Grand Rapids were lost in a fire after being transferred to the state archives in Lansing in the early 1950s. The New York City records were destroyed by Social Services due to the guidelines of their retention schedules.
[17] Published statistics by state are available in 1921 for selected states, and in 1928, but this publication's tabulations are substantially less detailed and comprehensive than the 1931 data.
[18] The published data report amounts for all children receiving pensions. We do not observe the duration of transfers for a large portion of our sample. We report amounts for new beneficiaries which constitute a substantially smaller sample and are not entirely comparable to the stock of beneficiaries.

(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 ended. For a single county (Clay, MN) we report data on a detailed 1930 study of 62 families in the MP program. We only collected county records in which the children's dates of birth were available—matching individuals without their date of birth proved to be very difficult (as we demonstrate later using census records).[19]

Because the Great Depression negatively affected local finances, many counties discontinued their MP programs beginning in 1929, only to have the federal government replace Mothers' Pensions with the ADC program in 1935. As a result, in our analysis we restrict our attention to transfers prior to 1930.

Table 2 shows summary statistics by state for the data on all the recipients born between 1900 and 1925 who applied before 1930. We drop individuals with no date of application or transfer amount. Our data contains information on approximately 19,000 families and 52,000 children who received transfers between 1911 and 1930. We also have data on rejected applicants and their characteristics for as many states, often with a reason for rejection, though they constitute a small share of our data (about 12%).

To better understand the generosity of the benefits, in Appendix Table 4 we compute how the monthly transfers compare to the average wages in manufacturing in the state. The average MP transfer was equal to 17% of manufacturing wages, ranging from 9% in Pennsylvania to 29% in Iowa. We have a few other estimates of the size of the transfer relative to resources. MP transfers represented an even greater share of maternal income. For example, in a subsample of counties in Illinois we observe MP transfers are about 29% of the median maternal weekly income of $14. A detailed 1926 study of the budget of 2,500 MP families (by the Mother's Assistance fund of the Pennsylvania Department of Welfare) revealed that on average, MP transfers accounted for 39% of total family income. All of our estimates suggest MP transfers increased family income substantially. If MP transfers crowded alternative sources of funds (from municipalities, private charities or family members), family incomes might have increased less than what we estimate.

[19] In our early attempts to match MP records to other databases, the match rate was substantially lower for individuals for whom only age was available.

The historical evidence does not support the idea of a strong crowd-out[20] but if there is crowd-out, this would lead us to find no effects of the transfer.

Overall the evidence suggests that MP transfers were generous relative to the level of income of the recipients. The level of generosity of MP transfers is higher than that of conditional cash transfer programs in developing countries, which range from 0.6% in Bangladesh to about 33% in Mexico, with many programs in the 5-15% range (Table 3.2, Fiszbein and Shady 2009).[21]

b-Sample selection

For this analysis we keep only males born 1900-1925, under age 18 at the time of application and whose mothers applied to the program prior to 1930. We limit analysis to cohorts born before 1925 because they are more likely to have died by 2012. We drop individuals without a year of birth or without a year of application and those with no first or last name. We keep only states and counties where data on rejected applicants is available. Finally we limit our analysis to males: matching females based on names is substantially more difficult than matching males because they typically changed their name when they married. The final sample includes 15,822 males in 60 counties from 11 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 23 percent in Ohio, which accounts for 23 percent of our records (see Table 2).[22]

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

[20] For example the 1922 investigation of practices across the country showed that many families were receiving supplementary aid in addition to MP, either from other state sources or from private charities. In fact MP agencies often collaborated with private charities to ensure that MP families receiving inadequate MP transfers would get additional aid from charitable sources. In Pennsylvania, the 1926 survey of families receiving MP pensions showed that 11% of families were receiving additional aid from private charity (Lundberg 1928).

[21] In real terms (1982 dollars), MP transfers were half the size of 2010 transfers, (Appendix table 4). 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. Figure 1a shows that states that were (relatively) generous in 1930 are also more generous today.

[22] In Ohio, which constitutes 38 percent of the estimation sample, the rejection rate was 16 percent.

has increased over time and has been near complete for older individuals since the early 1970s.[23] We matched individuals based on their first name, last name, middle name, and day, month and years of birth. (Details of the matching procedure are in Appendix I.)

Figure 3 shows the distribution of the number of matches in our estimation sample. We were able to match about 48% of our sample to a unique SSA death record. About 4% have multiple matches, and 48% have no match. Thus we have information on age at death for 52% of our sample. Using life tables, we computed the number of individuals that would be expected to die prior to the existence of comprehensive DMF data (beginning around 1975). These calculations suggest that about 32% 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 84% of the individuals whose death records should be in the DMF.[24] However Figure 3 shows that we are more likely to find boys whose mother were accepted into the MP program. We return to this issue later.

d-Other state and county data

We also include as controls various state and county characteristics of the MP programs. These include whether citizenship is required, the length of residency in the county required for eligibility, the maximum amount to be given for the first child, the amount to be given for an additional child, if available, (all of which varied over time), as well as whether the transfers were conditional on school attendance, maternal work or maternal non-work. We also include characteristics that we believe might have affected the existence or generosity of the program.[25] These include 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. For Ohio we were also able to obtain county-level

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

[24] For comparison, we computed similar statistics 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 whose mothers received welfare during their childhood, and within 20 years about 40% 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. For the PSID, we limit the sample to boys born between 1951 and 1968 and present in the household in 1968.

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

expenditures for several years. These include total expenditures on relief, total expenditures on outdoor relief, and total expenditures on children's homes. See Appendix II for details.

V. Empirical Strategy and identification of the effects on transfers

a. Basic empirical model

To estimate the effect of cash transfers on outcomes we begin with the following regression specification:

$$P(survived\ to\ age\ a=1)_{iftcs} = f(\theta_0 + \theta_1 MP_f + \theta_2 X_{if} + \theta_3 Z_{st} + \theta_c + \theta_{t\ +\varepsilon if}) \qquad (1)$$

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)*. The coefficient of interest, MP_f , can be defined as an indicator for whether the child's family received MP benefits, and X is a vector of relevant family and child controls (marital status, number of siblings, etc), and year of birth and age at application. We can also control for county-level characteristics 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 is identified by comparing accepted and rejected children 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.

b. Identification strategy: Rejected applicants

Mothers who applied for but were denied MP are likely to be similar to those who applied for and were granted the transfer, relative to a mother in the same economic conditions that did not apply. 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. To assess the comparability of these groups we investigate reasons for rejection and we compare the observed characteristics of accepted and rejected applicants.

The most common reason for rejection was insufficient need, in which case the rejected applicants represent an upper bound on what could be expected of the beneficiaries. Other less common reasons for rejection include ineligibility due to insufficient length of residency and non-citizenship (Table 3). Case studies from specific counties suggest similar patterns. 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, 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, though if anything the rejected seem better off, consistent with the reasons for rejection. On average, rejected applicants were slightly older, came from somewhat smaller families, and the average age of the children in the family was higher, particularly the age of the youngest child (Table 4). There is also a difference between the groups concerning the marital status of the mother: among the accepted, there are more children of widows and fewer children of mothers with unknown/unreported marital status. Interestingly, the information about the date of birth of the child is more likely to be missing among accepted children, suggesting their mothers were more likely to be illiterate (recall we collected data only from counties where this information was systematically collected). Thus based on these characteristics, accepted applicants appear to be slightly worse-off than the rejected applicants.[26]

In Table 5 we present estimates of who in our sample is accepted and how generous the program was to those accepted based on observables. The models include all individual characteristics, state characteristics at the time of application, county and cohort dummies. The results show remarkable consistency. 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.

These results are consistent with the idea that those in need were on average given more aid. On the other hand, divorced women were less likely to be accepted and systematically helped less, relative to widows. Although not always significant, the results suggest non-widows were also somewhat less likely to be accepted and less likely to be given generous amounts. Historical evidence also documents cases of many women deemed "unworthy", and there were

[26] The county characteristics, as measured in 1910, are roughly similar for the accepted and rejected applicants (Table 4, Panel B). Since we restrict our sample only 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, but otherwise the characteristics are similar.

many concerns about the morals or level of need of certain groups. In our records one woman in CT is denied transfers because she had an abortion. In the Clay county records, continuation of the transfers is conditioned on "not having men around the house". Table 3 shows some systematic evidence from our records that these were indeed reasons for rejection. However such cases are a small share of all cases.

One similar 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 U.S. Department of Labor determined that 96% 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 in significant numbers (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 pursue different strategies that suggest this is not 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 found among the rejected. Second, we attempt to identify the race of accepted and rejected applicants by linking them to their census records and/or inferring the race of the applicant based on name (following the methods of Cook et al. 2012) making use of the fact that some names were historically more commonly used among blacks. When we do, we find that blacks are not disproportionately represented among the rejected applicants.[27] Third, we present results in which we drop from the sample all counties with a disproportionate share of black residents and present results for Ohio only, one of just two states (Pennsylvania being the other) in which blacks appeared to receive MP benefits at appropriate rates (Children's Bureau 1933). Finally we can observe race for the subset of our sample found in WWII records.

[27] In 1931 in both and Pennsylvania and Ohio about 5% of recipients were black—and the percent black in the population in 1930 was about the same, although blacks probably made up a larger share among the poor (Children's Bureau 1933, table A II). However in some counties in Pennsylvania (Allegheny County and Philadelphia County) and Ohio (Montgomery) the share of blacks in the MP records was known to exceed their representation in the census. About 50% of our sample comes from these two states, and we have a few years of records for both of the Pennsylvania counties. Thus overall blacks are likely to be over-represented in our data relative to the MP population. However it is difficult to confirm this, we collected data on race whenever available but we have only a handful of observations because the data was not collected at the time—the 1933 Children's Bureau Report notes that a large number of counties and several states did not collected or report race, therefore even the numbers published then are also based on a small subset of counties.

c-Other econometric issues

Our data come from matched administrative sources. These data pose some challenges. First, there are a non-negligible number of MP records for which the outcomes of interest are missing because a match could not be found—this is the standard problem of sample selection. Second, among those records that we do find, a minority have multiple possible matches (and thus outcomes). Last, there is also measurement error in matching: even if we find a unique match, we do not know for sure that it is the correct match. To address these issues we follow the methods described in Bugni, Honoré and Lleras-Muney (2013).

We can address sample selection by estimating bounds. The theory for computing bounds when there is missing data on the outcomes is well-known. We can improve on these bounds by making use of the information from multiple matches and allowing for measurement error. However without making any parametric assumptions, bounds are typically large and uninformative.

The second approach consists of approximating the process that leads to missing observations, no matches, multiple matches or erroneous matches, by a reasonable parametric model. First assume there are multiple matches but no measurement error (observed individual characteristics are not sufficient to uniquely identify individuals). We model the correct matches as being generated by $f(.;\theta_1 | MP, X)$ the probability model in equation *(1),* and the incorrect matches as being generated from a different distribution $g(.;\theta_2 | X)$, which is an unknown function that does not depend on *MP*. Suppose that there are two possible matches for observation *i*: Y_{i1} and Y_{i2}. Then we can write the likelihood function for this observation as:

$$f(Y_{i1};\theta_1 | MP_i, X_i)\, g(Yi_2;\theta_2 | X_i) + f(Y_{i2};\theta_1 | MP_i, X_i)\, g(Y_{i1};\theta_2 | X_i)$$

where the first term corresponds to the case where Y_{i1} is the correct match and the second term corresponds to the alternative. The entire likelihood function can be derived by generalizing this approach to the case with multiple matches. Note that we can estimate g separately and non-parametrically, and thus this model is quite flexible, but it does rely on the functional form for *f()*.

To add measurement error, we assume there is a constant but unknown probability that a match is a correct match. Alternatively we can make this probability a function of the quality of

the match, if such information is available. We can then re-write the likelihood function weighting each "piece" by the probability that it is the correct case. In the case of two matches, there would be three possibilities, each with its associated probability: match one is correct, match 2 is correct, or neither match is correct. Bugni et al show that this method improves upon the standard approach which consists in estimating models using unique matches only: by making use of the multiple matches we can gain efficiency, and using the parametric model we can allow for measurement error.

This approach is very parametric and uses the logit. It does not allow for the use of instruments. Alternatively we implement a GMM approach that account for multiple matches to OLS or Instrumental Variable models. In particular we want to estimate AFT models of survival like

$$Log(Age\ at\ death)_{ifts} = \theta_0 + \theta_1 MP_f + \theta_2 X_{if} + \theta_3 Z_{st} + \theta_c + \theta_{t\ +\varepsilon if} \quad (2)$$

where the dependent variable (y) is the log of the age at death for a given individual, and all other explanatory variables are defined as in equation (1). Abstracting from attrition (non-matched recipients), the identification conditions can be expressed as a set of moments conditions $E(Xy)=E(X'X)\theta$ that identify our parameter of interest θ. Bugni, Honoré and Lleras-Muney (2013) show that we can compute these population moments using the matched data with multiple matches.

