The Impact of Cash Transfers to Poor Mothers on Family Structure and Maternal Well-Being

Anna Aizer, Sungwoo Cho, Shari Eli and Adriana Lleras-Muney*

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Abstract

We use newly collected data for 16,000 women who applied for Mothers' Pensions, America's first welfare program, to investigate the effect of means-tested cash transfers on lifetime family structure and maternal well-being. In the short-term, cash transfers delayed marriage and lowered geographic mobility. In the long run, transfers had no impact on the probability of remarriage, spouse quality or fertility. Cash transfers did not affect women's well-being, measured by longevity and family income in 1940. Given the lack of significant negative behavioral impacts, the benefits of transfers appear to exceed costs if they have, even modest, positive impacts on children.

JEL codes: I12, I14, I18, I32, I38, J16, N32

*Anna Aizer: Brown University and NBER, anna_aizer@brown.edu; Sungwoo Cho: UCLA, chosw13@g.ucla.edu; Shari Eli: University of Toronto and NBER, shari.eli@utoronto.ca; Adriana Lleras-Muney: UCLA and NBER, alleras-muney@gmail.com. We thank Joe Price who directed the team of researchers at the BYU Record Linking Lab, and without whom this project would not have been feasible. We also want to thank the excellent research assistants who have contributed to this project: Amanda Loyola Heufemann, Ariadna Jou, Keyoung Lee, Xuan Zhang, Tomas Guanziroli, James Uguccioni and Diego Zúñiga. We thank Nathan Hendren and Ben Sprung-Keyser, who helped us with the Marginal Value of Public Funds computations in this paper. We have benefited from conversations with Pedro Dal Bó, Bo Honoré, Caroline Hoxby, Petra Persson, Robert Pollack, Aloysius Siow and from the comments of the seminar participants at Carnegie Mellon University, the NBER Summer Institute, University of Minnesota, Princeton University, Queen's University, RAND, University of British Columbia, University of Toronto, University of Urbana-Champaign, Wharton, Washington University, UCLA, and Yale University. This project was supported by the California Center for Population Research at UCLA (CCPR), which receives core support (P2C- HD041022) from the Eunice Kennedy Shriver National Institute of Child Health and Human Development (NICHD) and from NIH grant 1 R01 HD077227-01. All errors are our own.

1 Introduction

Since the implementation of the first means-tested cash transfer program in the US in 1911, the Mothers' Pension (MP) program, critics have argued that welfare leads to the erosion of families by incentivizing mothers to remain single and have children out of wedlock, thus trapping women and their children in a cycle of poverty (Skocpol 1995; Chappell 2011). Because of their high poverty rates, single-headed households with children have always been the main target of means tested transfer programs. Indeed today, 87% of adult welfare recipients are unmarried (ACF 2021), suggesting to some that welfare disrupts family formation. However, there is little empirical evidence to support this claim. Existing research on the short run effects of welfare on marriage and fertility is ambiguous, and there is a lack of empirical research on the lifetime effects of welfare on family structure and maternal well-being.

In this paper, we construct a new dataset and exploit a novel identification strategy to estimate the lifetime effects of the MP program on family structure and maternal lifetime well-being. The MP program was first implemented in 1911 in Illinois, adopted across most states by 1920, and finally replaced in 1935 by the federal Aid to Dependent Children (ADC) program, the precursor to Temporary Aid to Needy Families (TANF), today's welfare program. Before 1911, mothers who could not care for their children were forced to place them in orphanages or training schools. In response to reports of high death rates and poor outcomes of children in these institutions, states established MP programs to provide cash transfers to poor mothers so that they could care for their children at home. Since the inception of MP, commentators have been concerned about the unintended effects of the program with respect to family structure (Leff 1973). Indeed, MP recipients who remarried lost transfers, encouraging women to remain unmarried; abandoned women could receive transfers, encouraging men to abandon their families; finally, the transfer was an increasing function of the number of children, encouraging out-of-wedlock fertility.

In order to estimate the short- and long-term impact of welfare receipt on marriage and fertility, we construct a novel dataset of over 16,000 women who applied for the program between 1911 and 1930, and follow them from the time of application until their death. It is challenging to track women over their lifetimes due to name changes, and for this reason historical work tends to focus on men (see Abramitzky et al. 2021 for discussion). To overcome this, we match data from the program's administrative records to family trees from FamilySearch.org, federal census records and vital statistics. The family trees represent a new source of data that aids in the tracking of changes in marital status (and names) as well as fertility.

The MP records include mothers who applied to the program and were accepted as well as those who passed an initial eligibility screening, but were ultimately rejected. This allows us to implement a novel identification strategy: comparing accepted and rejected applicants to estimate the causal impact of welfare receipt on family structure using OLS and machine learning approaches. Most existing work in this area leverages changes in state laws or policies over time that modify benefits or eligibility, an intensive margin where we might expect more limited effects (Bitler et al. 2004; Hoynes 1996; Blank 2002; Blundell, Pistaferri, and Saporta-Eksten 2016; Grogger and Karoly 2005). Moreover, the women are typically followed for only a short period of time. Our data allow us to estimate how the receipt of cash transfers at the extensive margin affected marriage market outcomes (remarriage, duration to remarriage, and characteristics of the new husband), and fertility over the mother's lifetime.

We find no difference in the lifetime remarriage rates of women who received transfers and those who did not: 47 percent of the women remarried, regardless of welfare receipt – our point estimates are small and we can rule out decreases in marriage rates above 10%. We also fail to find any effects on fertility: Although women who received transfers had more children *before* the application, they did not have more children *after* the application. We can rule out increases in fertility greater than 5%. However, among those who remarried, those with transfers took about a year longer (14 months) to do so.

Could delays in remarriage be welfare-enhancing? To answer this, we develop a model of welfare participation and search in the marriage market, similar to models of unemployment insurance in the market for labor and as suggested by Hutchens (1979). In the model, women search for husbands who are heterogeneous in quality. Receiving welfare benefits, like unemployment insurance, may cause a woman to be more selective when remarrying (her reservation "husband quality" increases) as it enables her to wait longer for the arrival of a preferred partner. Thus, the model predicts that receiving cash transfers can lead to delays in marriage but also increases in the quality of the husband, with implications for maternal and child welfare.

However, similar to the empirical literature on unemployment insurance (UI), which largely finds that UI results in longer unemployment durations but no improvement in the quality of the next job (Card, Chetty, and Weber 2007; Lalive 2007; Van Ours and Vodopivec 2008; Schmieder, Wachter, and Bender 2016), we find that welfare receipt results in longer time to remarriage but does not affect the characteristics of the new husbands. We provide two explanations for this. The first is that the marriageability of women declines with the duration of search. This is analogous to the explanation offered in the UI literature: as workers spend more time out of the labor force, their productivity declines, reducing the potential quality of their next match. Indeed, in our data, remarriage rates fall rather dramatically with maternal age, consistent with marriageability declining with age. The second possible reason is that welfare receipt is stigmatizing and also reduces marriageability, consistent with Moffitt et al. (1983). Although we cannot empirically test for stigma in our setting, we show that theoretical predictions regarding the impact of welfare on husband quality become ambiguous once stigma is introduced.

To estimate the impact of cash receipt on women's overall welfare, we compare the longevity and household income of accepted and rejected mothers. Longevity is an important determinant of overall lifetime welfare (Jones and Klenow 2016). We find no significant differences in the overall longevity of accepted and rejected mothers. These results are consistent with the results of Price and Song (2018) who find no effects on adult longevity among participants of a negative income tax experiment. We also find no changes in household income of accepted mothers in 1940 (another important determinant of well-being), at least a decade or more after the mother's application. Thus, fears regarding the negative influence of welfare on mothers do not appear to be borne out in the data: Welfare does not impoverish women in the long run. However, welfare does not lift them out of poverty either. Overall, welfare appears to have little impact (either positive or negative) on mothers in the long run. This contrasts with evidence of long term benefits to children of welfare participation (Aizer et al. 2016; Hoynes, Schanzenbach, and Almond 2016).

There is a large literature in economics that investigates the effects of welfare on marriage and fertility (reviewed in detail later). The early work, reviewed by Moffitt (1992), found small effects but was not well identified, typically comparing marriage rates and fertility across states with different benefit levels. The second generation of papers concentrated on the effects of the 1996 welfare reform, which increased work incentives and reduced incentives to remain single and to have more children. This work is better identified, though it shifts the focus away from estimating the effects of receiving welfare to estimating the impact of changes in the design of cash welfare benefits. Welfare reform has been found to reduce marriage rates in some studies (e.g. Low et al. 2018a) though not others (Grogger and Bronars 2001), with little evidence of any impact on fertility (Kearney 2004). One factor that complicates identification is that welfare reform in the US occurred amidst the backdrop of a strong economy and significant demographic changes (increases in non-martial fertility). Moreover, all these studies focus on short run effects and none consider either lifetime impacts of how welfare might affect the qualities of the new spouse.

We make several contributions to the literature. We are the first to follow a large sample of women over their lifespan. Our results show this matters: For marriage, the negative short-term effects of the program fade significantly over time resulting in small and insignificant lifetime effects.

Second, our data also allow us to study the quality of the new match, which has not previously been considered or estimated. Here, our results present a puzzle to be resolved which mirrors the identical puzzle in the UI literature. Third, because our data include the longevity of welfare applicants and family incomes in 1940, we can estimate the impact of welfare on two major determinants of lifetime well-being. Taken together, our results show that welfare receipt did not create perverse incentives as was feared by U.S. policymakers in the early twentieth century. These same concerns played a large role in the dismantling of cash welfare beginning in 1997.[1] Today, these same fears are expressed by policymakers during debates over expansions of non-work and non-means tested transfers such as the Child Tax Credit or Universal Basic Income. Concerns regarding incentives embedded in such program to remain unmarried or have more children are voiced by the general public as well.[2]Our results suggest that such concerns may be unfounded, that unconditional cash transfers would likely not generate perverse incentive effects with respect to family structure, nor consign parents to a lifetime of poverty. While family structure has evolved since the early-mid twentieth century, which might raise concerns about applying such lessons to the current setting, the short run evidence on the impact of welfare reform on fertility, from a more recent period, is certainly consistent with our findings (Kearney 2004).

Finally, we can incorporate the impact of maternal behaviors and outcomes in the evaluation of the MP program by computing the Marginal Value of Public Funds (MVPF) using the methodology of Hendren and Sprung-Keyser (2020). We show that the MVPF is less than 1 when we consider effects on maternal behaviors and outcomes only. However, if there are even modest benefits to the children in terms of longevity or income, as found in Aizer et al. (2016), the program pays for itself. This suggests that the overall evaluation of the program depends crucially on children's outcomes and less so on the outcomes or behaviors of mothers.

2 Background on the MP Program and Existing Literature

In this section we explain the structure of the MP program, including a description of eligibility and benefit determination to clarify the incentives of the program with respect to family structure (marriage and fertility). Then, we situate our contributions in the context of the existing literature.

[1]Welfare reform in 1996 significantly reduced the availability of cash assistance in the US and added significant conditions. In 1996, the year before welfare reform, 68% of poor mothers received welfare assistance compared with 22% in 2018. Moreover, the form of assistance has transformed from unconditional cash, to other, mainly non-cash, forms of assistance all of which included conditions.

[2]In "Working Class Americans' Views on Family Policy," participants in focus groups expressed differing opinions regarding the role of policy in shaping family structure, with older participants expressing more concern about incentives that reduce marriage and increase fertility than younger participants Brown (2021).

2.1 Structure and Incentives of the Mothers' Pension Program[3]

In the early twentieth century, widowed or abandoned mothers had few options to earn a living and support their children. Marriage was by far the most common way for these women to address their economic needs.[4] With limited ability to provide for their children, many poor, single mothers were forced to place their children in orphanages, the main form of poverty relief for children provided by local governments (see Skocpol 1995, p. 425). In response to poor outcomes for institutionalized children, states embraced cash transfers to poor mothers so they could care for their children at home. Illinois was the first state to do so in 1911, and by 1920, most states had followed suit. By 1931, every state in the continental US had an MP program with the exceptions of South Carolina and Georgia. The MP program inspired the eventual implementation of Aid to Dependent Children (ADC) with the passage of the Social Security Act in 1935.

The MP program was funded and administered by individual counties, after states passed the authorizing legislation. There was variation in how MP programs were administered from county to county within a state, and variation in the program's implementation across states. For instance, in most states the program was administered through the county's juvenile court or county clerk's office, but in some states, separate bureaus of child welfare were opened to specifically adjudicate the applications of poor mothers to the MP program.

Eligibility criteria for aid differed across states.[5] Widows, women with husbands in jail or in an asylum, and women with disabled husbands were almost always eligible.[6] However, women who had been deserted or divorced were eligible in some states but not others. Some states required periodic reapplication, while others granted payments until the child turned 14 or 16 years of age. In all states, income and property thresholds were not provided in explanations of eligibility requirements. Rather, need was determined by local administrators at the time of application who would also determine the amounts granted to each applicant. These amounts were capped at the state level but were otherwise discretionary. The pensions provided about one-third of family income at the time, and the median duration of transfers was about 3 years (Aizer et al. 2016).

Women would apply to the program without knowing the income thresholds for eligibility. They would then undergo an initial review that was usually conducted by a social worker. After the review, a judge or adjudication panel would make a final determination regarding the application and the amount of pension to be granted. The data we have are based on the judge or adjudication

[3]We provide a brief description of the program here. More details are provided in Aizer et al. (2016).

[4]In the 1910 Federal Census, the vast majority of white women with children were married (92%) and very few of them worked (4.7%) (Appendix Table 1).

[5]The details for the states we study are given in Appendix Table S1 of Aizer et al. (2016).

[6]In three out of the eleven states that we study, only widows were eligible.

panel decisions. That is, our sample consists not of all women who applied, but all women who "passed" the initial eligibility screening and whose eligibility was determined by the judge or panel.[7] Women were denied pensions for many reasons. Most commonly mothers were rejected because their income or wealth levels were deemed to be too high.[8] In Iowa, rejected applicants had a 35 percent higher predicted family income related to accepted applicants prior to receipt of MP.[9] Other reasons for rejection include the following: 1) ineligible (which may also include income in excess of the standard of need); 2) married or husband returns; 3) moved from county; 4) no children eligible; or 5) not a citizen.[10] For one county, Clay County, Minnesota, we have detailed information collected by a nurse who, through home visits, reviewed the living conditions and needs of all MP recipients in the county. The records from the nurse home visits are largely consistent with the above evidence on reason for termination: Families that appeared to have other sources of income were removed from the rolls whereas those that appeared to be in significant need remained on the rolls.

The program design created disincentives to remarry or move residences, as either would immediately disqualify mothers. Transfers increased with the number of children, creating incentives to have more children. Maximum transfers ranged from $9 to $15 per month for the first child and $4 to $10 for each additional eligible child depending on the state. Incentives with respect to work were less uniform across the states. This may be in part because maternal work outside the home was relatively rare at the time particularly among white women (Goldin 2006). In several states (6 out of the 11 that we study), women were required to stay home as a condition of the transfers, since the cash transfer was given in exchange for looking after the children. Other states limited the hours women could work; still others enacted a 100% benefit reduction rate on earnings. More generally, working women were by definition less likely to be deemed eligible since they had a source of income.[11] Given the variation across programs, for our analysis we include county of application FE to address any heterogeneity across counties in the administration of the program.

[7]For some counties, not all years are available, but for years in which records are available, we believe we have the universe of records for this second stage of the application process.

[8]See Table 2 of Aizer et al. (2016)

[9]See Aizer et al. (2016), p. 10-11.

[10]See Table 2 in Aizer et al. (2016) which shows the frequency of rejection by reason.

