

Child support instruments and labor supply: evidence from a large scale child benefit program

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Abstract

We investigate immediate effects of a large scale child benefit program introduction on labor supply of the household members in Poland. Due to nonrandom eligibility and universal character of the program standard evaluation estimators are likely to be inconsistent. In order to address this issues we propose a novel approach which combines difference-in-difference (DID) propensity score based methods with covariate balancing propensity score (CBPS) by Imai and Ratkovic (2014). The DID part solves potential problems with non-parallel outcome dynamics in treated and non-treated subpopulations resulting from non-experimental character of the data, whereas CBPS is expected to reduce significantly bias from the systematic differences between treated and untreated subpopulations. We account also for potential heterogeneity among households by estimating a range of local average treatment effects which jointly provide a reliable view on the overall impact. We found that the program has a minor impact on the labor supply in periods following its introduction. There is an evidence for a small encouraging effect on hours worked by treated mothers of children at school age, both sole and married. Additionally, the program may influence the intra-household division of duties among parents of the youngest children as suggested by simultaneous slight decline in participating mothers' probability of working and a small increase in treated fathers' hours worked.

Keywords: Child benefits, labor supply, program evaluation, difference-in-difference estimation, covariate balancing propensity score

JEL classification: C21, C23, I38, J22

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1 INTRODUCTION

The effects of unconditional non-equivalent income support instruments on labor supply are at the core of labor economics. The interest in child-care programs and labor supply has been growing since the seminal paper of [Heckman \(1974\)](#). Autonomous income in a form of subsidy pushes the budget constraint upwards, expanding the feasible set of choices between consumption and leisure. Depending on the properties of the utility function, one can expect both a contraction in hours worked and unchanged labor supply with an increased consumption. Income support instruments are frequently under scrutiny for the possible effect of discouraging employment: a utility flow generated by the social transfer might exceed the utility from working even if the earned income was higher than the transfer, because work induces some nonzero disutility ([Besley and Coate, 1992](#); [Spencer, 2003](#)). The direction of the effect may also be ambiguous because of nonlinearities in a budget set which appear due to a relationship between wage rate and hours worked ([Burtless and Hausman, 1978](#)), a stigma effect ([Moffitt, 1983](#); [Hoynes et al., 1996](#)) or tied wage–hours contracts ([Averett and Hotchkiss, 1997](#)). Child benefit program is also supposed to limit barriers in women’s access to the labor market constituted by costs of care ([Kimmel, 1998](#)) and affect labor supply through this channel.

Given these theoretical premises, the actual effects of income support program on the labor supply remain an empirical question even for the monetary non-equivalent transfers. The biggest challenge lies in the causal identification in the data: individuals adjust labor supply and consumption for a variety of reasons. Assigning that certain income support instrument has been the cause and labor supply adjustment has been the effect is at the heart of much of modern microeconometrics and labor economics ([Angrist and Pischke, 2010](#)). Empirical investigations of the effects of universal transfers on labor supply are difficult due to lack of an appropriate counter-factual. If eligibility is based on the number of children, ineligible households (i.e. those who do not receive the transfer) are not a good control group – there may be other, unobservable differences between the eligible and ineligible households ([Graham and Beller, 1989](#)).

The empirical literature approaches the estimation of an impact of child benefits on labor supply in various ways. Some authors explicitly derive a stochastic model from economic theory and estimate it by Maximum Likelihood (ML) methods. Structural models allow the interpretation of estimates to follow closely the economic theory, e.g. through model nonlinearity or budget set non-convexity, but suffer from the high level of complexity. Usually researchers need to impose many simplifying assumptions in order to achieve empirical identification. The standard solution is to discretize the set of possible allocations in order to apply a variant of discrete choice models ([Blau and Robins, 1988](#); [Fraker and Moffitt, 1988](#); [Ribar, 1995](#); [Hoynes et al., 1996](#)). More recent studies concern complex dynamic life-cycle models ([Blundell et al., 2016](#)), account for both the distribution of preferences and restrictions on the choice of hours ([Befy et al., 2015](#)) and derive sophisticated methods of estimation. Another approach is to estimate reduced form equations in order to uncover directions of the effects of interest [Mroz \(1987\)](#); [Moffitt and Wolfe \(1992\)](#); [Ribar](#)

(1992). The results of the studies tend to suggest negative impact of the child benefit programs on labor supply (for a more detailed review see [Del Boca et al. \(2015\)](#)).

Due to the recent strong development, program evaluation methods are getting more and more attention in studies of the labor supply. [Blundell et al. \(2004\)](#) perform difference-in-difference propensity score matching investigating the effects of a mandatory job search program. Other papers analyze directly child benefit programs. [Luna \(2011\)](#) investigates the introduction of an unconditional child benefit in Spain on family well-being using regression discontinuity design. The study suggests that mothers eligible for the program stayed longer out of the labor market after giving birth. [Koebel and Schirle \(2016\)](#) in turn exploits difference-in-difference strategy to measure the effects of Canadian Universal Child Care Benefit. They find out that the child care benefit program has a negative effect on legally married woman and increases labor supply of single mothers.