The main limitation of this approach is that we do not observe the age at death for all the unmatched records, and it is difficult to assess the impact of sample attrition in this case. The survival analysis however provides a reasonable alternative based on life tables: we can impute missing deaths by assuming that that missing records correspond to deaths prior to DMF. We present our results for both survival and age at death, and making various assumptions about attrition, which we investigate next.

Attrition

What is crucial for our identification strategy is to determine is whether accepted and rejected individuals are missing at different rates and whether they have different characteristics. Table 4 presents some evidence on this issue: it shows the summary statistics for the samples with death certificate matches and for the sample with unique matches. Although the samples decrease substantially in size, the characteristics of those individuals we find are very similar to the

characteristics of those we do not find, or of those with multiple matches for both accepted and rejected individuals.

We test for differential attrition and matching across accepted and rejected more formally in Panel A of Table 6. Accepted individuals are about 8% less likely to be missing. In other words we find them at higher rates whether or not we control for covariates. Without knowledge of the reason why we are not finding the records, this result is consistent with (1) lower mortality rates among the accepted, and (2) identical mortality but rejected children being harder to find. We return to this issue in our estimation. Among those we find, accepted individuals are not statistically more likely to have more than one match but we nevertheless investigate the robustness of our results to the use of these multiple matches.

VI. Mortality results

a-Main Results

Preliminary evidence of the effect of acceptance on mortality is presented in Figures 4A and 4B. These plot the density of the age at death for accepted and rejected applicants, using all matches (Figure 4A), and using unique matches only (Figure 4B). 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 lives. The largest differences are observed between ages 60 and 80, where the distributions are the densest.

Table 6 presents the results of estimating survival models using our ML estimator, and AFT models using our GMM estimator. These estimations use all matches, and for survival we assume those without matches died prior to the mid-1970s. In panel A, we estimate the probability of survival past age 60, 70 and 80. Column 1 includes only state and cohort dummies, column 2 adds all individual controls, county characteristics in 1910 and state characteristics at the time of application. Column 3, includes cohort dummies. The results show a positive and significant effect of acceptance on survival past 70 and past age 80, and a smaller but positive effect for survival past age 60. Using the date of birth from the death certificate instead of the one from our records (column 4), dropping those without age at death (column 5), or using unique matches only (column 6) does not appreciably change the results. In the last column we drop counties with a share of blacks in the top quarter for the state (never higher than 5 percent). The effects remain positive and are somewhat larger for surviving past age 60. The effects

appear to be quite robust to these alternative specifications and samples. The implied marginal effects suggest statistically significant increases in the probability of survival past age 70 (10 -20 percent), and in the probability of survival past age 80 (9-15 percent).

We also estimate an Accelerated Failure Time hazard model in Panel B, which consists of regressing the log of the age at death on covariates, and using all matches, but not imputing age at death for missing records. Again the coefficient on acceptance is positive and significant in all specifications. The implied effects are large: acceptance increased life expectancy by about a year, relative to a mean of 72.48, among the rejected with unique matches. The estimates range from .7 to 1.35 years of life.

b-Robustness

We conduct several additional robustness checks and present the results in Table 8 for the probability of surviving past age 70. Panels A and B show these results. The results are robust to using of Logit or AFT specification and to using only unique matches; using the best matches; using all matches; allowing for measurement error; matching accepted and rejected on propensity scores; or 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.

Our final set of robustness checks involves abandoning the arbitrarily chosen cutoffs of ages 60, 70 and 80. Instead, we estimate our survival model using ML and the fully saturated specification for each age at death between 58 and 88, which correspond to the 10^{th} and 90^{th} percentiles of the distribution of the age at death. Figure 5 shows the marginal effects as a percentage of the survival rate of rejected applicants, computed using coefficients from estimations 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.

c-Results for Ohio

We also present separate estimates for the state of Ohio, which makes up 38 percent of our estimation sample (Panel C of Table 8). As the 1931 Children's Bureau study reported, there was no evidence of discrimination against black women in Ohio's MP program. The estimates are unchanged when we limit the sample to the state of Ohio, suggesting 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 if there are other sources of support in the community. When we include these as controls, the results are unchanged. They are likewise unaffected by dropping the missing instead of imputing them as dead.

Third, Ohio maintains death records going back to 1958—we matched Ohio boys to these earlier state records. We also attempted to manually find unmatched records in Ancestry.com, a database that also includes deaths from WWII and other wars, as well as additional state records. Thus we can examine our assumption that those not found in the DMF records died prior to the mid-1970s. Figure 3 Panel C shows that we increase our match rate to about 60%, but as the graph shows (and panel C of table 6 confirms) we continue to find death records for accepted applicants at higher rates—suggesting that the difference does indeed corresponds to a real mortality effect.

The state records also show that the age at death among rejected applicants whom we did not find in the DMF records is 66.28, and the age at death among accepted applicants is 67.16. About 60 percent of the newly found death records show deaths prior to age 70, but only about 30% died before age 60. We draw two conclusions from this. First, that the assumption that the missing are dead is reasonable for survival past 70, but not for survival at younger ages. However, this does not appear to affect our results. Rather, the earlier state records showing a difference of one year of life between accepted and rejected applicants is exactly what we estimate using the full data (Table 8, Panel C), suggesting that our inability to link the MP applicants to death records prior to the mid-1970s is not affecting our results.

d-Heterogeneity

We next explore heterogeneous treatment effects. First we look at whether all groups that were targeted by the legislation benefitted. Mothers who were abandoned or divorced appear to have been less likely to be accepted into the program, and there were many states in which they were ineligible. Widows on the other hand were the primary target of the laws, and there was little controversy about their eligibility. In table 9, we show however that children whose fathers were away (abandoned, imprisoned or disabled) received the greatest benefits.

To understand these results we investigate the income and wealth of these groups using the 1915 Iowa census—the only data with family earnings prior to 1915. Figure 6 and Appendix Table 6 shows that children of widows were on average more similar to children in two parent families and they are substantially better off than children of abandoned/divorced/single women. These results suggest that the benefits were larger for these subgroups because their marital status is a better indicator of poverty than widowhood.

Next we stratify by family size: larger families were more likely to be accepted and were poorer. The historical record suggests that in many localities women with a single child were systematically turned down for help. We estimate separate effects for single children, those in families with 2-3 children, and those in larger families. There are no differences in the effects.

Previous literature suggests larger effects of investments early in life. Our evidence also shows that families with younger children were more likely to be accepted and the legislation did not always make teenagers eligible. Table 9, Panel A shows the results when we split the sample into 3 age categories: 0-4 years, 5-11, and 12+. Our results do not suggest larger effects for younger children but rather substantial benefits throughout the age distribution—though these results are somewhat sensitive to what we assume about missing data.

We also explore the extent to which different MP program features influence the impact of the program on mortality to learn about the type of legislation that appears to have been most successful. We find that requiring work had no differential effect on children's outcomes, although the effects are somewhat larger for this group—given how poor these groups were, the results suggest that prohibiting maternal work was detrimental to children, but even when work was not regulated children benefited from the transfers.

When we stratify by whether the MP legislation requires periodic reapplication, interestingly, we find that reapplication is associated with lower program effects. Finally, we find that in states where children were eligible until a later age, the estimated effects of program receipt are greater. Altogether the results suggest that generous, but stringent and targeted, transfers worked best.

e-Alternative counterfactuals from the 1900-1930 censuses

The main concern with our results is the comparability of the rejected and accepted applicants, in particular whether they differ based on unobserved characteristics. Despite the fact that we

observe that on average accepted children are slightly better off, it is possible that on unobservables these children were from worse-off families. We attempted to construct alternative comparison groups by linking children of poor mothers from the 1900, 1910, 1920 and 1930 censuses to their death certificates. This exercise is difficult for two reasons. First there is very little information about the socio-economic status of women who are not married: prior to 1940 the census does not report education or earnings, and although occupation is available, very few women worked.[28] Also the Iowa census data suggest that a randomly chosen widow might not be very poor, but we have no way of identifying who is poor (though we condition on living in a poor minor civil division, see Appendix I). Second, none of the census reports exact date of birth: 1900 and 1930 report year and month, and 1910 and 1920 only report year of birth. As a result it is substantially more difficult to find these individuals in death certificates. For this reason we allow a "softer" matching criteria for the census.

We selected all children under age 18 of women living in poor areas, and included black families and all children living in institutions. We further selected children by matching their characteristics to those of the MP applicants using propensity scores (see Appendix I) and matched them to death certificates. Appendix Table 7 shows that despite our best efforts, our census samples differ substantially from MP children on observables: they are older when we observe them, they are disproportionately drawn from more recent cohorts, and they come from smaller families. There is also more measurement error for these samples in the age at death. Panel D of Figure 3 (as well as the Table) shows that we match substantially fewer of these children to death certificates (in particular blacks).

With these caveats in mind we present here suggestive comparisons with two possible alternative counterfactuals from the census. Panel A of Figure 6 compares the age at death of our MP children with children living in institutions (which we label as orphans, although the reason for institutionalization is unknown). Historically this is the counterfactual that MP programs wanted to replace; that is, the program meant to prevent institutionalization of children by allowing them to remain home. The Figure shows that rejected applicants are very similar to orphans, and both groups lived shorter lives than accepted applicants. In Panel B, we compare MP children to children of single or divorced women in the census, in states where these women

[28] Labor force participation among women in 1910 in the US is about 22% (Olivetti 2013) and this number is substantially lower among those with children (NEED CITE).

were not eligible for the MP program. We drop widows from the accepted group and control group, so we are comparing children whose fathers are disabled or institutionalized with children of single or divorced women. Again the figure shows that children of accepted women fared better than children of single and divorced women.

VII. Results on educational attainment, health and labor market in the medium-term

We can match the MP records with WWII enlistment records for individuals who enlisted in the Army during 1938 -1946.[29] For all enlistees we observe education, marital status, civilian occupation, 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).

These data have two issues. First, our match rate is quite low—lower than our match rate for mortality. (Compare Appendix Table 6 and Table 4.) Match quality is low because WWII records do not contain date of birth, though they contain state of birth, which we match on. Second, the WWII records are not a complete or random subset of the male population of males because of induction rules and exemptions. Appendix Table 6 show that our matched sample is substantially younger—this is to be expected because males aged 18-25 in 1942 served at much higher rates than older men (Hogan, 1981). The WWII samples are not otherwise substantially different. As with the mortality analysis, we find that accepted applicants are more likely to be matched with the WWII records (Table 6, Panel C). Perhaps they were more likely to serve—ultimately we cannot assess why we find them at higher rates.

We estimate effects both without controls (Table 10, column 1) and with a full set of controls, which include county and cohort fixed effects, individual characteristics and state characteristics at the time of application (Table 10, column 2).

Preliminary evidence of the effects on educational attainment is shown in Figure 8: children of accepted families are more likely to have more than 8 years of school. In Table 10 we report the point estimates. Accepted applicants are 20 percent less likely to have exactly 8 years of school—no one in these records is coded as having fewer years of schooling. MP receipt increases educational attainment by 0.1 years, but this effect is statistically insignificant. When

[29] Enlistment records are available for 9 million (of the 16.5 million) individuals who served in WWII.

we estimate a censored model to account both for censoring at 8 and for the fact that some are still in school, the coefficient becomes 0.3 and it is marginally significant.[30] These results are suggestive but the magnitudes are somewhat sensitive to controls.

Figure 8 shows that MP receipt also reduces the probability of being underweight. Estimates in Table 10 imply a statistically significant 50% reduction. The estimates for height, weight and BMI (measured continuously) are positive but not significant. The results with and without controls are very similar, though for some outcomes, the results with controls are slightly smaller, though not statistically different.

The last panel looks at labor market outcomes. We do not find large or significant effects with respect to occupation score, and in fact the coefficient is negative and large in magnitude. On the other hand we find positive and large effects of the probability of having a white collar occupation. These results paint a mixed picture of labor market outcomes in young adulthood of MP recipients. It is possible the sample is too small.

In the last row we show that, in WWII records, accepted applicants are more likely to be black. Although this difference is not statistically significant it suggests again that in our sample black children are not over-represented among rejected applicants.

Based on OLS estimates of the impact of education on mortality from Cutler and Lleras-Muney (2008), an increase in schooling of 0.25 years should result in a 0.15 year increase in longevity. Underweight in adulthood is associated with increased mortality: the relative risk for underweight individuals ranges from 1.38 to 2.3 relative to those with normal BMIs (Flegal et al. 2005), though it is not clear how to estimate the impact on longevity. But these estimates are consistent with the large health benefits of MP transfers on longevity.