[11]While the MP program has many similarities to modern day welfare, there are important differences. Both are means-tested programs that offer unconstrained, but limited, cash transfers. The MP program terminated eligibility upon remarriage (to any man), creating strong disincentives to remarry. The modern-day welfare program terminates benefits upon marriage or cohabitation with the child's father, not necessarily any man. The MP program discouraged work —several states required women to stay home as a condition for the transfer, although some regulated the amount of work or simply lowered the transfers when women brought income home. This continued to be the case in most states until the 1996 welfare reform which capped lifetime benefits and required recipients to work.

2.2 Existing Literature on Cash Transfers and Maternal Behavior

Moffitt (1992) reviews the existing research on the theoretical underpinnings and empirical evidence regarding the incentive effects of the US welfare system with respect to labor supply, family structure, and migration. With respect to family structure, because welfare benefits have historically been paid only to single mothers with dependent children, Moffitt writes, "the program provides an obvious incentive to delay marriage, increase rates of marital dissolution, delay remarriage and have children outside of a marital union" (page 27). Empirically, women on welfare are indeed less likely to marry and have more children (Hutchens 1979; Teitler et al. 2009).

However, there is little evidence that these effects are causal. Women on welfare differ in important ways from women who are not on welfare. These differences may explain their lower marriage rates and their higher fertility rates. In fact, though the cross-sectional comparisons across states suggest a positive relationship between welfare generosity and single motherhood, the time series evidence does not. While welfare benefit levels were increasing between 1960 and 1976, so were rates of single motherhood. However, when benefit levels started to fall from 1976 to 1984, the share of single-parent headed families continued to rise, inconsistent with welfare benefits lowering marriage rates. More detailed analyses based on comparisons within states over time confirm this finding: Changes in benefit levels within a state are not accompanied by changes in single motherhood (see Moffitt 1992).

More recent work has focused on the effects of welfare reform efforts, namely the use of sanctions, time limits, and work requirements, on maternal behavior. This literature has found larger impacts of welfare on family structure. Moffitt, Phelan, and Winkler (2015) find that welfare reforms did increase the probability of cohabitation with a biological father, but not other males. Bitler et al. (2004), Bitler, Gelbach, and Hoynes (2006), and Low et al. (2018a) find that time limits on beneficiaries imposed by the 1996 welfare reform reduced divorce rates and increased the likelihood that children live with unmarried parents. Kearney (2004)finds that welfare reform efforts that reduced financial incentives to have more children did not increase fertility. Work estimating the impact of welfare reform on intergenerational correlations in welfare receipt suggest that welfare reform did reduce daughter's reliance on AFDC/TANF, but not other safety net programs such as SNAP or SSI (Hartley, Lamarche, and Ziliak 2022.)

There is a related literature investigating the effects of other redistributive programs on marriage. The earned income tax credit (EITC) also contains disincentives to marry (Hotz and Scholz 2007), but empirical work has found these effects to be economically small or insignificant (Dickert-Conlin and Houser 2002; Herbst 2011, Michelmore 2016).

Several papers investigate how the marital status requirements embedded in women's eligibility for pensions upon the death of their husbands affect remarriage. These papers find larger effects on marriage rates (Salisbury 2017; Brien, Dickert-Conlin, and Weaver 2004; Persson 2017). The subjects in these studies however tend to be older and richer than the average welfare-recipient. As a result, the opportunities for and benefits of remarriage may be lower.[12]

We make several contributions to this literature. First, we use a credible identification strategy at the individual level to investigate the effect of welfare receipt on remarriage decisions, examining the extensive margin where one might expect to see larger effects. Second, we follow women over their lifetime and establish not only whether they marry, but when they marry and who they marry, as well as whether they have more children. Third, we estimate the impact of welfare on a lifetime summary measure of well-being: longevity. Finally, we calculate the MVPF of the MP program taking into account both the behavioral impacts on mothers and the benefits of cash transfers to children.

3 Model of Welfare Receipt and Search in the Marriage Market

We adapt the canonical model of search in the labor market with unemployment insurance, first developed by McCall (1970), to model search in the marriage market with cash transfers.

In McCall's original model, an unemployed worker searches for employment. Offers of employment vary in quality, as measured by the wage, with a known distribution. Unemployed workers receive offers, which arrive at a given rate, and accept an offer if the offered wage exceeds the worker's reservation wage. If the worker rejects the offer and remains unemployed, they retain the option of waiting for another potentially better offer in the next period. In this model, unemployment insurance increases the value of remaining unemployed, thereby increasing the reservation wage. The model yields two predictions. First, workers with unemployment insurance will remain unemployed for longer than those without. Second, when workers with unemployment insurance do accept an offer, the wage will be higher.[13]

We adapt this model to the marriage market where women are searching for husbands and offers of marriage arrive at an expected rate. Like offers of employment, offers of marriage also vary in quality. Cash transfers (welfare) have the same effects on the marriage market that unemployment insurance has in the labor market: It increases the woman's outside option and therefore the "reservation quality of the match," extending her duration of search (the time to marriage), and

[12] Dillinder (2016) also considers effects of Social Security receipt. Additionally, (Fox 2017) investigates the effect of tax incentives on marriage as do Whittington and Alm (1997) and Fitzgerald and Ribar (2004).

[13] Other features have since been added to this model, such as simultaneous offers (Burdett and Judd 1983).

resulting in a higher quality husband when she does remarry. After describing the model, we discuss how to test its predictions with respect to the quality of the spouse in the data.

3.1 A Basic Model of Search in the Marriage Market

A single woman must decide every period whether to marry or to stay single. If she stays single, she has the option to marry the next period. If she marries, she will stay married forever.[14] Her patience level is given by her discount rate β. She searches for partners, and prospects arrive at a Poisson rate λ. Each prospect has a value of q, which summarizes his quality as a husband. This value has an unknown distribution in the population, $q \sim F(q)$ with support $\left[\underline{q}, \bar{q}\right]$ and $\bar{q} > b$. While she is single she receives a cash transfer of value b every period, but this transfer is lost upon remarriage.

The value of being single is given by

$$V_s = b + \beta\big(\lambda \int_{q=\underline{q}}^{\bar{q}} \max\left\{V_m(q), V_s\right\} \, \mathrm{d}F(q) + (1-\lambda)V_s\big).$$

and the value of being married to prospect q is given by:

$$V_m(q) = q + \beta V_m(q) = \frac{q}{1-\beta}.$$

In this set-up, the agent accepts an offer to marry prospect q if $V_m > V_s$. Since the value of marriage is strictly increasing in q, the agent will follow a cut-off rule. There is a q^* such that she will accept all prospects with $q > q^*$. The cut-off rule is implicitly defined as

$$V_m(q^*) = V_s.$$

Considering that, and rearranging the definition of V_s, we can write

$$(1-\beta)V_s = b + \frac{\beta\lambda}{1-\beta} \int_{q=q^*}^{\bar{q}} \big(1 - F(q)\big) \, \mathrm{d}q,$$

This function is continuous and positive at $q^* = b$ and negative at $q^* = \bar{q}$, so there exists a solution, and because it is strictly decreasing, the solution is unique. Intuitively, this equation states that the value of the minimum acceptable marriage, q^*, should be equal to the benefit, b, plus the option value of holding out for a good match. Given a reservation quality q^*, the probability of marriage is $\lambda\left(1 - F(q^*)\right)$ and the average match quality is $\mathbb{E}[q|q > q^*]$. The duration until

[14] This is a simplifying assumption, but it is well supported by the data. Most women in our sample marry only once (only 5.6% married more than once after the transfer).

marriage is given by $D = 1/\lambda\left(1 - F(q^*)\right)$. Duration is decreasing in the arrival rate and increasing in reservation quality.

3.2 Model predictions and testable implications

The following propositions are derived from this model. All proofs are provided in the Appendix.

Proposition 1. $\partial D/\partial b > 0$ and $\partial \mathbb{E}[q|q > q^*]/\partial b > 0$: *An increase in benefits b increases the number of periods the woman stays single and the average quality of the marriage.*

It is straightforward to test whether receiving a transfer leads to longer durations until remarriage. Testing whether the quality of the match increases among those who marry is more difficult because there is no single indicator of the quality of a match. Suppose instead that there are many traits X that matter but that prospects can be ranked using a single index function $q(X)$ as in Becker (1973). If this function is known, then we can test the predictions in Proposition 1 by constructing this index function. Alternatively, if the function is not known, then we can investigate how transfers affect each trait X. The following proposition holds under the assumption that q is increasing in all its arguments X:

Proposition 2. *Without further assumptions about the joint distribution of X and the production function $q(X)$, the sign of $\partial \mathbb{E}[x_i|q > q^*]/\partial b$ is ambiguous for all i. However, the sign of $\partial \mathbb{E}[x_i|q > q^*, x_{-i}]/\partial b > 0$ for all x_i so long as all relevant X are observed.*

This proposition states that the theory does not provide any guidance about the effect of transfers on any one "input" into quality without knowing their joint distribution and how women trade-off these characteristics.[15] But the proposition also states that conditioning on one measure of quality, the other measure of quality will unambiguously increase with an increase in the transfer. If both measures of quality are observed, we can test this empirically by conditioning on one trait and estimating the impact of the transfer on another trait.

[15] In fact, we might observe that the average quality for any one trait (or for all traits) might decrease with the transfer even though the actual match is better. For example, consider a quality function $q(x_1, x_2) = x_1 x_2$. The joint distribution of the traits is uniformly distributed over three mass points $(1, 10); (10, 1); (4, 4)$. Suppose that, initially, the cutoff is $q^* = 10$. The average of each trait conditional on a match is equal to 5. Consider a small increase in the cutoff ($10 < q^* \leq 16$). The new average of each trait is 4, lower than in the original situation, and suggesting that the average quality has gone down. However the quality of the match after the cutoff increase is 16, higher than the average quality before the cutoff increase.

4 Data

4.1 Data Collection

Administrative data on MP applicants were collected directly from state and county archives in 14 states, 10 of which included rejected applicants in their records.[16] We limit the sample to mothers from the 10 states with rejected applicants, and to those who applied before 1930, when most MP programs lost funding.[17] To track MP mothers and their children, we match these administrative data to family tree data available on FamilySearch.org, which includes more than 1.2 billion people.[18]The mother's name, combined with the names and dates of birth of her children, enables us to locate the mother on a family tree. Once a mother has been found, we observe her maiden name, her date of birth, her date of death, and the names, dates of birth and dates of death of all her husbands and children. For all women in our sample, we employed researchers at the BYU Linking Lab to search for any evidence that she married after the MP program using information in the trees. This strategy of using families to create matches was pioneered by Joseph Price at the BYU Lab (see Price et al. 2019). [19]

In addition, we had the BYU Lab researchers hand match all other records available on Ancestry.com and FamilySearch.org (e.g. the Social Security Death Master File, other state death records, cemetery records, birth certificates and marriage certificates). Therefore, we do not only rely on the family trees that were available. We improve on strategies in the automated linking literature as well as the tree-matching literature by individually searching for each mother's marriage information in available records on the genealogy sites. Finally, research assistants also manually linked mothers and their new husbands to 1910, 1920, 1930 and 1940 Census Records if these links are not already made in the family tree. We observe several measures of the characteristics of the new husband: his education, longevity, age, and occupation, as reported in various census years. We describe our matching methodology in more detail in the Online Appendix.

[16]We study 10 states with early programs (dates of passage in parentheses) for which we obtained data: North Dakota (1915), Idaho (1913), Illinois (1911), Iowa (1913), Minnesota (1913), Ohio (1913), Oklahoma (1915), Oregon (1913), Washington (1913) and Wisconsin (1913). See Aizer et al. (2016) for details.

[17]We also drop a small number of mothers who applied multiple times and those who did not appear to be mothers (grandmothers, sisters and step-mothers). Sometimes a woman appears more than once in our records. In this case, we kept a single record using the following rules: (i) Keep only the observations of the first successful attempt. (ii) If applied successfully more than once the same year, keep the application with more children listed. (iii) Keep the smallest family ID if applied successfully more than once the same year, with the same number of children.

[18]Recent research (Kaplanis et al. 2018) suggests that data from the trees are quite accurate when validated using genetic information. The information also appears to be roughly representative of the population, as life expectancy and other summary measures derived from the trees reproduce population patterns.

[19]While our paper represents the first example of implementation of the technique of linking mothers, more recently others have followed suit. These include working papers by Craig, Eriksson, and Niemesh (2021), Marchingiglio and Poyker (2021) and Withrow (2021).

The resulting dataset allows us to determine if a woman in our MP sample ever remarried, the duration until the marriage and the characteristics of her new husband. They also allow us to track all her children (and when they were born) as well as her own longevity. Thus, we have lifetime measures of marriage, fertility and maternal longevity, as well as the quality of her new husband. To our knowledge, this type of data has never been collected for a sample of welfare recipients. We can also observe employment and occupation in each census year (1920, 1930 and 1940) and income in 1940. These labor market measures, in contrast to our measures of family structure, are only spot measures. Because these data are more limited (and less novel), they are not the main focus of our work.

4.2 Summary Statistics

Our sample includes 16,228 applicants in 132 counties across 10 states. Summary statistics are presented in Appendix Table 2 for the full sample, and for the subsample of unmarried women at the time of MP application (13,383 mothers, or 82% of the full sample). About 53% of the applicants were widowed at the time of application and about 21% were married.[20] The husbands of married mothers were either disabled or in jail, mental institutions or sanatoriums. Very few (3%) were divorced. About 10% of the applicants are rejected. The average woman in our sample was 37 years old at the time she applied and listed 2.6 children under the age of 14 in the application. About 98% are white, and 17% are foreign born.[21]

Forty-eight percent of unmarried MP mothers eventually remarried, and they waited an average of 6.4 years to do so. Only 15% of all unmarried mothers married within 3 years of applying for welfare. When they remarried, they married men who lived almost as long as they did (71 years for men and 74 years for women) but who were less educated than them on average (the education gap is -0.23 years). Post-application fertility was low with only 0.26 children born on average after applying for welfare, already suggesting that any fertility effects are likely to be small.

The information on maternal work, income and location comes from decennial census data so we cannot observe the entire history of employment, income and location. Only 12% of MP mothers were in the labor force in 1910. Women's labor force participation remained low despite their high poverty rates: rising to a max of 37% in 1930 and falling to 26% by 1940. Women's wages and occupational scores were low, as were their incomes (Appendix Figure 1).

[20] The rest do not have marital status, in many cases this is because only widows are eligible.

[21] We have data on the duration of the transfer or reason for termination for only a small subset of the sample – for this reason they are not included here. Therefore, we cannot perform "common" tests in the UI literature such as testing whether people marry just before the end of the transfer.

4.3 Data Quality and Limitations

Historical administrative data have several advantages for this analysis. They allow a long follow-up period and have lower attrition than modern survey data. We discuss these aspects now.

Data Quality. Of the sample of 13,383 mothers who were unmarried at the time of application, we found remarriage data for 84% of the sample. Among those who remarry (5,435), we have the exact date of marriage for 70% of the sample. With respect to measures of new husband quality, we measure longevity for the entire sample, but for other measures such as his wage income from the 1940 census, we find only 52% (see Appendix Table 2). For the mothers, we determined maternal longevity for 80% of the sample and found maternal education for 68% of those who were alive in 1940.[22]

These match rates compare favorably with recent work using US census data from the early part of the twentieth century which hover around 10 to 30%.[23] These rates are also higher than follow-up rates in modern survey data tracking women on welfare. For example, the follow up rate in the SIPP is about 63% over 12 waves/years (Zabel 1998). In the PSID, the follow-up rate for mothers collecting welfare is lower than 40% over 35 years.

All of our data were hand-matched across multiple sources and all data entry were double checked. A validation exercise showed the accuracy of the matches to the tree, the death certificates and the 1940 census to be very high (above 97% in all three cases). We discuss strategies to address missing data and data quality below.

Limitations. We are unable to generalize our results to African American mothers as they accounted for only 1.3% of the population in the counties we study and 2% of applicants in our data.[24] Because of the small number of women who were rejected (only 10% of the sample), we cannot conduct heterogeneity analysis with any precision, though we do present results in an appendix and investigate it using random forest approaches. Last, as previously mentioned, the data on women's labor market outcomes are limited.