In this paper we contribute to the program evaluation literature by proposing a novel approach to estimate the effects of child support instrument on household labor supply in a situation, when the instrument design invalidate other estimation methods. Our estimator combines an idea of covariate balancing propensity score (CBPS, [Imai and Ratkovic \(2014\)](#)) with difference-in-difference (DID) estimators by [Abadie \(2005\)](#) and [Heckman et al. \(1997\)](#). The DID estimators solve potential problems with non-parallel outcome dynamics in treated and non-treated subpopulations, which might result from nonexperimental character of the data. CBPS is expected to reduce significantly bias resulting from systematic differences between treated and untreated subpopulations.

The new estimators are applied to investigate the immediate effects of introduction of a large scale child benefit program in Poland on labor supply of the household members. The eligibility to the program is not random because it is driven by the number of children within a household. Additionally, the universal character of the program makes it difficult to define appropriate control groups, as among eligibles there are various types of households on different steps of the life cycle. To account for this heterogeneity we pursue the local average treatment strategy in defining the control groups. Several strategies are considered. We compare labor supply of parents in treated and untreated families where both of them are present and separate the effects for males, females and age groups of the youngest child. We consider also effects on single treated mothers referred to untreated wives.

The study indicates that the introduction of a large scale child benefit program has a minor impact on women's labor supply and almost no effect on men's labor supply. All of these effects are immediate, i.e. they concern the period directly after the introduction of the program. We found a small encouraging impact on hours worked by treated mothers of children at school age, both sole and married. Additionally, there is a weak evidence for a limited decrease in probability of employment among married mothers of the youngest children (0-5) that participate in the program simultaneous with a slight increase in men's hours worked in the same child age

category. It would suggest a program-induced change in the division of household duties between men and women. Although this effect seems barely significant, we expect it to strengthen in the subsequent quarters. The labor supply for the rest of parents remains unaffected. One of the possible explanations for this result is that the program has been introduced quite recently which is why households might not have sufficient amount of time to fully adjust their behavior. Another key issue is whether they perceive the transfers as a long run increase in a family's income. With the time (and arrival of subsequent waves of data) we will be able to verify if the effects intensify.

The paper is organized as follows. In section 2 we discuss theoretical models of labor supply with child care and their predictions for the outcome of interest given an increase in non-labor income. In section 3 we describe the identification strategy that allows us to identify the treatment effects. In section 4 we propose a new estimator to account for potential incomparability between treated and control group. In section 5 we describe the data set. In section 6 we present and discuss the empirical results. Section 7 concludes.

2 THEORETICAL CONSIDERATIONS

Our analysis departs from the theoretical models of labor supply developed at the turn of '80 and '90 (Blau and Robins, 1988; Connelly, 1992; Ribar, 1992, 1995) which emphasizes child-care provision. It seems a key issue in the evaluation of non-equivalent child care benefit programs as the eligibility relies mainly on the number of children in the family. In addition, the program is universal and all eligible participate. A caveat related to the labor supply modeling in the presence of welfare program might appear as a kinked budget constraint (studied by among others Heckman (1974) and Burtless and Hausman (1978)¹). In our paper it is not an issue, because the benefit constitutes a lump transfer untied with the number of working hours or earnings. Thus it does not alter the slope of budget constraint but only the intercept and might be perceived as a positive shock in non-labor income.

We focus attention on the household optimization in the way proposed by Blau and Robins (1988). They include in the model so called *other* members of a household that bring monetary income and may provide child-care. One might interpret the *others* as a husband and therefore consider intra-household allocation of labor and leisure (including child-care) among parents. Blau and Robins (1988) suggest that even in the absence of father, the household might have an access to the informal child care at low (or zero cost) – other children, friends, relatives living in the household or elsewhere – as long as they contribute to household non-market work. Some models in this literature focus mainly on the woman's utility maximization (Connelly, 1992; Ribar, 1992, 1995). Although, as Michalopoulos et al. (1992) notice, labor supply decision within the household is likely to be made simultaneously, Mroz (1987) provides some evidence that the

¹For the review of methods see Moffitt (2002)

income earned by husband is largely exogenous to woman's labor supply².