VIII. Interpretation and policy relevance

We find that poor children receiving cash transfers in childhood live about a year longer as a result. These results are very robust to alternative specifications. Because, until 1950, income transfers were the *only* major public benefit for which poor children were eligible (with the exception of public schooling) we can interpret these as the effect of cash transfers alone. (In

[30] Educational attainment is censored from both the left because no one is listed as having less than 8 years of schooling, and from the right because many reported they were students prior to enlisting.

kind transfers such as Food Stamps or Medicaid constitute a much larger share of total transfers for welfare recipients today, Currie and Gahvari 2008.) We also find that poorer children benefited substantially more than others, as did children in states with targeted but generous programs.

In young adulthood, acceptance into the MP program increased educational attainment by 0.2 years. It reduced the probability of being underweight between ages 18 and 45 by 35 percent, consistent with the mortality results and from contemporaneous reports that malnutrition was common among MP recipients. The effects for labor market outcomes are insignificant and ambiguous, showing insignificant but large increases in the likelihood of a job market occupation, but also large and insignificant effects of the log of occupational score—our proxy for labor market earnings.

Although these results come from a smaller sample and have large standard errors, it is interesting to compare them to the evidence from *conditional* cash transfers (CCTs) in developing countries today. These are estimated to have decreased short run mortality by about 12 percent for the elderly, and by about 2 percent among infants (Barham 2011, Barham and Rowberry 2012). They raise enrollment in the short term substantially. But there is considerable heterogeneity and uncertainty about the long term effects of CCTs on learning, final years of education, wages or anthropometric outcomes. But consistent with the results here, the largest positive effects are observed for CCT that are generous and that reach the poorest populations (Fiszbein and Schady 2009).

These results have a number of important implications for public policy today. First, the results imply that today's cash transfer policies targeting poor families will likely have significant positive effects on the long term health and human capital of the children. Even though conditions prevailing in the early part of the 20th century differ significantly from current conditions, the relationship between parental income and child human capital has not diminished significantly over time. Second, our results showing how long-term outcomes can be predicted by early life conditions can help policy makers anticipate the needs of aging cohorts in order to better tailor policies geared toward the elderly (such as disability, pension and Medicare policies). Third, the conditions faced by poor families with dependent children in the early part of the 20th century are similar to those faced by families in developing countries today. Thus recently implemented cash transfer programs to poor families in developing countries such as

Progressa and Bolsa Familia are likely to generate similar long term effects. Lastly more generous but targeted policies were more successful in terms of their long-term effects.

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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 /divorced	Husband institution alized	Children age eligibility	Residency required	Citizenship required	property limits	work regulated	reapplicati on	1st child	add'l child	Max amount
Colorado (1913)	Yes	Yes	16	No	No				*	*	No
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	12 months	20	15	No
Montana[b] (1915)	No	yes	16	Yes	Yes		yes	3 months	15	10	50
North Dakota (1915)	No	no	14	Yes	No				15	15	No
Ohio (1913)	Yes	yes	16	Yes	No		yes	6 months	35	10	No
Oklahoma (1915)	No	yes	14	Yes	No				10	5	No
Oregon (1913)	No	yes	16	Yes	Yes	yes	yes	3 months	15	10	60
Pennsylvania (1913)	No	yes	14	Yes	No				20	10	No
Washington (1913)	No	no	15	Yes	No			12 months	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.

b. additional amounts were specified for the second child (15), third child (10) and for every additional child (5).

"Work regulated" 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.

Although

Table 2: Data on Children in Families Receiving Mother's Pensions 1911-1930
All children born 1900-1925, under age 19 at the time of application

	All data collected (boys and girls in all counties with records collected)					Boys in counties with rejected applicants (estimation sample)	
State	# counties	# children receiving transfers	# families receiving transfers	Average monthly allowance	# rejected applicants	# rejected	# accepted
1-Colorado	2	547	205	17.3	0		
2-Connecticut	1	192	67	12.1	20	10	89
3-Idaho	19	3,117	1,112	20.35	179	78	776
4-Illinois	10	2,271	829	17.95	116	56	670
5-Iowa	8	2,957	841	31.92	174	73	739
6-Minnesota	17	3,276	1,023	23.62	176	26	527
7-Montana	3	1528	559	29.42	0		
8-North Dakota	8	1,390	484	33.03	163	67	563
9-Ohio	24	13,643	5,607	12.42	2131	978	4,825
10-Oklahoma	5	1,504	582	13.27	181	72	700
11-Oregon	12	3,351	1,499	23.13	736	302	978
12-Pennsylvania	17	7,765	1,967	9.90	0		
13-Washington	23	10,681	4,661	22.67	911	500	3,701
14-Wisconsin	7	1,079	497	23.95	34	16	77
Total	145	51,912	19,374		4,658	2,178	13,645

Note: We collected an additional 30,000 records which are not included here because the application date is after 1930, the cohorts are out of the specified range, or the amount or date of application is missing.

Table 3: Reasons for rejection.
Distribution in all records and in estimation sample

	Reason MP denied			Reason MP ended
	All	first application	Re-application	All
Panel A: All data				
Ineligible/expired	63.02	41.68	97.43	43.13
Other means	21.04	32.49	2.57	17.38
Married or husband returns	4.33	7.02		27.25
Withdrew	1.83	2.97		
Divorced	0.94	1.52		
Immoral/unfit	4.20	6.80		3.22
Application incomplete	3.35	5.43		
Mother unable to provide	1.07	1.74		
child delinquent	0.04	0.07		1.62
mother died/hospitalized/in prison	0.18	0.29		4.23
administrative reasons				0.93
other				2.23
N observations with data	2,239	1382	857	13,794
Panel B: Estimation Sample				
Ineligible/expired	43.34			39.97
Other means	35.31			20.01
Married or husband returns	7.38			25.34
Withdrew	3.37			
Divorced	1.12			
Immoral/unfit	2.09			4.39
Application incomplete	7.38			
Mother unable to provide				
child delinquent				
mother died/hospitalized/in prison				4.11
administrative reasons				2.55
other				3.2
N observations with data	623			4,692

Panel A includes information from any record with non-missing values in original MP data.

Table 4: 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			72.52	73.25	72.48	73.41
A. Individual characteristics						
Year of application	1,920.7	1,921.6	1,920.8	1,921.8	1,920.8	1,921.8
YOB of child	1,912.0	1,913.3	1,912.0	1,913.5	1,912.0	1,913.5
Child age (years)	8.71	8.21	8.78	8.25	8.78	8.26
Number of kids in family	3.62	3.76	3.53	3.73	3.55	3.72
Age of oldest kid in family	11.87	11.47	11.8	11.45	11.83	11.43
Age of youngest kid in family	5.57	4.81	5.64	4.84	5.62	4.85
Length of family name	6.39	6.44	6.31	6.36	6.33	6.36
Widow	0.51	0.54	0.53	0.55	0.52	0.55
Divorced	0.04	0.03	0.04	0.03	0.04	0.03
Husband abandoned/prison/hospital	0.19	0.19	0.19	0.19	0.19	0.19
Mother's marital status unknown	0.26	0.24	0.25	0.23	0.25	0.23
Missing age at death	0.51	0.47	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	0.51	0.56	1.05	1.06	1	1
Quality of merge with DMF file	1.2	1.18	1.2	1.18	1.19	1.17
B. 1910 county characteristics						
Duncan socio-economic Index	24.22	24.46	24.27	24.7	24.3	24.92
SD of SEI	20.83	20.48	20.8	20.56	20.83	20.74
% 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 with mom's widowed/divorced/single/abandoned	0.06	0.05	0.06	0.06	0.06	0.06
% workers with SEI<20th percentile	0.11	0.1	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	84.37	93.36	82.32	95.37	82.33	96.42
Duration of transfers in years	0	4.14	0	4.27	0	4.29
Monthly amount - 1982 dollars	0	307.31	0	309.36	0	309.13
Number of children	2,178	13,645	1,068	7,253	1,031	6,954
Number of families	1,323	7,892	662	4,212	631	4015
Number of counties	73	73	73	64	73	63
Number of observations	2,230	14,084	1,120	7,692	1,031	6,954

Tables 5: Determinants of acceptance and generosity of transfers

	Accepted ==1	Log(monthly amount)	Log(duration in years)	Log (lifetime transfer)
Model	Logit	OLS	OLS	OLS
Child age (years)	-0.00110	0.0109***	-0.0252**	-0.0234*
	[0.003]	[0.003]	[0.011]	[0.012]
Number of kids in family (imputed)	0.00332	0.109***	0.0399***	0.133***
	[0.003]	[0.005]	[0.012]	[0.013]
Age of oldest kid in family record	0.000342	0.00140	0.0104*	0.0176***
	[0.002]	[0.002]	[0.006]	[0.006]
Age of youngest kid in family record	-0.0052***	-0.0261***	-0.0347***	-0.0565***
	[0.002]	[0.002]	[0.005]	[0.006]
Length of family name	0.00137	5.51e-05	0.00224	0.00168
	[0.002]	[0.003]	[0.008]	[0.009]
(Widowed is the excluded category)				
Divorced	-0.0587**	0.00126	-0.108	-0.0999
	[0.025]	[0.023]	[0.066]	[0.073]
Husband abandoned, in prison/hospital	-0.0112	-0.00212	-0.121***	-0.100**
	[0.011]	[0.013]	[0.040]	[0.043]
Mother's marital status unknown	-0.0989**	-0.0378	0.00965	0.00505
	[0.040]	[0.039]	[0.103]	[0.118]
Day or month of birth missing	-0.0180	0.0610*	0.419**	0.519***
	[0.030]	[0.034]	[0.182]	[0.197]
County and cohort FE?	yes	yes	yes	yes
State characteristics (year of application)	yes	yes	yes	yes
Mean of Y	0.862	5.497	1.309	6.644
Number of individuals	15,822	13,538	6,766	6,719

* 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 6: 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	-0.165**	-0.142**	-0.146**	-0.119**	-0.112**
(M=0.498, N=15,822)	[0.051]	[0.051]	[0.051]	[0.053]	[0.055]
More than one match=1					
(M=0.0786, N=8321)	0.163	0.177	-0.0294	-0.0175	-0.0596
	[0.187]	[0.188]	[0.199]	[0.198]	[0.216]
Panel B: Ohio MP matched to additional death records					
Missing match=1	-0.168*	-0.161*	-0.164*	-0.0520	-0.138
(M=0.272, N=5,469)	[0.090]	[0.090]	[0.090]	[0.095]	[0.097]
More than one match=1					
(M=0.576, N=3,495)	-0.165	-0.168	-0.180	-0.204*	-0.246**
	[0.107]	[0.109]	[0.110]	[0.113]	[0.115]
Panel C: MP matched to WWII records					
Missing match=1	-0.182***	-0.161**	-0.167**	-0.180**	-0.129*
(M=0.841, N=15,822)	[0.068]	[0.069]	[0.069]	[0.071]	[0.075]
More than one match=1 (if					
matched) (M=0.144, N=2,874)	0.111	0.110	0.114	0.154	0.169
	[0.171]	[0.172]	[0.172]	[0.178]	[0.198]
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 7: Main results for mortality.

