# 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 19 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 the 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 (Mitchell and Gray, 2007; Aarssen, 2005). 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, by assessing 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 the sense that they were collected in the first randomized controlled trials (RCTs) conducted in industrialized countries for policy purposes together with those in the United States at the same time, and that since then, no such RCT has taken place in an industrialized country. To the best of our knowledge, the fertility effects of this experiment have never been analyzed before. Our analysis looks at not only whether a monthly paid 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. The highest support level in the experiment corresponded to approximately half of the median income in Canada at the time. Not all households receiving income were able to pass the threshold of low-income levels through the payments made.

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 meant 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, 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).[1]

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. A strand of literature, following Galor and Moav (2002) has explained this hump-shaped relationship through the so-called subsistence consumption constraint (Micevska and Zak, 2002; Vogl, 2016). If the households can't secure the necessary 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, the number of children the households have will be lower than optimal, and thus increase with income. Households with income high enough for the subsistence consump-

[1] As childcare is predominantly performed by women, it is especially the female labor supply and the use of the woman's time that weighs in on the cost of a child and is thus more critical to fertility decisions (Willis, 1973; Jones et al., 2008). This difference between the influence of female and male wage is reflected by Merrigan and Pierre's (1998) findings, examining Canadian fertility dynamics from 1950 to 1990. While an increase in the wages of Canadian women fostered declining fertility, the male wage effects had a significantly positive, nevertheless small, effect on family dynamics.

tion constraint to be non-binding can allocate the optimal level of resources to the quantity and quality of their children. Together with heterogeneous preferences for child quality, which are 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.

Not only the absolute levels of income, but also the risk of not being able to meet subsistence consumption, negatively impacts the possibility of having a child, since having children limits the household's ability to secure against potential wage shocks (Micevska and Zak, 2002). 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 a certain level of income and providing 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 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).

A number of studies have also specifically looked at the effect of child subsidy programs to assess the relationship between fertility, income, and income security (Cohen et al., 2013; Riphahn and Wiynck, 2017; Blundell et al., 2018). Specifically in Canada, Hyatt and Milne (1991) find that a personal exemption, a family allowance, and a child tax credit significantly and positively affect fertility rates, whether being considered separately or jointly. Also looking at the effects of three tax policies (child tax credit, family allowances, maternity leave benefits) of fertility from 1921 to 1988, Zhang, Quan, and van Meerbergen (1994) find that all three transfer programs significantly alleviate declining fertility. Correspondingly, McNown (2004) finds a positive elasticity of the fertility rate to the child benefit policies. Milligan (2005) shows that a transfer policy in Quebec, Canada that gave all families having a child C$8,000 led to a strong positive effect on fertility.

Unlike government policies aimed explicitly at increasing fertility rates at times where fertility is below the optimal level (the analysis of which would be plagued by endogeneity concerns), MINCOME was not designed with the purpose of increasing fertility. In the same spirit, 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. Keeley (1980), employing data collected from the Seattle and Denver income maintenance experiments (SIME/DIME) in the 1970s, which are similar to MINCOME experiment, finds that while there seemed to be negative effects on fertility for white women who received the treatment for five years, fertility increased for Chicanas and stayed the same for Black women with the same treatment.

The results of MINCOME 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 ranges between 7 and 19 percentage points, depending on the specification. The estimates for some treatment plans are higher, but 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 present 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. We then 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. 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 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 $\tau$, 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 $\gamma$ 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 $\gamma$ 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 so 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 $\gamma/\tau$ 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 as in Galor and Moav (2002); Vogl (2016), 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 in having more children. 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 themselves in the second and third cases and all households will be able to choose the optimal solution, and allocate $\gamma$ 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, on 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, but who are still eligible for payments. We should observe no effect on households whose income certainly or with very high probability does not 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.

# 3 The MINCOME Experiment

## 3.1 Background of the Experiment

Towards the end of the 60s and in the 70s, there was heightened interest in assessing the impact of a guaranteed annual income (GAI) or a negative income tax (NIT)[2] in the United States and in Canada, motivated by high levels of poverty. In 1964, President Lyndon Johnson called for a "war against poverty" in the United States. 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 had 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.

These policy discussions and concerns about work behavior motivated experimental research in both countries. 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. In Canada, a negative income tax program was launched in 1974 in Manitoba to be continued for three years with the main purpose of estimating the labor supply effects of this program. These experiments were all conducted with the explicit aim of informing policy discussions on transfer schemes (Hum and Simpson, 1991).