[22] There are several reasons we might not find a person. Many of our outcomes come from censuses, which undercount the population particularly in the past (Hogan and Robinson (1993)). We might also fail to find them due to spelling errors or other inaccuracies in the data. Finally birth, marriage and death records are not always available.

[23] For example, Abramitzky, Boustan, and Eriksson (2014) estimating the impact of migration on earnings trajectories achieve match rates of 16% for the native born and 12% for foreign born men. On the higher end, the Life-M Project matches about 30% of birth certificates to death certificates in the states of Ohio and North Carolina (Bailey et al. (2022)).

[24] States and counties with large black populations often did not implement the Mothers' Pension program (Eli et al. (2022)), and when they did, they appear to have systematically discriminated against them as many were never deemed eligible (Eli and Salisbury 2016; Roberts 1993; Ward 2009).

5 Empirical Strategy and Identification

5.1 Empirical Strategy

We test the model's predictions using the following equation:

$$y_{ict} = \beta_0 + \beta_1 Accepted_{ict} + \theta X_{ict} + \gamma_c + \gamma_t + \varepsilon_{ict}$$

where y_{ict} is an outcome for woman i applying to the program in county c in year t. *Accepted* is an indicator equal to one if the mother was given a cash transfer and it is equal to zero if she applied for the transfer but was denied after investigation. We also include county and year of application fixed effects γ_c and γ_t in all baseline specifications to account for the fact each county had a different program that varied over time, and to account for secular trends in outcomes over this period. We can also include a vector of controls (X_{ict}) that includes the characteristics of the mother and family at the time of application: the number of children, age of the oldest and youngest, her marital status at application (widowed, divorced or missing), maternal age at application, and county-level and state-level time varying covariates.[25][26] We report standard errors clustered at the county level. We also estimate standard errors just correcting for heteroskedasticity or clustering at the county-by-year level. The results are robust to these alternatives. Finally, although our main model is linear for many outcomes, we consider alternative function forms.

Our main coefficient of interest is β_1, which represents the impact of welfare receipt on the outcome. Thus, our strategy consists of comparing the mean outcomes of accepted and rejected mothers who applied in the same county and year and were similar on observables. For rejected mothers to be an appropriate counterfactual, it must be the case that they are not otherwise different than mothers who were accepted, as discussed below.

[25] A difference in differences analysis using variation across counties or states over time in the creation of a MP program cannot be conducted given likely violation of identifying assumptions for the following reasons. First, eligibility and generosity varied considerably across states and counties, complicating our ability to use other states or counties as "counterfactuals" in our specification. Second, we do not know for all counties whether/how quickly after a state authorized MP programs the counties developed their MP programs. It could be that a state authorized an MP program but it took years for most counties to develop their programs. As a result, it's not clear how much of a state is actually treated. Third, we cannot identify likely eligible mothers in counties/states before the MP program from available data (e.g., marital status alone does not determine eligibility). Given this, the strongest identification strategy involves comparison of outcomes for mothers who applied in the same county, under the same eligibility rules.

[26] County controls include: sex ratio (M/F) aged 18-55, share females in the labor force aged 18-55, share Black aged 18-55, share rural aged 18-55. County controls match linear interpolated information from the 1910, 1920 and 1930 census with the year of application to the program. State-varying controls 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 expenditure in social programs), 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).

In addition to estimating standard OLS models with and without covariates, we also estimate the average treatment effect (ATE) of the cash transfer on outcomes using the causal random forests methods recently developed by Wager and Athey (2018) and Athey, Tibshirani, and Wager (2019). This approach has several advantages over OLS. First it is a matching approach which provides consistent estimates of the ATE under the standard assumption of unconfoundedness. Like other matching estimators it assumes that untreated observations with similar propensities to be treated as treated observations provide appropriate counterfactuals. This method leverages machine learning, specifically random forests, to find the "nearest neighbors" and computes treatment effects for each treated unit using these untreated but similar observations.[27]

Since we obtain individual level treatment effects, this method allows to investigate if treatment effects are heterogeneous, a second major advantage of this alternative approach. Sloczynski (2022) shows that in the presence of heterogeneity, OLS estimates a weighted average of the treatment effects across groups, where larger groups get smaller (rather than larger) weights. In our case the rejected group is substantially smaller than the treated group, and thus if the treatment effect is heterogeneous, it is possible that the OLS differs from the ATE, the Average Treatment on the Treated (ATT) and the Average Treatment on the Untreated (ATU). The random forest approach allows us to investigate this possibility without imposing any specific functional form in the estimation of the propensity score (and without imposing linearity in the treatment as OLS does). We report the ATE and the ATT that results from this approach. Details of the implementation of this procedure are in the Appendix.

5.2 Identification: Comparing Accepted and Rejected Mothers

Three pieces of evidence presented in Aizer et al. (2016) showed that rejected mothers were slightly better off. We summarize these here and also offer additional evidence based on our new data.

First, investigating the basis for rejection (when available), we found the most common reason (35%) was "other means of support," suggesting rejected mothers had greater incomes. Second, comparing accepted and rejected mothers, we found that the rejected had on average fewer children and that their children were older. We used these characteristics and marital status to predict family income using the 1915 Iowa State Census—the only income data available in the US prior to 1940. Women who were rejected from the program have higher predicted income than those who were accepted, consistent with the evidence on reasons for rejection.A third piece of evidence comes from a comparison of the *pre-application* characteristics of accepted and rejected mothers whom we can

[27] In this method the nearest neighbors are the observations that fall under the same leaves of a given tree.

find in either the Iowa State Census of 1915 (for the Iowa sample of mothers) or in the 1900-1920 US Federal Census for the Ohio sample of mothers.[28] In both cases, we find that for the majority of the variables we observe, accepted applicants were worse off (Aizer et al. 2016)

We use our newly collected data to further assess the pre-determined differences between the two groups. Specifically, we now have information on the mother's educational attainment (from 1940 census records), her date of birth, place of birth, race and ethnicity, the longevity of her first husband, and information on all her children, including those who died prior to applying for the pension, and those who were too old to be eligible (and were therefore not listed in the MP records) but could potentially provide income or other resources to their mothers. We also observe the number of siblings the mother had who could also serve as alternative means of support.

We continue to find that rejected mothers were slightly better off than accepted mothers when comparing them on these newly collected predetermined characteristics (Appendix Table 3).[29] To assess the magnitude of the observed differences between accepted and rejected mothers, we repeat our previous analysis and predict maternal income again but include these newly collected measures. Accepted mothers are more likely to be at the lower end of the distribution of predicted income (Figure 1), but these differences are modest. The predicted income of accepted mothers is about 50 dollars (6 percent) lower than that of rejected mothers (Appendix Table 3). Thus with the newly collected data on mothers, we confirm our previous findings that, on average, accepted mothers appear to be slightly poorer than rejected mothers.[30]

Based on this finding, we may be biased towards finding more harmful effects of welfare on maternal outcomes. For example, this slightly negative selection into MP receipt would likely bias downwards any positive impact of cash transfers on maternal longevity, and lead to overestimates of the impact of welfare receipt on marriage delay and fertility. We conduct two exercises to assess the extent of omitted variable bias. First, we report bounds for β_1 using the Oster (2019) proposed correction to assess the extent to which our assumptions about unobservables affect the coefficient estimates.[31] Second we estimate causal random forest treatment effects, which as ex-

[28]We focused on Ohio because a large portion of our records come from Ohio.

[29]Controlling for county and year of application fixed effects, accepted mothers had more children who died before the application (which is significant for the sample of unmarried mothers) and fewer children over the age of 14. They were also younger, and had husbands who died more recently and at a younger age. All other differences (number of siblings, race, foreign born status, work and occupation in 1910 or education levels in 1940) are not statistically significant in the full sample or in the sample of unmarried mothers.

[30]The mean predicted income of the accepted and rejected groups using the Iowa samples are both higher than in the original AER paper. The main reason is that we can now observe the age of the mother and use this age in the prediction. This results in significantly higher predicted family incomes. We have predicted incomes using many different specifications and control variables and we find very similar results across all of these: Although the means vary, the accepted group is always slightly poorer than the rejected group.

[31]To compute these bounds we assume that the R-max is 1.3 times greater than the R-squared that is estimated in the regression with controls, as suggested by Oster. We assume that δ = (-1, 1) for lower and upper bounds to

plained above flexibly account for observables to construct counterfactual groups, in the spirit of matching methods.

5.3 Assessing the Impact of Missing or Low Quality Data on our Estimates

Missing data. Although attrition in our data is low, missing data can bias our results if the data are missing differentially for accepted and rejected mothers. We investigate whether accepted mothers are differentially missing outcomes by regressing an indicator for missing on the indicator for accepted (Appendix Table 4A). We find no differential attrition in our data for all outcomes related to family structure (remarriage, duration, husband quality and fertility).[32]

We do however find evidence of differential attrition for our labor market outcomes in 1930 and 1940. Labor force participation, occupation scores and family income in 1940 are all less likely to be missing for accepted mothers (Appendix Table 4B). Conditional on controls, the differences are about 10%. The same is true for location and family income in 1940 (Appendix Table 4B).

To address this issue we take two approaches. First, we estimate OLS models that account for attrition using the semi-parametric two-stage approach proposed by Newey (2009). In the first stage we predict attrition, including a predictor (an instrument) that is not part of the main equation of interest. Our instrument for selection is research assistant (RA) finding rates. RAs are assigned arbitrarily to the mothers in our data. RA quality affects the likelihood of finding a match. Thus differences in finding rates reflect RA ability rather than underlying likelihood that the record can be matched based on observables. In the second stage, we estimate a linear regression of the outcome on controls and a fourth degree polynomial of predicted values from the first stage, i.e. a semi-parametrical selection correction term. Second we estimate Inverse Propensity Weight (IPW) OLS models that use the estimated probability of a match as an (inverse) weight in the regressions, as recommended by Bailey, Cole, and Massey (2020) when matching historical data sets. We report both of these alternative estimates in the tables.[33]

Mismatched data. There is considerable debate among economic historians regarding the quality of linked data and how it varies based on various matching methods (Bailey et al. 2017; Abramitzky et al. 2019). We test whether the quality of the match influences our results. To do this, we compute measures of the quality of matches and re-estimate results using only high quality

capture that the omitted variables are positively or negatively correlated with the regressor of interest.

[32] Accepted status predicts only one marriage related outcome at the 5% level (whether the new husband's age at death is observed).

[33] We do not implement these robustness checks for the random forest estimators for which these adjustments have not been developed. Since we find little evidence of heterogeneity (see results section), we view the OLS adjustments to be informative.

matches.[34] We also present results using data from multiple sources – for example we can compare our marriage information from the trees to the information that is derived from the census. If the results are similar across different data sets, this reduces concerns that matches to one source of information may be incorrect.

6 The Effects of Welfare on Marriage and Fertility

6.1 How does Welfare Affect Marriage Decisions?

Unmarried mothers on welfare are not less likely to remarry over their lifetime (Table 1, Column 1). Accepted mothers are slightly (1.4 percentage points) less likely to remarry than rejected mothers (conditional on controls), but the difference is not statistically significant and it is small relative to the average remarriage rate for rejected mothers (47 percent). This effect is not sensitive to how we estimate the standard errors, correct for missing data or whether drop the lowest quality matches. The causal random forest ATE and ATT are somewhat more negative (-0.02 and -0.026 respectively) but they are also statistically insignificant and small in magnitude. Interestingly the ATE, ATT and OLS are similar – in fact we cannot reject the null that they are the same, i.e. that there is no heterogeneity in the treatment effects.[35]

How large are these effects? Using the largest Oster bound, being accepted lowered the probability of remarriage by 0.02 percentage points, an economically small effect. If we use the confidence interval from our main OLS specification or the one from the random forest, we can reject declines in marriage rates larger than 10%. If we compare the estimated impact of MP receipt on remarriage to the impact of age at MP application, we find that the impact of MP receipt on marriage is roughly equivalent to a one year increase in maternal age.

Next we investigate the impact of welfare receipt on duration until remarriageA histogram of the duration to remarriage suggests that rejected mothers were more likely to marry soon (within two years) after applying (Figure 2a). Kaplan-Meier survival estimates of the probability of remaining single, where the clock starts the day of the MP receipt and ends at death, show a similar pattern: accepted mothers remain single for longer and are more likely to remarry later (Figure 2b). While women on welfare are not less likely to ever remarry, they wait longer to do so.[36]

[34] A high quality match is a match with quality above the median. The quality measure is a weighted sum of Jaro-Winkler distance assessing the similarity of the name, place of birth and age match between the different datasets. The data codebook details how we compute each quality measure.

[35] A test that OLS is equal to ATE cannot be rejected. We also test for heterogeneity as suggest by Athey, Tibshirani, and Wager (2019) and find no evidence of significant heterogeneity.

[36] We corroborate these findings using another source of data on marriage in the Census. While there are no differences in marriage rates in 1930 or 1940, there is a statistically significant 25% decline in the likelihood of being

How much longer? A regression of time to remarriage on accepted status suggests 1.3 years longer, which is identical to the causal random forest ATE and ATT estimates (Table 1, Column 2). The coefficient is similar with the Oster bound (1.4) but smaller (0.9) if we drop low quality matches or use IPW. Relative to the duration of 5.47 years to remarriage for rejected mothers, this represents an increase of 20-24 percent relative to the mean. Estimates from an Accelerated Failure Time model (AFT), using the log of the duration as the outcome, are very similar around 24% (column 3).

To further explore timing, we estimate regressions where the dependent variable is whether the mother remarries within a year, two years, five years, etc. For these regressions, mothers who did not ever remarry are coded as zero. Mothers whose marital status could not be defined, or who are missing marriage dates are excluded. We find a marginally significant effect of receiving welfare on short durations but no significant differences on longer durations, consistent with Figure 2 (Table 1, last 5 columns). The coefficient estimate suggests that remarriage within one year is 2.4 percentage points lower for mothers on welfare. Because the baseline is low in the first year (0.04), the relative effects are large: Welfare receipt lowers the likelihood of remarriage by 60% within a year. This falls to 15% within 5 years and is small and insignificant after five years.[37] The year-by-year estimates are presented in Figure 2c and show that the effects, as a percentage of the baseline, are large but decline and become insignificant. The short run effects are larger if we drop low quality matches, but lower if we use the IPW. Overall they are still small and insignificant after the fifth year. The ATE and ATT estimates are somewhat larger but they also decline magnitude relative to the mean. By the 10th year the effect is about a 10% decline in the probability of remarriage.

In sum, duration to remarriage increases between 0.9 and 1.4 years with welfare receipt. This effect is accounted for by short run behavior: women are less likely to remarry but only in the short run. After five years, there are no large differences in marriage rates. Over the lifetime we can reject declines in marriage rates greater than 10%. Our results are consistent with previous research finding of immediate effects of welfare reform on remarriage (Low et al. 2018b), but we are the first to show that over a longer follow-up period, the difference falls to zero. Overall, we conclude that the effects of welfare on marriage are modest, and not as large as short-term estimates imply.

married in 1920 (See Appendix Table 5, Columns 2-4). These results also suggest that cash transfers increased the duration until marriage in the short run, but not the in medium or long run.

[37] We also estimate Logit models. The results are very similar to those reported here.

6.2 Who Do Mothers on Welfare Remarry?

Were these marriage delays associated with increases in the quality of the husband and match as theory predicts? In this section, we describe how we construct our measures of husband and match quality. We follow this with an analysis of whether waiting does increase quality and conclude with an analysis of whether welfare receipt, which leads to delays in remarriage, results in a higher quality husband or match.