	1	2	3	4	5	7	8
	Only state and cohort dummies	Individual controls state*year, county 1910 and cohort FE	Individual controls state*year, county FE and cohort FE	DOB from SSN	Drop missing age at death	Unique matches only	Drop counties with % black>p75 in state
P(survived to 60)	0.157**	0.110**	0.106**	0.106**	0.117	0.116	0.143**
	[0.047]	[0.048]	[0.051]	[0.051]	[0.114]	[0.115]	[0.058]
N	15,823	15,822	15,822	15,822	8,320	7,984	11,228
% effect	9%	6%	6%	6%	1%	1%	8%
P(survived to 70)	0.227**	0.190**	0.202**	0.196**	0.257**	0.265**	0.276**
	[0.051]	[0.052]	[0.054]	[0.054]	[0.073]	[0.074]	[0.063]
N	15,823	15,822	15,822	15,822	8,320	7,984	11,228
% effect	16%	13%	14%	14%	10%	11%	20%
P(survived to 80)	0.180**	0.161**	0.159**	0.158**	0.129*	0.132*	0.214**
	[0.065]	[0.066]	[0.069]	[0.069]	[0.078]	[0.079]	[0.080]
N	15,823	15,822	15,822	15,822	8,320	7,984	11,228
% effect	15%	14%	13%	13%	9%	9%	18%
Log(age at death)	0.0118**	0.0099	0.0126*	0.0134**		0.0162**	0.0185**
	[0.006]	[0.006]	[0.007]	[0.007]		[0.006]	[0.007]
Effect (yrs)	0.86	0.72	0.92	0.98		1.18	1.35
N	8,329	8,328	8,328	8,325		7,984	5,816
Ohio only	0.0080	0.0072	0.0112	0.0112		0.0341**	0.0146
	[0.012]	[0.012]	[0.013]	[0.013]		[0.015]	[0.013]
N	3,679	3,679	3,679	3,679		2,492	2,565

* p<0.10, ** p<0.05. Columns (4)-(7) include all individual controls, county and cohort FE, as in column (3). 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 8: Robustness checks
Coefficient on Accepted on the probability of surviving past age 70

	Accepted=1			
	beta	se	Obs	% effect
Panel A: Logit				
Unique matches and missing dropped	0.257**	[0.077]	7,960	10%
Unique matches and missing imputed as dead	0.206**	[0.058]	15,486	15%
Random match and missing imputed as dead	0.204**	[0.057]	15,822	14%
All matches treated as observations, missing imputed as dead	0.192**	[0.056]	16,313	13%
Keep highest quality match	0.210**	[0.092]	5,871	8%
Panel B: MLE (Logit model)				0%
All matches and missing imputed as dead	0.202**	[0.054]	15,822	14%
Allowing for measurement error in matching			15,822	
Sample matched on propensity score	0.204**	[0.054]	15,822	14%
Drop individuals with 3 or matches	0.200**	[0.054]	15,729	14%
Individuals with 1 or 2 matches, missing dropped	0.245**	[0.074]	8,227	10%
Less Stringent matching criteria	0.1871**	[0.0564]	14,987	
Panel C: results for Ohio				
a. original data				
No county controls	0.308**	[0.099]	5,469	24%
Add county*year controls	0.311**	[0.099]	5,469	24%
Drop missing	0.324**	[0.113]	3,042	14%
b. OH state death records added				
No county controls	0.270**	[0.093]	5,469	18%
Add county*year controls	0.293**	[0.094]	5,469	20%
Drop missing	0.270**	[0.114]	3,494	14%
c. Additional deaths, manual search				
No county controls	0.221**	[0.090]	5,469	14%
Add county*year controls	0.224**	[0.091]	5,469	15%
Drop missing	0.201*	[0.109]	3,678	11%

* p<0.10, ** p<0.05. All models are estimated using county and cohort fixed effects and include state characteristics at the time of application which are 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 work is required, whether reapplication is required, the maximum legislated amount for the first child and the legislated amount for each additional child). County controls for Ohio consist of total expenditures on relief, total expenditures on outdoor relief and total expenditures on children's homes (see Data Appendix for details).

Table 9: Exploring heterogeneity of effects of the MP program
Table reports the coefficient of accepted.

Dependent var	P(died after 70).				Log(age at death).			
Specification:	Logit				OLS			
	All matches, missing imputed as 0.				Unique matches, missing dropped			
	beta	se	N	% effect	beta	se	Effect (yrs)	Mean Y for rejected
Panel A: Demographics								
Widow	0.0910	[0.076]	8,535	6%	0.0001	[0.008]	0.01	73.51
Not widow	0.3270**	[0.079]	7,287	24%	0.0325**	[0.009]	2.36	71.35
Marital status missing	0.3151**	[0.108]	3,781	23%	0.0265**	[0.012]	1.92	71.48
Divorced/in prison/etc	0.3287**	[0.119]	3,506	24%	0.0359**	[0.014]	2.61	71.48
1 kids	0.2042	[0.161]	1,403	14%	0.0051	[0.018]	0.38	73.55
2-3 kids	0.2190**	[0.083]	6,671	15%	0.0210**	[0.010]	1.53	72
4 or more kids	0.1995**	[0.083]	7,748	14%	0.0180**	[0.009]	1.32	72.61
Under 5	0.2403**	[0.112]	4,489	17%	0.0033	[0.013]	0.24	72.19
Ages 6-12	0.2484**	[0.084]	7,232	17%	0.0194**	[0.010]	1.42	72.32
Ages 13+	0.0517	[0.119]	2,869	3%	0.017	[0.012]	1.26	73.34
Panel B: Legislation type								
Work regulated	0.2330**	[0.070]	8,702	16%	0.0170**	[0.008]	1.23	71.68
Work not regulated	0.1393	[0.088]	7,120	10%	0.0131	[0.010]	0.98	73.96
Duration unspecified	0.2690**	[0.078]	7,240	19%	0.0166*	[0.009]	1.19	71.31
Reapplication required	0.1346*	[0.077]	8,582	9%	0.0139	[0.009]	1.03	73.88
kid eligible to age 16+	0.2306**	[0.070]	8,745	16%	0.0186**	[0.008]	1.34	71.53
kid eligible to age 14 or 15	0.1327	[0.088]	7,077	9%	0.009	[0.010]	0.67	74.32

* p<0.10, ** p<0.05. All models are estimated using county and cohort fixed effects and include state characteristics at the time of application which are 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 10: Preliminary results from WWII records. Coefficient on Accepted reported. All matches used, OLS or Logit coefficients reported

Dependent variable	No Controls	All controls	N	Mean rejected
A. Education				
Education (yrs)	0.149	0.107	2,874	10.38
	[0.151]	[0.152]		
Has exactly 8 years of school	-0.355***	-0.293		0.333
	[0.130]			
Education -- left and right censored	0.343*	0.255		10.38
	[0.196]	[0.207]		
B. Anthropometrics				
Height (cms)	0.679	0.290	2,250	174.90
	[1.018]	[1.210]		
Weight (pounds)	1.977	1.089	2,219	146.4
	[2.149]	[2.323]		
BMI	0.533*	0.383	2,097	22.17
	[0.306]	[0.395]		
Underweight	-0.688***	-0.608		0.0909
	[0.246]			
Obese	0.362	0.609		0.02
	[0.433]			
C. Labor market and race				
Log occupation score	-0.0104	-0.00861	2,132	3.130
	[0.025]	[0.033]		
White collar	0.0832	0.293	2,509	0.13
	[0.195]	[0.247]		
Black=1	0.261	0.102	2,861	0.0370
	[0.309]			

* p<0.10, ** p<0.05. 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).

Figure 1: Passage of Mother's Pension Laws

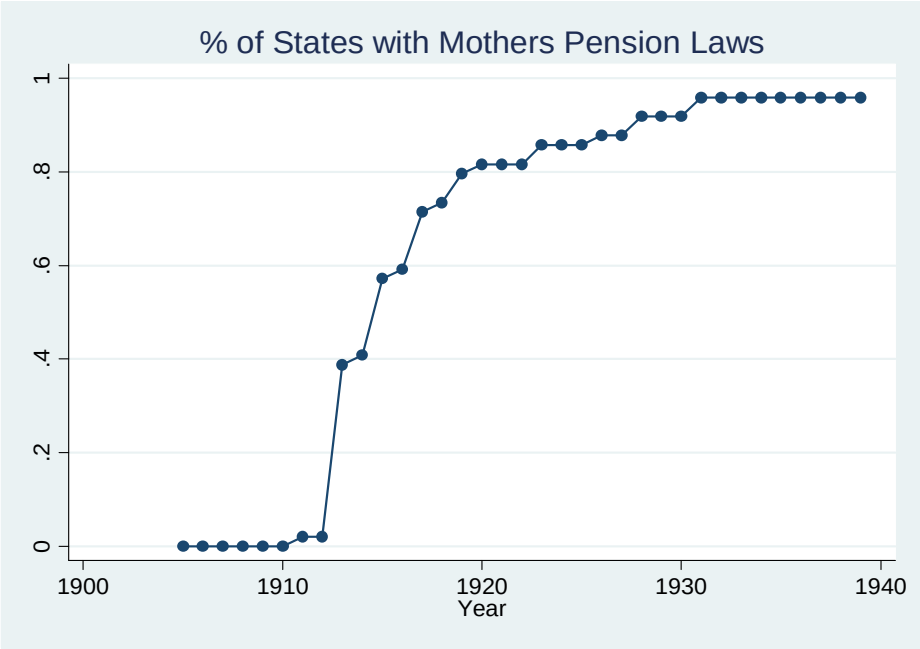


Figure 2: Relative generosity of means tested transfers by state: 2010 vs 1930.

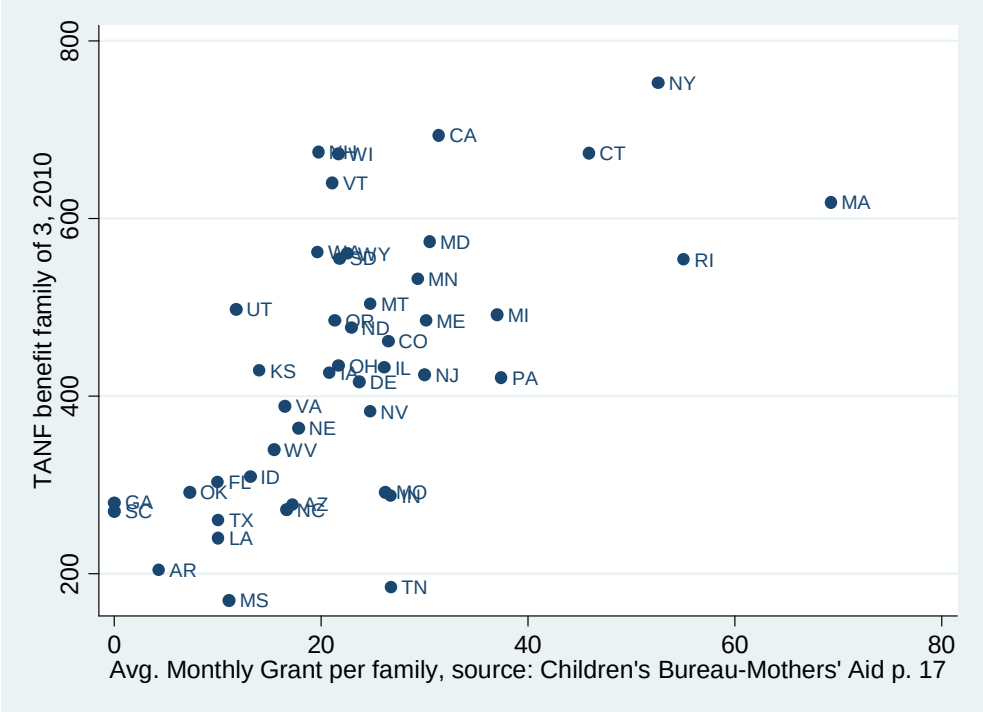
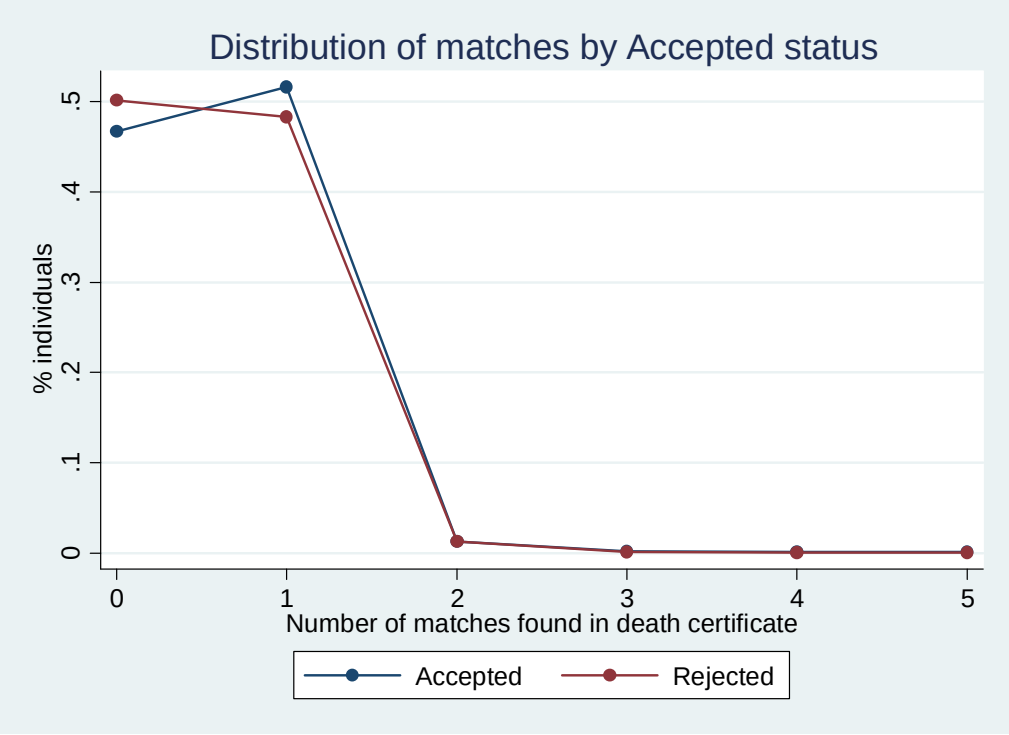
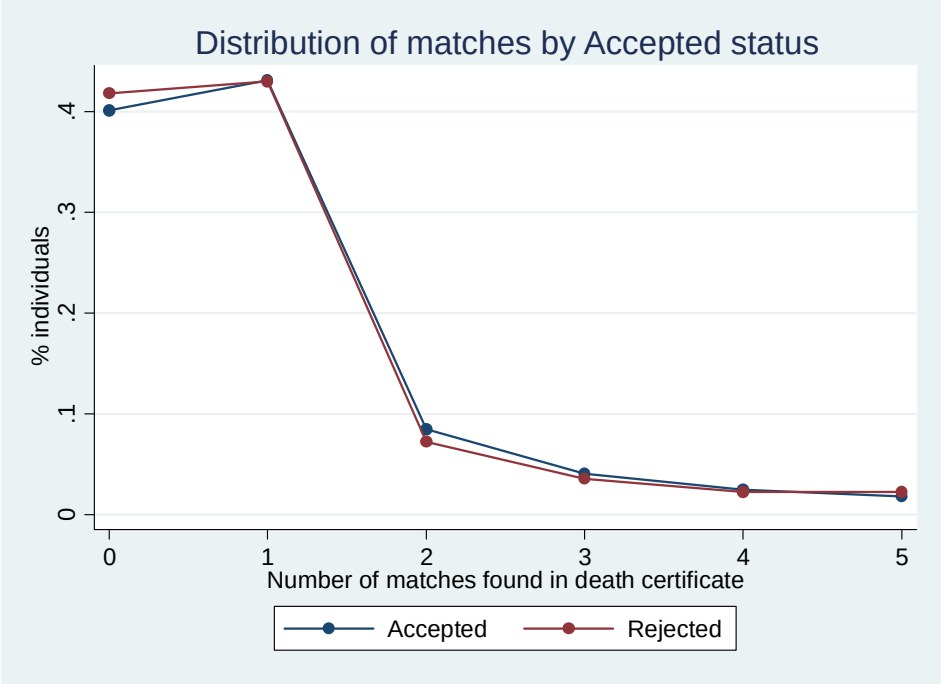