[2]We use GAI and NIT interchangeably, since the experiment is composed of a guaranteed income that decreases with income.

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.

## 3.2 Program description

The design of the treatment groups 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. 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 worth. For instance, if a household was in a treatment group with a 50% tax rate, every dollar the household made would reduce the payment by 50 cents. The final payment of the household $i$ in treatment group $k$ was given by $P_{ik} = G_k - (t_k * Y_i) - (r * W_i)$ where $G_k$ is the support level, $t_k$ the treatment group-specific offset tax rate, $Y_i$ monthly earnings of the household, $W_i$ 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 had 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 the following table:

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 main purpose of the MINCOME experiment was to provide evidence on labor supply responses of such a program, in order to inform the ideal design and implementation of a future negative income tax policy. The treatment with the highest income level and lowest tax rate would be more generous than any negative income tax scheme that the Canadian government would realistically implement in terms of financial costs. This treatment plan was thus considered to be irrelevant for the main purpose of the experiment and was eliminated from the start. The least generous plan, Plan 6, was merged with Plan 7 after a while due the high drop out rate (Mason, 2016).

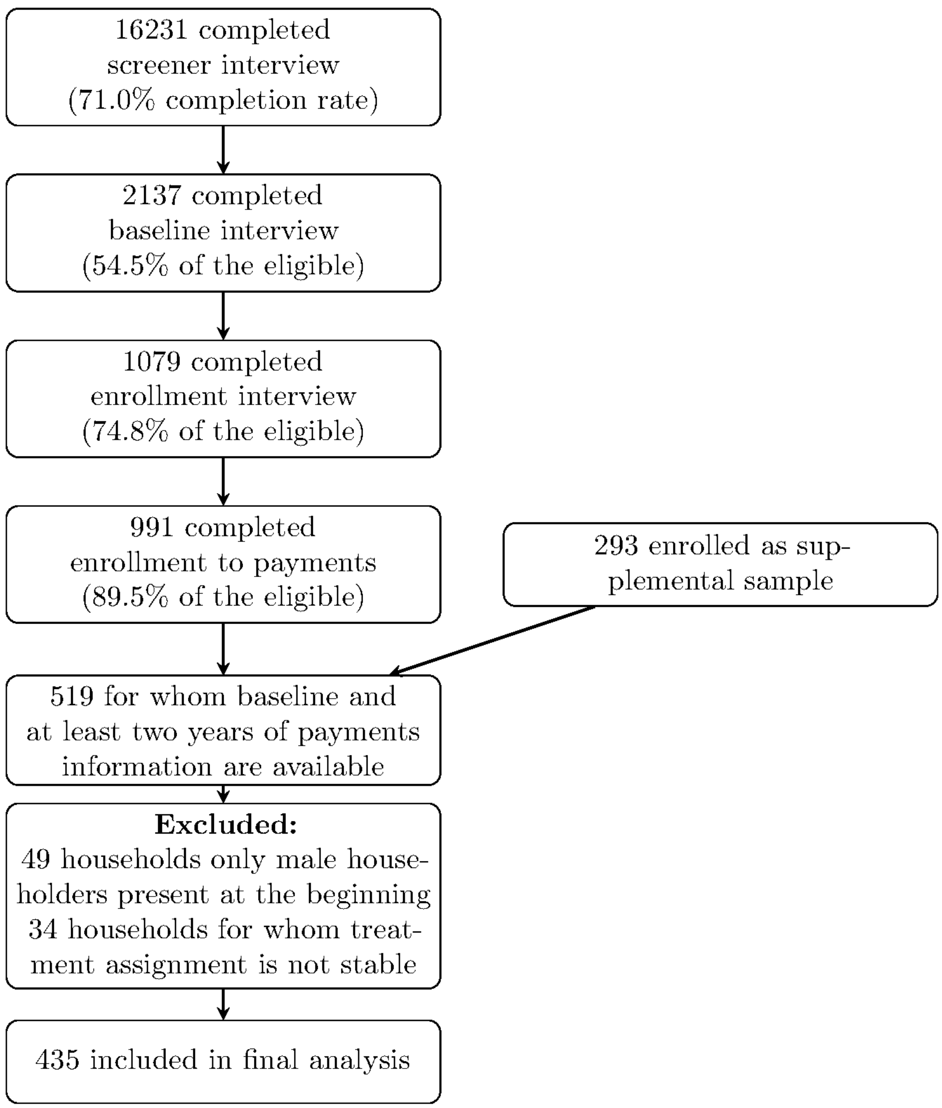
The most generous annual support level corresponds to approximately $28,000 Canadian dollars (or 21,000 USD), the second one to $24,000 Canadian dollars (18,000 USD), and the least generous level to $19,500 (15,000 USD) if we adjust for inflation. Median income in Canada in 1972 was $3,311 Canadian dollars, and $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 beyond 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 previous section, these households would most likely fall under the second and third categories, for whom the subsistence consumption constraint is binding.