6.2.1 Measuring Husband and Match Quality

We calculate five measures of the quality of the new match: three characteristics of the husband and two of the match. The former includes his longevity, his education and his predicted income based on occupation score. Longevity is an excellent measure of health and also an indirect measure of his lifetime resources, as it partly reflects the socioeconomic conditions he experienced as a child and as an adult.[38] Education is a good predictor of permanent income and is also associated with marital stability (Lundberg, Pollak, and Stearns 2016), but it can only be observed in the 1940 Census and therefore not observed for all.[39] Finally, we predict the husband's lifetime income (in 1950 dollars) using the latest pre-marriage occupation observed in census data. [40]

We construct two measures of the quality of the match: the age and education gaps between spouses. We assume that the optimal age gap is 2.5 years based on previous work.[41] For the second measure of match quality, the education gap, a more equal distribution is preferred (Doss 2013; Hitsch, Hortaçsu, and Ariely 2010). We can only compute the latter for couples in which neither has died prior to 1940.

Finally, we combine these measures of husband and match quality into a single index, using two methods. In the first, we standardize all the measures and sum them, giving each equal weight.[42]

[38] Many papers document that conditions in utero affect health and longevity (for a review see Almond and Currie 2011). Another extensive literature shows that individuals nutrition as well as their parents' income and education while growing up predict health (Case, Lubotsky, and Paxson 2002; Hayward and Gorman 2004, see Almond, Currie, and Duque 2017 for a review). Finally, socio-economic status (education, occupation and income) in adulthood are very large predictors of longevity (Cutler, Deaton, and Lleras-Muney 2006; Chetty et al. 2016).

[39] Because 18% of remarried husbands died prior to 1940, it is not observed for all men.

[40] We use the IPUMS constructed "occscore." This measure assigns income to individuals based on their occupation, imputing income in that occupation in 1950. We assign each man the occupation score we observe in the latest census where he is observed before marriage under the assumption that this is the most likely occupation that the MP woman would have observed at the time of her marriage decision.

[41] Empirically, small age gaps predict greater satisfaction (Lee and McKinnish 2018) and lower divorce rates (Lillard, Brien, and Waite 1995), and they are preferred in online dating (Hitsch, Hortaçsu, and Ariely 2010). The optimal gap of 2.5 is based on work by Grow and Van Bavel (2015).

[42] To do this, we first normalize each measure (subtracting the mean and dividing by the standard deviation) and then sum them together as in Kling, Liebman, and Katz (2007). To maximize sample size we use any measure available, so the index is defined for those that have any measures.

In the second, we combine them into an index using the model calibrated by Grow and Van Bavel (2015) which is based on marriage patterns in contemporary Europe.[43] This index corresponds to the utility associated with a given match, which is a function of both the woman's and the man's traits.

6.2.2 Duration to Remarriage and the Quality of the Husband and Match

The basic model predicts that if women delay marriage they will marry more desirable husbands. Duration to remarriage is indeed positively and statistically significantly related to husbands' education, occupation, and longevity; duration is also statistically significantly associated with smaller education gaps and age gaps (Figure 3). To our knowledge this is the first paper documenting that there is a strong correlation between waiting to marry and the quality of the husband.

6.2.3 Welfare Receipt and Quality of the Husband and Match

Comparing the estimated densities of the quality measures for accepted and rejected mothers does not support the prediction that welfare receipt improves the quality of the husband or the quality of the match (Figure 4). The new husbands of welfare recipients do appear to live a bit longer, but they are not more educated or likely to be employed in higher paying occupations. The distribution of match quality (age and education gaps) is also very similar for both groups. We cannot reject the null that the distributions of any trait are identical for accepted and rejected.

Regression analysis yields similar findings. The results (Table 2, Panel A) suggest that mothers on welfare marry husbands who are roughly similar: Except for longevity, all the coefficients for accepted are statistically insignificant. The results without covariates are very similar (Appendix Table 6). While some point estimates are positive (longevity), several are negative (predicted income and education). Estimates of the impact of welfare receipt on match quality (age and education gaps) are also insignificant and often of different signs. A joint test (Column 6) shows that we cannot reject the null that all coefficients are equal to zero at the 5 percent level.

Using the index based on equal weights (Table 2, Panel A, columns 7 and 8), we find a positive and significant effect of welfare receipt on husband and match quality, but this result is mostly driven by the positive impact on longevity and it is small, on the order of 10 percent of a standard

[43] We use the utility function and the parameters defined and calibrated in Grow and Van Bavel (2015). The index is given by $v_{ij} = \left(\frac{S_{max}-|si-sj|}{S_{max}}\right)^{w_s}\left(\frac{y_i}{Y_{max}}\right)^{w_y}\left(\frac{A_{max}-|\alpha_i-\alpha_j|}{A_{max}}\right)^{w_a}$. The first term of the equation is the similarity of education, the second term is the earnings prospects and, the last term is the age gap. We follow the same categorization of variables as in the original paper, except for education, where we divide it in 4 quartile categories instead of the four categories in the paper (no schooling, primary, secondary and tertiary). The calibration parameters are given by $S_{max} = 4; Ymax = 5; A_{max} = 800; w_s = 0.385; w_y = 1.201; w_a = 10.833$. Note that this is **not** a sorting index like those used in Becker's assortative matching models.

deviation in the index. Using the index based on Grow and Van Bavel (2015), the coefficient is small and insignificant (Table 2, Panel A, Column 9). The results using causal random forests are very similar and suggest no overall effect on husband quality (Panel D).

However recall that this test may not be informative since the theory is ambiguous about the effect of transfers on any given trait and there is uncertainty about how to combine the traits into a single index. To address this we repeat the analysis but controlling for other husband traits, as proposition 2 of the model (Table 2, Panel B) suggests. The results are roughly similar and do not unambiguously point to an increase in quality, except for longevity. None of these results are affected by Oster corrections, corrections for missing data or quality of the data (Appendix Table 6). We worried in particular about our use of occupation as a means to assess income since the mapping between occupation and wages/income varied overtime. Our results are very similar if we use two alternative measures of occupation-based income computed by Olivetti and Paserman (2015).[44]

We also rule out that the transfers affected assortative mating (Appendix Figure 2). More educated women were more likely to marry more educated men as they do today. However, this is equally true among both accepted and rejected women. A final test of the hypothesis that quality of the match increased is to examine whether husband and wife live together in 1930 or 1940: these are indicators that the marriage was long-lasting and therefore a good match. We find that accepted mothers are less likely to be living with their spouses in 1930 and in 1940, suggesting that if anything these matches are of worse, not better quality (columns 9 and 10 of Appendix Table 7). We conclude that the transfers did not meaningfully improve the quality of the matches.

6.3 Why Does the Theory Fail?

We consider five possibilities. First, it may be that the attractiveness of women declines with age, just as the human capital of workers declines when they are unemployed. If so, waiting to marry a higher quality husband would result in a depreciation of the mother's own quality or attractiveness (her age and fertility). Appendix Figure 3 shows that, as in other settings, women are much less likely to marry as they age. Theory (proposition 3 in the Appendix) suggests this should not affect the predictions of the effects of the cash transfer–those who receive the transfer should still find better men. But it does suggest that the effects might be small if waiting to marry reduces her

[44] In Appendix Table 6, we show results for these alternative occupation measures and also show additional specifications for the results. In Appendix Table 7 we show results for several other traits of the new husband (1940 income or earnings score, foreign born status, farming status and number of children). The coefficients on accepted are never statistically significant and vary in their sign. In Appendix Table 10, we show heterogeneity in results.

attractiveness. If we control for the age at marriage of the mother, our conclusions are unchanged (Table 2, Panel C).

A second possibility is that there is negative selection into marriage among those who delay (the "best" women marry first). There is little evidence of this once age is accounted for. There are no predetermined characteristics that predict duration to remarriage, aside from her age and number of children (Appendix Table 8), suggesting that negative selection likely does not explain this.

A third possibility is that stigma associated with welfare receipt reduces the quality of the husband. Proposition 6 in the Appendix states that if transfers lower the rate of arrival of prospects or worsens their quality, then the predictions of the model become ambiguous.[45] Thus once stigma is included in the model, the predictions with respect to partner quality can reverse, even if duration is increasing. [46] We cannot provide empirical evidence for stigma but historical accounts suggest that there has always been strong stigma from receiving charitable help, from private or public institutions, and this was also true during the period we study (Skocpol 1995).

Fourth, perhaps welfare did not affect marriage prospects because it created incentives not to move since mothers would loose the transfer upon moving. Though welfare would reduce incentives to move, mothers who do move, should move to "better" places (see Appendix), because location influences marriage prospects and determines long-term outcomes of children (Chyn and Katz 2021). We do find women who receive welfare are about eight percent more likely to live in the same county where they applied for welfare compared those that were rejected (Table 3Columns 1 and 2).[47] Thus welfare receipt significantly lowers geographic mobility. However accepted women move to similar places as rejected women. Moreover, neither group appears to move to better areas relative to where they applied, where "better" places are defined as having higher levels of education, or higher sex ratios. Thus while geographic mobility was affected by transfers, it would seem that marital prospects were not.

Finally, our insignificant results maybe be due to our large standard errors. While for some measures of husband quality we can rule out large differences in quality (e.g. education and occupation),

[45]Cutoff quality moves in the same direction as the benefits, the change in the probability of proposals, λ , and the distribution of quality, $F(q)$. With stigma, the program increases b but lowers λ or the distribution of quality. The original effect increases the cutoff but the stigma effect lowers it. It is unclear which one we should expect to dominate.

[46]The predictions of the model with respect to quality are still ambiguous even though duration increases. This is because a duration increase is to be expected even if quality doesn't change. The only way duration could decrease is if the quality cutoff was substantially lower with the transfer. In other words, both an increase and a small decrease in quality are consistent with duration increasing.

[47]We find no effects on the likelihood of staying in the same state. Thus, the reduction in mobility is local. The Oster bounds are tight for these outcomes (Table 3, Panel B). The largest upper bound we estimate for the effect is 0.10 (from the CI of the Newey estimates), which is a 15 percent increase in the likelihood of remaining in the same county.

for others we cannot (eg, longevity, the indices) due to lack of statistical precision. Specifically using the 95% confidence intervals, we can rule out increases in the education of the husband greater than 3%, or increases in his occupation-based income larger than 5%. For longevity we can only rule out increases larger than 4 years of life (a 6% increase). As a result, the indices that use weights could improve as much as 20% driven largely by the longevity gains.

The bulk of the evidence presented here suggest that even though the transfers did initially delay marriage, in the long run, women who received welfare married similar men and at similar rates relative to women who did not receive welfare. This is most consistent with aging and stigma effects, though our large standard errors do not allow us to completely rule out the possibility quality increased particularly in terms of the husband's longevity. Because most women had lost their husbands, its possible that they valued health and longevity highly when finding a new husband.

6.4 Effects on Fertility

The MP program incentivized fertility. We test empirically whether welfare recipients had more children after receiving welfare. Fertility post application to the MP program was modest: Only 14% of mothers had any children post welfare application and the differences across the two groups are very small (Figure 5). Women on welfare did have 0.421 more children on average, but this difference existed pre-welfare receipt (Appendix Table 3). As Table 4 shows, there is no effect of getting welfare on post-welfare fertility, among all mothers or among unmarried mothers only. To rule out that this is due to the relatively old age of mothers in our sample (median age 37), we show that the results are identical if we look at only the youngest mothers in the sample (Table 4, last two columns).

These conclusions do not change when we correct for missing data, drop observations with low quality, or compute Oster bounds. The causal random forest ATE and ATT are very similar to the OLS estimates: negative, small and statistically insignificant. Nor are they changed when we estimate fertility from census data which only include the number of children in her household in the 1930 and 1940 census (Appendix Table 8). These results, like the results for marriage are closer to precisely estimated zeroes: Among all mothers, we can rule increases in fertility larger than 0.01 children, a small number relative to the mean number of children of 4.5.

In sum, we find no significant effects on fertility post-welfare receipt, although there are significant differences before.

7 Lifetime Maternal Welfare and Implications for Program Evaluation

7.1 Overall Maternal Welfare

Critics of welfare often argue that welfare is harmful to women as it traps them in a cycle of poverty and dependence.[48] To shed light on this, we collected two measures of maternal long-run well-being: longevity and her household income in 1940. In Figure 6, we compare the distributions of longevity (6a) and 1940 household income (6b) of the mother by acceptance status. In both cases, we cannot reject that the distributions are identical (p-values reported in the figure). We confirm this in our regression analysis. There do not appear to be any large or significant effects of welfare receipt on long run maternal well-being (Table 5): Receiving welfare has a small and positive but insignificant effect of 0.25 years on maternal longevity (Column 1), and a small and negative but insignificant effect of roughly $60, -6% decline relative to the rejected applicants' meanincome (Column 2). A difference of 6% is almost identical to the difference in predicted income at the time of application (last column of Appendix Table 3). These estimates are more sensitive to accounting for attrition: The IPW are positive and statistically significant for longevity (0.9) and essentially 0 for income, perhaps suggesting there are indeed some improvements.

However, these results are imprecise. The OLS CI for longevity ranges from -0.8 to 1.4 years . While not statistically different from the OLS point estimates, the causal random forest estimates are larger and the confidence interval for the ATE is [-0.35; 1.4], which includes small to moderate negative effects and large positive effects.

For income the OLS CI ranges from -12% to 1%, and the causal random forest CI is [-9%; 5%]. Given an initial gap of 6%, this suggest that welfare could have decreased income by 6% (3%) or increased by 7% (11%). While not insignificant, these magnitudes do not suggest large effects on economic outcomes. Thus mothers who applied for welfare were poor and remained so by 1940, regardless of welfare receipt, having roughly half of a typical household's income.

Consistent with these results for income, we find that cash transfers had no large disincentive effects on labor market outcomes (Appendix Table 11), though the data on this are more limited in great part because we observe these only in 1920 and 1930.[49] To estimate these effects we restrict the samples only to women who applied for the MP transfer between 1918 and the census in 1920 (and similarly for 1930) and investigate whether their labor market outcomes in 1920 (1930) differ

[48] For example, see Cato Handbook for Policymakers, 8th Edition (2017) chapter 41 Poverty and Welfare

[49] Also because these data are missing at higher rates and differentially by accepted status, see the data section.

as a result of the transfer. Although we cannot confirm that all women in this sample are still receiving welfare, the median duration in our records is of 3 years so we expect most are still in the program. We find no statistically significant effects of receiving welfare on the likelihood that women were in the labor force or that they worked. In addition, we find no statistically significant effects of receiving welfare on their earned incomes or their occupational scores when they worked (Appendix Table 11). The labor supply estimates are very noisy though — they are statistically insignificant and include both large positive and negative effects of the transfers. The lowest 95% CI for the estimates correspond to labor supply effects that range from -6% (a small response) to -30% (a more substantial response) associated with a 30% increase in income due to the MP transfer. For comparison, the estimated extensive margin elasticity in the EITC range from 0.7 to 1 (Bastian 2020 and Nichols and Rothstein 2016). Our estimate is so imprecise likely because we are missing these data for much of our sample and the measure, when we do observe it, is a spot measure. Indeed, the inadequacies of these data are the main reason maternal labor supply response is not the focus of this paper. Interestingly by 1940 (when most applicants would no longer be on welfare) our estimates are positive and significant for labor force participation and work. Thus our estimates suggest that in the long run welfare moms returned to work.

Given that welfare did not affect marriage rates and the type of husbands women married, and that it doesn't change labor market outcomes for women, it is not surprising that family income is unchanged, and that ultimately health was not affected either.

If mother's longevity did not change as a result of the transfer why did the longevity of her children increase? Together these findings are consistent with the idea that childhood is a critical period for physical development, and is in line with other research that finds that the returns to various government programs is largest for children and young adults (Hendren and Sprung-Keyser 2020).

In sum we find very few significant changes in the economic and demographic circumstances of women associated with welfare receipt, explaining why we also find no significant improvements or declines on their long-term wellbeing. Thus, the long run evidence from the first welfare program in the US does not support the claim that welfare harms women.