We assume preferences of a household depend on the leisure hours (L), the quality of the child-care (Q) and a market composite good (C). Leisure hours are defined as the maximum hours available minus hours worked. We prefer [Blau and Robins \(1988\)](#) approach who separated the leisure of mother and other household member/supporter (L_M and L_O respectively). An important assumption is that parental child care is included and non-separable from the leisure time. The quality of child care is a function of the child-care qualities from various sources – maternal, from other members and market services – and time devoted by each provider. Depending on the problem specification it might depend also on the number of children and age structure of the children within a household ([Connelly, 1992](#)) or other goods ([Ribar, 1995](#)). The market composite good reflects the typical consumption part in the consumer optimization and is a numeraire good. The utility function is typically optimized with respect to three constraints. The first regards the limited amount of time that might be distributed on work and leisure. The second reflects the structure of child-care quality function. The third is a typical budget constraint which requires the total monetary inflow (earnings from working and non-labor income) to be equal to spending on consumption and child-care.

Participation in the child-benefit program should be perceived as an exogenous increase in autonomous income Y . The main prediction from the theoretical models regarding the increase in non-labor income is that one should expect an increase in allocation with no market child-care ([Blau and Robins, 1988](#)). Even though the model does not imply an unambiguous allocation of child care between mother and other members of a household, it is likely to decrease especially mother's probability of work.

However, the increase in overall household's income leads also to a relative decrease of market child-care costs. It is connected with the upward movement of the budget constraint. One might easily imagine a family with strong preference for working (and therefore utilizing formal care), which cannot afford market care because of the limited income. In this situation at least one of the household's members is forced to decrease the labor supply. An additional non-labor income shifts the budget constraint upwards. An allocation with full employment and sole market child-care utilization, which used to be unavailable, enters the choice set if the number of children is high or their age is low, as these factors increase costs of care ([Connelly, 1992](#)). Considering this channel, a child-benefit program might result in increased labor supply.

The decision whether to utilize paid and unpaid care depends not only on the absolute monetary cost but also on relative quality of paid care. The likelihood of formal care utilization increases with marginal contribution of paid care to overall child quality ([Ribar, 1995](#)). Additional hours worked are associated with increase in non-maternal care which might be of inferior quality by various reasons. [Baker et al. \(2008\)](#) describe the effects of highly subsidized, universally accessible

²[Connelly \(1992\)](#) in turn provides the conditions under which single parent optimization is consistent with household optimization.

child care introduction in Quebec. The program led to a significant rise in maternal labor supply. Nevertheless, children that were provided with market care demonstrated deterioration in characteristics such as aggression, motor and social skill or illnesses. Additionally, maternal care seems to be important for a proper development of children, especially in the initial years of life (Gupta and Simonsen, 2010; Berger et al., 2005). This is why even the best market based child-care in terms of quality might not be able to exceed the quality of mother's care. Finally, parents may demonstrate strong preferences for spending more time with their children, as a factor vitally contributing to hearth (Bianchi, 2000). This would be reflected in (subjective) perception of mother's care quality and affect the equilibrium quantities. Blau and Robins (1988) model predicts that an increase in maternal care quality decreases the probability of working. Hence, if a mother wants to have increase working hours she must take into account the utility loss or gain from not providing care for her children by herself (Michalopoulos et al., 1992). For women participating in the labor market the equilibrium market wage is equal the shadow value of their leisure time³. They are expected to adjust L and H until the net benefit of parental care is equal to the wage (Connelly, 1992). Therefore, if leisure time (including non-formal child care) is relatively expensive in terms of utility, for instance by a strong preference for hearth, the probability of working falls with an additional unit of non-labor income.

The changes in household labor supply after a unexpected income increase might be partially affected also by the changes in consumption, as additional utility from C is offset by the marginal decrease or increase in utility from additional non-mother care in optimum (Michalopoulos et al., 1992).

Michalopoulos et al. (1992) discuss a range of simplifying assumptions that make the child-care models tractable. Some of them might influence the model predictions in the presence of an unexpected non-income increase. First, the model requires an assumption that all leisure time is spent on providing child care. If families tend to maximize both their leisure and market child care, a household would decrease the labor supply in the presence of child benefit program.

Second, it is implicit in our model that household's utility is increasing in the quality of the child care. Although it seems unconvincing to think about the negative marginal utility from the quality of child care, the parents might be indifferent between any level of care quality provided it assures a certain level of basic needs satisfaction. If they tend to separate leisure and time spent on child care provision and simultaneously have stronger preference on leisure (working), the child-benefit program will result in a decrease (increase) of their labor supply.

Finally, we assume that working mother is able to set freely the number of working hours and employ as many hours of free care as she spends in the job. The latter might be justified by a presence of other members in household but does not necessarily hold in general. The former does not seem to be satisfied for Polish labor market. In our data approximately 80% of parents

³This result was obtained also in previous research not directly linked with child-care services, e.g. in (Heckman, 1974).

work 32, 40, 50 or 60 hours. Therefore, probably the number of hours worked might not be freely chosen by an optimizing household. Inflexibility in the choice of working hours lead to potential discontinuities in the choice set.