### 3.3 Sample development

A few words should be said on the sample selection. 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 MINCOME, the first step of sampling was not random, but followed an assignment model, which we explain below. Furthermore, the experiment targeted low-income families only. However, the assignment to different treatment or to the control group was completely randomized.

The sample selection for MINCOME consisted of several steps. The experiment was launched on three sites: the urban dispersed site in the provincial capital city Winnipeg; a rural dispersed sample composed of households in 18 rural communities in Manitoba; and a saturation site in Dauphin. Due to the noise in the data in the rural sites and Dauphin, we focus our analysis solely on Winnipeg. The households were randomly chosen, and then screened for eligibility depending on their income. The families 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.

Already 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 would be enrolled in the program. The households in this sample comprised re-contacted households who had not initially completed the baseline survey; households that were incorrectly excluded from the baseline interview due to a mistake; households who were initially not contacted for the baseline interview; and finally a subsample of households that received welfare in the past three years, but who were no longer receiving welfare payments (Sabourin, 2016). A supplementary sample consisting of 293 households was thus enrolled in the program.



In Winnipeg, the allocation to different treatment groups was realized using the Conlisk-Watts model. This model was first developed for the New Jersey experiment, and was followed by subsequent NIT experiments (Hum and Simpson, 1991; Keeley, 1980). In the context of a policy experiment with a limited budget, several trade-offs were considered. As the payment households would receive depended on household earnings and the number of people in the household, the cost of assigning a household to a specific treatment group depended on the family size and income. This payment system meant that some units were more expensive than the others. Minimizing this cost per unit leads to a larger sample size by giving the possibility of making payments to more households.

The trade-off between randomization and cost and policy considerations was tackled with the Conlisk-Watts assignment model, which optimizes the experimental design given certain constraints. The 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 implies that the assignment to treatment cells is not orthogonal to household characteristics that also affect the outcome variable. It is therefore important to employ a multivariate analysis where the stratifying variables – family structure and income group of the household – are included in the regression (Keeley and Robins, 1980). Importantly, once a household is 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 we can assume that refusing to participate and dropping out of the program is random, there is no problem other than the sample size. If, however, 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. Especially if the reasons of self-selection also positively affect the outcome variable, this would lead to the overestimation of the effect of the treatment.

The participation in an experiment such as MINCOME is associated with certain costs, not least in terms of time. 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. 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. This would create the problem of self-selection into the sample and lead to an overestimation of the effect of the program on fertility. It is plausible to think that families for whom the benefits are less important will have less incentive to go through the interview and filing processes.

In the Appendix, we provide detailed information on 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 less likely to drop out of the program, and that households with double earners were the most likely to drop out. Greater numbers of adults in the household decrease the probability of continuation in the program. We conduct a complementary analysis by comparing households that initially completed all steps but who dropped out within the first two years to those who continued to participate. Our findings suggest that households who remained in the experiment have on average more children than the non-participant group, as well as higher actual or expected childcare expenses. This pattern might suggest that the households that self-selected themselves into the experiment did so to financially support their already born children. On the other hand, there is a possibility that these households were also more likely to want more children. With the assumption that this unobserved characteristic is correlated with the number of children, we include the number of children a household already has in the analysis in order to control for this effect.

# 4 Data and Descriptive Statistics

The final sample we use for analysis includes 435 households in Winnipeg who participated in the experiment for at least two years, and where a female householder has been present since the program began. We have information on the treatment plan to which the household was assigned, their income bracket, the family composition, the ages and educational attainment of the householders and the ages of the children living in the household.

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. Every month, households reported the number of children present in the household, which allows us to see if there has been an increase. The fact that we only observe the numbers, and not the new child's relationship to the householder, might raise a concern of whether or not the child in question is indeed a new born child of the householders. Fortunately, for the Winnipeg site, there is a separate data set for family composition. In this data set, 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 s/he is the brother, sister, 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 birth date data is 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.

