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Economic Security and Fertility: Evidence from the Mincome Experiment*

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Abstract

Using experimental data, this paper analyzes the relationship between households' economic security and their fertility decisions for low-income households. Between 1974 and 1977, a randomized controlled trial was conducted in Manitoba, Canada in which the treatment groups received differing levels of guaranteed annual income. All of the program participants were low-income households. We find positive effects of the program on the probability of child birth that range between 7 to 10 percentage points.

Keywords: fertility, economic security, policy analysis, guaranteed annual income, negative income tax

JEL Codes: I38, J13, J18

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1 Introduction

Over the last decades, fertility rates declined below population replacement levels in most industrialized countries. The major decline in fertility rates occurred between 1960 and 1980, where fertility rates dropped from 2.88 to 1.87 children per woman in OECD countries. In Canada, the country we analyze here, the fertility rate went from 3.90 to 1.73 children per woman in the same period (Castles, 2003). There are many factors contributing to this trend, not least the increase in female labor market participation; female empowerment; and changes in individual attitudes and preferences (Engelhardt et al., 2004; Aarssen, 2005; Mitchell and Gray, 2007). As this development poses many challenges for industrialized societies, the drop in births per woman has encouraged research on the economics of fertility to analyze potential determinants of fertility rates, such as household income, economic security, and public policy changes.

This paper studies the effects of increased income, and more specifically economic security, on low-income households' decision to have a child. We assess the fertility effects of the 1970s-era Manitoba Basic Annual Income Experiment (hereafter referred to as Mincome). The data from this experiment are unique in that they were collected in the first randomized controlled trials (RCTs) conducted in industrialized countries for policy purposes, and that since then, no such RCT has taken place in an industrialized country. Mincome was conducted in three sites: Winnipeg, Dauphin, and a rural-dispersed site in Manitoba. Of the three places, only Winnipeg featured experimental variation in the level of guaranteed income and the rate at which total income was taxed. Moreover, assignment to treatment versus the control group in Winnipeg was random. This study therefore looks at the fertility effects of Mincome's guaranteed income in Winnipeg only.

Forget (2011) has assessed the effect of Mincome on fertility, finding no effects - but that study is not truly comparable to this one. First, that study used data from the Dauphin site; we study effects in Winnipeg. Second, that paper defined fertility as having had a child before age 25, which leaves out the large section of the population that has their first child after age 25. Finally, our analysis looks at not only whether a monthly guaranteed income impacts the decision to have a child, but also how differences in the magnitude of a basic income may differently impact fertility of low-income households.[1]

In the long-standing demand theory of fertility, parents are considered consumers maximizing their utility as they decide their preferred number of children while simultaneously considering budgetary constraints (Hotz et al., 1997); higher income means more children. Ever since the belief of a positive relation between income and fertility was challenged by the economic transition (Kreyenfeld et al., 2012), there have been several explanations put forward for a negative association between income and fertility, such as the quality-quantity trade-off (Becker, 1960), the opportunity costs of not participating in the paid labor market (Mincer, 1963; Becker, 1965), and women's educational attainment (Becker, 1981).[2]

However, there is reason to believe that a positive relationship between income and fertility turns negative only after a certain level of income has been reached and the minimum level of income required for survival has been secured. Galor and Moav (2002) have explained this hump-shaped relationship between income and fertility through the so-called "subsistence consumption constraint." While originally developed to illustrate the historical link between fertility, the preference for children's quality, and economic growth, the framework has also been used to explain modern connections between fertility and income (Micevska and Zak, 2002; Vogl, 2016). The main idea is that if households can't secure the necessary

[1] Green (2022) re-analyzes the data used in Forget (2011), noting that pre-trends in the data on health care call the validity of Forget's (2011) use of difference-in-differences into question; Forget (2022) responds that Green's choice of years to include in the analysis drives the identification of pre-trends. The outcome variable, experiment site, data used, and method in Forget's original 2011 analysis all differ from the present paper, but this discussion reflects the continued interest in understanding the effects of the Mincome experiment.

[2] While the gender gap in time devoted to childcare has been decreasing more recently (Gimenez-Nadal and Alberto, 2022), it is worth nothing that the Mincome data were collected in the 1970s – a time where the gender gap in childcare was even greater than it is now. It was thus female labor supply and women's time use in particular that were more central in considering the the cost of a child and subsequent fertility decisions (Willis, 1973; Jones et al., 2008). The difference in the role of female versus male wages on fertility is reflected by Merrigan and Pierre's (1998) study of Canadian fertility dynamics between 1950 to 1990; while an increase in the wages of Canadian women fostered declining fertility, male wages had a positive, though small, effect on family composition dynamics.

income for their own subsistence consumption, they will not have children. Once this level of income is secured, but the subsistence consumption constraint still binds, fertility (the number of children had by households) will be lower than optimal. Households with income high enough for the subsistence consumption constraint to be non-binding can allocate the additional resources to the quantity and quality of their children.[3] higher for high-income families, the relationship between the number of children and income can turn negative as parents spend more time and money on the education of their children, rather than having more children (Galor and Moav, 2002; Vogl, 2016). Along with absolute levels of income, the risk of not being able to meet subsistence consumption will negatively impact the possibility of having a child, since having children limits the household's ability to secure against potential wage shocks and adjustments in expected income (Micevska and Zak, 2002; Adsera and Menendez, 2011). Therefore, economic security - or the lack thereof - is an important factor to consider in understanding the fertility decisions of households.

The Mincome experiment targeted low-income families, ensuring treated households with a certain level of income and providing increased economic security through compensating potential decreases in family earnings up to the threshold of the respective plan. For families with income either below the subsistence level, or who face the risk of falling below it, we predict that a guaranteed income scheme will increase completed fertility. Our prediction is in accordance with the theory of spacing out births (Wolpin, 1984), which predicts that women delay motherhood until they can expect a steady and sufficient flow of economic resources (Hill and Tsehaye, 2018; Sear et al., 2016). Additionally, from an evolutionary psychological perspective, humans respond to the perception of environmental and economic adversity by delaying and lowering reproduction (MacDonald, 1999). There is a theoretical and empirical literature showing that households faced with economic uncertainty postpone

[3]Note that our application of the subsistence consumption constraint framework to Canada in the 1970s departs markedly from the model's original application to pre-modern societies. Low-income households may still meet their "subsistence consumption" needs, especially as compared to pre-modern civilizations. Even though our use of the framework deviates from the original meaning, we still find the core insights - that fertility choices are related to some basic necessary income level - illustrative here. Together with heterogeneous preferences for child "quality," which the literature models as

the decision to have a child, resulting in a decrease of completed fertility (Sommer, 2016; Adsera, 2004; Karaman Örsal and Goldstein, 2010; Kreyenfeld et al., 2012; Busetta et al., 2019).

Aside from Forget's 2011 paper studying Mincome's effects in Dauphin, there is very limited literature assessing the fertility effects of a basic income grant. The one exception is Keeley (1980), who studies data collected from the Seattle and Denver income maintenance experiments (SIME/DIME) in the 1970s. That analysis finds that while there seemed to be negative effects of five years of treatment receipt on fertility for white women, fertility increased for "Chicanas" and stayed the same for Black women.

Wider related literature looks at the relationship between fertility, income, and income security by studying child subsidy programs (Cohen et al., 2013; Riphahn and Wiynck, 2017; Blundell et al., 2018; González, 2013; Raute, 2019). Across the board, these studies find that more economic support is associated with higher birth rates. Milligan (2005) shows that a transfer policy in Quebec, Canada that gave Can$8,000 to all families expecting a child led to a strong positive effect on fertility. The effect of the birth subsidy was large, with an increase of up to a 25% in fertility for eligible families. A Can$1,000 increase in first-year benefits was found to increase the probability of childbirth by 16.9%. Further, Hyatt and Milne (1991) find that a 1% increase in unemployment insurance maternity benefits in 1980s Canada would result in a 0.09% - 0.26% increase of the total fertility rate. Looking at the effects of three tax policies (child tax credit, family allowances, and maternity leave benefits) on fertility from 1921 to 1988, Zhang, Quan, and van Meerbergen (1994) find that all three transfer programs significantly alleviate declining fertility. Estimated elasticities of fertility with respect to the cumulative effect of all three programs range from 0.05 to 0.11. Finally, McNown (2004) finds the elasticity of the fertility rate relative to the child benefit policies to be 0.7.

Other studies show that other causes of a change to personal income affect fertility decisions, as well. Lovenheim and Mumford (2013) show that an exogenous positive shock in housing prices, which affects personal wealth of homeowners, increases their probability of having a child. Similarly, Lindo (2010) finds that a permanent income shock caused by the husband's job displacement reduces total fertility. Analyzing the effect of increasing earning and employment potentials of non-college educated men employed in fracking production, Kearney and Wilson (2018) find a positive effect on both marital and non-marital birth rates.

The results of our analysis of the Mincome data indicate a positive effect of receiving a guaranteed income on child birth rates, even when controlling for a rich set of covariates that can explain fertility. We complement our analysis with a dynamic event history analysis, which equally indicates a positive effect of plan treatment. The magnitude of the positive effect of the treatment ranges between 7 and 10 percentage points, depending on the specification. The estimates for some treatment plans go up to 22 percentage points. It is not the case that the households assigned to more generous plans are more likely to have a child.

In the following section, we explain the Mincome experiment and the political context in which it was conducted. We further provide a description of sample development, discuss potential self-selection issues, and give a comparison of the treatment and control groups. Section 3 presents a simple model illustrating our hypothesis that a guaranteed income scheme would increase fertility for families whose income is below the threshold for the subsistence consumption constraint to be binding. In Section 4, we present our data and descriptive statistics. Section 5 explains our estimation strategy, and presents our results. Section 6 concludes and discusses the results.