Figure 3: Number of matches to death certificates

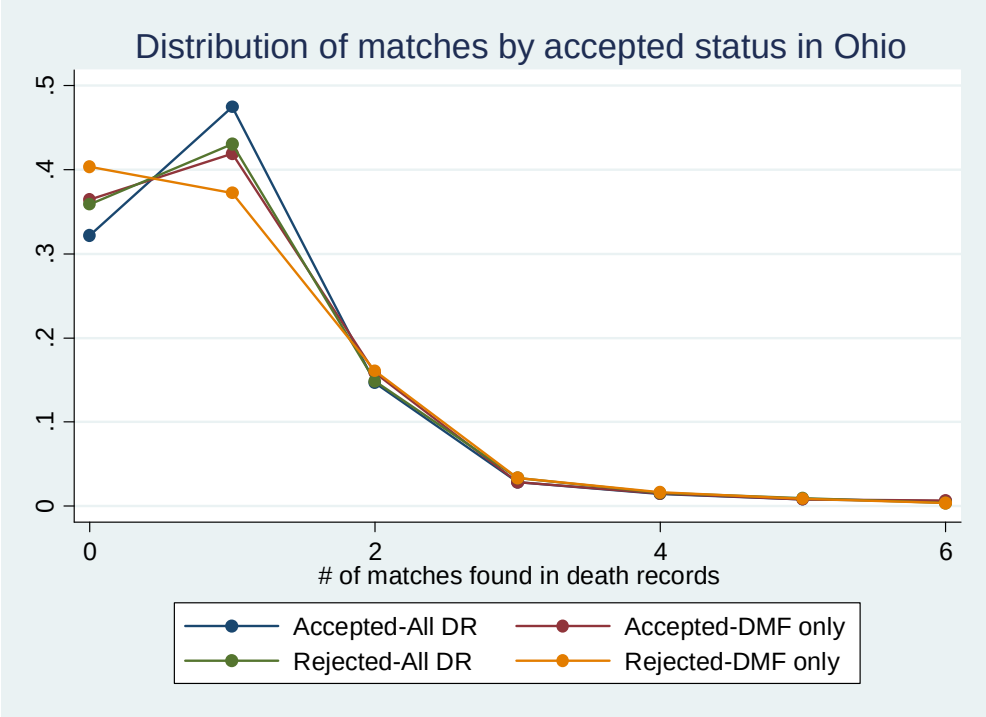
Panel A: Estimation Sample



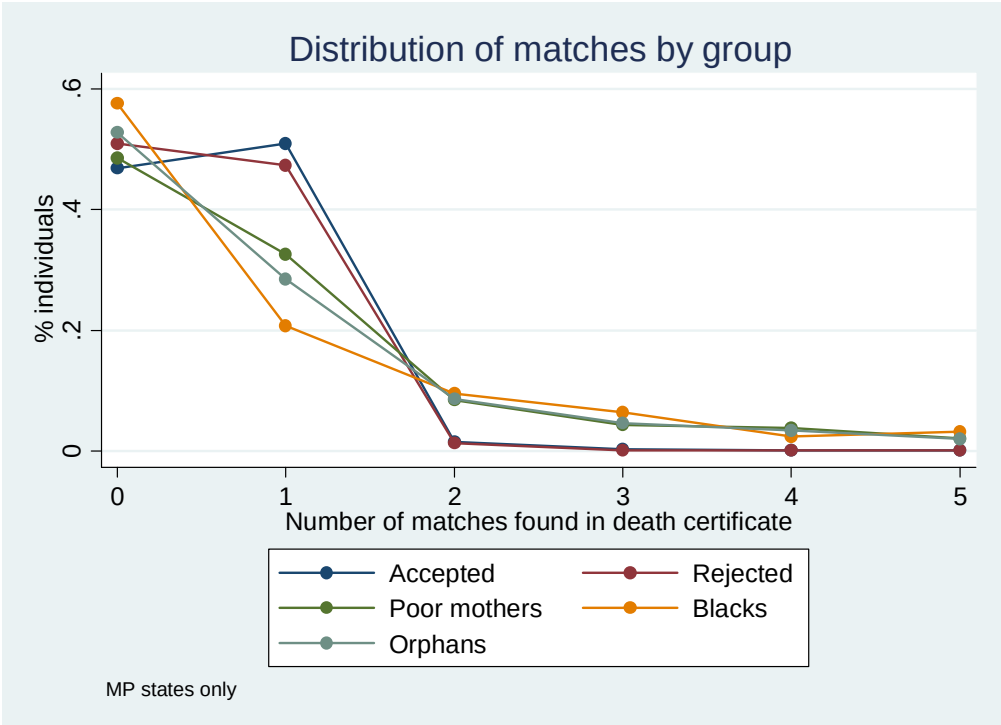
Panel B: Distribution of matches with less stringent matching criteria



Panel C: Ohio Sample with additional matches to state death certificates



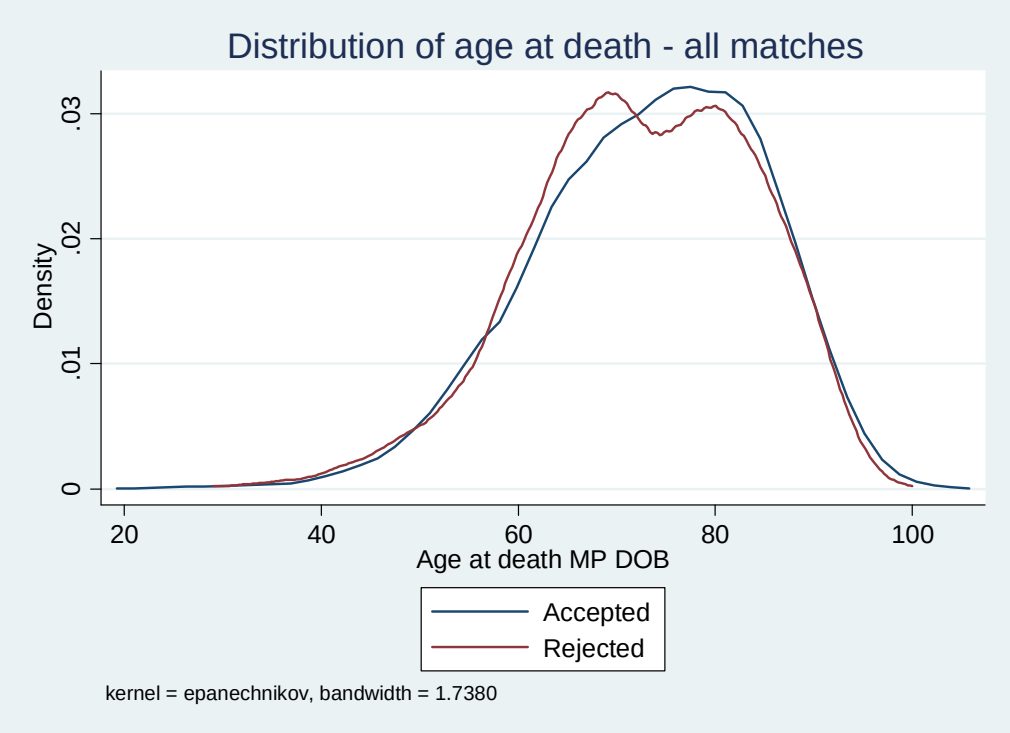
Panel D: matches for possible census controls



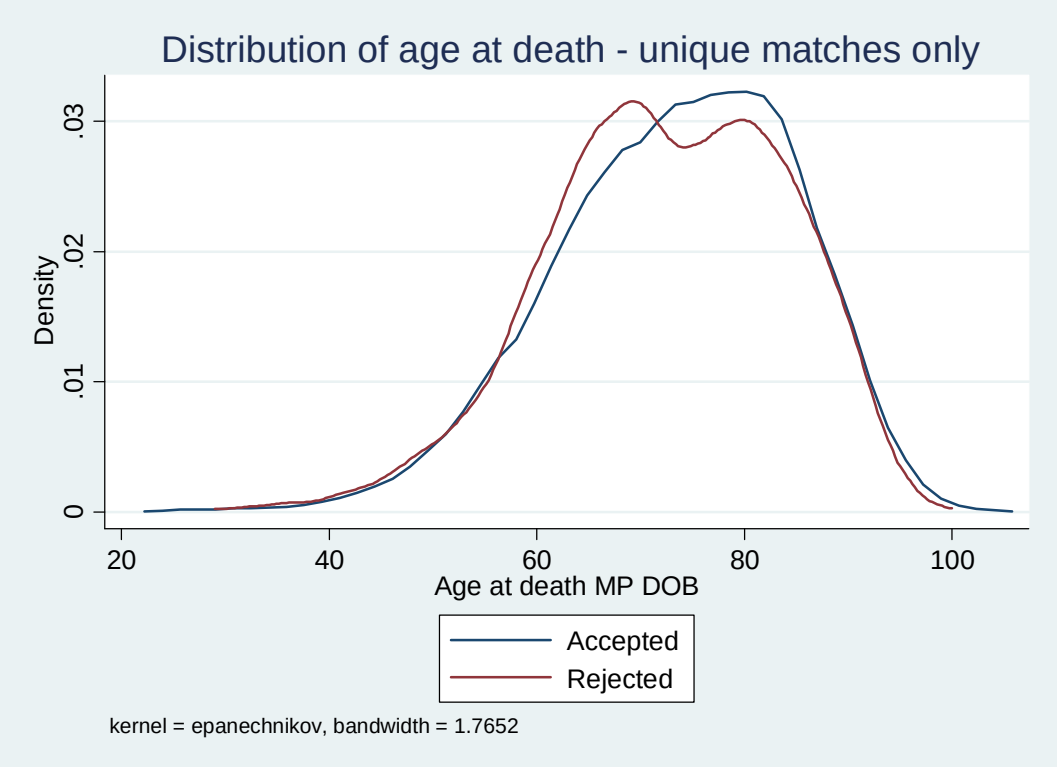
Note: MP matches are stringent but not census controls.