7.2 Was the Program Worth it? Marginal Value of Public Funds Computations.

Our previous work documented large positive effects of welfare receipt on the education, income and longevity of their sons (Aizer et al. 2016). Here we find that cash transfers resulted in marriage delays of about a year and decreases in geographic mobility. But they otherwise had no statistically significant negative impacts on maternal behavior and no positive effects on maternal outcomes. We now compute the MVPF of the program using the methodology of Hendren and Sprung-Keyser (2020) to determine how these estimates change the overall evaluation of the program.

The computations are in Table 7 Panel A lists the dollar value (in 2019 dollars) of all the benefits and costs associated with the program, using the results from this paper and our previous paper documenting increases in the education, income and longevity of recipients' sons. The benefits of the program are given by the total willingness to pay of recipients. This includes the value of the transfer which lasted three years (about \$20,000), plus the value of the spillovers to sons, minus the dollar value of the negative behavioral responses. In Column 1, we ignore spillovers to children. The negative behavioral response we estimate is a delay in remarriage of about a year, costing about \$3,500. The total costs of the program are given by the size of the transfers (\$20,715) plus or minus the changes in taxes received by the government. Since we estimate that labor supply increases (though this is statistically insignificant), the total cost of the program is a bit lower (\$500) as a result. Considering only the benefits to mothers, once we include the dollar value of behavioral responses, the MVPF of the program is 0.84, below one.

However, a more realistic and comprehensive calculation would also consider whether the transfers benefitted children. Aizer et al. (2016) find that boys' longevity increase by about 1.5 years and that labor market income increased by 10% as a result of the transfer. We use their results on the effects of the transfer on the survival curves, along with estimates of the value of life to compute the present discounted value of children's longevity and earnings increases (using a 3% discount rate). These amount to about \$61,000 which are added to the total willingness to pay estimates. More earnings also reduce the cost of the program through increased taxes, which, assuming at 10% tax rate, amounts to a savings of about \$5,000. Once we incorporate these benefits to the sons, we find that the MVPF of the program is greater than 5, even with maternal behavioral responses (Column 2). The results are similar if we only include spillovers in the benefits and do not count the transfers itself (Column 3, MVPF 3.86).

These computations are subject to uncertainty. Aizer et al. (2016) only tracked the longevity of about 50% of the sons, and the incomes of roughly 15% of the sons in 1940. Additionally they could not track outcomes for daughters. Our computations so far include only benefits for sons and assume there are no benefits for daughters.To address this issue, we compute the smallest increases in income or longevity of the sons that would be needed for the MVPF to be larger than one. We find that if the sons' income over their lifetime increases by only 0.75% then the MVPF exceeds one. Alternatively if their longevity increases by 0.3 years of life, the MVPF would also exceed one.

Thus, relatively small benefits for at least some children allows the program to pay for itself, in part because behavioral responses from the mother are relatively minor, and the benefits accrue to sons over a long time horizon.[50]

8 Conclusion

Tracking over 16,000 women who applied for the first welfare program in the US between 1911 to 1930, we establish that cash transfers to poor women had no effect on lifetime remarriage rates and fertility. Those with transfers were not less likely to remarry over their lifetime, and they delayed remarriage only in the short-term. The cumulative effect was to delay time to remarriage by about a year. These findings underscore the importance of conducting long-term evaluations, as short-term effects can be misleading.

Why were the effects of the program on marriage so modest? One reason is that the transfers were small relative to the lifetime income that a marriage would bring. The average woman that remarried in our sample was 39 years old and married a 43 year old man who died at age 71. Marriage would bring 22 years of income with relative certainty (assuming retirement at age 65). Cash transfers instead accounted for less than half of the income these women needed to live, and receipt was not guaranteed: Women had to reapply and could lose the transfer if they moved, for instance. The median duration of transfers was three years. Thus, a very rough back-of-the-envelope calculation shows that cash transfers represent only 7% of what a marriage would bring over a lifetime.[51]

We also find that women who received transfers did not marry different men. Although women who wait to remarry do marry better husbands in general, delays induced by welfare receipt are not associated with improvements in the quality of the matches. Thus our findings reject the predictions of a simple search model of welfare and the marriage markets. Other forces such as age and stigma may be more important determinants of marriage behaviors than the monetary incentives embedded in these government programs. Incorporating these forces into standard models of behavior and further assessing their empirical importance is an important area for future research.

We conclude that the program did not generate large negative incentive effects as predicted by economic models and as feared by policy makers, nor did it help mothers escape poverty. It did,

[50] The table also shows alternative computations. For example in the benefits of the program we count the transfer as a benefit. If we do not count it, and instead only count the benefits for the children, then we require a 6% increase in child income or a 1.5 increase in longevity for the MVPF to be greater than one.

[51] Assuming that a marriage brought in 100% of family income and that the transfer brought in 50% of that income, we compute that the ratio of cash transfer income to marriage income is 3*50/22*100 = 7%)

however, appear to help alleviate short-term cash constraints. Thus, ultimately the program should be judged largely by the impact it had on its intended beneficiaries — the children.

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Appendix

Model and Proofs

A woman is either single or married. While single, she receives a flow benefit of b and, with probability λ, she finds a potential partner with quality q (the flow utility she would get from marriage) and decides whether to marry him or stay single. For simplicity, we say that marriage lasts forever. The quality of a partner q is distributed $F(q)$ with support $\left[\underline{q}, \bar{q}\right]$ and $\bar{q} > b$. She discounts the future at rate β.

The value of being single is

$$V_s = b + \beta \left(\lambda \int_{q=\underline{q}}^{\bar{q}} \max \left\{ V_m(q), V_s \right\} \; \mathrm{d}F(q) + (1-\lambda) V_s \right).$$

The value of being married to a partner with quality q is

$$V_m(q) = q + \beta V_m(q) = \frac{q}{1-\beta}.$$

Since the value of marriage is strictly increasing in q, the agent will follow a cut-off rule. There is a cutoff quality, q^*, such that she will accept all prospects with $q > q^*$. The cutoff rule is implicitly defined as

$$V_m(q^*) = V_s.$$

Considering that, and rearranging the definition of V_s, we can write

$$\begin{aligned}
V_s &= b + \beta V_s + \beta\lambda \int_{q=\underline{q}}^{\bar{q}} \big(\max \left\{ V_m(q) - V_s, 0 \right\} \big) \; \mathrm{d}F(q), \\
V_s &= b + \beta V_s + \beta\lambda \int_{q=q^*}^{\bar{q}} \big(V_m(q) - V_s \big) \; \mathrm{d}F(q), \\
V_s &= b + \beta V_s + \frac{\beta\lambda}{1-\beta} \int_{q=q^*}^{\bar{q}} \big(1 - F(q) \big) \; \mathrm{d}q, \\
(1-\beta) V_s &= b + \frac{\beta\lambda}{1-\beta} \int_{q=q^*}^{\bar{q}} \big(1 - F(q) \big) \; \mathrm{d}q,
\end{aligned}$$

where the third line followed from integration by parts. From the definition of q^*, we have obtained an implicit equation for q^* (which contains no other endogenous variables)

$$q^* = b + \frac{\beta\lambda}{1-\beta} \int_{q=q^*}^{\bar{q}} \big(1 - F(q) \big) \; \mathrm{d}q. \tag{1}$$

$$0 = -q^* + b + \frac{\beta\lambda}{1-\beta} \int_{q=q^*}^{\bar{q}} \big(1 - F(q) \big) \; \mathrm{d}q.$$

We can see that this function is continuous and positive at $q^* = b$ and negative at $q^* = \bar{q}$, so there exists a solution. Also, the function is strictly decreasing so its solution is unique.

Intuitively, this equation says that the value of the minimum acceptable marriage, q^* should be equal to the benefit, b, plus the option value of holding out for a good match. Given a reservation quality, q^*, the probability of marriage is $\lambda(1 - F(q^*))$ and the average match quality is $\mathbb{E}[q|q > q^*]$. The duration until remarriage is given by $D = 1/\lambda(1 - F(q^*))$.

Before proving Proposition 1, we establish the following useful result.

Lemma. *The reservation quality, q^*, is increasing in benefits, b. Moreover, the reservation quality is also increasing in the probability of finding prospects, λ, and the distribution of quality $F(q)$ (in the senses of first-order stochastic dominance).*

Proof. This result can be seen on equation 1. An increase in b, λ, or the distribution F increases the right-hand side of the equation which corresponds to the value of waiting. In order to preserve the equality, the cutoff must be higher. Waiting is more attractive when the benefits are higher, the offers appear more often, or the offers are stochastically better. Then, the woman will only find it worthwhile to settle for a higher cutoff quality. □

Now, we are ready to prove Proposition 1.

Proposition 3. *$\partial D/\partial b > 0$and $\partial \mathbb{E}[q|q > q^*]/\partial b > 0$: An increase in benefits, b, increases the number of periods the woman stays single and the average quality of the marriage.*

Proof. From our previous lemma, an increase in benefits will increase the cutoff quality. Since the probability of marriage is decreasing in the cutoff quality, the increase in benefits decreases the probability of marriage and increases the expected number of periods the woman stays single. The average quality of the marriage increases because the woman now rejects relatively lower quality proposals. □

In order to test the second prediction of Proposition 1, we would need to observe the quality of the marriage. what we observe are several traits that matter for the quality. We assume that there exists a quality function, $q : \mathcal{X} \to [\underline{q}, \bar{q}]$, that maps a vector of characteristics into a single quality index. For exposition, and without loss of generality, we assume that the function q is increasing in each trait.

Proposition 4. *Without further assumptions about the joint distribution of X and the production function $q(X)$, the sign of $\partial \mathbb{E}[x_i|q > q^*]/\partial b$ is ambiguous for all i. However the sign of $\partial \mathbb{E}[x_i|q > q^*, x_{-i}]/\partial b$ is positive for all x_i so long as all relevant X are observed.*

It might seem natural to expect that higher benefits would result in higher (better) traits in the accepted marriages. This is not necessarily true and it could be that every trait becomes worse.

Example 1. Consider a quality function $q(x_1, x_2) = x_1 x_2$. The joint distribution of the traits is uniformly distributed over three mass points $(1, 10); (10, 1); (4, 4)$. Suppose that, initially, the cutoff is $q^* = 10$. The average of each trait conditional on a match is equal to 5. Consider a small increase in the cutoff $(10 < q^* \leq 16)$. The new average of each trait is 4.

As the example shows, each trait could be, on average, lower with a higher cutoff quality. Still, we can predict an increase in a particular trait when conditioning for all the other relevant traits. In order to see this, notice that for a given value of the other traits, a higher cutoff will only eliminate matches where the trait we are interested in was low.

Extensions

Age

We show that the predictions of the model still hold when we incorporate aging considerations. In order to maintain the simple recursive structure of the model, we model aging as a random independent process that moves the agent from a young state to an old state. In the young state, a woman receives a proposal with probability λ_Y. In the old state, she receives a proposal with probability $\lambda_O < \lambda_Y$. There is a probability π of transitioning from young to old and, naturally, no probability of the reverse transition. The transition, or lack of, is realized at the end of each period after the offer has been accepted or rejected.

The old single woman's problem is the same as the original problem. Let us define $V_{s,O}$ and q^*_O as the value of being single and the cutoff quality when old.

The young woman's problem is slightly different. The opportunity cost of accepting a proposal is given by $V := (1 - \pi)V_{s,Y} + \pi V_{s,O}$, where $V_{s,Y}$ is the value of being single when young.

$$V_{s,Y} = b + \beta \left(\lambda_Y \int_{q=\underline{q}}^{\bar{q}} \max \{V_m(q), V\} \, \mathrm{d}F(q) + (1 - \lambda_Y)V \right).$$

The cutoff rule is defined by $V_m(q^*_Y) = q^*_Y/(1 - \beta) = V$. Then, $\pi(V_{s,Y} - V_{s,O}) = \frac{\pi}{1-\pi}(V - V_{s,O}) =$

$\frac{\pi}{1-\pi}\frac{q_Y^*-q_O^*}{1-\beta}$.

$$
\begin{aligned}
V_{s,Y} &= b + \beta V + \beta\lambda_Y \int_{q=\underline{q}}^{\bar{q}} \big(\max\{V_m(q) - V, 0\}\big)\, \mathrm{d}F(q), \\
V_{s,Y} &= b + \beta V + \beta\lambda_Y \int_{q=q_Y^*}^{\bar{q}} \big(V_m(q) - V\big)\, \mathrm{d}F(q), \\
V_{s,Y} &= b + \beta V + \frac{\beta\lambda_Y}{1-\beta} \int_{q=q_Y^*}^{\bar{q}} \big(1 - F(q)\big)\, \mathrm{d}q, \\
(1-\beta)V &= b + \pi(V_{s,O} - V_{s,Y}) + \frac{\beta\lambda_Y}{1-\beta} \int_{q=q_Y^*}^{\bar{q}} \big(1 - F(q)\big)\, \mathrm{d}q, \\
q_Y^* &= b - \frac{\pi}{1-\pi}\frac{q_Y^* - q_O^*}{1-\beta} + \frac{\beta\lambda_Y}{1-\beta} \int_{q=q_Y^*}^{\bar{q}} \big(1 - F(q)\big)\, \mathrm{d}q,
\end{aligned}
$$

This equation takes into account the probability of transitioning into old age. It is easy to see that the cutoff quality will not be the same if $\lambda_Y > \lambda_O$.

Proposition 5. *If the arrival rate λ falls with age then $\partial\mathbb{E}[q|q > q^*]/\partial b > 0$ and $\partial D/\partial b > 0$.*

Proof. First, for the old woman, the analysis of the basic model applies and the result follows immediately. Second, for the young woman, we can apply the same kind of analysis. Higher benefits increase the value of waiting both directly and indirectly. The direct effect comes from enjoying the benefits while single and young and the indirect effect comes from the benefits while old (which shows up through the cutoff quality of old). Thus, all cutoff qualities increase which implies higher expected qualities conditional on a match and a higher duration of single-hood. $\square$

Stigma

Getting the benefits could also bring about negative effects if there is stigma associated with participating in the program. In the model, we can think of this issue in two ways. First, being in the program lowers the probability of receiving an offer. Second, the distribution of offers gets worse.

In either case, the presence of the stigma makes the predictions of the model ambiguous.

Proposition 6. *If b lowers the rate of arrival of prospects λ or worsens the distribution $F(q)$ in the sense of first-order stochastic dominance (in addition to increasing the per period utility) then the sign of $\partial\mathbb{E}[q|q > q^*]/\partial b$ and $\partial D/\partial b$ becomes ambiguous.*

Proof. Lemma 1 established that the cutoff quality moved in the same direction as the benefits, the change in the probability of proposals, λ, and the distribution, $F(q)$. With a stigma effect, the program increases b but lowers λ or F. The original effect increases the cutoff but the stigma effect lowers it. It is unclear which one we should expect to dominate. $\square$

Work

The initial predictions are maintained when we introduce a labor decision in the model. In this extension, a woman has a probability λ_E of receiving an employment opportunity. A job offer is characterized by its wage w which is distributed $G(w)$ with support $[\underline{w}.\bar{w}]$ and $\bar{w} > b$. We assume that marriage lasts forever and that an employed woman loses her job with probability δ each period. We also assume that an employed woman can receive marriage offers at rate $\lambda_{m,e}$ and with quality distributed $\hat{F}(q)$.