2 The Mincome Experiment

2.1 Background of the Experiment

Motivated by high levels of poverty, the end of the 1960s and 1970s brought heightened interest in assessing the impact of a guaranteed annual income (GAI) or a negative income tax (NIT) (we use GAI and NIT interchangeably, since Mincome is composed of a guaranteed income that decreases with income). In 1964, US President Lyndon Johnson called for a "war on poverty" in the United States. In the same year, Congress established the Office of Economic Opportunity to design programs to combat poverty and inequality of opportunity. In 1968, the Economic Council of Canada declared that "poverty among Canadians was widespread beyond belief" (Hum and Simpson, 1991). In this context, negative income tax schemes were proposed to alleviate poverty and provide financial security for low-income families. The opponents of this proposal argued that it could negatively affect labor market participation by decreasing incentives to work, which led the public and academic debate to concentrate on the labor supply responses to such schemes. At the same time, another policy change was impacting labor supply. In 1971 in Canada, maternity leave became available to mothers with 20 or more insurable weeks. The introduction of maternity leave provided parents with greater economic stability and more time to care for their newly born offspring (Marshall, 2003). By the end of the 1970s, women's labor market participation had increased to 60%, up from just 30% just ten years prior (Congress, 2021).

The changing policy landscape and questions about income and work behavior motivated experimental research in both the United States and Canada. In the United States, the first such experiment was conducted in New Jersey in 1967 (Kershaw and Skidmore, 1973). Similar programs followed in Seattle, Washington; Denver, Colorado; and Gary, Indiana. Mincome was launched in 1974 in Manitoba to be continued for three years.

The main purpose of the Mincome experiment was to provide evidence on labor supply responses of a basic income program, in order to inform the ideal design and implementation of a future negative income tax policy (Hum and Simpson, 1991; Hum and Powell, 2016). The provincial government of Manitoba in Canada submitted a proposal on the project mainly with the intention of understanding the effect of a GAI on labor supply as well as the financial feasibility of this transfer scheme for the federal government (Farthing, 1992). Mincome was conducted in this context under the joint sponsorship of the Manitoba government and the federal government of Canada. The data from the Mincome experiment has been used to study not just the labor supply changes to basic income (Calnitsky and Latner, 2017; Riddell and Riddell, 2021), but also wages (Calnitsky, 2018), education and birth outcomes (Forget, 2011), and marriage stability (Choudhry and Hum, 1995). Simpson et al. (2017) provide a detailed review of Mincome's implementation and a modern-day reflection on the income experiment's success.

2.2 Program description

The Mincome program targeted low-income households. Once households were selected to participate in the program (conditional on having low-enough income), their assignment to a treatment group or a control group was randomized. The design of the treatment groups in Winnipeg was based on a combination of three different levels of guaranteed income payments and three tax levels. The annual support level represented what the household would receive if they had no other source of income or wealth; Mincome was unique among the income maintenance programs of the day in its inclusion of net worth in calculating benefits. This amount was calculated for a four-person household and was then adapted for households of other sizes. The assigned tax rate determined the rate at which the income support declined with a marginal increase in income or net wealth. For instance, if a household was in a treatment group with a 50% tax rate, every additional dollar the household made would reduce Mincome payment by 50 cents. The final payment of household i in treatment group k was given by $P_{ik} = G_k - (t_k Y_i) - (r_k W_i)$, where G_k is the support level, t_k the treatment group-specific tax rate, Y_i the monthly earnings of the household, W_i the household wealth, and r the tax rate on wealth, which was fixed for all groups (Hum and Powell, 2016). As such, the scheme applied is different than a basic income grant, as households that earned more than a certain amount would not receive any payments that month. However, the design ensured that even when a household had income above the threshold, they were guaranteed the security that they would receive money in case of job loss or a decline in earnings. The overview of different treatment groups, where the guaranteed annual income is normalized for a four-person household, can be seen in table 1.

Table 1: Treatment Plans

Tax Level \ GAI Level	$3800	$4800	$5480
35%	Plan 1	Plan 2	-
50%	Plan 3	Plan 4	Plan 5
75%	Plan 6	Plan 7	Plan 8

The treatment with the highest income level and lowest tax rate (the top-right field in table 1) would be more generous than any negative income tax scheme that the Canadian government would realistically afford to implement. This treatment plan was thus consideredirrelevant for the main purpose of the experiment and was left out from the start. The least generous plan, Plan 6, was merged with Plan 7 after a while due to the high drop out rate (Mason, 2016).

The most generous annual support level was $5,480 Canadian dollars (approximately Can$28,000 in current value), and the least generous was Can$3,800. The median income in Canada in 1972 was Can$11,234 for a four-person household (Hum and Powell, 2016). As such, the highest support level corresponded to approximately half of the median income in Canada at the time. The threshold to be considered a low-income household lies between the lowest and highest support levels (ibid.). The households that could benefit from or realistically receive the payments were low-income households, and only a fraction of these households could actually go above the threshold of low income with the Mincome payments. It is possible to say that the payments only affected households that were either subsistence-constrained or that risked becoming so in the future. Within the framework we developed in the introduction, these households would most likely fall under the second and third categories, for whom the subsistence consumption constraint is binding.

2.3 Study Sample

A few words should be said on the sample selection in the Winnipeg section of the Mincome experiment. Ideally, both the selection of the sample and the allocation to treatment and control groups would be completely random, with each group comprising more or less the same number of units, in this case households. In our setting, the first step of sampling was not random, but followed an assignment model. Random households were chosen to be screened for eligibility based on their income. Households were selected for participation if their income was low enough. The households deemed eligible were asked to complete a baseline survey for a more detailed second screening. The final sample of families chosen were then asked to enroll in the payments program. Once households were selected for the program, assignment into treatment group or to the control group was random. Figure **??** shows the development of the sample of households analyzed here.

In the first year of the experiment, the drop-out and refusal rates were very high. Furthermore, too few households received non-minimal payments. It was thus decided that a supplemental sample of 293 households would be enrolled in the program.[4] The trade-off between randomization and cost and policy considerations was tackled in Winnipeg with the Conlisk-Watts assignment model, which optimizes the experimental design given certain constraints. This model was first developed for the New Jersey experiment, and was followed by subsequent NIT experiments (Hum and Simpson, 1991; Keeley and Robins, 1980).

The Winnipeg sample was stratified using two variables: family structure and income. The family structure was stratified along four states: double-headed families with multiple earners; double-headed families with a single earner; single-headed families; and single-headed individuals (Hum and Sabourin, 2016). The assignment is of particular importance for this study looking at the effect of guaranteed income on fertility decisions, because it

[4]The households in this supplemental sample comprised re-contacted households who had not initially completed the baseline survey; households that were incorrectly excluded from the baseline interview; households who had initially not been contacted for the baseline interview; and a sub-sample of households that received welfare in the past three years, but who were no longer receiving welfare payments (Sabourin, 2016).

implies that the assignment to treatment cells is not orthogonal to household characteristics that also affect the outcome variable, fertility. It is therefore important to employ a multivariate analysis where the stratifying variables - family structure and income group - are included in the regression (Keeley and Robins, 1980). An important aspect of the survey design here was that once a household was selected into the experiment, the assignment to the treatment plan, including to the control group, was random (Sabourin, 2016).

A further concern regarding sample selection is non-participation and attrition. If households with certain characteristics are more prone to refuse to participate in the interviews or to drop out of the program, we would have a bias in the sample. If the reasons of self-selection to participate are also positively correlated with the outcome variable, we would have an overestimation of the treatment effect. In our case, a family planning on having a child might find it more important to receive the financial benefits of the program and thus be less likely to drop out. The Mincome experiment required the participants to take part in interviews every three months and to file monthly income reports; the interviews were composed of a very extensive set of questions that went beyond labor supply and included socio-psychological variables, leisure time use, satisfaction with marital life, and so on. It is plausible to think that families for whom the benefits were less important had lower incentive to go through the interview and filing processes.

In the appendix, we provide a detailed analysis and discussion of the patterns of refusal and non-response behavior. The technical reports of the experiment provide this analysis for the refusals and non-response between the initial stages of the experiment, such as between the screening interview and the baseline interview and between the baseline interview and enrollment to the program. The analysis suggests that single individuals without children were least likely to drop out of the program, and households with double earners were most likely to drop out. We conduct a complementary analysis, comparing households that initially completed all steps to be considered for the program, but who dropped out within the first two years with those who continued participation. We identify two important