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. 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 6.

An important and statistically significant difference between the households in the treatment and control group is the household composition. 67.8% of the house-

Table 2: 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 |

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

# 5 Identification Strategy and the Results

## 5.1 Baseline Estimation

Our main specification is a static logit 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.

$$\Pr(\text{Child birth}_i = 1) \quad = \quad \frac{\exp(\beta_0 + \beta_1 \text{Treated}_i + \beta_2 \text{Controls}_i)}{1 + \exp(\beta_0 + \beta_1 \text{Treated}_i + \beta_2 \text{Controls}_i)} \tag{7}$$

We run the same regression by replacing the dummy "treated" with a 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. 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 in 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 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. 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, fourth, fifth and sixth models as these information are missing for some households. 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. 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 of our sample.

In this first specification, the overall treatment dummy and the dummies for treatment plans 3 and 7 have coefficient estimates that are positive and are statistically significant. Being in the treatment group has a marginal effect of 7% on the probability of having a child, and being assigned to treatment groups 3 and 7, 14% and 13% respectively. The estimates for other treatment groups are all positive, although not significant at 90%.

In the second specification, we add control variables for the male householder, including his age, whether he is ill or has disabilities, whether he is in school, and his educational attainment. Our sample size is reduced to 252 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 1, 3, and 7 are statistically significant and positive in this specification. Furthermore, the magnitude of the marginal effects are larger; for the treated group the marginal effect is 13%, and they are 25%, 22%, and 29% for treatment groups 1, 3, and 7, respectively. 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.

Finally, we also add information on the female householder's mother's educational attainment in the final specification. The sample size in this case is reduced to 193. Nevertheless, we believe that this is an important variable to add as the control and treatment groups differ significantly in terms of mother's educational attainment, and because it is a good indicator for the socio-economic background of the woman. Furthermore, it is possible that the households in this subsample were those who participated from the first interview, and that they have all the information for that reason. They would be exposed to treatment for longer periods of time, or at least know for a longer time that they were going to receive payments. In this final specification, the coefficient estimates of being treated and being assigned to treatment plans 1, 3, and 7 remain positive and statistically significant, and their magnitudes increase considerably. The estimate for treatment plans 4, 5, and 8 are likewise positive and statistically significant.

We report the odds ratios in the Table 3 below, and the average marginal effects of the treatment dummies in Section 7.3 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 3 and 7 are always positive and statistically significant. The estimate for plan 1 is positive and significant when the sample is restricted to households with a male and a female householder. Coefficient estimates for other treatment plans such as 4 and 8 also appear to be significant depending on the variables used, but they are not robust to all specifications. One possible reason for the sensitivity of treatment plan dummies to different specifications and to the inclusion of different control variables, could be the small size of each treatment group. The comparison between the control and treatment group is likely to be more informative on 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, indicate that the guarantee of a certain level of income is more important in the decision of having a child, rather than the absolute level of increase in income.

Table 3: Odds ratios of treatment plans

| | *Dependent variable:* | | | | | |
|---|---|---|---|---|---|---|
| | Dummy of childbirth | | | | | |
| | (1) | (2) | (3) | (4) | (5) | (6) |
| Treated | 2.490* | | 4.770* | | 21.511** | |
| | (0.526) | | (0.806) | | (1.303) | |
| Plan 1 | | 2.308 | | 5.666* | | 24.329** |
| | | (0.746) | | (1.041) | | (1.480) |
| Plan 2 | | 1.561 | | 2.495 | | 11.162 |
| | | (0.705) | | (1.041) | | (1.485) |
| Plan 3 | | 4.406* | | 8.027* | | 72.066** |
| | | (0.765) | | (1.078) | | (1.676) |
| Plan 4 | | 2.078 | | 4.805 | | 26.386** |
| | | (0.725) | | (0.961) | | (1.451) |
| Plan 5 | | 2.510 | | 2.634 | | 6.138 |
| | | (0.755) | | (1.098) | | (1.725) |
| Plan 7 | | 3.503* | | 10.065** | | 36.435** |
| | | (0.709) | | (1.004) | | (1.444) |
| Plan 8 | | 3.088 | | 3.351 | | 22.167* |
| | | (0.728) | | (1.066) | | (1.597) |
| Observations | 435 | 435 | 252 | 252 | 192 | 192 |
| *Female householder controls* | Yes | Yes | Yes | Yes | Yes | Yes |
| *Male householder controls* | No | No | Yes | Yes | Yes | Yes |
| *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 | −71.545 | −69.860 | −55.959 | −54.043 |
| Akaike Inf. Crit. | 365.666 | 375.404 | 267.091 | 275.720 | 231.918 | 240.086 |

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

## 5.2 Extension: Event History Analysis

As an extension to our baseline estimation, we conduct a dynamic analysis in an event history framework. In the 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 question whether and when people experience the 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 outcome 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 allows 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 duration to 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 suffers 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 has finished.