Figure 4: Distribution of age at death and treatment

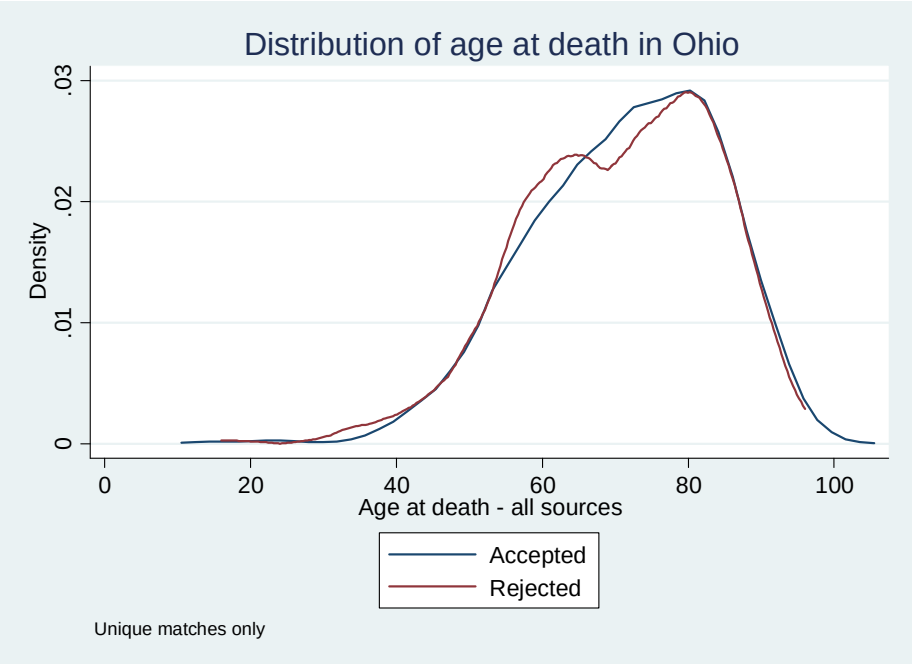
a. All matches



b. Unique matches

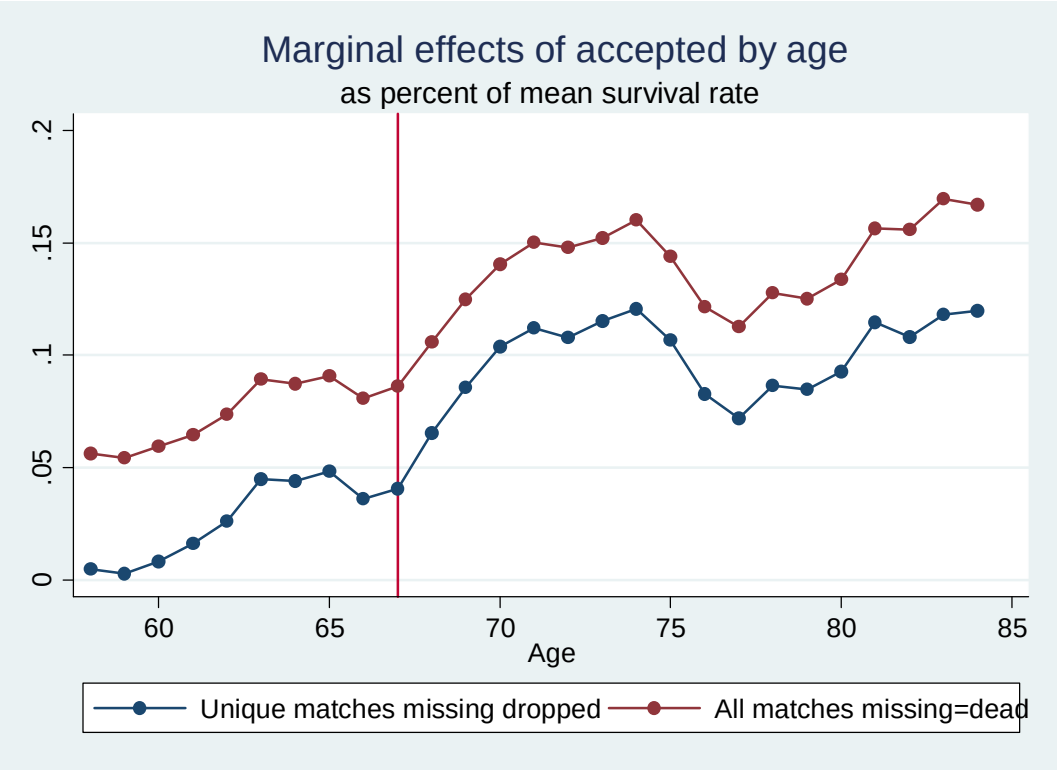


c. Ohio, matching to additional death records.



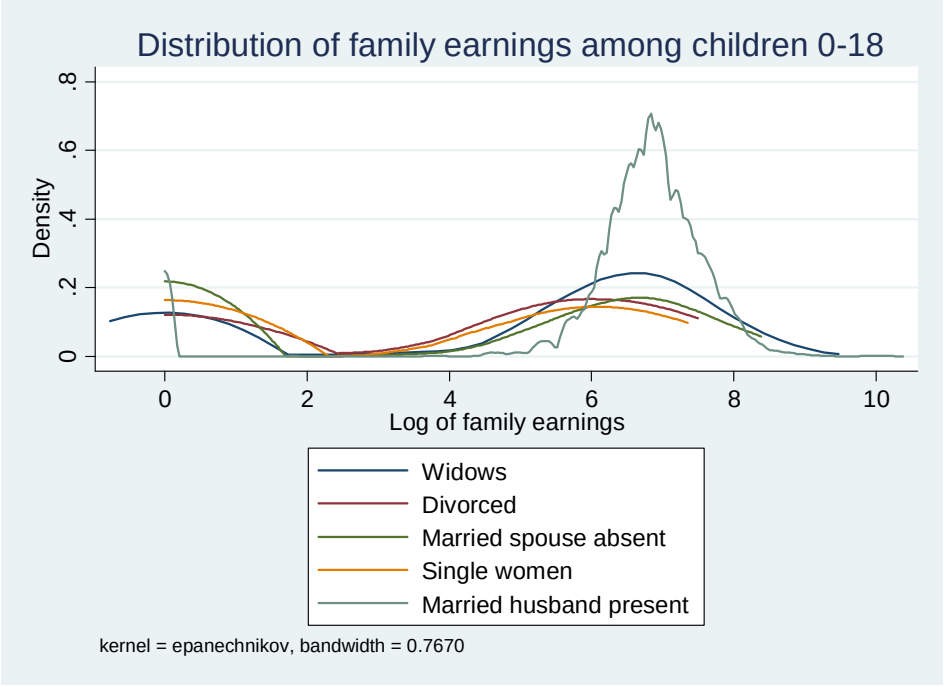
Records matched first to Death Mortality Files (DMF). Unmatched records were then matched to Ohio and Illinois state death records. Unmatched records were then manually imputed by searching individual records in Ancestry.com.

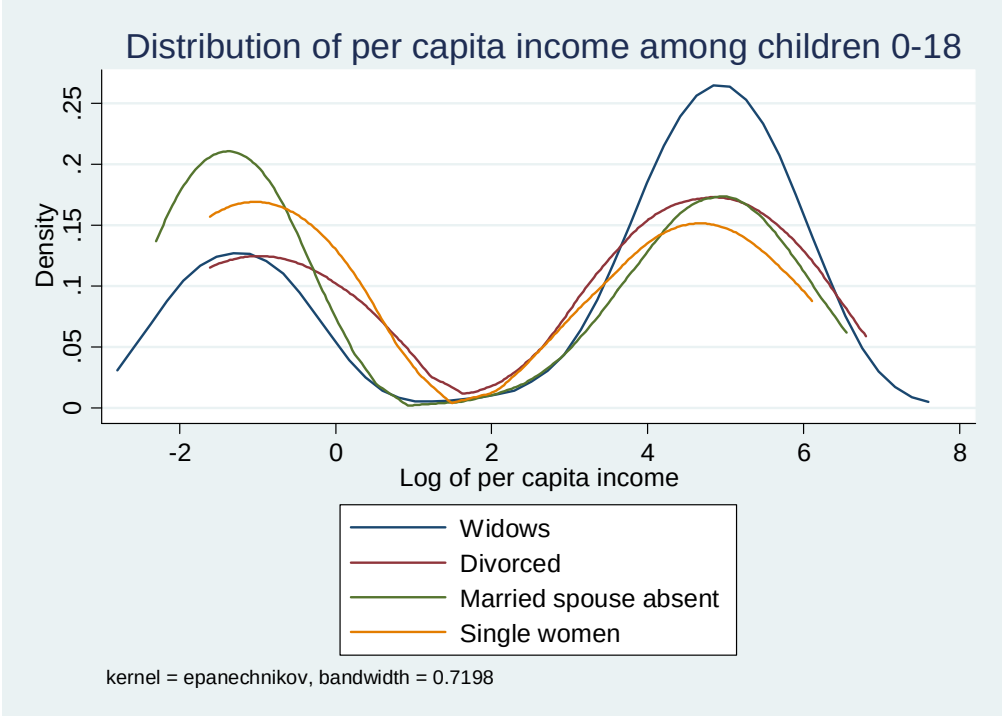
Figure 5: Effects by Age



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 67 are significant at the 10 percent or higher for both sets of estimates.

Figure 6: SES in 1915 census by marital status

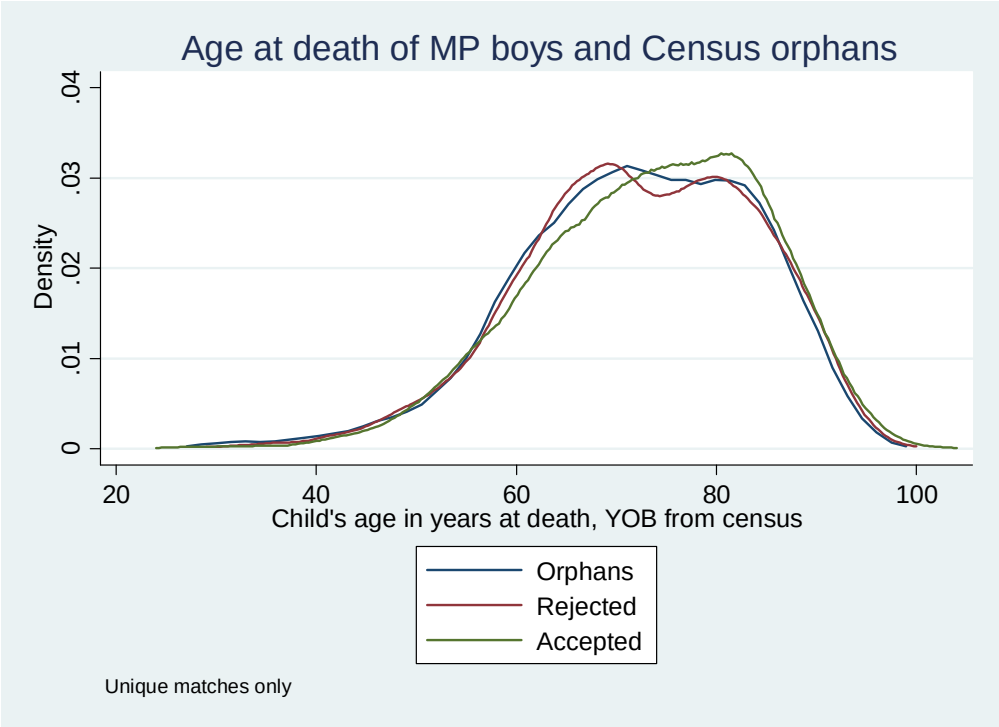




Sample is restricted to individuals ages 0-18 living in families with at least one child under 14.