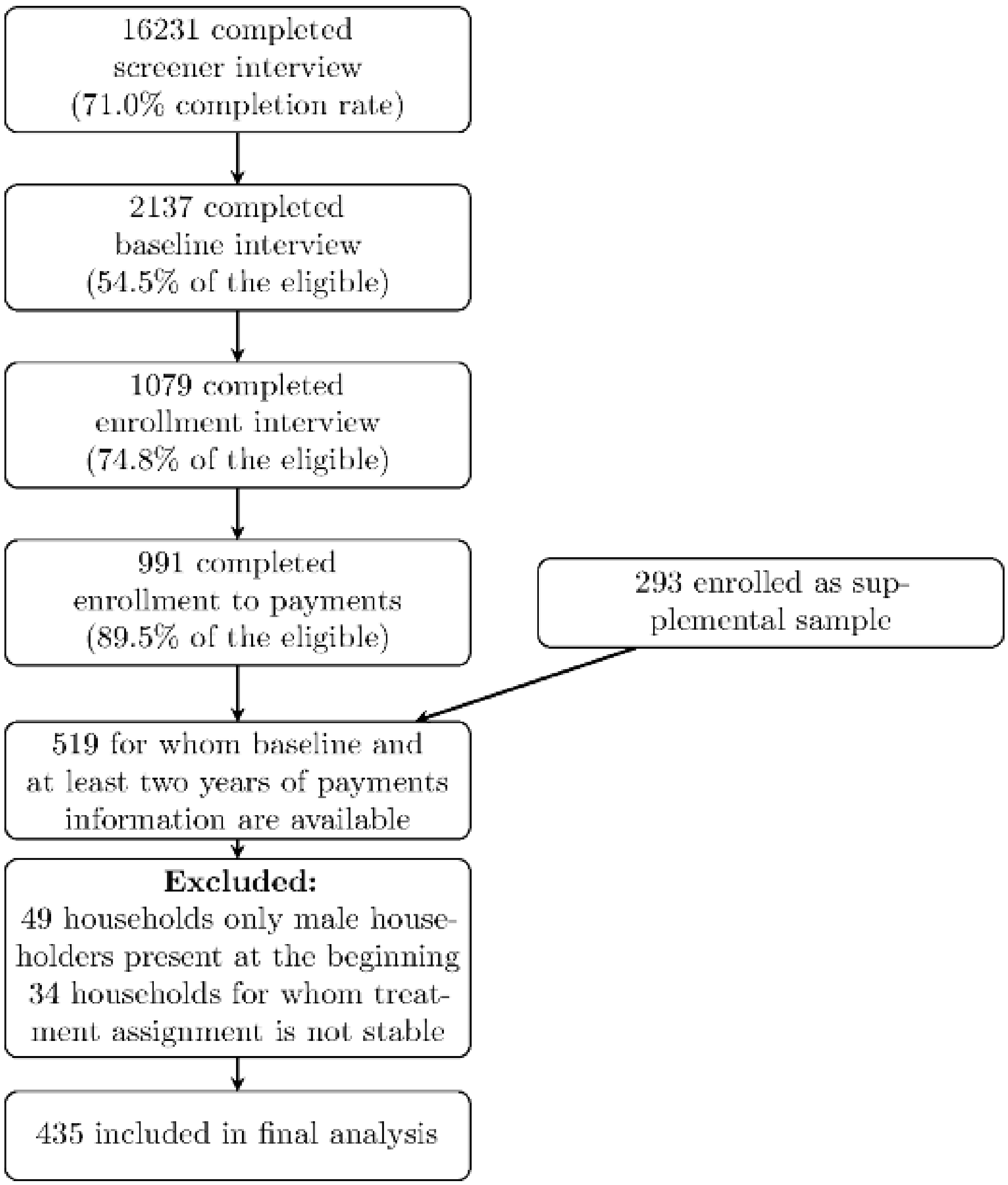

Figure 1: Sample development over time

differences between households who remained in the experiment and those who dropped out. First, the households who continued participation had, on average, more children than the non-participant group. Second, and relatedly, the participant group had higher actual or expected childcare expenses. These differences may suggest that the households that self-selected into participation did so to financially support their already existing children. On the other hand, these same households may have been more likely to want more children. With the assumption that the unobserved desire for another child is correlated with the number of existing children, we include the number of children a household already has in the analysis in order to control for this effect.

3 Conceptual Framework

In order to form our hypothesis on how a guaranteed income scheme can affect fertility decisions in the presence of a subsistence consumption constraint, we illustrate here a modification of the seminal model of Galor and Moav (2002). As we focus on the number of children born as an outcome variable in our empirical analysis, and we cannot observe the investment in children's educational level in our data, the households in our model decide only on their consumption and the number of children. We augment the model by adding insecurity on future income, depending on the human capital of the householders.

We first consider the case without any payment scheme, where a household's income is only their wage. We have two periods. In the first period, the households do not derive any utility; they draw and observe a level of human capital, h_i; and decide on their consumption and the number of children they want to have in the second period. The value they draw assigns them a certain range of wages that will be available to them in the next period, although they do not know with certainty what wage they will receive. In the second period, they draw a wage from a uniform distribution with support of $[\bar{w}_i(h_i), \underline{w}_i(h_i)]$. Each child costs a fraction of time, denoted τ, and time is normalized to 1. There is no leisure in the model, and labor supply is inelastic to the wage. The decision of how many children to have is simultaneously the decision of how many time units the household will spend on the labor market.

The households maximize the following utility function, where c denotes own consumption, and n the number of children:

$$u = (1 - \gamma) \ln c + \gamma \ln n \tag{1}$$

where γ represents the weight assigned to the children in the utility function, subject to the subsistence consumption and budget constraints:

$$c \geq \tilde{c} \geq 0 \tag{2}$$

$$c \leq w_i(1 - n\tau) \tag{3}$$

Because there is uncertainty, we need to take expectations, and we can write the budget constraint as follows:

$$\tilde{c} \leq c \leq \mathbb{E}(w_i) - \mathbb{E}(w_i)n\tau \tag{4}$$

where the first part of the right hand-side represents expected income, and the second part the expected cost of n children. Note that, if unemployed, so that w_i is 0, the time cost per child is 0 since time cost is measured by the wage offered on the market.

Plugging the budget constraint in the utility function, we see that the optimization problem can be reduced to choosing the number of children in order to maximize the following function:

$$u = (1-\gamma)[\ln \mathbb{E}(w_i) - n(\tau \mathbb{E}(w_i)] + \gamma \ln n \tag{5}$$

Since time is normalized to 1, $\mathbb{E}(w_i)$ represents potential income, that is, the income the household would have if they spent all their time on the labor market. $\tilde{w}$ denotes the threshold above which the subsistence constraint is no longer binding, defined as $\tilde{w} = \tilde{c}/(1-\gamma)$. There are three solutions to the number of children to have, depending on the expected potential income:

$$n^* = \begin{cases} \frac{\gamma}{\tau}, & \text{if } \mathbb{E}(w_i) \geq \tilde{w} \\ \frac{E(w_i)-\tilde{c}}{\tau \mathbb{E}(w_i)}, & \text{if } \tilde{c} < \mathbb{E}(w_i) \leq \tilde{w} \\ 0, & \text{if } \mathbb{E}(w_i) \leq \tilde{c} \end{cases}$$

We will call this set of three solutions the benchmark case, which is the Galor and Moav (2002) model without investment in education. In the first case, where the household's expected wage is high enough for the subsistence consumption constraint to be no longer binding, the household can realize the optimal solution and allocate the γ fraction of their resources to child-rearing. When the subsistence consumption constraint no longer binds, the optimal solution is not increasing in income, since the child-rearing cost is also a function of income. It can be thought of as the opportunity cost of having a child and working less, measured by the wage offered on the labor market. In the second case, the household can consume enough for survival, but the subsistence consumption constraint is still binding. The household allocates all resources to the subsistence level of consumption, and then invests the remaining resources in child-rearing. In the third case, the household does not have the necessary means to purchase the subsistence level of consumption, and cannot afford to have any children.

We then consider the case with a guaranteed income scheme, similar to the one implemented in Mincome. The guaranteed level of income is denoted by G, and it decreases with labor income by a negative tax rate t. The payment is thus $P = G - tw_i(1 - n\tau)$ so that the total potential income is $G + w_i(1 - t)$. There are no payments made in the cases where a household's wage is above the threshold defined by $w' = G/t$.

Since the income now not only comprises the wage but also the guaranteed income payment, let $\mathbb{E}(y_i)$ be the total potential income of a household, with $\tilde{y} = \tilde{c}/(1 - \gamma)$, the income level above which subsistence constraint does not bind. The new set of solutions can be written as:

$$n^* = \begin{cases} \frac{\gamma E(y_i)}{\tau E(w_i)}, & \text{if } \mathbb{E}(y_i) \geq \tilde{y} \\ \frac{E(y_i) - \tilde{c}}{\tau \mathbb{E}(w_i)}, & \text{if } \tilde{c} < \mathbb{E}(y_i) \leq \tilde{y} \\ 0, & \text{if } \mathbb{E}(y_i) \leq \tilde{c} \end{cases}$$

How these solutions correspond to the benchmark case depends on two factors. First of all, there are three types of households defined by the wage range they face, which determines whether or not they are covered by the guaranteed income scheme. Secondly, it depends on how the two threshold wages, one for the receipt of the guaranteed income and the other for the subsistence consumption constraint to no longer be binding, relate to each other. In the following, we discuss how the optimal number of children changes for each three types of households, as well as the role of the generosity of the program.

First, there are households whose lowest possible income $\underline{w}_i(h_i)$ is above the threshold wage, $\underline{w}_i(h_i) > w'$, so that they will definitely not be covered by the guaranteed income scheme and the expected total potential income equals $\mathbb{E}(w_i)$. The budget constraint will be the same as in the case without the guaranteed annual income, and with the cost of a child remaining constant, the number of children these households has will not change. Since

$\mathbb{E}(w_i) = \mathbb{E}(y_i)$, the expression for the optimal number of children will be the same as in the benchmark case.

Secondly, there are households whose highest possible income is still lower than w'. The expected total potential income will change from $\mathbb{E}(w_i)$ to $G + (1-t)\mathbb{E}(w_i)$, and will thus increase. A guaranteed income scheme's effect on these households would happen through the channel of an increase in the absolute level of income. As $\mathbb{E}(y_i) > \mathbb{E}(w_i)$, the number of children for these households increases, unless the expected total potential income is still less than $\tilde{c}$. In that case, the household will still not be able to afford to have any children. For a guaranteed income scheme that is generous enough, characterized by a high G and low t, such that $G + (1-t)\mathbb{E}(w_i) > \tilde{c}$ is ensured, even the households in the lowest income group will not find themselves in the third solution. If the threshold wage is high enough so that $G + (1-t)\mathbb{E}(w_i) > \tilde{y}$, the subsistence consumption constraint would not be binding for any household.