Particularly suitable for fertility analyzes 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 each year where a woman is fertile for each 435 women included in the baseline analysis. If the person 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 has the value 1 for that year. The variable married takes the value 1 for each year a woman is married.

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

$$\Pr(\text{Child birth}_{it} = 1) = \frac{\exp(\beta_0 + \beta_1 \text{Treated}_i{*}\text{Experiment}_t + \beta_2 \text{Married}_{it} + \beta_3 \text{Age}_{it} + \text{j}_{it} + \text{u}_i + \text{Year}_t)}{1 + \exp(\beta_0 + \beta_1 \text{Treated}_i{*}\text{Experiment}_t + \beta_2 \text{Married}_{it} + \beta_3 \text{Age}_{it} + \text{j}_{it} + \text{u}_i + \text{Year}_t)} \tag{8}$$

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. $\text{j}_{it}$ 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 period of observation. 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 affecting 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 problem 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 have been 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 have been divorced before and are now single, as the full marital and child birth histories for these women cannot be constructed.

The regression results can be found in Table 4 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 second and third regressions above. The second sample included the 252 households for whom we have information on the male householder and the third sample includes the 193 households for whom we have information on the educational attainment of the female householder's mother. The baseline analysis with these two subsamples had more pronounced results, and the effect of most plans were estimated in a statistically significant way. In the event history analysis as well, the treatment dummy's coefficient estimate are higher, and in the last specification, it is statistically significant at the 95% level. The coefficient estimates for most treatment plans are likewise positive and statistically significant in the event history regressions with these two subsamples. Overall, our results show positive effects that range from 4 to 16 percentage points depending on the sample and the treatment plan.

Table 4: Results from event history analysis, average marginal effects

| | Dependent variable: | | | | | |
|---|---|---|---|---|---|---|
| | Dummy for childbirth | | | | | |
| | (1) | (2) | (3) | (4) | (5) | (6) |
| Treated*Experiment | 0.044 | | 0.061 | | 0.100** | |
| | (0.030) | | (0.039) | | (0.045) | |
| Plan 1*Experiment | | 0.050 | | 0.053 | | |
| | | (0.045) | | (0.058) | | (0.065) |
| Plan 2*Experiment | | 0.021 | | −0.002 | | 0.016 |
| | | (0.042) | | (0.054) | | (0.064) |
| Plan 3*Experiment | | 0.066 | | 0.088 | | 0.115* |
| | | (0.047) | | (0.058) | | (0.065) |
| Plan 4*Experiment | | −0.040 | | −0.011 | | 0.019 |
| | | (0.042) | | (0.051) | | (0.058) |
| Plan 5*Experiment | | 0.079* | | 0.085 | | 0.136* |
| | | (0.046) | | (0.057) | | (0.070) |
| Plan 7*Experiment | | 0.078* | | 0.110** | | 0.154*** |
| | | (0.042) | | (0.053) | | (0.060) |
| Plan 8*Experiment | | 0.079* | | 0.125** | | 0.167*** |
| | | (0.046) | | (0.058) | | (0.064) |
| Observations | 4,026 | 4,026 | 3,169 | 3,169 | 2,489 | 2,489 |
| Log Likelihood | −922.347 | −947.581 | −955.661 | −978.890 | −782.363 | −804.827 |
| Akaike Inf. Crit. | 1,972.693 | 2,047.163 | 2,039.321 | 2,109.781 | 1,692.727 | 1,761.654 |
| Bayesian Inf. Crit. | 2,375.927 | 2,526.003 | 2,427.236 | 2,570.430 | 2,065.183 | 2,203.947 |
| *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, but also 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 6%, 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 the unique opportunity to test the effect of an exogenous source of guaranteed income security on low-income household fertility decisions. 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 7 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.