To understand the nature and consequences of the discontinuities in the choice set consider a woman that wants to work 20 hours weekly and devote the rest of the time to her children. Assume that given the market wage she is able to afford employment of only 20 market care hours, whereas informal care other than maternal is not available. Due to the labor market inflexibility she might choose either 0 hours with no labor income or full employment — which in turn is not available because of the budget constraint. Hence, she would not work, even though it is not the optimal solution in the world in which all constraints are continuous. Assume now the woman is given an additional lump transfer from a child benefit program which enables her to afford additional hours of child care. The non-equivalent subsidy moves her budget constraint upwards. Because of discontinuities it makes some qualitatively different choices available. Now the solution to woman’s optimization problem depends in higher degree on the structure of her preferences, whereas previously she was forced to stay at home.

Given all above considerations, direction of the overall effect of the non-equivalent child benefit program is ambiguous and depends on many factors. Its sources lie in the quality and accessibility of the market child-care services, household preferences as well as labor market imperfections which produce discontinuities in the choice set.

3 IDENTIFICATION STRATEGY

The program *Rodzina 500 Plus* is one of the most significant policy interventions affecting the household sector in Poland in the recent years. Introduced in the second quarter of 2016, this large scale program (cost of app. 2% of GDP per year) provides a monthly non-equivalent benefit of roughly 20% of net average wage to each family with two children, and another 20% of net average wage for each third and next child. Additionally, families below a certain income level are entitled to obtain the benefit also for the first-born child. The eligibles are supposed only to register in local authorities in order to obtain the benefit. This kind of simplicity strongly stimulates participation among the eligibles. The authorities predict that around 2.7 million families bringing up to 3.7 million children are or will be enrolled in the program. The main goal of the program is to improve financial well-being of families upbringing children and stimulate fertility (for more details see the official [MRPiPS \(2016\)](#)).

The program was announced in the middle of the first quarter of 2016. The first payment should have been distributed in April, but some households obtained the benefit for the initial months with some delay. The short time before announcement and realization ensure the absence of any anticipation effects that would interfere the causal inference. However, we shall discuss a caveat that results from the introduction of the program in the middle of 2016Q2 and the delay. In

2016Q2 some of the households were interviewed before the program has actually begun. Others might have not obtained the benefit even though they have satisfied all the legal requirements. Therefore, one may question whether they have succeeded to adjust the economic behavior to the program, which actually have not yet affected them. This is why we prefer to perceive 2016Q2 as still a pre-treatment period. The data provide some evidence supporting this claim. We elaborate more on that issues in the following sections.

Furthermore, it is not clear if households perceive the benefit as a long run increase in the non-labor income. If they predict the program to be finished in the reasonably short horizon or believe that the eligibility requirements might change, one should not expect them to adjust the labor supply. Some evidence on the possibly short-term perception of the program comes from the aggregated household consumption, which has not increased by leaps and bounds neither in 2016Q2 nor in 2016Q3 (Premik et al., 2017). Life cycle theory predicts that only a change in permanent income would raise the consumption dynamics (Friedman, 1957; Bewley, 1977). Finally, the process of quitting the job is not immediate. Even if households do perceive the additional income as stable and therefore decide to leave the labor force, it might be observed in data with some delay due to for instance a dismissal period. The decision to quit the job may be also postponed in order to collect accumulate precautionary savings. This is why the impact of the program *Rodzina 500 Plus* on the labor supply might be more intense in the subsequent quarters. This version of the paper presents the estimates of the immediate effects of the program utilizing data for 2016Q3. With the publication of new waves of the survey we will have an opportunity to refresh the estimates and verify whether the effects intensify.

The estimation of the effects of non-equivalent child benefit program on labor supply falls into the category of program evaluations. However, the eligibility to the program is not random as long as we believe that the number of children within a household is not random. Indeed, economic theory predicts that the number of children might be set as a result of household optimization (Rosenzweig and Schultz, 1985). It implies that families that do not obtain the benefit may differ systematically from the treated subpopulation. Additionally, the number of the children is also likely to affect the outcome variable i.e. the labor supply measures (Connelly, 1992; Nakamura and Nakamura, 1992; Black et al., 2013).