Finally, there are households who face a wage range that includes the threshold wage in its support. The expected wage might be above the threshold wage, but even then the possibility of receiving guaranteed income increases the expected potential income. Let us denote with q the probability that the household receives a wage below the threshold wage, such that $q = \frac{w' - \bar{w}_i(h_i)}{\underline{w}_i(h_i) - \bar{w}_i(h_i)}$. The probability of receiving a wage above the threshold is then $1-q$. The expected income can be written as:

$$q(G + (1-t)\mathbb{E}(w_i | w_i \leq w')) + (1-q)\mathbb{E}(w_i | w_i > w') \tag{6}$$

Comparing this expression to the case without guaranteed income, $q\mathbb{E}(w_i | w_i \leq w')) + (1-q)\mathbb{E}(w_i | w_i > w') = \mathbb{E}(w_i)$, we see that the first part of the equation increases while the second part stays the same, so that the expected total potential income increases. The increase becomes more pronounced as the probability of being below the threshold increases.

At the one extreme where $q = 1$, we are in the second case, and when it is 0, we are in the first case. For the households in the third case, the effect of a guaranteed income scheme is that of the effect of economic security, which manifests itself as an increase in expected potential income.

In the case that the household's highest possible income is lower than the threshold wage, or in which the threshold wage is within the support of the wage distribution faced by the household, we have shown that the expected income is higher than in the benchmark case, so that the optimal number of children increases as long as the household can expect to receive the guaranteed income. Note that this is because the additional income is non-labor income, and the time cost of a child is measured with the wage a household is offered on the labor market. The guaranteed income payments ensure that the total expected income increases without changing the cost of a child, given by $\tau\mathbb{E}(w_i)$.

We have stated that for a guaranteed income scheme generous enough such that even the lowest expected potential income is high enough to make sure that the subsistence consumption constraint is not binding, all households will have $\gamma E(y_i)/\tau E(w_i)$ number of children. For the households whose lowest expected wage is above the threshold w', the expression will simplify to γ/τ and stop increasing with income. For other households, the relationship $\gamma E(y_i)/\tau E(w_i) > \gamma/\tau$ holds, and the additional income as well as the security of payment in case of going below the threshold, increase household fertility. Of course, within this framework without investment in children's quality, a very generous guaranteed income would increase fertility for even high income households, as w' increases. With choice of educational investment and importance assigned to children's quality that increases with the human capital of parents, it is likely that high income households would spend this extra income on their children's education, rather than on having more children as in Galor and Moav (2002); Vogl (2016). Realistically, such schemes are likely to have a wage cap, ensuring that only the low-income households receive payments. In Mincome, the highest possible payment corresponded roughly to half of the median income at the time so that it would not have any effect on households whose income was above this threshold.

A guaranteed income scheme increases the aggregate fertility through a second channel. Above, we discussed how the optimal number of children increases with expected potential income. Additional income also changes the probability of a household to be in any one of the three cases. As $\mathbb{E}(y_i)$ increases, the first inequality in the set of solutions, $\mathbb{E}(y_i) > \tilde{y}$, is relaxed. In the case where the guaranteed income is above the threshold for the subsistence constraint to be binding, no household will find itself in the second and third case and all households will be able to choose the optimal solution, and allocate γ of their resources to child-rearing. Note that, in the benchmark case without the guaranteed income scheme, there might not be aggregate implications of uncertainty as the households are not risk-averse. It is possible that a household does not have any or only a less than optimal number of children, because they expect to have a wage below $\tilde{w}$, even though the realization of the wage in the second period is actually above this level. It is however equally likely that a household that expects to have a wage income above $\tilde{w}$ decides to have the optimal number of children, but that the wage realization is below the expectation. Whether or not the proportion of the former group is higher than the latter group would depend on the distribution of human capital, as well as the corresponding wage ranges in an economy, about which we do not make any assumptions. This result is due to the risk neutrality of the households, and the uniform distribution of individual wages. We choose these assumptions to keep our argument tractable, and show the effect of guaranteed income in a qualitative way. With risk aversion, which is a more realistic assumption, it is likely that the aggregate fertility rates would be negatively affected and the quantitative results would be more pronounced. We also do not make any assumptions on the aggregate wage and human capital distribution, as we are primarily interested in the households' individual decisions rather than the aggregate implications and due to what our data allows us to measure. With the inclusion of guaranteed annual income, however, uncertainty plays an important role for households whose wage range includes the threshold wage. All else equal, the aggregate fertility also increases with the guaranteed income scheme, reducing economic insecurity as compared to the benchmark case. It is not solely the income effect, but also the economic security effect of this scheme, as some households might end up not receiving the payment, but the knowledge in period 1 that they might increases their expected potential income and thus the number of children.

This simple model illustrates our hypothesis. A guaranteed annual income scheme with payments decreasing with wage income would allow low-income households and households with low income security to relax their financial constraints. With a certain level of income secured in case of job loss or negative shocks to wages, which occur more frequently for households at the lower end of the income distribution, we can expect a guaranteed income scheme to increase fertility. This increase would be less pronounced for households above the subsistence consumption constraint, who are still eligible for payments. We should observe no effect on households whose income certainty does not or is very unlikely to fall below the threshold wage for receiving payments. The Mincome participants were all low-income families. As such, our empirical analysis will deliver results for low-income families, for whom we expect fertility to increase.

4 Data and Descriptive Statistics

Our main data sources are the Mincome Baseline Data and the Mincome Payments Data (Mason, 2017).The final sample we use for analysis includes 435 households in Winnipeg who participated in the experiment for at least two years and in which a female householder has been present since the program began (see Figure 1 for more detail).

The first dataset is cross-sectional, comprising data collected at the beginning of the experiment. Here we have information on the household's treatment plan, income bracket, family composition, the ages and educational attainment of the householders, and the ages of the children living in the household. Table 2 gives an example observation (one household) in the baseline data. We supplement these data with the so-called Mincome Payments Data.

These data were collected on a monthly basis throughout the experiment. They include information on the number of children and adults living in the household, among various other variables. An example observation from the payments data is given in table 3. We merge these two datasets to create a pseudo panel with fixed household characteristics coming from the baseline data, and dynamic variables that come from the payments data. As such, in our final data, one household has one observation, with information on certain outcome variables from points throughout the experiment.

ID	Treatment	Two HH	Age M	Age F	Nr child	Homeowner	Earn '74 M	Earn '74 F
1352	4	1	34	32	2	0	183	190

Table 2: An example of an observation from the baseline data: A household with two householders, and two children, assigned to treatment group 4. We see (among other things) their homeownership status before the experiment started, and their earnings in the past year.

ID	Nr child 1	...	Nr child 14	...	Nr child 36	Empl F 1	...	Empl F 14	...	Empl F 36
1352	2	...	3	...	3	1	...	0	...	1

Table 3: An example of an observation from the payments data: For each month, numbered 1-36, we observe multiple variables regarding labor market status, number of adults and children at home, and earnings. Here, we show number of children in a household for months 1, 14, and 36 along with the employment status of the female householder.

We create a dummy variable that takes the value of 1 if the household had a child during the three years of the experiment, excluding the births that happened within the first nine months of the experiment. As mentioned above, every month, households reported the number of children present in the household, which allows us to see if there had been an increase. The fact that we only observe the numbers, and not the new child's relationship to the householder, might raise the concern of whether or not the child in question is indeed a newborn child of the householders. Fortunately, there is a separate data set for family composition. In this dataset, we can see the relationship between different members of the household, as well as their birth dates. However, these data were collected not monthly, but a maximum of four times for each family. They thus do not account for the births that happened after the last interview. For that reason, we concentrate our analysis on the increases of the number of the children in a household, rather than using the family composition data to detect births. However, we use the latter to check if there is reason to believe that a child who joined the household was not the child of the householders, i.e. that they are the sibling, cousin, or not related to the householders. We match these two datasets to check for increases in the number of children that would not correspond to births calculated with the family composition data. In only two of the unmatched cases is the child a brother or a grandchild to the householder. In other cases, either the birthdate data are missing for the family, or the last interview for family composition data was conducted before we observe the increase in the number of children.

A first look at the data shows us that there is a higher birth rate for every treatment group than for the control group. The overall birth rate in the treatment group during the experiment, excluding the births that happened within the first nine months of the experiment, is 15%, while it is 8% for the control group.

Table 4: Birth rates by treatment status

Treatment status	Number of households	Number of births	Birth Rate
Treated	335	49	0.15
Plan 1	40	8	0.20
Plan 2	56	6	0.11
Plan 3	42	7	0.17
Plan 4	59	7	0.12
Plan 5	47	6	0.13
Plan 7	51	9	0.18
Plan 8	40	6	0.15
Control	100	8	0.08

In the analysis below, we ask whether or not the households in treatment and control groups differ in certain observable characteristics that could drive the differences in the birth rates. Overall, we look at means of 77 variables related to household composition, demographics, labor market experience, and social background, as they were measured at the beginning of the experiment, and reported in the baseline data. We find that the households in the two groups differ in 20 of these characteristics. The full list of variables and the balancing statistics can be seen in Table A1.

An important and statistically significant difference between the households in the treatment and control group is the household composition. 67.8% of the households in the treatment group have two householders, as compared to 48.0% in the control group. Since being a married couple should be a significant factor determining whether or not a household has a child, the difference between the birth rates could be driven by this difference. Moreover, the average age of the female householders in the treated group is slightly lower than that of the control group. In both groups, about half of our sample does not have any children, 30% one child, and about 14% have two children. The treatment group is slightly wealthier than the control group, as can be seen in the percentage of households owning a house and vehicles.

The descriptive analysis of the characteristics of the households in the different treatment groups and the control group shows that although the allocation was random, the population in each group is different in certain observable characteristics that affect the probability of childbirth. In order to account for these differences, we control for these variables in a multivariate regression whenever they are available for the whole sample. We run different specifications in which we include some variables not available for the whole sample, but which we deem important for the analysis, and report the results as well.