In this extension, there exist three possible states: single and unemployed, single and employed, and married. The value of being single and unemployed is

$$V_{s,u} = b + \beta\lambda_m \int_q \max\{V_{m,u}(q), V_{s,u}\} dF(q) + \beta\lambda_e \int_w \max\{V_{s,e}(w), V_{s,u}\} dG(w) + \beta(1 - \lambda_m - \lambda_e)V_{s,u}.$$

The value of being married to a partner with quality q is

$$V_{m,u}(q) = q + \beta V_{m,u}(q) = q/(1 - \beta).$$

The value of being employed at wage w is

$$V_{s,e}(w) = w + \beta\lambda_{m,e} \int_{q=\underline{q}}^{\bar{q}} \max\{V_{s,e}(w), V_{m,u}(q)\} \mathrm{d}\hat{F}(q) + \beta\delta V_{s,u} + \beta(1 - \lambda_{m,e} - \delta)V_{s,e}(w).$$

Letw^* be the cutoff wage and q^* be the cutoff quality for the single, unemployed woman. Then, by definition of cutoff wage and quality

$$(1 - \beta)V_{s,u} = (1 - \beta)V_{s,e}(w^*) = (1 - \beta)V_{m,u}(q^*) = q^*.$$

Evaluating the expression above at w^*, we get

$$q^* = w^* + \frac{\beta\lambda_{m,e}}{1 - \beta} \int_{q=q^*}^{\bar{q}} [q - q^*] \mathrm{d}\hat{F}(q) = w^* + \frac{\beta\lambda_{m,e}}{1 - \beta} \int_{q=q^*}^{\bar{q}} [1 - \hat{F}(q)] \mathrm{d}q. \tag{2}$$

For each wage w, there will be a cutoff marriage quality, $q(w)$, such that all proposals with quality $q > q(w)$ will be taken. The cutoff marriage quality is implicitly defined by

$$V_{s,e}(w) = V_{m,u}(q(w)) = \frac{q(w)}{1 - \beta}.$$

Then, we can write,

$$[1 - \beta(1 - \delta)]V_{s,e}(w) = w + \beta\lambda_{m,e} \int_{q=\underline{q}}^{\bar{q}} \max\{0, V_{m,u}(q) - V_{s,e}(w)\} \mathrm{d}\hat{F}(q) + \beta\delta V_{s,u}.$$

$$[1-\beta(1-\delta)]V_{s,e}(w) = w + \frac{\beta\lambda_{m,e}}{1-\beta}\int_{q=q(w)}^{\bar{q}}[1-\hat{F}(q)]\mathrm{d}q + \beta\delta V_{s,u}.$$

$$[1-\beta(1-\delta)][V_{s,e}(w)-V_{s,u}] = w + \frac{\beta\lambda_{m,e}}{1-\beta}\int_{q=q(w)}^{\bar{q}}[1-\hat{F}(q)]\mathrm{d}q - (1-\beta)V_{s,u}.$$

$$[1-\beta(1-\delta)][V_{s,e}(w)-V_{s,u}] = w + \frac{\beta\lambda_{m,e}}{1-\beta}\int_{q=q(w)}^{\bar{q}}[1-\hat{F}(q)]\mathrm{d}q - q^*.$$

$$[1-\beta(1-\delta)][V_{s,e}(w)-V_{s,u}] = w - q^* + \frac{\beta\lambda_{m,e}}{1-\beta}\int_{q=q(w)}^{\bar{q}}[1-\hat{F}(q)]\mathrm{d}q.$$

$$q(w) = q^* + \frac{1-\beta}{1-\beta(1-\delta)}(w - q\tilde{n}^*) + \frac{\beta\lambda_{m,e}}{1-\beta(1-\delta)}\int_{q=q(w)}^{\bar{q}}[1-\hat{F}(q)]\mathrm{d}q. \tag{3}$$

We can directly establish the existence and uniqueness of the solution of $q(w^*)$ (the cutoff marriage quality at the reservation wage) by evaluating this expression at $w = w^*$. The cutoff marriage quality accounts for the current wage, the search value, and the possibility of the job being lost.

Now, the value of being single and unemployed is given as before.

$$(1-\beta)V_{s,u} = b + \beta\lambda_M\int_{q=\underline{q}}^{\bar{q}}\big(\max\{V_m(q)-V_{s,u},0\}\big)\,\mathrm{d}F(q) + \beta\lambda_E\int_{w=\underline{w}}^{\bar{w}}\big(\max\{V_{s,e}(w)-V_{s,u},0\}\big)\,\mathrm{d}G(w),$$

$$(1-\beta)V_{s,u} = b + \beta\lambda_M\int_{q=q^*}^{\bar{q}}\big(V_m(q)-V_{s,u}\big)\,\mathrm{d}F(q) + \beta\lambda_E\int_{w=w^*}^{\bar{w}}\big(V_{s,e}(w)-V_{s,u}\big)\,\mathrm{d}G(w),$$

$$(1-\beta)V_{s,u} = b + \frac{\beta\lambda_M}{1-\beta}\int_{q=q^*}^{\bar{q}}\big(1-F(q)\big)\,\mathrm{d}q + \frac{\beta\lambda_E}{1-\beta(1-\delta)}\int_{w=w^*}^{\bar{w}}\big(1-G(w)\big)\,\mathrm{d}q(w),$$

$$(1-\beta)V_{s,u} = b + \frac{\beta\lambda_M}{1-\beta}\int_{q=q^*}^{\bar{q}}\big(1-F(q)\big)\,\mathrm{d}q + \frac{\beta\lambda_E}{1-\beta(1-\delta)}\int_{w=w^*}^{\bar{w}}\big(1-G(w)\big)\,\mathrm{d}q(w),$$

$$q^* = b + \frac{\beta\lambda_M}{1-\beta}\int_{q=q^*}^{\bar{q}}\big(1-F(q)\big)\,\mathrm{d}q + \frac{\beta\lambda_E}{1-\beta(1-\delta)}\int_{w=w^*}^{\bar{w}}\big(1-G(w)\big)\,\mathrm{d}q(w). \tag{4}$$

Then, we can solve for all cutoffs in the following way. We first solve for the cutoffs at the single, unemployed state. Those cutoffs are w^* and q^*. Equation (2) is increasing in w^* while equation (4) is decreasing in w^*. This means that if a solution exists, it is unique. We can also solve for the cutoff marriage quality at a job with wage w using equation (3). Clearly, $q(w^*) = q^*$ and $q(w)$ is a strictly increasing function.

We can now establish the comparative statics with respect to the benefits.

Proposition 7. *An increase in benefits b increases the number of periods the woman stays single and the average quality of the marriage. An increase in benefits b also increases the number of periods the woman stays unemployed and the average wages of the women that become employed.*

Proof. As before, all we need to do is establish that the increase in benefits increases the cutoff qualities and wages. For the single and unemployed cutoffs, notice that equation (4) is the only one affected by the change in benefits and that this equation is decreasing in w^*. Therefore, q^* and w^* must increase.

For the single and employed cutoffs, the higher benefits have an indirect effect through the single and unemployed cutoff which we already established was increasing. Intuitively, higher benefits make it better to wait before marrying even when employed because if the woman were to lose the job, she would enjoy those benefits. □

Fertility

An extra dimension that we can consider is fertility. A woman's incentives to have more children are affected by the program. We model this dimension as a binary decision that a woman makes in each period. If a woman decides to have children, she gets one next period with probability π_c. In the model, we limit the number of extra children a woman can have to one. We do this by considering a small state space. That is, a woman can be single with n children, single with $n+1$ children, or married with n and $n+1$ children. A decision to have children while married does not affect the analysis and is thus omitted.

Let us compare the decision of having children when enrolled in the program and when not. The value of being single with n children is

$$V_{s,n}^i = b_n^i + a_n + \beta \left(\lambda_n^i \int_{q=\underline{q}}^{\bar{q}} \max \left\{ V_m(q), \hat{V}_{s,n}^i \right\} \, \mathrm{d}\tilde{F}_n(q) + (1-\lambda_n^i)\hat{V}_{s,n}^i \right),$$

where the i superscript is either 0 or 1, indicating if the woman is participating in the program. $\hat{V}_{s,n}^i$ is the optimal continuation (next period) value of a single woman who has n children in this period. $\hat{V}_{s,n}^i = \max\{V_{s,n}^i, \pi_c V_{s,n+1}^i + (1-\pi_c)V_{s,n}^i\}$.

Also, a_n is the utility flow of having n children. Finally, b_n^i is the transfers that a woman who has n children receives. Some conditions change when a woman enrolls in the program. For instance, if a woman is enrolled in the program, she will receive a transfer $b_n^1 > b_n^0 = 0$. If $b_{n+1}^1 > b_n^1$, the program provides extra incentives to have children (because$b_{n+1}^0 = b_n^0$). At the same time, if $\lambda_{n+1}^i < \lambda_n^i$ and$\lambda_{n+1}^1 - \lambda_n^1 < \lambda_{n+1}^0 - \lambda_n^0$ (the effect of an extra child on the arrival of prospects is more negative when participating in the program), there are fewer incentives to have children. When combined with the effect of the higher transfers, the overall effect of the program on fertility is ambiguous.

Proposition 8. *If b is an increasing function of the number of children then fertility will increase when b increases. But if more children while single lower the rate of arrival of prospects in the labor and marriage market, then the predictions about fertility become ambiguous.*

Mobility

Now, we introduce the possibility of moving to a new location. Locations are indexed by j and have different characteristics (λ_j). We consider the case where the transfer is lost upon moving to a new location. Opportunities to move to a new location arrive randomly with probability μ. We assume that a married woman does not receive moving opportunities.

$$V_s = b + \beta \left(\lambda \int_{q=\underline{q}}^{\bar{q}} \max \{V_m(q), V_s\} \, \mathrm{d}F(q) + \mu \int_j \max \{V_{s,j}, V_s\} \, \mathrm{d}H(j) + (1 - \lambda - \mu)V_s \right).$$

The value of being married to a partner with quality q is

$$V_m(q) = q + \beta V_m(q) = \frac{q}{1-\beta}.$$

We take the value of being single in the new location, $V_{s,j}$, as exogenous. While we could make it endogenous, the only relevant assumption is that for each specific new location, the value of being single there is not affected by b.

The decision to migrate is governed by $\max\{V_{s,j}, V_s\}$. Define the set of locations the agent would move to as $J^* := \{j|V_{s,j} \geq V_s\}$. The probability of moving to a new location is given by $\mu H(J^*)$. The expected quality of new locations a woman moves to is given by $\mathbb{E}[V_{s,j}|j \in J^*]$.

Proposition 9. *If b increases, then mobility falls, and those who do migrate, move to better locations.*

Proof. By applying standard dynamic programming arguments, we can show that V_s is a strictly increasing function of b,. [First, the Bellman operator satisfies Blackwell's sufficient conditions for a contraction so there is a fixed point and it is unique. Second, the operator preserves the property of being an increasing function of b, and the operator maps weakly increasing functions of b to strictly increasing functions of b.] Since V_s is a strictly increasing function of b and each $V_{s,j}$ is constant on b, the set J^* is decreasing in b (i.e., when b increases, the set gets smaller as some locations are now excluded). Thus, the probability of moving is lower. Finally, the expected quality of a new location a woman moves to is higher when b is higher. That is because the expected quality when b is lower is a weighted average of the locations that remain when b is higher and the locations that were excluded. By construction, the latter has a lower value than any of the former which proves the result. □

Causal Forest

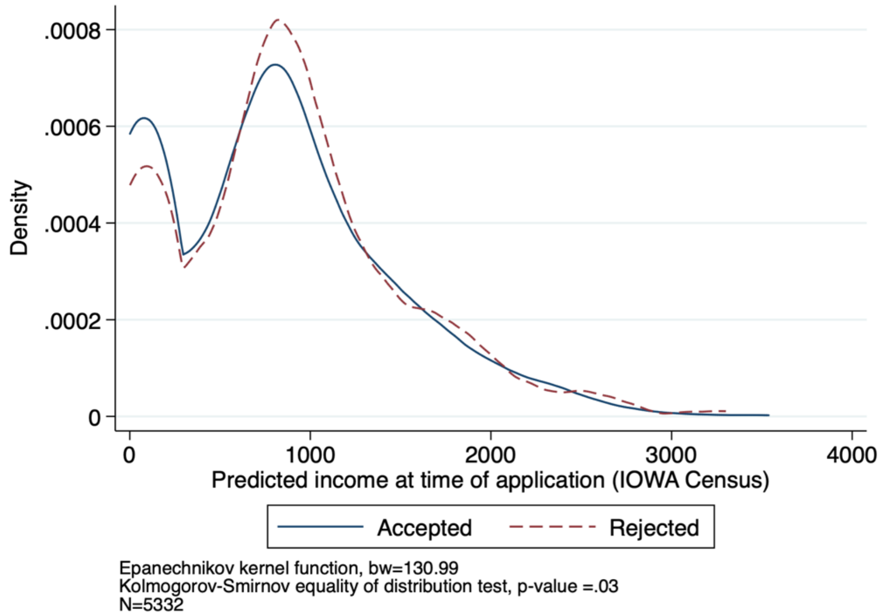
We implement the generalized random forest algorithm proposed by Athey, Tibshirani, and Wager (2019). The algorithm, first, trains a causal forest using a full set of covariates and second, estimates conditional average treatment effects (CATE).

An individual tree in a causal forest is trained by drawing a random subsample from the dataset and the sample is split into several nodes to form a tree. Each node in a tree is split using a random subset of covariates and some value of the covariate. The GRF algorithm measures the goodness of a split using heterogeneity across nodes and maximizes the difference in treatment effects across nodes. Then, a prediction is made using a weighted average of each tree's prediction where the weight is the similarity across trees.

We make the following decisions to train a causal forest. First, we use 50% of the full dataset to grow each tree. Second, we train 100,000 trees in a causal forest. Davis and Heller (2017) use 100,000 trees and Beaman et al. (2021) use 250,000 trees but find no meaningful increase in stability when increasing the number of trees from 100,000. In training each tree, we consider $\sqrt{x}+20$ number of variables for each tree where x is the number of variables and set 20 as the minimum number of observations in each leaf.

We estimate the average treatment effects using a doubly robust augmented-inverse-propensity weighting estimation method (Robins, Rotnitzky, and Zhao 1994). We report the average treatment effects on the full and treated samples. We also estimate the overlap-weighted average treatment effect recommended by Li, Morgan, and Zaslavsky (2018) that addresses an issue of estimated propensities being close to 0 or 1 and find similar results to ATE.

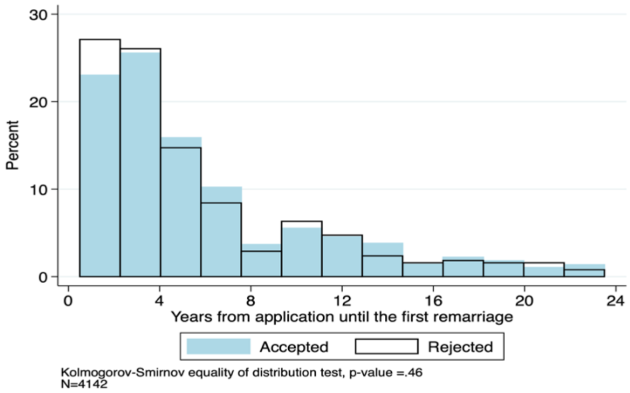
Figure 1: Welfare Recipients Have Lower Predicted Incomes Pre-Application



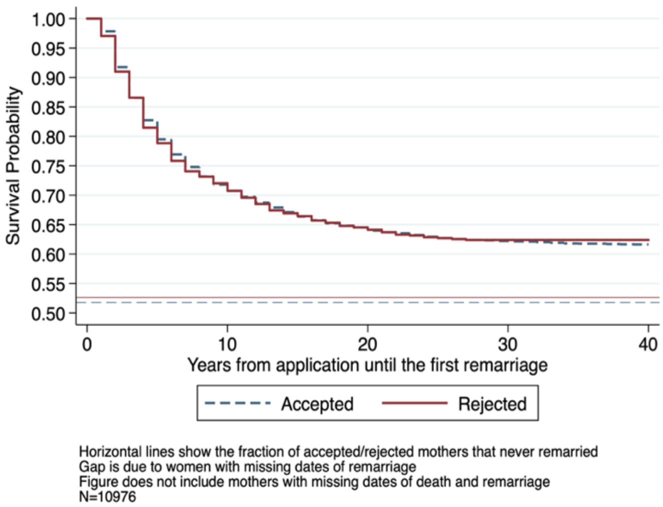
Notes:
Data come from administrative data collected by the authors. Sample includes women with non missing predicted income. Income<1 set to =1. Sample includes 5332 individuals for whom we could compute predicted income using the Iowa Census. The predicted income was computed by running a regression of family income on covariates (widow, mother age at application, number of kids at each age (0-18), age of the youngest and oldest kid, number of kids over 14, mother is foreign, black, education and occupation score. We include interactions of the covariates with the variable widow, and some of the covariates are included in a dummy format.) in the Iowa Census and then using the estimated betas to predict income for all mothers in the MP sample. In the MP sample we use the 1910 census occupation scores and 1940 census education.