However, 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, as 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 – as the MINCOME experiment compensated for decreases in family earnings up to the threshold of the respective plan – 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, there is reason to believe that the estimates provide a lower bound on the impact of a guaranteed annual income on fertility, as MINCOME provided low-income households with an income guarantee for just three years. Our results suggest that this guarantee has increased the probability that families in treatment groups will have children. It is plausible that a guaranteed annual income or a basic income scheme that has no time limit would provide an even larger extent of income security, thus having a more pronounced effect on fertility.

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# 7 Appendix

## 7.1 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. This analysis concerns itself 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 this household characteristic is the experience with other welfare programs. Households that happen to 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 is reported to increase 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, tables in the appendix of the report show that the completion rate decreases with more adults. If there are no other adult is present in the household than the householder, the completion rate is 60%, as compared to 45.8 if there is another adult, 54.3% if there are two others and 51.9% if there are 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 double householders but a single earner, we can observe a similar trend, albeit with less difference 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. The enrollment in the latter is unsurprisingly dependent on the generosity of the treatment plan. It is higher 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 analysis of non-participation, we analyze the household characteristics of those households versus that took part in the baseline interview and participated in the payments for at least two years, and 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 which 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 5: Summary descriptives table by groups of 'Remained in the experiment'

| | 0<br>N=918 | 1<br>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 5 – *continued from previous page*

| | 0<br>N=918 | 1<br>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 5 – *continued from previous page*

| | 0<br>N=918 | 1<br>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, as the number of the adults increase in a household, the rates of continuation in the experiment drop. 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 for childcare as reported by the female householder differ 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 children a household already has 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. The participant male householders participate in the labor market more often than the 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 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.

## 7.2 Descriptive statistics

Table 6: 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 6 – *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 6 – *continued from previous page*

| | Control<br>N=100 | Treatment<br>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 6 – *continued from previous page*

| | Control<br>N=100 | Treatment<br>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 6 – *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 6 – *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 6 – *continued from previous page*

| | Control<br>N=100 | Treatment<br>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 |

## 7.3 Results of the baseline model

Table 7: Average marginal effects of treatment plans

| | (1) | (2) | (3) | (4) | (5) | (6) |
|---|---|---|---|---|---|---|
| Treated | $0.07^{*}$ | | $0.13^{**}$ | | $0.19^{***}$ | |
| | (0.03) | | (0.04) | | (0.04) | |
| Plan 1 | | 0.08 | | 0.25 | | $0.43^{***}$ |
| | | (0.08) | | (0.14) | | (0.12) |
| Plan 2 | | 0.04 | | 0.10 | | 0.26 |
| | | (0.07) | | (0.13) | | (0.16) |
| Plan 3 | | 0.16 | | 0.22 | | $0.40^{**}$ |
| | | (0.09) | | (0.14) | | (0.13) |
| Plan 4 | | 0.07 | | 0.16 | | $0.33^{**}$ |
| | | (0.08) | | (0.12) | | (0.11) |
| Plan 5 | | 0.09 | | 0.20 | | $0.35^{*}$ |
| | | (0.09) | | (0.14) | | (0.16) |
| Plan 7 | | 0.13 | | $0.29^{*}$ | | $0.37^{**}$ |
| | | (0.08) | | (0.12) | | (0.12) |
| Plan 8 | | 0.11 | | 0.20 | | $0.32^{*}$ |
| | | (0.08) | | (0.13) | | (0.15) |
| Observations. | 435 | 435 | 252 | 252 | 192 | 192 |
| Controls: | | | | | | |
| *For female householder* | Yes | Yes | Yes | Yes | Yes | Yes |
| *For male householder* | No | No | Yes | Yes | Yes | Yes |
| *Household characteristics* | Yes | Yes | Yes | Yes | Yes | Yes |
| *Fem. householder's mother's education level* | No | No | No | No | Yes | Yes |
| Log Likelihood | −120.83 | −119.70 | −76.33 | −74.88 | −61.06 | −59.87 |
| Deviance | 241.67 | 239.40 | 152.65 | 149.77 | 122.12 | 119.73 |
| AIC | 365.67 | 375.40 | 254.65 | 263.77 | 224.12 | 233.73 |
| BIC | 618.34 | 652.53 | 434.65 | 464.95 | 390.25 | 419.41 |

$^{***}p < 0.001$, $^{**}p < 0.01$, $^{*}p < 0.05$. 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.

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## Conflict of interest

There are no conflicts of interest to be disclosed.