5 Identification Strategy and Results

5.1 Causal Framework

Our goal is to estimate the effect of having three years of minimum income security on realized fertility outcomes. As laid out in out theoretical model, we expect the presence of income security for low income households to have a positive impact on fertility. Following the potential outcomes framework (Angrist and Pischke, 2009), we denote with Y_{1i} the probability of household i of having a child if they were assigned to treatment, whereas Y_{0i} is the child birth probability of the same household if they were assigned to the control group, hence if they were not guaranteed minimum income. The average treatment effect that we try to estimate is given by $\mathbb{E}[Y_{1i} - Y_{0i}]$: the population average of differences in potential outcomes for all individuals.

The observed difference in outcomes can be written and decomposed as

$$\begin{aligned} \mathbb{E}[Y_i|D_i = 1] - \mathbb{E}[Y_i|D_i = 0] =& \mathbb{E}[Y_{1i}|D_i = 1] - \mathbb{E}[Y_{0i}|D_i = 1] + \\ & \mathbb{E}[Y_{0i}|D_i = 1] - \mathbb{E}[Y_{0i}|D_i = 0] \end{aligned} \tag{7}$$

where $D_i = 1$ implies that the household was assigned to treatment. The first expression of the equation is the population mean of the observed birth rate between treatment and control group, which in our case is 0.07. The first expression on the right-hand side is the average treatment effect on the treated (ATT), and the second half of the right-hand side is selection bias. Selection bias would be zero if we can expect the potential outcome for the treated group in the absence of treatment to be on average the same as the control group.

In the case of random assignment, the last two terms cancel out: units assigned to the treatment and control groups are on average the same given randomization, so we would expect the same outcome for them in the absence of the treatment, i.e. when $D_i = 0 \; \forall \; i$. order to be able to interpret the difference of 0.07 as the average causal effect, we would need to be able to argue that the assignment was indeed random.

As mentioned above, once selected into the Micome program, the assignment to control and treatment was purely random. However, we have shown that the control and treatment groups differed from each other in important aspects, which may have been due to attrition, posing a threat to the identification of the treatment effect.

We alleviate this concern by controlling for the set of characteristics upon which the treatment and control groups differ. We therefore rely on the Conditional Independence Assumption (CIA) to identify the causal effect of income security: that the assignment to treatment and control is random once we control for this set of covariates. The baseline data includes rich information on household characteristics such as education, the value of durables held by the household, and recent labor market history.

We think that the CIA is justified in this case since assignment to treatment and control was random, and that we can control for a large set of relevant household characteristics. Under the CIA, our baseline estimation explained in the section below results in causal estimates of the effect of the program on fertility. The CIA may not hold if the households in control and treatment groups differ from each other in unobserved characteristics. In order to account for potential unobserved heterogeneity, we later further adopt an event history approach, developed in the section following the baseline estimation.

5.2 Baseline Estimation

Our main specification is a static logistic regression whose outcome variable is whether or not a household has experienced a child birth. At first, we regress this probability on a dummy variable indicating that the household has been exposed to treatment and a set of controls.

$$\Pr(\text{Child birth}_i = 1) = \frac{\exp(\beta_0 + \beta_1 \text{Treated}_i + \beta_2 \text{Controls}_i)}{1 + \exp(\beta_0 + \beta_1 \text{Treated}_i + \beta_2 \text{Controls}_i)} \quad (8)$$

The coefficient of interest in this analysis is β_1, which reflects the average treatment effect of monthly payments on the probability of childbirth. Our theoretical framework delivers the prediction that this coefficient is positive.

We run the same regression by replacing the dummy "treated" with different dummy variables for each of the treatment plans, by leaving out the control group in order to detect differential effects of different plans. We run several regressions with different sets of control variables. We do not have information on some control variables for all of the families.

Our first set of controls includes the stratifying variables that were used to determine the assignment to treatment plans, as well as the characteristics in which the control and treatment groups differ. The family size index and the income brackets were created as stratifying variables, and as they do not have a numerical meaning, they also enter the regression as a set of dummies. By controlling for the stratifying variables, we deal with the problem that the assignment was not completely randomized.

Beyond the stratifying variables, we also control for the household characteristics that are relevant for fertility decisions, and in which control and treatment groups differed. We control for age of the female householder, the number of children present in the household, the number of children outside of the household, the family size, whether or not a male householder is present, and the income bracket in which the household was at the beginning of the experiment. All of the controls enter the regression as sets of dummy variables. Age is coded as a dummy variable to allow for a non-linear relationship between the probability of childbirth and the age of the female householders. The relationship between how many children a household has and the probability of having a new child is probably not linear either, which is why these variables also enter the regression as dummy variables. In our theoretical framework, factors that affect economic security such as wealth, savings, and income play a role in whether or not additional income would have any effect on fertility, so we also control for household wealth by including the number and the value of the vehicles the household has and whether or not the household owns the house in which they live in the third through sixth specifications (this information is missing for some households, hence omitted from the first specifications). Further household characteristics include the number of adults living in the household apart from the householders, dummies stating whether the female householder is in school, whether she is ill or has disabilities, and a dummy that takes the value 1 if the household had a child within the first nine months of the experiment (and was thus pregnant before the program started). We also control for whether or not the female householder was out of the labor market, as this could be related both to childbirth and to the participation in the program. We account for a change in the householder composition during the experiment, which happens either by one householder leaving or a new householder joining. An important difference between the treatment and control groups is the educational attainment of the female householder's mother. We add this information in the last two specifications, as this information is missing for 61 households in our sample.

In the first specification (columns 1 and 2 of table 5), the overall treatment dummy and the dummies for treatment plans three and seven have coefficient estimates that are positive and are statistically significant. Being in the treatment group has an average marginal effect of seven percentage points on the probability of having a child, and being assigned to treatment groups three and seven, 17 and 14 percentage points, respectively. The estimates for other treatment groups are all positive and range between 4 and 14 percentage points, although not significant at 90%.

Table 5: Odds ratios of treatment plans

	Dependent variable:					
	Dummy of childbirth					
	(1)	(2)	(3)	(4)	(5)	(6)
Treated	2.490*		3.791*		3.262*	
	(0.526)		(0.719)		(0.699)	
Plan 1		2.308		3.547		3.461
		(0.746)		(0.926)		(0.913)
Plan 2		1.561		2.527		2.213
		(0.705)		(0.910)		(0.883)
Plan 3		4.406*		6.055*		5.479*
		(0.765)		(0.991)		(0.966)
Plan 4		2.078		3.534		3.097
		(0.725)		(0.869)		(0.856)
Plan 5		2.510		2.515		2.362
		(0.755)		(0.943)		(0.937)
Plan 7		3.503*		5.259*		4.681*
		(0.709)		(0.874)		(0.866)
Plan 8		3.088		3.831		3.617
		(0.728)		(0.942)		(0.925)
Observations	435	435	275	275	275	275
Female householder controls	Yes	Yes	Yes	Yes	Yes	Yes
Male householder controls	No	No	Yes	Yes	No	No
Household characteristics	Yes	Yes	Yes	Yes	Yes	Yes
Fem. householder's mother's education level	No	No	No	No	Yes	Yes
Log Likelihood	−120.833	−119.702	−87.042	−87.154	−89.107	−88.386
Akaike Inf. Crit.	365.666	375.404	300.085	310.309	286.214	296.772

Note: *p<0.1; **p<0.05; ***p<0.01

In the second specification, we add control variables for the age of the male householder, whether he is in school, and his educational attainment. Our sample size is reduced to 275 in this case, as it only includes households where there is both a male and female householder. The coefficient estimates for being treated and being assigned to plans three and seven are statistically significant and positive also in this specification. Furthermore, the magnitude of the marginal effects are larger; for the treated group the marginal effect is 11 percentage points, and they are 22 and 20 percentage points for treatment groups three and seven, respectively. Again, the estimates for all plans are positive and range between 11 and 16 percentage points.

For comparison purposes, we report the results obtained from running the same regression asin the first two columns, but including mother's education, for the subsample with two householders. Both the point estimates and significance levels of the coefficients change barely, implying that the change between the two regressions is due to the change in the sample rather than the controls. These results have a very intuitive interpretation. Economic security through a guaranteed annual income might make it possible for households with partners to have children, but it will not affect the probability of finding a partner with whom the single householder can then have children.

We report the odds ratios in the Table 5 below, and the average marginal effects of the treatment dummies in Section A.3 of the Appendix. It would be wrong to compare the estimates obtained at different specifications as the sample of households analyzed are different. We can however see that across different specifications, dummies for being in the treatment group and being assigned to treatment plans three and seven are always positive and statistically significant. Given the small size of each plan, the comparison between the control and the overall treatment group is likely to be more informative about the effect of the program. However, the result that the most generous plans are not those for which the households had higher odds of childbirth indicates that the guarantee of a certain level of income is more important in the decision of having a child than the absolute level of increase in income.

It is important to note that in order to obtain estimates of a treatment effect that could be generalized to the overall population, we would further need to interact the treatment variable with the stratifying variables. There are two reasons why this is not our baseline estimation. First, we are interested in understanding the effect of secured income on low-income households, not on the general population. Our theoretical model suggests a positive effect only for the households that face subsistence constraints, or have a high risk facing them. Secondly, we are unable to do so in a practical sense, as each of the stratifying variables had over 20 categories. Given our sample size of 435 households and births being rare events, we are not able to estimate the coefficients of these interaction terms. Nevertheless, we do this exercise by including income as a continuous variable and taking the number of people in the household instead of the family size index. We interact the treatment variable with these two variables, and find positive but statistically insignificant coefficients of the treatment variable. The results of this exercise are in the appendix table A.4.