Figure 2: Welfare Recipients Delay Time to Remarriage

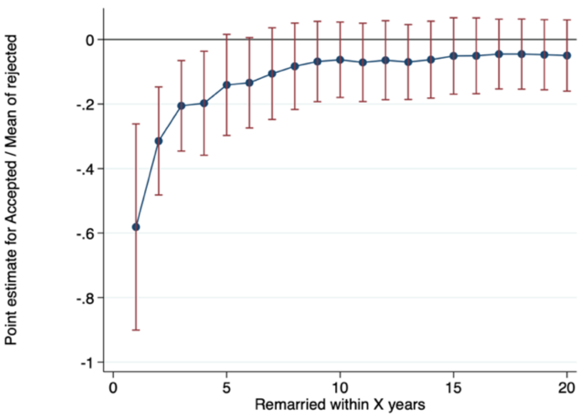
2a. Histograms of duration until the first remarriage (in years) by welfare receipt



2b. Survival curves over 40 years: probability of remaining single by welfare receipt



2c. Effect of obtaining cash transfer on probability of remarriage by year, as a function of baseline probability of remarriage



Notes: Panel a. The figure plots the duration until the first remarriage by accepted among women who were not married at the time of the application. We cannot reject that the distributions are equal. Sample includes only women that remarried. **Panel b**: The figure plots the survival curves by accepted for the duration until the first remarriage. **Panel c**: The figure plots the estimated coefficients of "accepted" divided by the baseline probability of remarriage among rejected applications and 95% confident intervals. Coefficients come from regressions where we regress a dummy indicating that the mother remarried within x years on accepted status and all predetermined characteristics. Standard errors are clustered at the county level. See information in Table 1.

Figure 3: Delaying Remarriage Improves the Quality of the New Husband

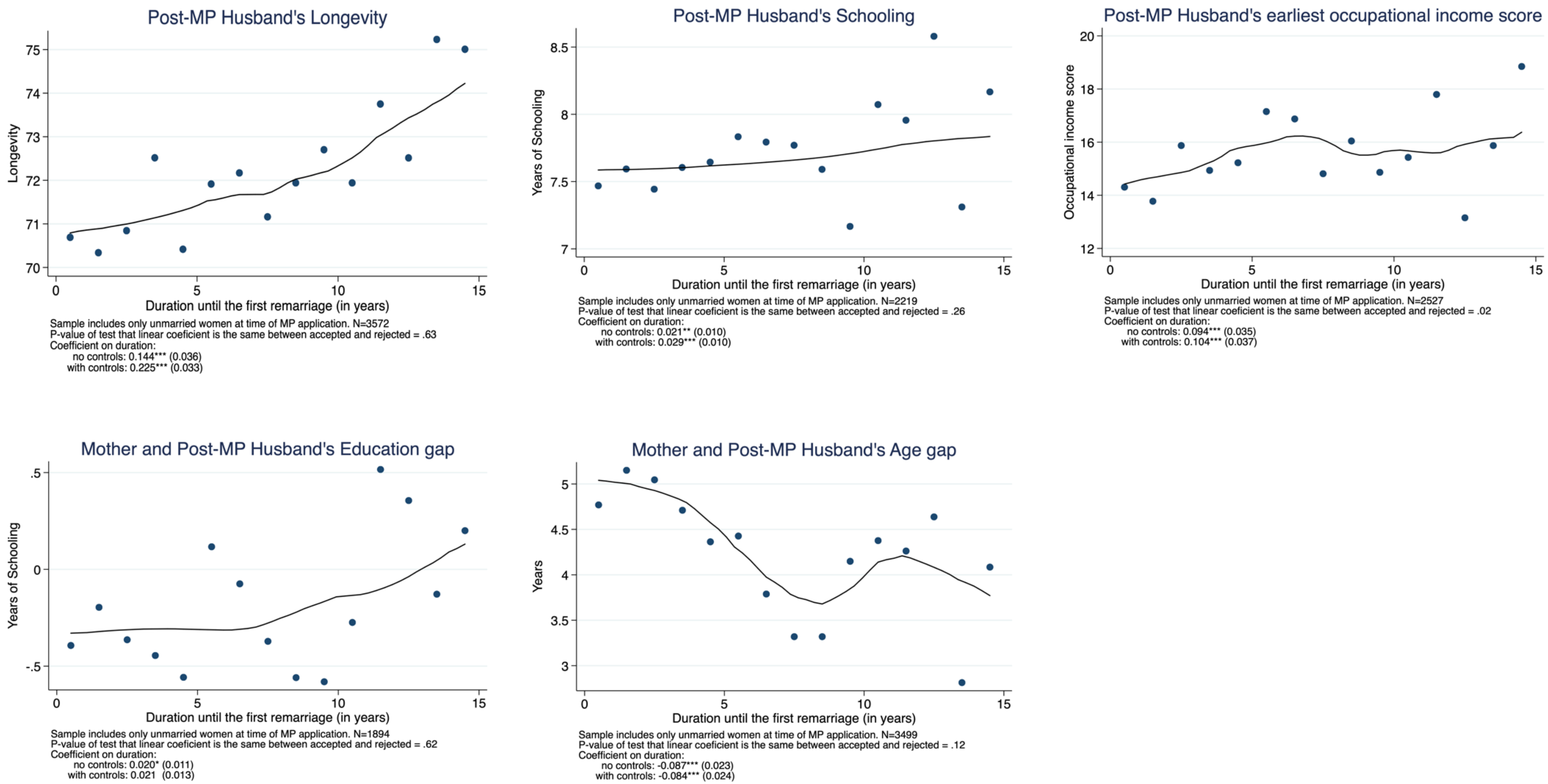
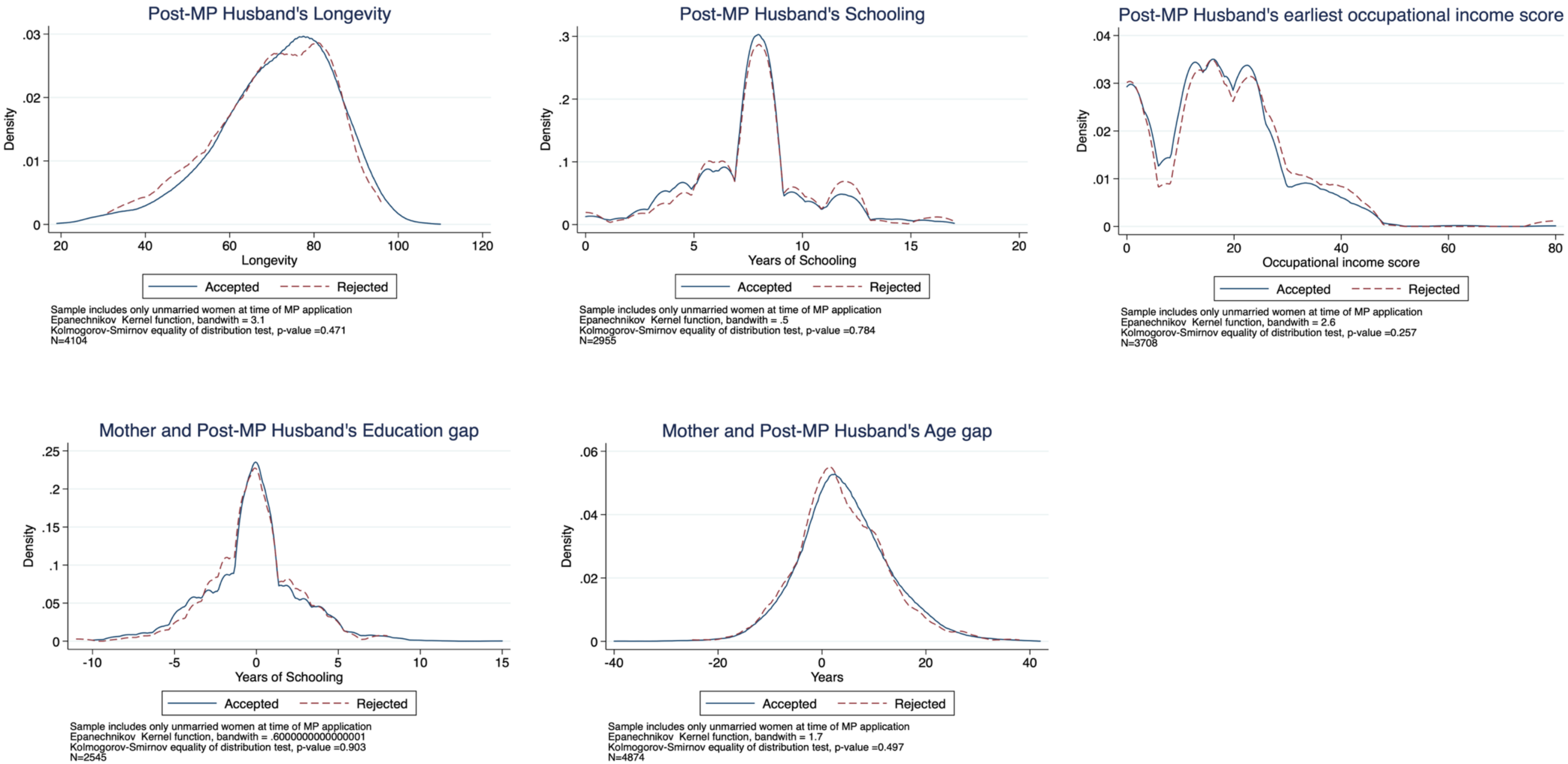
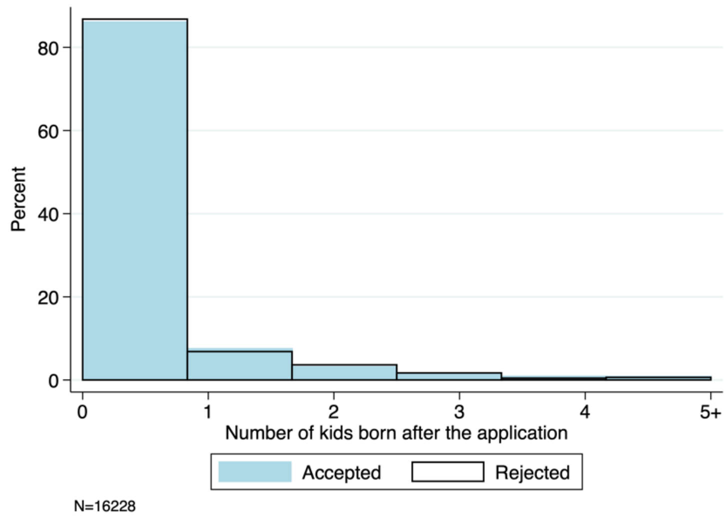


Figure 4: Welfare Recipients Do Not Marry Better Men



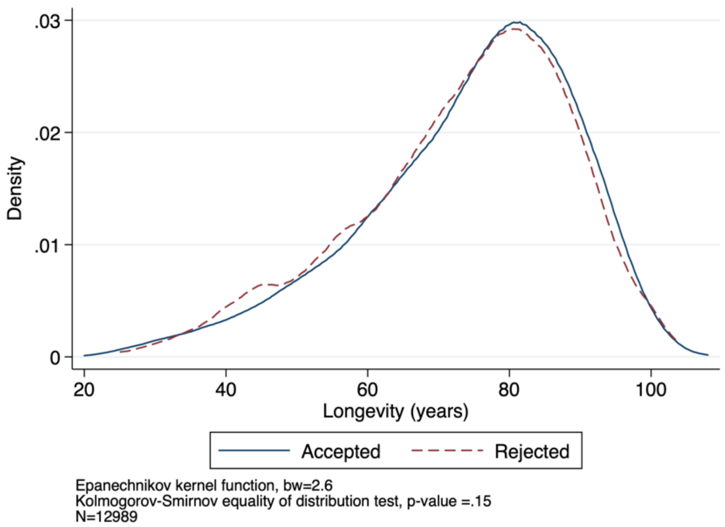
Notes: All figures are estimated densities. The number of observations varies because we do not always observe a given outcome. We use the maximum number of observations available for each figure.

Figure 5: Welfare recipients do not have more children after receiving welfare

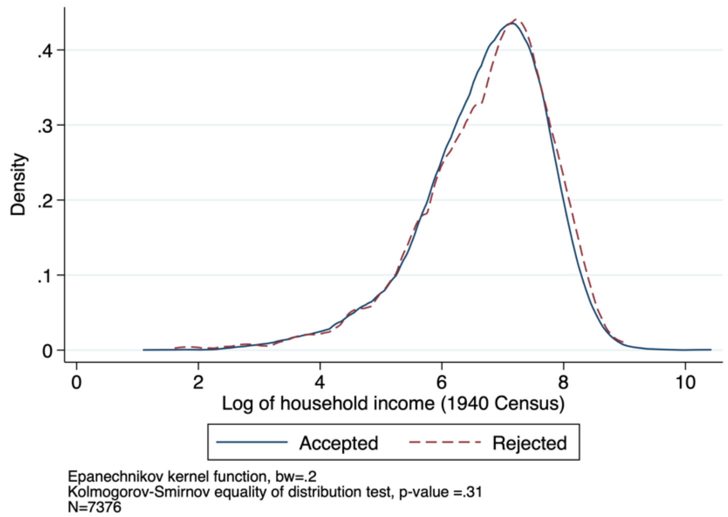


Note: The figure plots the distribution of kids born after application by accepted. The sample includes all women.

Figure 6: Welfare Recipients' Long-Term Well-Being Is Not Affected By Receiving Welfare



a. Distribution of longevity of the mother by accepted



b. Distribution of 1940 household income of the mother by accepted
Sample: Women with non-missing and non-zero household income

Note: Panel a The figure plots the distribution of the longevity of the mother by accepted. We cannot reject that both distributions are equal. The sample includes all women with non-missing longevity. **Panel b** The figure plots the distribution of 1940 household income by accepted. We cannot reject that both distributions are equal. The sample includes all women with non-missing and non-zero household income.