Figure 7: Alternative counterfactuals

Panel A: Orphans from the 1900-1930 Census



Panel B: States where single and divorced are ineligible

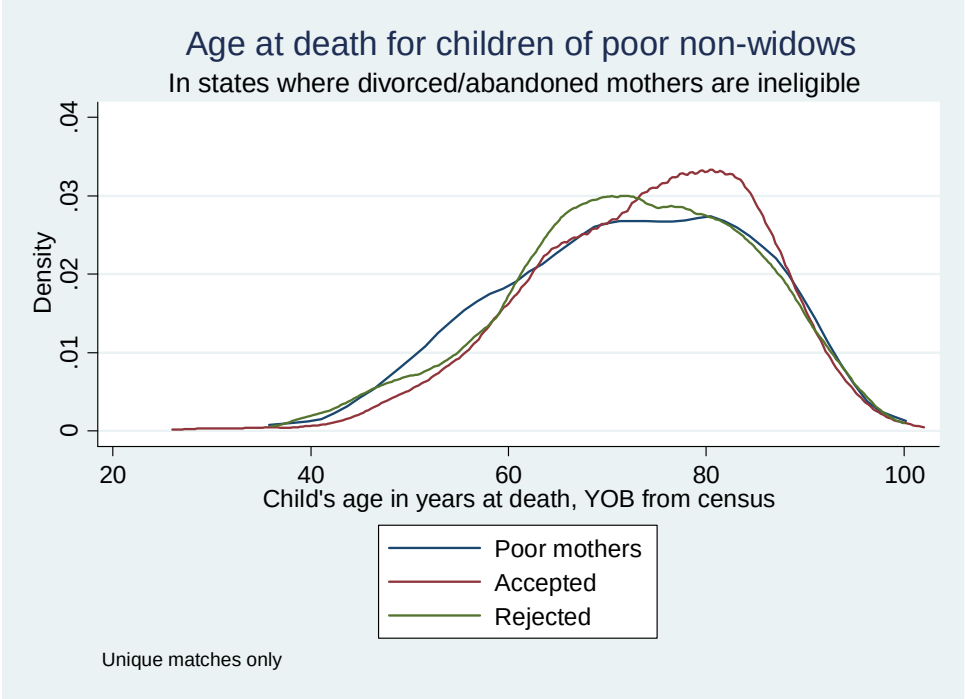
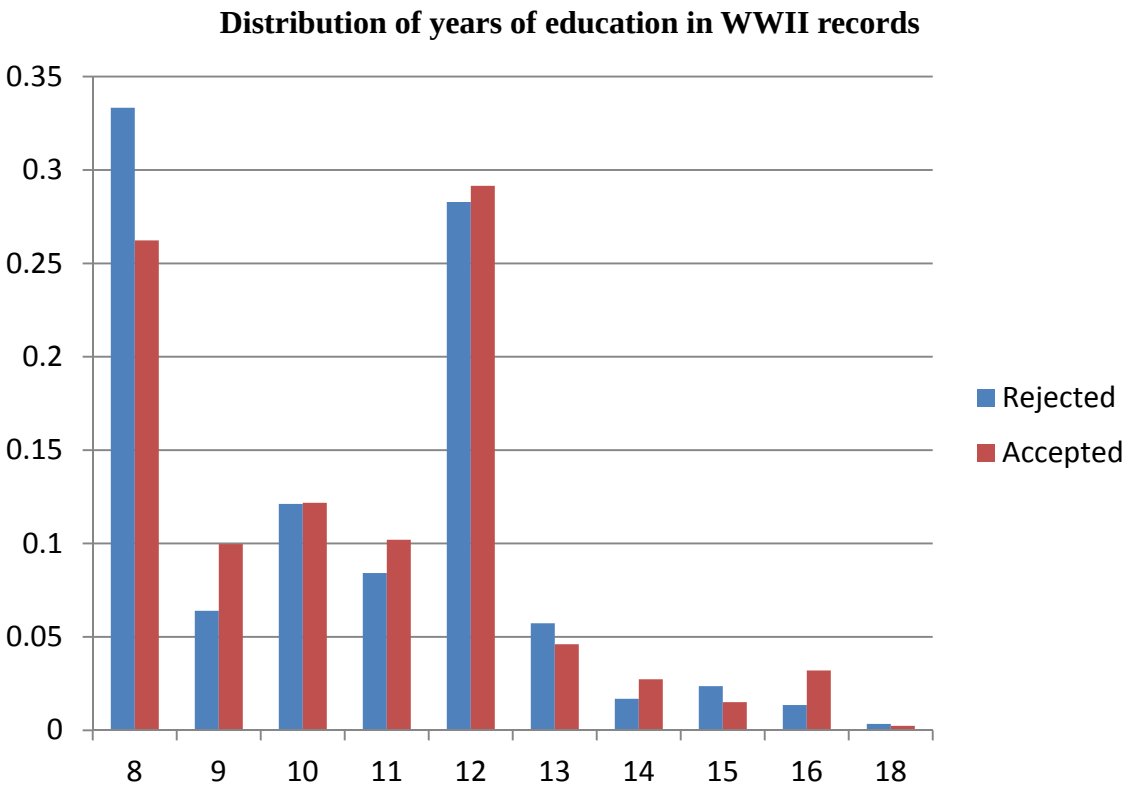
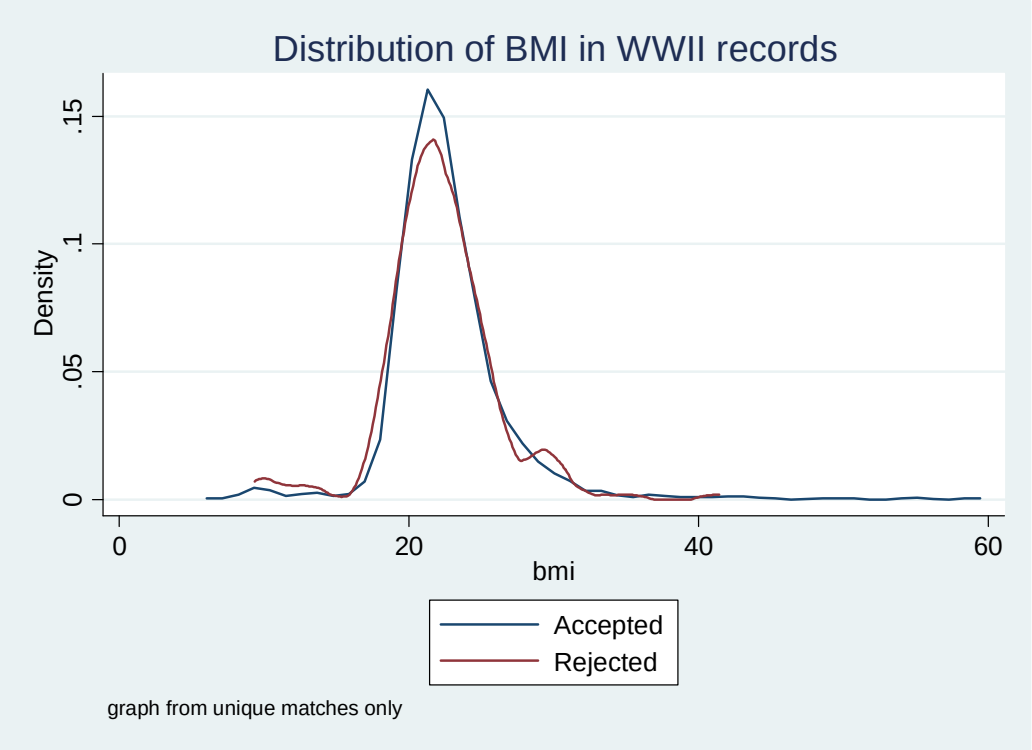


Figure 8


Distribution of years of education in WWII records
0.35
0.3
0.25
0.2
0.15
0.1
0.05
0
8
9
10
11
12
13
14
15
16
18
Rejected
Accepted


Distribution of BMI in WWII records
Density
0
.05
.1
.15
0
20
40
60
bmi
Accepted
Rejected
graph from unique matches only

Appendix I: Selection of controls from the 1900 (5%), 1910 (1%), 1920 (1%), and 1930 (5%) Censuses

1-Black sample: All black children 18 and younger in poor minor civil divisions within MP states living in households and whose mother is marital status was "widowed", "single/never married" "divorced" or "abandoned"

2-Controls in MP states:

- All white children 18 and younger
- living in poor minor civil divisions within MP states
- mothers' marital status was "widowed", "single/never married" "divorced" or "abandoned"
- matched by propensity score using gender, gender-specific year-of-birth dummies, age at application/observation dummies, number of siblings in each age category, dummies for (imputed) mother's birthplace, number of latters in last name and state dummies.

3-Controls from neighboring states.

- All white children 18 and younger
- Living in poor minor civil divisions in control/neighboring states.
- Control states were selected based on a-geographic proximity, b-having no MP program, c-having an MP program with very few recipients according to Children's Bureau publications. States chosen based on the following table:

MP state	Control state
Colorado	New Mexico (passed law in 1931).
Ohio	Kentucky (passed law in 1928) and Indiana
Connecticut	New Hampshire
Oklahoma	Missouri
Iowa	Missouri
Illinois	Indiana and Missouri

- matched by propensity score using the same covariates as listed above

NB: A *poor* minor civil division is defined as a minor civil division in which a-average earnings of adults in households were below the 50th percentile of socio-economic index in the combined 1900-1930 censuses or b-average earnings of adults in households were below the 50th percentile of Duncan occupational score in the combined 1900-1930 censuses, or c-average earnings of married men in dual households was below the 50^{th} percentile.

Appendix II: State and county level data

a. State data. State characteristics at the time of application include:
1-manufacturing wages (computed as the national manufacturing wages * ratio of state to national manufacturing earnigns), education/labor laws (age must enter school age can obtain a work permit and whether a continuation school law is in place). These were obtained from Price Fishback at:
http://www.u.arizona.edu/~fishback/Published_Research_Datasets.html. The information is available for all years from 1900 to 1930.
2-state expenditures (education, charity and total expenditures on social programs). These were collected from various volumes of the Financial Statistics of the States and are available for 1915-1919, 1923-1930. We imputed missing values for 1923-1930 using linear interpolation within states.

b. State Mother's Pensions Laws. Available for years 1914, 1916, 1919, 1922, 1925, 1926, 1929 and 1934. Missing values were imputed using the last observation within state. The data for each year of the MP Laws was gleaned from the following resources:

- **1914**: "Laws Relating to 'Mothers' Pensions' in the United States, Denmark, and New Zealand" from the U.S. Department of Labor, Children's Bureau, Dependent Children Series, No. 1, Bureau Publication No. 7
- **1916**: "Widows' Pension Legislation" from the Bureau of Municipal Research and Training School for Public Service in New York, No. 85, May, 1917
- **1919**: Thompson, Laura A. 1919. "Laws Relating to 'Mothers' Pensions' in the United States, Canada, Denmark, and New Zealand." Washington, DC: U.S. Government Printing Office (U.S. Department of Labor, Children's Bureau, Legal Series No. 4, Bureau Publication No. 63)
- **1922**: Eckman, Lulu L. 1923. "Public Aid to Children in Their Own Homes: A Tabular Summary of State Laws in Effect November 1, 1922." Washington, DC: U.S. Government Printing Office (U.S. Department of Labor, Children's Bureau, Legal Chart No. 3)
- **1925**: Eckman, Lulu L. 1925. "A Tabular Summary of State Laws relating to Public Aid to Children in Their Own Homes in effect January 1, 1925 and the Text of the Laws of Certain States." Washington, DC: U.S. Government Printing Office (U.S. Department of Labor, Children's Bureau, Chart No. 3)
- **1929**: "A Tabular Summary of State Laws relating to Public Aid to Children in Their Own Homes in effect January 1, 1929 and the Text of the Laws of Certain States." Washington, DC: U.S. Government Printing Office (U.S. Department of Labor, Children's Bureau, Chart No. 3)
- **1934**: "A Tabular Summary of State Laws relating to Public Aid to Children in Their Own Homes in effect January 1, 1934." Washington, DC: U.S. Government Printing Office (U.S. Department of Labor, Children's Bureau, Chart No. 3)

c. County data for Ohio
We include three county level variables available for a few years: total expenditures on relief, total expenditures on outdoor relief and total expenditures on children's homes. These were collected from various volumes of the Ohio General Statistics, available for 1915-1922. We imputed missing values for using linear extrapolation within counties.

Appendix III: Matching algorithm

We start by matching our MP records to DMF. The match uses 6 variables: first name, middle initial, last name, day, month and years of birth. The match allows for errors in strings and in single digits for DOB. Specifically we convert all names/strings into sounds using the SOUNDEX function and match individuals based on this rather than based on the original strings to avoid dealing with changes in spelling and spelling mistakes. The procedure SPEDIS computes a measure of distance between strings and we use it to measure the quality of a match. We also allow for error in the dates.

Step 1: take as a possible match any pairs of observations meeting at least one of these criteria

a. surname SOUNDEX codes match AND first letter of given names match AND year of birth is within 2 years

b. first letter of surnames match AND given name SOUNDEX codes match AND year of birth is within 2 years

c. first letter of surnames match AND first letter of given names match AND date of birth matches exactly

d. surname SOUNDEX codes match AND date of birth matches exactly

Step 2: group the matches based on quality

First Best:

-birthdate matches exactly AND average SPEDIS score across surname and given name is 0 OR

-(birthdate matches exactly OR 2 of [birth month, day, year] match) AND (surname matches exactly OR one surname contains the other OR one surname contains the 1st three letters of the other) AND (given name matches exactly OR one given name contains the other OR one given name contains the 1st three letters of the other)

Second Best: if no "First Best" group

(year of birth matches exactly OR date of birth differs by a month or less) AND (surname matches exactly OR one surname contains the other OR one surname contains the 1st three letters of the other) AND (given name matches exactly OR one given name contains the other OR one given name contains the 1st three letters of the other)

Third Best: if no “First best” or “Second best” group

(year of birth is within 2 years) AND (surname matches exactly OR one surname contains the other OR one surname contains the 1st three letters of the other) AND (given name matches exactly OR one given name contains the other OR one given name contains the 1st three letters of the other)

Step 3: Retain only observations only if they were in one of the three categories in Step 2 AND average SPEDIS score across surname and given name is less than 9

Step 4. Retain only those observations with the "best" match AND the lowest average SPEDIS score across surname and given name is zero

Step 5. Retain only those observations with 5 or fewer matches to the DMF or state death records

These procedures relay heavily on two functions:

1. SOUNDEX: The SOUNDEX function encodes a character string according to an algorithm that was originally developed by Margaret K. Odell and Robert C. Russel (US Patents 1261167 (1918) and 1435663 (1922)). The algorithm is described in Knuth, The Art of Computer Programming, Volume 3. (See References.) Note that the SOUNDEX algorithm is English-biased and is less useful for languages other than English.