5.3 Extension: Event History Analysis

As an extension to our baseline estimation, we conduct a dynamic analysis in an event history framework. In an event history analysis, the focus is on the occurrence and the timing of events. Event occurrence is an individual's transition from one state to another: from being unemployed to employed, from not being pregnant to being pregnant, from being married to being divorced, and so on. As such, it is suitable for analyses that are concerned with the questions of whether and when people experience particular events and why the timing and the occurrence differ from individual to individual. Unlike other statistical applications, event history analysis does not take means of outcomes and standard deviations of the variable to be explained, but rather the probabilities of an individual experiencing a certain event at a given time in their life course (Singer et al., 2003). It allows us to detect whether or not the economic security provided by Mincome increases the occurrence rate of births. While the multivariate logistic regression in section 5.2 allowed us to control for multiple factors that influence fertility decisions and outcomes, a dynamic analysis such as the event history analysis has certain advantages over a static one. Most importantly, it allows previous childbirths to influence not only the probability of, but also the time until the next childbirth, and for us to include a full marital history so that we account for not only whether the person is married, but also since when. Furthermore, the data are organized as panel data, which allows us to use individual random effects, capturing unobserved characteristics that influence the point by which a woman has a child, and how many. Finally, event analysis is useful to analyze data that suffer from censorship, and especially non-informative and right censorship. As our data end with the last year of experiment, we do not see whether a family had a child in the following months after the experiment was finished.

Particularly suitable for fertility analyses where we need to account for the possibility of multiple childbirths is the so-called frailty analyses, where an individual random effect is incorporated. This random effect represents the unobserved heterogeneity in the sample that leads to some units being at a higher risk of experiencing an event (Amorim and Cai, 2015). We organize the data in person-year format. We have one observation per woman for each of the 435 women in the baseline analysis. If the woman is younger than 50 in 1977, then the age they reach in 1977 is the last observation we have. In this case, the data are right-censored. We take the period in which a woman is fertile as the ages between 15 and 50. Using the family composition data, we reconstruct the childbirth and marital history for each woman. When a woman has a child, the variable birth takes the value 1 for that year. The variable married takes the value 1 in each year a woman is married.

Similar to the framework suggested by Van Hook and Altman (2013), our econometric model is a logistic regression with age dummies as well as the inclusion of the episode:

$$\Pr(\text{Child birth}_{it} = 1) = \frac{\exp(\beta_0 + \beta_1 \text{Treated}_i {*} \text{Experiment}_t + \beta_2 \text{Married}_{it} + \beta_3 \text{Age}_{it} + \text{j}_{it} + \text{u}_i + \text{Year}_t)}{1 + \exp(\beta_0 + \beta_1 \text{Treated}_i {*} \text{Experiment}_t + \beta_2 \text{Married}_{it} + \beta_3 \text{Age}_{it} + \text{j}_{it} + \text{u}_i + \text{Year}_t)} \quad (9)$$

As in the previous analysis, we run the same regression by replacing the dummy "treated" with a different dummy for each of the treatment plans, by leaving out the control group. j_{it} is the variable representing the parity so that we differentiate between births of different order. It takes the value 1 until the first childbirth, then 2 until the second, 3 afterwards, and so on. The dummy "treated" and the dummies for each treatment plan are the same during the whole observation period. The dummy "experiment" takes the value 1 in the years 1974 through 1977. The coefficient of interest is that of the interaction of these two variables. We also include an individual random effect that captures the unobserved characteristics related to how many times a woman experiences childbirth as well as the time intervals between each birth. The random effect allows us to capture this heterogeneity in the sample, allowing for more accurate estimates. Finally, we include a year fixed effect in order to control both for generational differences and time-variant economic factors in the region that might have affected the childbearing in the whole population in the years preceding the experiment.

There are two limitations that arise while constructing the full marital and childbirth history for each woman in our sample. The first is that for some units, the childbirth history is likely to suffer from left truncation, particularly for older women in our sample if they have more than one child living outside of the household. Second, single householders who were married before were not asked how long their marriage was, or when the marriage started. Married women who were previously married were, however, asked this question. As such, we only have information on the date of separation and cannot reconstruct a complete marital history for the women who were divorced or separated and who are now living alone. For that reason, we exclude women born before 1934 and women who were divorced before and are now single.

The regression results can be found in Table 6 below. The treatment dummy does not appear to be statistically significant, but is positive and of magnitude 4 percentage points. The coefficient estimates for plans 5, 7, and 8 are positive and statistically significant at the 90% level. They show an effect of 7 to 8 percentage points on the probability of childbirth.

For purposes of comparison with the baseline analysis, we run all the regressions with the same samples we had for the first and the second regressions above. The second sample included the 275 households for whom we have information on the male householder. The baseline analysis with this subsample had more pronounced results, and the effects of most plans were positive and statistically significant. In the event history analysis as well, the treatment dummy's coefficient estimates are higher. The coefficient estimates for the seventh and eighth plans are likewise positive and statistically significant in the event history regressions for both subsamples. Overall, our results show positive effects that range from four to 12 percentage points depending on the sample and the treatment plan.

Table 6: Results from event history analysis, average marginal effects

	Dependent variable:			
	Dummy for childbirth			
	(1)	(2)	(3)	(4)
Treated*Experiment	0.044		0.061	
	(0.030)		(0.039)	
Plan 1*Experiment		0.050		0.053
		(0.045)		(0.058)
Plan 2*Experiment		0.021		−0.002
		(0.042)		(0.054)
Plan 3*Experiment		0.066		0.088
		(0.047)		(0.058)
Plan 4*Experiment		−0.040		−0.011
		(0.042)		(0.051)
Plan 5*Experiment		0.079*		0.085
		(0.046)		(0.057)
Plan 7*Experiment		0.078*		0.110**
		(0.042)		(0.053)
Plan 8*Experiment		0.079*		0.125**
		(0.046)		(0.058)
Observations	4,026	4,026	3,169	3,169
Log Likelihood	−922.347	−947.581	−955.661	−978.890
Akaike Inf. Crit.	1,972.693	2,047.163	2,039.321	2,109.781
Bayesian Inf. Crit.	2,375.927	2,526.003	2,427.236	2,570.430

Note: *p<0.1; **p<0.05; ***p<0.01

The event history analysis allows us to compare the treated women both with the women in control group during the experiment and with all other women at the same age and with the same marital history before the experiment. Incorporating an individual random effect allows us to control for the unobserved heterogeneity in our sample and a year fixed effect controls for time trends. At the same time, we are not able to use as rich a set of control variables as in our baseline analysis, since we do not have information on the full history of the variables such as education, earnings, and employment. The two analyses can thus be seen as complementary; while our baseline analysis allows us to directly control for many variables, the event history allows us to take the effect of previous marital and childbirth histories into consideration in a more precise way and to incorporate an individual-specific random effect.

Taken together, results from both estimation strategies show a positive impact of the program on fertility. The quantitative estimation of the effect of the program on the probability of childbirth varies across different specifications, and the smallest estimates are of four percentage points, which represents a considerable impact on fertility. Importantly, the effect is not necessarily higher or more significant for the more generous treatment plans. This finding suggests that the security of receiving a certain amount of money in case of economic hardship was more important for households than the absolute increase in their income as a factor in the decision of having a child.

6 Discussion and Conclusion

Mincome as an RCT offers a unique opportunity to test the effect of an exogenous source of guaranteed income security on the fertility decisions of low-income households. Our analysis shows positive effects of receiving a guaranteed annual income on the probability of having a child. While the marginal effect of the program on fertility is seven percentage points for the whole sample, it is 13 for the households living as a couple. The effect is higher in magnitude for some treatment plans, and goes up to 14 percentage points for the whole sample, and to 29 for the sample with couples only. Event history analysis, which allows us to account for the dynamic nature of childbirth and for unobserved heterogeneity, confirms our results from the baseline analysis with positive results of similar magnitudes.

Estimating the effect of a positive exogenous income effect for multiple plans, we find that the probability of childbirth did not increase with the generosity of the guaranteed income. This finding is in line with the literature on the subsistence consumption constraint: once the constraint no longer binds, the positive relation of income and fertility no longer holds.

The results thus add to an important strand of research on the economics of fertility for low-income households. The payments received by the families in the treatment group facilitated securing subsistence consumption, and thus enabled them to fulfill their desire to have a child through mitigating the financial burden and pressure to secure against potential wage shocks that come with that decision.

On top of the absolute level of secured income, having the possibility to fall back on additional income when necessary may have been relevant for the decision to have a child. Having known about this safety net reducing the risk of falling below subsistence consumption may have alleviated the participants' concerns about being able to financially support a child in future periods of time, thus increasing the probability of childbirth. In addition to lowering the risk of potential income loss, the guaranteed annual income decreased the opportunity costs of the parent, most commonly the mother, of not participating in the labor market, as the payments received per family were based on the employment status of the adults in the household.

Finally, since Mincome provided low-income households with an income guarantee for just three years, the estimates here might be a lower bound of the impact of a guaranteed annual income on fertility. A guaranteed annual income or a basic income scheme with no time limit might have an even more pronounced effect on fertility. However, there are some limits to the extent that the results presented here can be extended to make this claim. First, it could be that some families incorrectly expected the financial support to be permanent. To the extent that this is true, our results overestimate of the effect of a (known) temporary income support program. We have no information on the extent to which the participants understood that the financial support was only for three years, so we cannot rule out this possibility. If many participants believed that the income support was permanent, then the results would be more in line with the effect of a policy offering lifetime support. Moreover and more generally, as discussed by Simpson et al. (2017), the institutional and historical specificity of Mincome calls us to be cautious about making claims about the external validity of the findings and the application to contemporary economies and societies.