To address this issues and consistently estimate the effects of interest we develop a new econometric approach. We combine an idea of covariate balancing propensity score (Imai and Ratkovic, 2014) with difference-in-difference estimator by Abadie (2005) and Heckman et al. (1997). The strategy based on DID exploits the quasi-natural experiment character of the child benefit program – it was introduced swiftly, not allowing for the anticipation effects. The estimators of Abadie (2005) and Heckman et al. (1997) solve potential problems with non-parallel outcome dynamics in treated and non-treated subpopulations, which might result from non-experimental character of the data and systematic differences between treated and non-treated. However, for the difference-in-difference method to be a reliable estimator of the causal effect, one needs to

assure a commonality of trends prior to the treatment (Imbens and Wooldridge, 2009). Given the stark differences between eligible and ineligible households, this assumption is not likely to be satisfied. Hence, we utilize CBPS estimates of propensity score to assure proper adjustment of the control to the treated group. The CBPS estimator employs moment-based approach to force balancing of the conditional distributions of covariates. It is expected to reduce significantly bias resulting from the systematic differences between treated and untreated subpopulations. It should also help to assure commonality of trends prior to the treatment, thus validate the DID approach. A thoughtful selection of covariates solves the potential endogeneity problem in the number of children. The details of our empirical approach are provided in the next sections.

Finally, the universal character of the program makes it difficult to define appropriate control groups. Among the treated there are plenty of households on different steps of the life cycle. Graham and Beller (1989) indicate a potential threat of incomparability of mothers in the context of child benefits and labor supply. Previous research proposed to utilize local average treatment effect to be more confident about the causal identification (Angrist and Evans, 1998; Jacobsen et al., 1999; Agüero and Marks, 2008; Cristia, 2008; Lefebvre and Merrigan, 2008). Therefore, we pursue the local average treatment strategy in defining the control groups. Several approaches are considered. The first idea consist of comparing households with (at least) two children and the youngest child under the age of 6 to the households with one child under the age of 6. The idea behind this identification scheme relies on the premise that children require the most attention when they are the youngest and this attention cannot be easily replaced through the market for caring service. At this period of child development, the mother in general is more likely to be inactive (the caring leave after the maternity leave) which implies that the least disincentive may be sufficient to turn working mothers into non-working mothers. We justify the choice of a threshold by the fact that even though the school duty is imposed on children from 7, children at the age 6 might go to a so called zero-class, which is frequently run in a proper primary school. Another concern relates to comparing women as second earners in households with two or more children to households with women as single earners eligible for the program. Employment elasticities are larger for single than married mothers (Connelly and Kimmel, 2003; Blundell et al., 2016). Additionally, both single and married woman obtain kind of a non-labor income: single mothers in a form of benefit, full family mothers in a form of husband's earnings. However, single women are supposed to be more constrained by means of providing child care services. One may also expect that more frequently single women are financially constrained in comparison with the married. Therefore, the benefit may loosen the constraints and in this way enable single women to move to new optimum, which is not necessarily a corner-solution.

4 ESTIMATION

We propose a novel approach in estimation of average treatment effects on the treated that combines the balancing propensity score matching (or weighting) with difference-in-difference. The following paragraphs introduce the notation and describe our approach in detail.

We use the standard program evaluation notation throughout the paper. We observe a household i in two periods indexed by $t \in \{0, 1\}$. Treatment D_{it} is binary and is observed only in $t = 1$, hence $D_{it} \equiv D_{i1} \equiv D_i$. Therefore, $\forall_i D_{i0} = 0$. Potential outcomes are denoted as Y_{it}^D , where $t \in \{0, 1\}$, $D \in \{0, 1\}$. For instance, Y_{i1}^1 is the value of outcome variable in the world a unit i receives the treatment. The outcome variable observed by an econometrician is $Y_{it} = Y_{it}^0(1 - D_{it}) + Y_{it}^1 D_{it}$. We consider a set of pre-treatment covariates $X_i \equiv \{x_{i0}\}$, assuming population conditional independence $Y_{it}^0, Y_{it}^1 \perp\!\!\!\perp D_i | X_i$. We are interested in average treatment effect on the treated (ATT):

$$\tau^{ATT} \equiv \tau = \mathbb{E}[Y_{it}^1 - Y_{it}^0 | D_{it} = 1] = \mathbb{E}[Y_{i1}^1 - Y_{i1}^0 | D_i = 1] \quad (1)$$

or its conditional-on-covariates versions. The conditional probability of treatment assignment is given by $P[D_i = 1 | X] = F_\beta(X_i)$, where β is a set of parameters. Let $f_\beta(X_i)$ denote the first derivative of $F_\beta(X_i)$ with respect to the parameters. If nothing else is mentioned, $F_\beta(X_i)$ is the logistic cumulative distribution function.