2. SPEDIS: SPEDIS returns the distance between the query and a keyword, a nonnegative value that is usually less than 100 but never greater than 200 with the default costs. SPEDIS computes an asymmetric spelling distance between two words as the normalized cost for converting the keyword to the query word by using a sequence of operations.

Stringent and non-stringent matching procedures

Appendix Table 1: Mothers' Pension Program Characteristics
In states with no individual-level data

State	State funds	Deserted or divorced eligible	Children age eligibility	Residency required	Citizenship required	Benefit for 1st child	Benefit for each add'l child
1-Alaska	yes	No	17	*	no	15	10
2-Arizona	no	No	16	Varies	yes	20	*
3-Arkansas	yes	Yes	15	Yes	yes	10	5
4-California	yes	No	15	Yes	no	*	*
5-Delaware	yes	Yes	14	Yes	no	9	5
6-Florida	no	Yes	16	*	no	25	8
7-Indiana	no	Yes	16**	*	no	*	*
8-Kansas	no	Yes	16	Yes	no	*	*
9-Louisiana	no	No	16	*	no	15	10
10-Maine	yes	Yes	14	*	no	*	*
11-Maryland	yes	No	14	Yes	no	12	*
12-Massachusetts	yes	Yes	14	Yes	no	*	*
13-Michigan	no	Yes	17	*	no	12	12
14-Missouri	no	Yes	16	Yes	no	16	8
15-Nebraska	no	Yes	14	Yes	no	10	10
16-Nevada	no	Yes	15	Yes	no	25	15
17-New Hampshire	yes	Yes	16	Yes	no	10	5
18-New Jersey	yes	No	16	Yes	no	9	*
19-New York	no	*	16	Yes	yes	*	*
20-South Dakota	no	Yes	16	*	no	15	7
21-Tennessee	no	No	15	*	yes	10	5
22-Texas	no	No	16	*	no	12	*
23-Utah	no	No	16	Yes	no	*	*
24-Vermont	yes	Yes	*	*	no	8	8
25-Virginia	no	No	16	*	no	12	*
26-West Virginia	no	Yes	13	*	yes	15	5
27-Wyoming	no	No	14	*	no	20	10

Note: States not included in Table 1A or 1B did not enact Mothers' Pension programs by 1920.
*No information provided in report. **Boys eligible until age 16, girls until 17.

Appendix table 2 Representativeness of MP data collected

State	MP data collected for 1930 New beneficiaries				Published Statistics 1931* All beneficiaries receiving transfers			
	# families	# children	Mean monthly grant	Mean family size	# families	# children	Mean monthly grant	Mean family size
Colorado	11	28	17.6	2.55	650	2166	26.5	3.33
Connecticut	6	11	12.11	1.83	959	2679	45.91	2.79
Idaho	75	184	20.22	2.45	230	619	13.16	2.69
Illinois	68	192	19.99	2.82	6087	17004	26.11	2.79
Iowa	81	185	29.51	2.28	3242	7829	20.81	2.41
Minnesota	40	123	28.54	3.08	3455	9990	29.35	2.89
Montana								
North Dakota								
Ohio	365	800	19.63	2.19	7708	21262	21.68	2.76
Oklahoma	78	158	11.53	2.03	1896	5166	7.29	2.72
Oregon	76	174	25.86	2.29	862	2127	21.35	2.47
Washington	316	723	23.75	2.29	2517	5605	19.66	2.23
Wisconsin	37	74	25.93	2.00	7052	18188	21.66	2.58

County	MP data collected for 1923-24 New beneficiaries	Published Statistics 1923-24** New beneficiaries
Allegheny (PA)	36	39.10
Cook (IL)	186	50.31
King (WA)	44	20

*Published data come from Children's Bureau (1931). Pennsylvania not represented because we only have data for 4 early years

**Published data come from Children's Bureau (1928a).

Appendix Table 3: Legislation to help widows and poor dependent children in developed countries 1900-1930

Country (year))	Eligibility
Belgium (1900)	Aged 45+, disabled, or caring for a child.
Czech Rep. (1906)	Deceased met pension conditions or was a pensioner
Slovak Rep. (1906)	Deceased met pension requirements or was a pensioner.
Iceland (1909)	Eligible survivors are a spouse or cohabiting partner (including a same-sex partner)
France (1910)	At least 55 years and married for 2 years.
Luxembourg (1911)	Insured had 12 months of coverage in 3 years prior to death or was pensioner
New Zealand (1912)	1-poor widows with children under 14 2-wives of inmates of insane hospitals
Romania (1912)	Insured met pension requirements or was a pensioner at the time of death.
Netherlands (1913)	Residents eligible. Payable to widow(er)/unmarried permanent partner
Sweden (1913)	Residents eligible. Deceased credited with pension points for at least 3 years.
Denmark (1914)	Widows with children under 14 with low wealth and assets
Canada (1916)	Max at 3/week/child (Saskatchewan), flexible in the 2 other provinces.
Italy (1919)	Deceased was a pensioner or had 5 years of contribution.
Spain (1919)	Deceased had 500 days of contribution in the last 5 years, was pensioner at time of death, or had 15 years of contribution.
Russia (1922)	Eligible survivors are widows older than age 55 (widowers 60+) or unemployed and caring for children younger than age 14 or disabled
Latvia (1922)	Deceased was insured or was a pensioner
Lithuania (1922	Deceased must have been a pensioner or had adequate coverage for disability pension at the time of death.
Slovenia (1922)	Deceased met pension (old age or disability) requirements or was a pensioner and had 5 years of coverage and contribution.
Croatia (1922)	Eligible survivors are a widow(er) aged 50 or older, caring for eligible children, or disabled
Ukraine (1922)	Eligible survivors are nonworking dependents
Kyrgyzstan (1922)	Eligible survivors are the spouse
Bulgaria (1924)	Deceased had 5 years of service, or was a pensioner.
Estonia (1924)	Widow(er) not capable of gainful activity; deceased had sufficient coverage.
UK (1925)	Widows of all insured men with children under 14
Austria (1927)	Deceased eligible for insurance/disability pension or was a pensioner
Poland (1927)	Deceased was a pensioner or eligible for old age pension/disability benefits.
Hungary (1928)	Deceased was pensioner or met requirements for pension at death

Sources: http://mchlibrary.info/history/chbu/20375.PDF
http://books.google.com/books?id=3cUKAAAAIAAJ&printsec=frontcover&source=gbs_ge_summary_r&cad=0#v=onepage&q&f=false
http://www.historyandpolicy.org/papers/policy-paper-42.html#first
http://www.ssa.gov/policy/docs/progdesc/ssptw/2010-2011/europe/index.html
http://books.google.com/books?hl=en&lr=&id=ktXg_fEWdSMC&oi=fnd&pg=PA225&dq=+italy+widows%27+pension&ots=Sni-X26-3S&sig=WZ4sY5oDaoKsuQODloP91m7gIeQ#v=onepage&q&f=true

Appendix Table 5: Generosity of transfers in real terms

State	Monthly amount	Monthly wages in manufacturing	Amount as a share of manufacturing wage	Estimated % of total family income	Amount in 1982 dollars	2010 TANF benefit In 1982$	Amount as a share of 2010 benefit
Colorado	14.95	115.24	13%		100.54	211.9	47%
Connecticut	11.39	99.9	11%		69.86	309.1	23%
Idaho	19.76	121.88	16%		114.07	141.7	81%
Illinois	16.46	117.23	14%	29%[a]	106.04	198.1	54%
Iowa	30.59	103.91	29%	44%[b]	174.14	195.4	89%
Minnesota	23.67	104.11	23%	50%[c]	131.07	244	54%
Montana	29.42	134.38	22%				
North Dakota	33.03	120.31	27%				
Ohio	11.07	115.2	10%	100%[e]	74.71	199	38%
Oklahoma	13.27	108.46	12%		71.8	133.9	54%
Oregon	19.42	114.6	17%		133.6	222.4	60%
Pennsylvania	9.9	106.87	9%	39%[d]	56.23	193.1	29%
Washington	20.94	121.01	17%		133.53	257.7	52%
Wisconsin	20.31	102.04	20%		127.81	308.6	41%
Mean	19	113.22	17%		107.78	217.91	52%

Monthly amount and monthly wages in manufacturing are averages over 1911-1930.

a. N=. Computed from MP records using observations with maternal earnings.
b. N=244. Computed using the average family pension 1915-1919 in Iowa MP records and average income from the 1915 Iowa Census.
c. N=62, Clay County MP records 1930.
d. N=2,404. 1926 Pennsylvania Study.
e. N=100. First 100 cases in Hamilton County Ohio. 1914 (Bullock, 1915). Computed as the average MP pension divided by monthly wages of mothers. Other sources of income not reported.

Appendix Table 6: Summary statistics for Ohio and WWII samples

	Ohio large sample with unique matches		WWII sample with unique matches	
	Rejected	Accepted	Rejected	Accepted
Individual characteristics				
Year of application	1921.0	1920.9	1922.7	1923.1
YOB of child	1912.6	1912.7	1914.8	1915.7
Child age (years)	8.41	8.21	7.87	7.39
Number of kids in family (imputed)	3.42	3.75	3.55	3.76
Age of oldest kid in family record	11.46	11.57	11.37	10.97
Age of youngest kid in family record	5.3	4.72	5.09	4.46
Length of family name	6.34	6.36	6.18	6.24
Widow	0	0	0.54	0.53
Divorced	0.01	0	0.03	0.03
Husband abandoned, in prison/hospital	0.15	0.19	0.18	0.22
Mother's marital status unknown	0.32	0.32	0.25	0.22
Day or month of birth missing	0	0.01	0.02	0.02
Number of children	375	2190	297	2,127
Number of counties	13	17	49	79

Appendix Table 7: Assessing possible counterfactual groups from the 1900-1930 censuses

Group	Rejected	Accepted	Poor Blacks	Orphans	Poor Mothers	widow	not widow
Panel A: All							
Year of application	1,920.7	1,921.6	1925.84	1927.10	1926.54	1926.47	1926.89
YOB of child	1,912.0	1,913.3	1915.59	1914.91	1914.79	1914.43	1916.70
Child age (years)	8.71	8.21	10.25	12.18	11.75	12.04	10.19
Number of kids in family	3.62	3.76	3.54		3.09	3.20	2.48
Age of oldest kid in family	11.87	11.47	13.42		14.23	14.66	11.97
Age of youngest kid in family	5.57	4.81	6.17		8.76	8.89	8.09
Length of family name	6.39	6.44	6.14	6.54	6.39	6.39	6.40
Widow	0.51	0.54	0.83		0.84		
divorced/single/deserted	0.04	0.03	0.17		0.16		
Missing age at death	0.51	0.47	0.58	0.53	0.49	0.49	0.47
Day or month of birth missing	0.02	0.03	0.58	0.53	0.49	0.49	0.47
Number of matches	0.51	0.56	0.85	0.83	0.89	0.88	0.91
Quality of merge with DMF file	1.2	1.18	1.09	1.08	1.06	1.06	1.05
N			125	1797	3756	3152	604
Panel B: Unique matches							
Year of application	1,920.8	1,921.8	1926.15	1927.68	1927.10	1926.94	1927.88
YOB of child	1,912.0	1,913.5	1916.50	1915.13	1914.94	1914.56	1916.86
Child age (years)	8.78	8.26	9.65	12.55	12.16	12.39	11.02
Number of kids in family	3.55	3.72	3.85		3.12	3.20	2.72
Age of oldest kid in family	11.83	11.43	13.31		14.62	14.93	13.07
Age of youngest kid in family	5.62	4.85	5.00		9.04	9.13	8.59
Length of family name	6.33	6.36	6.58	6.78	6.51	6.52	6.45
Widow	0.52	0.55	0.85		0.83		
divorced/single/deserted	0.04	0.03	0.15		0.17		
Day or month of birth missing	0.01	0.02	0.00	0.00	0.00	0.00	0.00
Quality of merge with DMF file	1.19	1.17	1.08	1.12	1.08	1.08	1.08
N			26	512	1227	1024	203

Appendix Table 8: SES of children ages 0-18 based on marital status and presence of children in the 1915 IOWA census

Sample: Children 0-18 in families with at least one child under 14.	Widow	Divorced	Single	Married women no married man	Married woman and married man
1915 Iowa Census					
N of individuals ages 0-18	604	97	143	970	14,792
Total family earnings	684	316	280	504	1,109
% below 30th percentile of family earnings	52	73	80	66	17
Number of kids under 14 in family	2.6	1.8	1.9	2.7	3.1
Number of people in family	5.4	3.4	3.7	5.3	6
Earnings per capita	127	93	76	95	185
% in family that owns house/farm	50%	7%	15%	27%	47%
Value of house/farm (if owns)	7,372	2,146	6,697	12,498	11,042