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A Appendix

A.1 Descriptive statistics

Table A1: Summary descriptives table by groups of ‘treated’

	Control N=100	Treatment N=335	Balancing stats
Household composition:			
Two householders	48 (48.0%)	227 (67.8%)	0.001
Single householder	35 (35.0%)	58 (17.3%)	<0.001
Age, male	35.1 (9.92)	32.8 (9.16)	0.142
Age, female	33.8 (11.5)	30.6 (9.86)	0.011
Number of children:			0.530
0	48 (48.0%)	170 (50.7%)	
1	31 (31.0%)	101 (30.1%)	
2	14 (14.0%)	51 (15.2%)	
3	7 (7.00%)	11 (3.28%)	
4	0 (0.00%)	2 (0.60%)	
Number of adults:			0.073
0	82 (82.0%)	285 (85.1%)	
1	15 (15.0%)	24 (7.16%)	
2	2 (2.00%)	20 (5.97%)	
3	1 (1.00%)	5 (1.49%)	
5	0 (0.00%)	1 (0.30%)	
Number of children living outside house:			0.182
0	95 (95.0%)	326 (97.3%)	
1	2 (2.00%)	2 (0.60%)	
2	0 (0.00%)	1 (0.30%)	

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Table A1 – *continued from previous page*

	Control N=100	Treatment N=335	Balancing stats
3	1 (1.00%)	4 (1.19%)	
4	1 (1.00%)	0 (0.00%)	
5	1 (1.00%)	0 (0.00%)	
7	0 (0.00%)	1 (0.30%)	
8	0 (0.00%)	1 (0.30%)	
Home ownership:			0.516
No	73 (73.0%)	231 (69.0%)	
Yes	27 (27.0%)	104 (31.0%)	
Home Value	23815 (10785)	22466 (8784)	0.552
Mortgage	5312 (5797)	5785 (5963)	0.795
Rent	77.9 (55.1)	94.2 (46.9)	0.025
Other Property? :			0.769
Yes	4 (4.00%)	12 (3.58%)	
No	96 (96.0%)	323 (96.4%)	
Sell price of other property	15100 (15320)	11756 (7456)	0.702
Number of vehicles	0.59 (0.82)	0.81 (0.88)	0.020
Value of vehicles	486 (985)	943 (1354)	<0.001
Liquid assets	1448 (3728)	1538 (3733)	0.839
Durables value	1275 (1707)	1572 (1896)	0.140
Total unemployment insurance 1974	877 (1581)	1565 (1961)	<0.001
Total unemployment insurance 1973	120 (491)	216 (968)	0.186
Total welfare 1974	216 (599)	163 (396)	0.408

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Table A1 – *continued from previous page*

	Control N=100	Treatment N=335	Balancing stats
Total welfare 1973	771 (1356)	121 (532)	<0.001
Total other unearned income 1974	1.62 (0.49)	1.51 (0.51)	0.365
Total other unearned income 73	895 (1182)	970 (1647)	0.612
Tot non-head earnings 1974	611 (1083)	587 (958)	0.843
Tot non-head earnings 1973	270 (827)	363 (1295)	0.444
Tot family income 74	162 (632)	245 (924)	0.354
Number of jobs, last week (male)	6760 (3855)	8223 (3808)	0.001
Labor market participation (male):			0.592
Yes	42 (82.4%)	188 (83.9%)	
No	8 (15.7%)	26 (11.6%)	
n/a	1 (1.96%)	10 (4.46%)	
Hours paid last week x10 (male)	311 (144)	384 (117)	0.001
Wage Rate x100 (male)	342 (152)	371 (130)	0.214
Gross Earnings (male)	132 (64.8)	158 (64.4)	0.011
Flexible hours (male):			0.573
Yes	5 (11.6%)	18 (9.09%)	
No	38 (88.4%)	180 (90.9%)	
Job satisfaction (male):			0.310
n/a	7 (13.7%)	14 (6.28%)	
Very satisfied	13 (25.5%)	65 (29.1%)	
Somewhat satisfied	20 (39.2%)	95 (42.6%)	
Neutral	5 (9.80%)	18 (8.07%)	

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Table A1 – *continued from previous page*

	Control N=100	Treatment N=335	Balancing stats
Somewhat dissatisfied	2 (3.92%)	21 (9.42%)	
Very dissatisfied	4 (7.84%)	10 (4.48%)	
Wage rate unit (male)	4.16 (1.65)	3.85 (1.51)	0.254
Expected childcare cost (male)	0.00 (0.00)	0.33 (3.19)	0.127
Number of jobs held in 1974 (male)	1.18 (0.77)	1.18 (0.62)	0.955
Ever unemployed in 1974? (male):			0.214
Yes	9 (17.6%)	23 (10.3%)	
No	42 (82.4%)	201 (89.7%)	
Total earnings 1974 (male)	5822 (3264)	6052 (3141)	0.658
Tips, boni, commissions (male)	53.9 (377)	62.3 (375)	0.887
Tot earnings 1973 (male)	5355 (3192)	5360 (2963)	0.993
Number of weeks worked 1974 (male)	35.9 (21.9)	38.0 (20.3)	0.540
Number of weeks worked 1973 (male)	37.3 (20.2)	39.2 (17.2)	0.543
Average weekly hours (male)	358 (140)	349 (129)	0.699
Ill or disabled? (male):			0.064
Yes	4 (7.84%)	5 (2.23%)	
No	47 (92.2%)	219 (97.8%)	
Years worked full time (male)	12.1 (11.4)	10.8 (9.68)	0.462
Finished high school (male):			0.121
No answer	5 (9.80%)	7 (3.12%)	
Yes	17 (33.3%)	86 (38.4%)	
No	29 (56.9%)	131 (58.5%)	

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Table A1 – *continued from previous page*

	Control N=100	Treatment N=335	Balancing stats
Still in school? (male):			0.010
No answer	5 (9.80%)	7 (3.12%)	
Yes	1 (1.96%)	28 (12.5%)	
No	45 (88.2%)	189 (84.4%)	
Father's years of schooling (male)	6.53 (4.30)	7.42 (4.35)	0.252
Mother's years of schooling (female)	6.76 (4.14)	7.50 (4.00)	0.323
Number of jobs held last week (female)	0.32 (0.49)	0.47 (0.53)	0.008
Labor participation (female):			0.021
Yes	34 (34.0%)	160 (47.8%)	
No	66 (66.0%)	175 (52.2%)	
Hours paid last week (female)	303 (158)	339 (125)	0.045
Wage rate x100 (female)	209 (111)	230 (95.9)	0.111
Gross earnings (female)	78.1 (43.2)	87.0 (39.5)	0.077
Flexible hours (female):			0.856
No answer	0 (0.00%)	1 (0.65%)	
Yes	9 (29.0%)	41 (26.8%)	
No	22 (71.0%)	111 (72.5%)	
Job satisfaction (female):			0.098
n/a	61 (61.0%)	160 (47.8%)	
Very satisfied	15 (15.0%)	58 (17.3%)	
Somewhat satisfied	18 (18.0%)	72 (21.5%)	
Neutral	0 (0.00%)	15 (4.48%)	

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Table A1 – *continued from previous page*

	Control N=100	Treatment N=335	Balancing stats
Somewhat dissatisfied	5 (5.00%)	21 (6.27%)	
Very dissatisfied	1 (1.00%)	9 (2.69%)	
Wage rate unit (female)	3.71 (1.40)	3.82 (1.61)	0.688
Expected childcare cost (female)	11.9 (18.5)	13.7 (19.9)	0.411
Number of jobs 1974 (female)	0.52 (0.73)	0.67 (0.76)	0.085
Ever unemployed in 1974? (female):			0.002
Yes	21 (21.0%)	30 (8.96%)	
No	79 (79.0%)	305 (91.0%)	
Total earnings 1974 (female)	1258 (2187)	1581 (2159)	0.197
Tips, boni, commissions (female)	12.2 (65.3)	12.8 (92.9)	0.943
Total earnings 1973 (female)	1007 (1498)	1223 (1641)	0.221
Number of weeks worked 1974 (female)	17.3 (23.3)	21.7 (23.6)	0.097
Number of weeks worked 1973 (female)	13.5 (18.7)	16.9 (19.7)	0.118
Average weekly hours (female)	272 (143)	262 (148)	0.687
Ill or disabled? (female):			0.003
Yes	11 (11.0%)	10 (2.99%)	
No	89 (89.0%)	325 (97.0%)	
Years worked full-time (female)	3.33 (4.95)	4.04 (5.94)	0.235
Finished high school (female):			0.009
No answer	11 (11.0%)	10 (2.99%)	
Yes	37 (37.0%)	139 (41.5%)	
No	52 (52.0%)	186 (55.5%)	

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Table A1 – *continued from previous page*

	Control N=100	Treatment N=335	Balancing stats
Years of schooling	9.19 (4.12)	10.2 (3.13)	0.024
In school?:			0.008
No answer	11 (11.0%)	10 (2.99%)	
Yes	10 (10.0%)	36 (10.7%)	
No	79 (79.0%)	289 (86.3%)	
Father's year of schooling (female)	6.39 (4.25)	7.83 (4.24)	0.013
Mother's year of schooling	6.66 (4.57)	7.88 (3.73)	0.038
If birth happened within 9 months	0.04 (0.20)	0.05 (0.22)	0.642

A.2 Patterns of non-participation to the experiment

In the Technical Report No 6, which deals with sample development over time, participation, and attrition, trends of non-participation are analyzed. That analysis deals with patterns of non-completion of and refusal to participate in the baseline interview, which was the first detailed and lengthy interview after the initial short screening interview. Overall, non-completion and refusal rates are lower for single individuals than for double-headed households. On the other hand, moving rates are higher for the former group. A factor of non-response related to moving is the experience with other welfare programs. Households that have more experience with welfare have higher response rates. These households are mainly those with a single householder (54.5%), followed by single individuals (24.4%), households with double householders and one earner (18.5%), and finally households with multiple earners (11.7%).