4.1 COVARIATE BALANCING PROPENSITY SCORE

Since the influential paper of [Rosenbaum and Rubin \(1983\)](#), the propensity score related methods have been continuously gaining attention in research, especially in a context of causal analysis (see for example [Dehejia and Wahba, 1999](#); [Caliendo and Kopeinig, 2008](#); [Austin, 2011](#)). Propensity score as a conditional probability of treatment assignment serves mainly for matching or weighting. The true propensity score is rarely known, so it must be estimated by a researcher. The standard framework is limited to maximum likelihood binomial models. However, as it stems from the likelihood theory, misspecification of the propensity score model leads to inconsistent estimators of binomial model parameters and therefore to inconsistent estimates of propensity score. That in turn might result in potentially heavily biased causal analysis ([Zhao, 2004](#)). Usually we do not know the correct functional form of data generating process, which is why researchers tend to experiment with many probably incorrect specifications and choose *the best* one. Due to the already mentioned reasons, the estimation of conditional probability of being eligible to the child benefit program is excessively exposed for those threats. Therefore, instead of estimating many probably misspecified propensity scores and checking their quality by a covariate balance check ([Dehejia and Wahba, 2002](#)), we employ the approach of [Imai and Ratkovic \(2014\)](#). They exploit the dual nature of propensity score. It is both conditional probability of treatment assignment and covariate balancing score. They derive a set of moment conditions that identify a propensity score vector that automatically weight the control group such that their weighted distribution

matches with that of the treatment group:

$$\mathbb{E}\left[D\tilde{X} - \frac{F_\beta(X_i)(1-D)\tilde{X}}{1-F_\beta(X_i)}\right] = 0 \quad (2)$$

One may set $\tilde{X} = X$ to balance the first moments, $\tilde{X} = X^2$ to balance second moments, etc. Restrictions that impose balance on a chosen moment for all covariates are sufficient to assure just-identification of the parameters β from the probability function $F_\beta(X_i)$. In general, imposing restrictions for more than one class of moments leads to an over-identified estimator, expected to be more biased in finite samples but asymptotically more efficient. One may add either first order conditions from binary ML model with $F_\beta(X_i)$ as the cumulative probability function or balancing conditions for (higher order) moments of covariates. Additionally, the over-identified version enables the researcher to perform a standard specification test of over-identifying restrictions. In the context of CBPS estimation, the specification test might be perceived as a verification of the reliability of unconfoundedness assumption (Imai and Ratkovic, 2014).

Let start with the just-identified case. The sample moment conditions are:

$$\frac{1}{N} \sum_i \frac{N}{N_1} \cdot \frac{T - F_\beta(X_i)}{1 - F_\beta(X_i)} \cdot X_i = 0 \quad (3)$$

The just-identified models are at the core interest in the paper as we have rather a small sample. We consider over-identified version only to perform the specification tests.

In summary, we prefer to use the CBPS method as it is robust for misspecification of the probability model instead of ML estimator. Additionally, it balances the distribution of covariates which is advantageous in the context of the paper due to expected heterogeneity between control and treated groups.

4.2 ABADIE'S DIFFERENCE-IN-DIFFERENCE

Difference-in-difference estimators exploit the time dimension in data to generate means of a natural experiment. Following Abadie (2005), we maintain the assumption of parallel path of outcomes:

$$\mathbb{E}[Y_{i1}^0 - Y_{i0}^0 | X_i, D_i = 1] = \mathbb{E}[Y_{i1}^0 - Y_{i0}^0 | X_i, D_i = 0] \quad (4)$$

which states that in the absence of treatment outcomes in both groups would behave in the same way⁴ and an overlap assumption $P[D_i = 1 | X_i] < 1$ – a well known identification condition for selection on observables. These assumptions are enough to uncover ATT.

⁴Abadie (2005) notices that this assumption identifies only $\mathbb{E}[Y_{i1}^1 | X_i, D_i = 0]$. $\mathbb{E}[Y_{i1}^1 | X_i, D_i = 1]$ remains unrestricted so it does ATE. However, we focus only on ATT so it is of minor concern in the paper.

Abadie (2005) proposes an estimation procedure that relies on the least squares approximation to $\tau_{X_k} = \mathbb{E}[Y_1^1 - Y_1^0 | D_i, X_k]$:

$$\tau_{X_k} = \arg \min_{\theta \in \Theta} \mathbb{E} \left[F_{\beta}(X_i) \cdot \left(\frac{D_i - F_{\beta}(X_i)}{(F_{\beta}(X_i))(1 - F_{\beta}(X_i))} \cdot (Y_{i1} - Y_{i0}) - g(X_k; \theta) \right)^2 \right] \quad (5)$$

where $X_k \subset X$ and $g(X_k; \theta)$ is the approximating function. If $g(X_k; \theta) \equiv \theta$ then $\tau_{X_k} = \tau$. We are interested mainly in the *homogeneous*⁵ average effect on the treated. Considering sample analogs, the equation 5 implies the following first order conditions:

$$\frac{1}{N} \sum_i -F_{\beta}(X_i) \left(\frac{D_i - F_{\beta}(X_i)}{1 - F_{\beta}(X_i)} \Delta Y_i - F_{\beta}(X_i) \tau \right) = 0 \quad (6)$$

Our contribution is to combine moment conditions from equations 3 and 6 in order to obtain a GMM representation for a one-step ATT estimator. It is appealing by many aspects, as it combines advantages of both CBPS and Abadie’s DID methods. First, it is a one-step method so there is no need to adjust the DID estimation for the stochastic nature of estimated propensity score. Second, we move an estimation process into the GMM framework. It allows us to obtain the (efficient) covariance matrix immediately. Third, the one-step estimation is justified because the way propensity score is defined produces balancing properties that are automatically satisfied. Fourth, it exhibits good numerical performance. Fifth, it is robust for functional form misspecification, as neither Imai and Ratkovic (2014) nor Abadie (2005) assume any functional form for the underlying model. In further discussions we refer to this estimator as ACBPS.