Table 1: Welfare recipients with cash transfers delay remarriage

Dep. Var. Y:	Ever remarried = 1	Duration[1]	Log duration[1]	Remarried within 1 year	Remarried within 2 years	Remarried within 3 years	Remarried within 5 years	Remarried within 10 years
Notes:	OLS	OLS	OLS	OLS specification. Women that never married are coded as				
Mean of Y for rejected	0.47	5.47	1.23	0.04	0.11	0.16	0.22	0.30
Panel A: Main results (full controls)								
Accepted	-0.014	1.275***	0.238***	-0.024***	-0.035***	-0.033***	-0.032*	-0.019
	(0.020]	(0.444)	(0.061)	(0.007)	(0.009)	(0.011)	(0.018)	
R-squared	0.228	0.338	0.115	0.039	0.091	0.121	0.170	0.228
Observations	11286	3572	3572	9423	9423	9423	9423	9423
Panel B: Checks								
1- Correction for OVB (Oster 2017)	[-0.02;-0.01]	[1.17;1.39]	[0.22;0.26]	[-0.03;-0.02]	[-0.04;-0.03]	-0.04;-0.03]	[-0.04;-0.02]	[-0.03;-0.01]
2- Semi-parametric sample selection correction (Newey, 2009)								
Accepted	-0.014	1.305	0.243	-0.024	-0.035	-0.033	-0.032	-0.019
95% Confidence interval	[-0.05;0.02]	[0.42;2.19]	[0.12;0.36]	[-0.04;-0.01]	[-0.05;-0.02]	[-0.05;-0.01]	[-0.07;0.00]	[-0.05;0.02]
F-Stat	72.37	32.81	32.81	28.01	28.01	28.01	28.01	28.01
3- Drop if quality of match low								
Accepted	-0.027	0.979**	0.213***	-0.045***	-0.061***	-0.045*	-0.029	-0.005
Clustered at county	(0.028)	(0.424)	(0.067)	(0.013)	(0.021)	(0.023)	(0.030)	(0.025)
Observations	5463	3334	3334	4495	4495	4495	4495	4495
4 - IPW	0.009	0.971**	0.174***	-0.017***	-0.021*	-0.013	-0.004	0.011
	(0.025)	(0.387)	(0.054)	(0.006)	(0.012)	(0.016)	(0.026)	(0.029)
5 - Causal Forest ATE	-0.020	1.330***	0.224***	-0.021***	-0.037***	-0.039***	-0.044***	-0.030**
	(0.014)	(0.305)	(0.055)	(0.007)	(0.011)	(0.014)	(0.015)	(0.015)
6 - Causal Forest ATT	-0.026	1.375***	0.226***	-0.021**	-0.039**	-0.045**	-0.054***	-0.039*
	(0.020)	(0.363)	(0.066)	(0.009)	(0.016)	(0.020)	(0.021)	(0.021)
Observations	11286	3572	3572	9423	9423	9423	9423	9423

Note: Sample includes only women who were not married at the time of application. Standard errors clustered at the county level. Controls for county and year-of-application fixed effects and individual, county and state controls. Individual controls: Kids: MP age of the youngest and oldest, MP dummies for number, FS number older than 14, FS number that died before MP, FS number with dates missing. Mother: last name length, dummies for divorced, widowed and missing marital status, age at application, missing age, number of siblings, foreign, missing nativity, first husband's longevity, first husband's longevity is missing. County controls: for ages 18-55: sex ratio (M/F), shares of white married mothers in the labor force, black and rural. County controls match linear interpolated information from the 1910, 1920 and 1930 census with the year of MP application. State controls: manufacturing wages, education/labor laws (age must enter school, work permit age, and continuation school law in place), state expenditures in logs (education, charity, and social programs), state laws concerning MP transfers (work required, reapplication required, maximum amount for the first child and for each additional child). [1]The duration measure starts at 0.5 (the variable is duration + 0.5, so we assume that marriages occur uniformly within a year). We also assume that if women married the same year they applied for the pension (and the exact data of marriage is missing) that the marriage took place after the MP application. [2] Low quality of match is defined as observations with remarriage dates that do not include day, month and year of marriage. Omitted variable bounds: We use Oster (2017) to construct ommited variable bias (OVB) bounds. We assume that the R-max is 1.3 times greater than the R-squared from panel B. We assume delta = (-1, 1) for lower and upper bounds. Sample Selection Correction: We follow the two-step estimation suggested by Newey (2009) to correct for sample selection. First, we regress the dummy indicating whether the outcome is mising on RA fixed effects (73 dummies) and all other controls. We report the F-statistic of the test of relevance of these dummies. Second, we estimate a linear regression of the outcome on controls and on a fourth degree polynomial of predicted values from the first stage. We jointly bootstrap the two stages and report the 95% bias corrected confidence interval clustered at the county level, from 200 repetitions. Quality of match: Regressions that drop low quality matches (quality measure below its median) include all controls and cluster the standard errors at the county level. The quality of match between census, family search and administrative data is constructed as the weighted sum of variables that access the similarity between first name, last name, full name, age and place of birth in each dataset. IPW: We estimate the average treatment effect using the estimated probability weights to address for potential missing outcomes. The stnadard errors are clustered at the county level and a logit model is used to predict the accepted status. Causal Forest: We implement the generalized random forest algorithm proposed by Athey, Tibshirani, and Wager (2019). We estimate the average treatment effects using a doubly robust augmented-inverse-propensity weighting estimation method and report the ATE and ATT. See Appendix for more details.

Table 2: Welfare receipt does not increase quality of Post-MP husband

	New husband's traits			New match characteristics			Overall Index		
Outcome:	Post-MP Husband Longevity	Post-MP Husband Education	Occ Score[2]	Age gap (shifted by 2.5 years)[1]	Education gap[3]	P-value (H0: all = 0)	Equal weights [4]	Equal weights (no age, education gap)	Satisfaction weights [5]
	(1)	(4)	(3)	(2)	(5)	(6)	(7)	(8)	(9)
Mean of outcome for rejected	70.130	7.798	21.220	6.661	1.821		-0.0470	-0.0465	0.361
Panel A: Main results (full controls)									
Accepted	1.821**	-0.226	-0.828	0.275	-0.064	0.095	0.095**	0.087*	-0.006
	(0.903)	(0.228)	(0.574)	(0.289)	(0.185)		(0.046)	(0.044)	(0.021)
Observations	4,104	2,955	3,556	4,874	2,545		4,894	4,606	2,540
Panel B: Control for other traits (proposition 2)									
Mean of outcome for rejected	73.99	7.946	20.18	6.345	1.818				
Accepted	1.368	-0.334	-0.425	0.247	0.031	0.719			
	(1.309)	(0.279)	(0.749)	(0.599)	(0.239)				
Observations	1,887	1,887	1,887	1,887	1,887				
Panel C: Control for mom's age at marriage									
Mean of outcome for rejected	71.08	7.905	20.28	6.826	1.924		0.0214	0.0184	0.360
Accepted	0.906	-0.362	-1.293**	0.133	-0.044	0.115	0.103*	0.103*	-0.013
	(0.960)	(0.221)	(0.624)	(0.346)	(0.198)		(0.058)	(0.056)	(0.022)
Observations	3,116	2,218	2,424	3,499	1,893		3,505	3,333	1,889
Panel D: Main Results using Causal Forest									
Causal Forest ATE	1.430*	-0.313*	-0.714	0.092	0.019		0.063	0.072	0.001
	(0.732)	(0.168)	(0.609)	(0.272)	(0.123)		(0.043)	(0.044)	(0.015)
Causal Forest ATT	1.475	-0.348*	-0.659	0.110	0.010		0.059	0.070	-0.001
	(0.919)	(0.206)	(0.771)	(0.352)	(0.146)		(0.055)	(0.056)	(0.018)
Observations	4104	2955	3556	4874	2545		4894	4606	2540

Note: Standard errors clustered at county level. See Table 1 for description of controls, restrictions and checks. Panel B includes the other inputs (Post-MP Husband longevity, age gap, Post-MP Husband latest occ. score, Post-MP Husband 1940 education and education gap) as controls (except if the input is the regression dep. var.). In column 6, we present the P-value of the test with null hypothesis that the estimates from columns 1 to 5 are jointly equal to zero. [1]Age gap is defined as the absolute value of the husband's age minus the mother's age minus 2.5. [2] Defined from pre-marriage data: uses 1940 if available, then 1930, then 1920, then 1910. Never uses a measure that is observed post-MP marriage. [3] Education gap is defined as the absolute value of difference in highest grade between the mother and the husband. [4] Equal Weights regressions give the same weight to each of the quality measures. Values are standardized to zero mean and variance equals one. [5] Satisfaction weights include husband's occ. score, education and longevity. We use the utility function and the parameters defined and calibrated in Grow and Van Bavel (2015) to construct the dependent variable. The equation below presents the utility function. The first term of the equation is the similarity of education, second term is the earnings prospect and, last term is the age gap. We follow the same categorization of variables as in the original paper, except for education, where we divide it in 4 quartile categories instead of the four categories in the paper (no schooling, primary, secondary and tertiary). αi = ai+25 To take into account that female agents prefer partners who are about 2.5 years older. The parameters are: Smax=4; Ymax=5; Amax=800; ws=0.385; wy=1.201; wa=10.833.

$$v_{ij} = \left(\frac{S_{\max} - |s_i - s_j|}{S_{\max}}\right)^{w_s}\left(\frac{y_j}{Y_{\max}}\right)^{w_y}\left(\frac{A_{\max} - |\alpha_i - a_j|}{A_{\max}}\right)^{w_a}$$

Table 3: Welfare receipt lowered geographic mobility

Sample:	All mothers		All mothers who moved			
Outcome:	lives in MP county in 1930	lives in MP county in 1940	lives in more educated county in 1930	lives in higher sex ratio county in 1930	lives in more educated county in 1940	lives in higher sex ratio county in 1940
Mean of Y for rejected	0.65	0.59	0.50	0.50	0.51	0.54
Panel A: Main results (Full controls)						
Accepted	0.048**	0.063***	0.028	0.038	0.021	-0.004
	(0.024)	(0.018)	(0.024)	(0.031)	(0.024)	(0.032)
R-squared	0.176	0.114	0.399	0.333	0.405	0.289
Observations	11178	9358	3123	2009	3177	3136
Panel B: Checks						
1- Correction for OVB (Oster 2017)	[0.04;0.06]	[0.06;0.07]	[0.03;0.03]	[0.03;0.05]	[0.02;0.02]	[-0.02;0.01]
2- Semi-parametric sample selection correction (Newey, 2009)						
Accepted	0.049	0.063	0.028	0.037	0.022	-0.003
95% Confidence interval	[0.00;0.10]	[0.03;0.10]	[-0.02;0.08]	[-0.02;0.10]	[-0.03;0.07]	[-0.07;0.06]
F-Stat	25.57	116.82	22.13	27.58	29.86	27.52
3- Drop if quality of match low						
Accepted	0.069***	0.053*	-0.023	0.080	-0.016	0.049
Clustered at county	(0.026)	(0.028)	(0.042)	(0.062)	(0.042)	(0.041)
Observations	5589	4679	1249	775	1362	1352
4 - IPW	0.080***	0.078***	0.032	0.019	0.024	-0.047
	(0.027)	(0.028)	(0.048)	(0.053)	(0.044)	(0.049)
5 - Causal Forest ATE	0.085***	0.101***	0.005	-0.009	0.008	-0.026
	(0.017)	(0.019)	(0.027)	(0.038)	(0.028)	(0.028)
6 - Causal Forest ATT	0.097***	0.111***	0.003	-0.009	0.010	-0.026
	(0.024)	(0.026)	(0.031)	(0.041)	(0.033)	(0.033)
Observations	11178	9358	3123	2009	3177	3136

Note: Sample: all mothers in application. Refer to Table 1 for a full description of the controls, restrictions and checks. Counties are ranked by the average schooling in the population between 18 and 55 years old in the 1940 census. Counties are ranked by the sex ratio at the year of application (interpolated between 1910, 1920 and 1930 censuses). We then estimate whether women moved to places above of below the median.

Table 4: Welfare recipients do not have more children

Outcome: Number of kids born after application for welfare	All ages		Women below median age (37)	
Sample:	All mothers	Mothers that were not married at time of application	All mothers	Mothers that were not married at time of application
Mean of Y for rejected	0.25	0.22	0.42	0.40
Panel A: Main results (Full controls)				
Accepted	-0.023	-0.009	-0.032	-0.005
	(0.018)	(0.021)	(0.036)	(0.045)
R-squared	0.160	0.162	0.157	0.160
Observations	16228	13383	9014	7168
Panel B: Checks				
1- Correction for OVB (Oster 2017)	[-0.04;-0.01]	[-0.03;0.01]	[-0.06;-0.01]	[-0.03;0.02]
2- Semi-parametric sample selection correction (Newey, 2009)				
Accepted	-0.023	-0.009	-0.030	-0.003
95% Confidence interval	[-0.06;0.01]	[-0.05;0.03]	[-0.10;0.04]	[-0.09;0.09]
F-Stat	.	75.57	42.64	16.13
3- Drop if quality of match low				
Accepted	-0.014	0.008	-0.048*	-0.003
Clustered at county	(0.028)	(0.031)	(0.026)	(0.034)
Observations	7577	6266	5738	4782
4 - IPW	0.022	0.038	0.032	0.057
	(0.025)	(0.025)	(0.040)	(0.043)
5- Causal Forest ATE	-0.010	0.009	-0.025	0.002
	(0.020)	(0.019)	(0.035)	(0.038)
6- Causal Forest ATT	-0.008	0.014	-0.028	0.002
	(0.029)	(0.028)	(0.050)	(0.052)
Observations	16228	13383	9014	7168

Note: Standard errors clustered at the county level. Refer to Table 1 for a full description of the controls, restrictions and checks.

Table 5: Welfare receipt did not benefit or hurt mothers in the long run

Data source	Family search	1940 census
Outcome	Mom longevity	Household income in 1940
Mean of Y for rejected	73.43	979.57
Panel A: Main results (Full controls)		
Accepted	0.247	-58.241*
	(0.567)	(31.877)
R-squared	0.028	0.080
Observations	12989	9358
Panel B: Checks		
1- Correction for OVB (Oster 2017)	[-0.02;0.49]	[-76.55;-41.74]
2- Semi-parametric sample selection correction (Newey, 2009)		
Accepted	0.254	-59.762
95% Confidence interval	[-0.86;1.37]	[-122.81;3.29]
F-Stat	46.25	116.82
3- Drop if quality of match low		
Accepted	0.215	-107.325
Clustered at county	(0.742)	(72.547)
Observations	8007	4679
4 - IPW	0.950**	-9.027
	(0.402)	(57.412)
5 - Causal Forest ATE	0.527	-17.824
	(0.447)	(36.364)
6 - Causal Forest ATT	0.488	-17.240
	(0.637)	(49.581)
Observations	12989	9358

Note: Sample includes all mothers regardless of marital status. Please refer to Table 1 for a full description of the controls, restrictions and checks. The quality measure uses the standardized Jaro-Winkler distance for longevity in column 1, and the standarized Jaro-Winkler distance for the 1940 census match in Column 2.

Table 6: Marginal Value of Public Funds for the Mothers' Pension Program

All values expressed in 2019 dollars

	Mothers	Including Spillovers on Boys and Assuming no Spillovers to Girls	
		Income and longevity benefits on kids	Transfer not counted as a benefit
Panel A: computations based on the results of this paper and of Aizer et al. (2016)			
Dollar value of maternal behavioral response (marriage delay and mobility decrease)	3,660.68	3,660.68	3,660.68
Dollar value of spillover for kids (mortality + income)	0	61,481	61,481
Dollar value of increased income taxes from kids (10% tax rate)	0	5,225	5,225
Dollar value of increased income taxes from mom (10% tax rate)	507.59	507.59	507.59
Total transfer	20,715	20,715	20,715
Total benefit or WTP (transfer + spillovers - cost of behavioral responses)	17,054	78,535	57,820
WTP excluding cost of behavioral responses	20,715	82,196	61,481
Total cost (transfer - taxes from increased earnings)	20,207	14,982	14,982
MVPF without behavioral responses from mother	**1.00**	**5.49**	**4.10**
MVPF including behavioral responses	**0.84**	**5.24**	**3.86**
Panel B: Minimum gains for children needed for an MVPF of 1			
Minimum change in kids' life expectancy[1] in years (for a MVPF=1)		0.34	1.45
Minimum percentatge change in kids' income[2] (for a MVPF=1)		0.75%	5.67%

Note: This table computes the Marginal Value of Public Funds (MVPF) using the methodology of Hendren and Sprung-Keyser (2019). We correct for discounting using a 3% rate, and we do not consider the implications of life extensions on Medicare and SSA pensions. We ignore the effects of the pension on marriage rates, type of husband, and years of schooling of the children. These are treated as intermediate outcomes whose ultimate value is reflected in increases in income and longevity. The dollar value of maternal behavioral response includes the discounted effects on marriage delay and mobility decrease. The value of spillover for kids includes the discounted effects on mortality from age 10 to 85 and discounted income effects for the children's average working period, 45 years. We assume a 10% tax rate that is discounted for mothers and children average working periods (27 and 45 years, respectively). The total transfer takes into account that mothers are in the program, on average, for 3 years. [1]Assumes no change in kids income. [2]Assumes there is no change in kids longevity and takes into account the increase in income taxes from kids.