The refusal rate increases slightly with the family size. The authors of the report attribute this pattern to the number of adults rather than to the number of children present in the household, as the length of the interview increases with the number of adults present in the household. Indeed, the tables in the appendix of the report show that the completion rate decreases with more adults. The completion rate for households comprising only one adult is 60%, as compared to 45.8 for those with another adult, 54.3% for those with two others, and 51.9% for those with three or more.

The age of the householder(s) also seems to play a role in probability of continued participation. Generally, we observe that younger households with multiple earners have a higher completion rate than their older counterparts (65.9% for those under 25, as compared to 62% for householders aged between 25 and 34, and around 50% for older age groups). For households with two householders but a single earner, we observe a similar trend, albeit with less dramatic differences across age groups (55.2% for those under 25, as compared to 57.9% for householders aged between 25 and 34, and around 50% for older age groups). For single householders, the opposite trend is true, with the completion rates increasing with age for both genders.

After the baseline interview, the households had to participate first in the enrollment interview, and then to the payment package once they found out about their assignment cells. Unsurprisingly, the enrollment in the latter is dependent on the generosity of the treatment plan. Participation is higher in households assigned to for plans 1, 2, 4, 7 and 8 (94.3%, 93.1%, 91.6%, 91.4% and 91.6% respectively) as compared to less generous plans 3, 5 and 6 (83.5%, 86.4%, and 84.7%, respectively).

In order to complement this existing analysis of non-participation, we analyze the household characteristics of households that took part in the baseline interview and participated in the payments for at least two years versus those who dropped out of the experiment during the first two years. We do so by matching the households in the baseline file with the households in the baseline payments dataset, which we use for the main analysis and that includes only households that filed the reports and agreed to be interviewed for at least two years. We compare the matched households with those that did not match.

In the table below, we look at the differences in observed characteristics between the non-participants and the participants to see if any observed characteristic could shape the decision of the household to enroll and continue to participate in the program or drop out. The p-value in the last column tells the probability of rejecting the null hypothesis that the mean of the given variable is different among two groups. A small p-value indicates that the two groups differ significantly for that variable.

Table A2: Summary descriptives table by groups of 'Remained in the experiment'

	0 N=918	1 N=519	Balancing stats
Age of the female householder	32.2 (11.3)	31.0 (10.5)	0.049
Age of the male householder	33.4 (11.3)	32.7 (9.92)	0.298
Number of adults in the household:			<0.001
0	697 (75.9%)	443 (85.4%)	
1	114 (12.4%)	43 (8.29%)	
2	71 (7.73%)	26 (5.01%)	
3	27 (2.94%)	6 (1.16%)	
4	8 (0.87%)	0 (0.00%)	
5	1 (0.11%)	1 (0.19%)	
Number of children in the household:			0.001
0	586 (63.8%)	283 (54.5%)	
1	217 (23.6%)	144 (27.7%)	
2	100 (10.9%)	70 (13.5%)	
3	12 (1.31%)	20 (3.85%)	
4	2 (0.22%)	2 (0.39%)	
5	1 (0.11%)	0 (0.00%)	
Household composition:			
Two householders present	504 (54.9%)	290 (55.9%)	0.763
Single householder	148 (16.1%)	90 (17.3%)	0.601
Single individual	266 (29.0%)	139 (26.8%)	0.408
Total family income in 1974	7905 (3938)	7823 (4207)	0.726

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Table A2 – *continued from previous page*

	0 N=918	1 N=519	Balancing stats
Years of schooling (male householder)	8.99 (4.80)	10.4 (4.25)	<0.001
Years of schooling (female householder)	9.73 (3.20)	9.84 (3.51)	0.585
Completed high school (male householder):			<0.001
Did not answer	88 (13.7%)	18 (5.29%)	
No	72 (11.2%)	47 (13.8%)	
Yes	483 (75.1%)	275 (80.9%)	
Completed high school (female householder):			0.032
Did not answer	30 (3.87%)	27 (5.77%)	
No	60 (7.74%)	52 (11.1%)	
Yes	685 (88.4%)	389 (83.1%)	
Labor participation in the past year (male householder):			<0.001
Did not work	163 (25.3%)	47 (13.8%)	
Worked	480 (74.7%)	293 (86.2%)	
Labor participation in the past year (female householder):			0.379
Did not work	416 (53.7%)	264 (56.4%)	
Worked	359 (46.3%)	204 (43.6%)	
Total Earnings in 1974 (female householder)	1666 (2158)	1486 (2149)	0.157
Total Earnings in 1973 (female householder)	1295 (1689)	1164 (1595)	0.172
Total Earnings in 1974 (male householder)	5256 (3699)	5461 (3307)	0.390
Total Earnings in 1973 (male householder)	4272 (3439)	4759 (3128)	0.026
Expected or actual childcare cost (male householder)	0.59 (4.89)	0.21 (2.59)	0.116
Expected or actual childcare cost (female householder)	9.45 (16.4)	13.1 (19.2)	0.001

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Table A2 – *continued from previous page*

	0 N=918	1 N=519	Balancing stats
Job Satisfaction Index (male householder):			0.001
N/A	140 (21.8%)	41 (12.1%)	
Very satisfied	163 (25.4%)	88 (26.0%)	
Somewhat satisfied	238 (37.1%)	138 (40.7%)	
Neutral	53 (8.26%)	26 (7.67%)	
Somewhat dissatisfied	31 (4.83%)	28 (8.26%)	
Very dissatisfied	17 (2.65%)	18 (5.31%)	
Job Satisfaction Index (female householder):			0.073
N/A	354 (45.8%)	240 (51.3%)	
Very satisfied	132 (17.1%)	78 (16.7%)	
Somewhat satisfied	188 (24.3%)	95 (20.3%)	
Neutral	49 (6.34%)	16 (3.42%)	
Somewhat dissatisfied	33 (4.27%)	26 (5.56%)	
Very dissatisfied	17 (2.20%)	13 (2.78%)	
Total non-householder earned income 1973	458 (1488)	226 (864)	0.002
Total non-householder earned income 1974	621 (1823)	356 (1269)	0.006

We observe that the households that dropped out within the first two years and those that did not do not differ significantly in terms of age of the male or female householders. As in the previous analysis, an increase in the number of adults corresponds with lowering rates of continuation in the experiment. This pattern can again be explained by the length of interview, which increases with each additional adult in the household.

The number of children and the actual or expected cost of childcare as reported by the female householder differs significantly across the two groups. Households who remained in the experiment have more children on average. They also report higher actual or expected cost of childcare. This pattern suggests that the households who continued to file in the income reports and responded to interviews did so in order to receive the payments to support their existing children, rather than with the intention to have new children. Of course, families who already have children, and those that have more children than the average, might be more likely to want to have a new child through an unobserved characteristic. The contrary is also possible: the already born children might decrease the likelihood of the household having another child. With the assumption that this unobserved characteristic is correlated with the number of children a household has, we include the number of existing children in the analysis in order to control for this effect.

The households also differ in terms of earnings, education, and labor market participation. The households who remained in the experiment are on average more educated, with the effect stronger for the educational attainment of the male householder. Male householders participating in the experiment are more likely to be in the labor force than non-participants. Job satisfaction also plays an important role for both male and female householders. The male householders in the participating group more often report dissatisfaction with their last regular job. This is the case for female respondents as well, but the difference is much less pronounced as compared to male respondents. This pattern could suggest that a motivation to remain in the program is to leave the current job. Finally, while the total earnings by the householders do not differ much between the two groups, the reported non-householder earned income is almost double in the non-participant group.

A.3 Results of the baseline model

Table A3: Average marginal effects of treatment plans

	(1)	(2)	(3)	(4)	(5)	(6)
Treated	0.07**		0.11**		0.10**	
	(0.03)		(0.04)		(0.05)	
Plan 1		0.09		0.16		0.16
		(0.09)		(0.13)		(0.13)
Plan 2		0.04		0.11		0.09
		(0.07)		(0.11)		(0.11)
Plan 3		0.17*		0.22*		0.21
		(0.10)		(0.13)		(0.13)
Plan 4		0.07		0.15		0.14
		(0.08)		(0.11)		(0.11)
Plan 5		0.09		0.11		0.10
		(0.09)		(0.12)		(0.12)
Plan 7		0.14		0.20*		0.20*
		(0.08)		(0.11)		(0.11)
Plan 8		0.12		0.16		0.16
		(0.09)		(0.12)		(0.12)
Observations.	435	435	275	275	275	275
Controls:						
For female householder	Yes	Yes	Yes	Yes	Yes	Yes
For male householder	No	No	Yes	Yes	No	No
Household characteristics	Yes	Yes	Yes	Yes	Yes	Yes
Log Likelihood	−120.18	−118.96	−87.86	−87.15	−88.10	−87.37
Deviance	240.36	237.93	175.71	174.31	176.20	174.73
AIC	378.36	387.93	295.71	306.31	294.20	304.73
BIC	659.40	693.40	512.72	545.02	507.59	539.82

$^{***}p < 0.01$; $^{**}p < 0.05$; $^{*}p < 0.1$.Controls for household characteristics include income bracket, family size, number of children in and out of the household, number and value of vehicles a household has, and whether the household owns a house and if the female householder stays at home. Controls for householders are age, years of schooling and a dummy indicating disability, retirement and/or illness.

A.4 Estimates when treatment is interacted with stratifying variables

Table A4: Estimates of the average treatment effects, in terms of odds ratios

	Dependent variable:
	Birth probability
Treated	1.414
	(1.355)
Treated×Total Fam Income in 74	0.00002
	(0.0001)
Treated×Family size	−0.170
	(0.319)
Observations	434
Controls:	
For female householder	Yes
For male householder	No
Household characteristics	Yes
Log Likelihood	−134.426
Akaike Inf. Crit.	326.852
Note:	*p<0.1; **p<0.05; ***p<0.01

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Conflict of interest

There are no conflicts of interest to be disclosed.