In the empirical part we apply just-identified ACBPS as a two step GMM, where the parameters needed to estimate consistently the moment covariance matrix come from logit and logit-based Abadie’s DID.

4.3 HECKMAN’S DIFFERENCE-IN-DIFFERENCE

Heckman et al. (1997) contribute to the program evaluation literature by proposing so called difference-in-difference matching estimator. They exploit the time dimension by performing matching on the first difference in the outcome variable. They propose to match observations using propensity score in order to overcome the curse of dimensionality. The estimator might be written down as:

$$\tau = \sum_{i \in \text{treated}} \omega_i (\Delta Y_{i,\text{treated}} - \sum_{j \in \text{control}} w_j \Delta Y_{j,\text{control}}) \quad (7)$$

where ω_i are sample weights and w_i are weights generated by a matching algorithm.

We propose to match observations using the CBPS propensity score. However, we maintain the two-step nature of the estimator, as matching weight in general cannot be obtained via

⁵The quality of any non-constant approximation for the effects of interest is expected to be poor due to the limited number of observations in our sample.

moment-based estimation. This estimator should be advantageous over the standard version using conventional propensity scores in the presence of improperly balanced control group. In particular, small sample properties of Heckman’s estimator rely critically on the availability of a proper (constructed) control group. Additionally, we expect it to be less biased in small samples than ACBPS as it performs matching instead of GMM estimation.

4.4 ROBUSTNESS CHECKS

We compare estimates from our estimators with other estimators for ATT to verify the robustness of the results. We investigate:

- ↳ Basic DID (as a baseline, [Card and Krueger \(1994\)](#))

$$\tau = (\bar{y}_{11} - \bar{y}_{01}) - (\bar{y}_{10} - \bar{y}_{00})$$

- ↳ Regression-based DID controlling for covariates:

$$\Delta Y_i = \beta_0 + \tau D_i + X_i \beta + \varepsilon_i$$

- ↳ Abadie’s estimator in the original two step formulation, with ML logit estimation for the propensity score.
- ↳ Heckman’s estimator in the original formulation with ML logit estimation for the propensity score and Epanechnikov kernel matching.

The Stata codes for ACBPS and CBPS estimation are available on request. In the paper we also make use of codes by [Houngbedji \(2016\)](#) and [Leuven and Sianesi \(2003\)](#).

5 DATA

We utilize data from Labor Force Survey (LFS) collected by the Central Statistical Office in Poland. It is a rotating panel in which households are interviewed in two consecutive quarters and then reinterviewed in two respective periods a year after. The time dimension allows us to apply the DID strategy. As it is already mentioned, this version of paper presents the estimates of immediate effects of the program which concern solely 2016Q3. Following the previous discussion, due to some ambiguity whether to perceive 2016Q2 as a post-treatment period we take 2016Q3 as the only post-treatment period for the main results. Hence, if data on both 2016Q2 and 2016Q3 for a given household is available we use the latter. Finally, we obtain a balanced panel with $T_0 = 2015Q3$ and $T_1 = 2016Q3$.

We consider only families with children and drop households in which another child was born between T_0 and T_1 . Depending on the definition of control groups we keep households with various characteristics in the sample. We distinguish five main groups of covariates that we control for in order to satisfy the unconfoundedness assumption. They are described in table 1.

Table 1: Covariates controlled for in the models' specifications

(1)	household	presence of non-parental household members, small city and village indicators, mother's age at birth of the youngest child
(2)	mother demographic	age, age squared, work experience, education
(3)	mother's job	indicators for public company, working as an employee, tenure contract, full-time job, binary variable for working in the field of education
(4)	father demographic	age, age squared, work experience, education
(5)	father's job	indicators for public company, working as an employee, tenure contract, full-time job, binary variable for working in the field of education

6 RESULTS

This section presents estimates of a range of local treatment effects concerning the impact of introducing non-equivalent child support on labor supply. We consider various definitions of control and treated subpopulation to provide an assessment of the total effect. Wherever the sample size is sufficient for identification, we separate effects on households with respect to the age of the youngest child, as it is a significant determinant of (especially women's) labor supply (Jacobsen et al., 1999). The sets of control variables are described in the previous section.

The main results are presented in tables 2-7 and are calculated assuming that 2016Q2 is a pre-treatment period. One might compare them with tables 8-12 in the appendix A which contain the estimates using both 2016Q2 and 2016Q3 as post-treatment periods. Even though using more data limits problems with small number of observations, especially in the subgroups defined over a certain age of the youngest child in household, the estimated impact is usually even weaker and less precise. It suggests that 2016Q2 should not be perceived as a quarter when the program has already been introduced. This claim is additionally supported by the fact that estimates obtained solely using 2016Q2 as a post-treatment period are not only statistically insignificant but also numerically close to zero in almost all specifications.

The specification test mentioned in the tables is the over-identifying restrictions test for CBPS propensity score. It is valid both for Heckman's and Abadie's approaches. They contain just-identified models, hence the estimates of propensity score probability function parameters are the same. Rejection of the null hypothesis might suggest violation of the conditional independence assumption indicating failure in accounting for observable factors.

6.1 FULL FAMILIES WITH CHILDREN AT THE SAME AGE

We begin with comparisons between families in which both mother and father are present. The treated group is defined as a subset of households that have at least two children — thus are eligible for the participation in the child benefit program, whereas families with only one children are classified as a control group. However, some of the control group households may also be eligible. The data gives no opportunity to verify their participation status as household's income is not observed. However, one should not expect a high percentage of potentially treated in the control group, as they contain at least both father and mother so their total income is likely to exceed the eligibility threshold. We separate the effects for parents who worked in the pre-treatment period and those who did not.

First, we analyze paths of labor supply of parents who worked in the pre-treatment period. Table 2 presents the estimated effects for women, table 3 for men. The average effects on the pooled sample are insignificant for both men and woman. However, they do not pass the specification test, thus one might doubt whether the necessary identification assumptions have been satisfied. However, we do not reject the null hypothesis for models estimated on subsamples with regard to the age of the youngest child. It suggests that households differ significantly with age of the youngest child and so their outcomes should not be compared in a correct causal analysis.

Most local effects for both women and men who worked in T_0 and have obtained a benefit indicate that they worked in T_1 with approximately the same probability as those employed in $T = 0$ who have only one child. This finding is robust to the choice of specification. In the majority of comparison groups, not only are the parameters statistically insignificant but they tend to zero also numerically for all subgroups except for the last one. There appear some exceptionally significant OLS or logit-based DID estimates, but they are likely to be biased because of inability to account for the systematic differences between households. Additionally, other estimates in their comparison group are way smaller and insignificant.

Two comparisons require further comments. First, all estimates consistently suggest a small decrease in labor participation for treated mothers whose youngest child is below 6. The estimates are obtained using a sample of moderate size. Even though they are not significant, the considerable number of observations suggest that this tendency does not necessarily come from the sampling error. It might indicate a weak regularity in quitting the job by participating married females. This regularity is consistent with economic theory, as additional money from non-equivalent transfer shifts the budget line so the allocation with no labor (and full-time maternal care) becomes available. The youngest children need maternal care in the highest degree. However, due to the immediate character of the estimates the effect is weak, as potentially more time is required to effectively quit a job in a response to the benefit program introduction. Moreover, estimates on the sample containing both 2016Q2 and 2016Q3 (table 8) confirm the presence of slight decline in labor market participation. Second, similar situation takes place in case of married mothers whose youngest child is above 11. However, here the effects are likely

to equal to zero. Significant regression estimates emphasize rather the fact that each treated mother from this subpopulation in the sample is employed in both T_1 and T_0 whereas 4% of the untreated dropped out from the employment state. Moreover, only 80 treated observations do not increase the credibility of the result.

Table 2: Effects on mother's who worked before

	OLS		Abadie		Heckman	
	raw	with x	cbps	logit	cbps	logit
Probability of working in T_1						
all households						
τ	0.005 (0.582)	0.003 (0.764)	0.006 (0.548)	0.010 (0.622)	0.004 (0.748)	0.005 (0.635)
N= 1664, N treated = 820, Specification test for CBPS p-val = .001						
youngest child younger than 6						
τ	-0.021 (0.161)	-0.032 (0.069)	-0.021 (0.131)	-0.032 (0.541)	-0.026 (0.144)	-0.026 (0.145)
N= 702, N treated = 408, Specification test for CBPS p-val = .872						
youngest child between 6 and 11						
τ	0.027 (0.074)	0.017 (0.278)	0.020 (0.187)	0.057* (0.029)	0.016 (0.410)	0.011 (0.573)
N= 575, N treated = 332, Specification test for CBPS p-val = .899						
youngest child older than 11						
τ	0.039*** (0.000)	0.039** (0.004)	0.036 (0.186)	0.033 (0.359)	0.045* (0.025)	0.030 (0.104)
N= 387, N treated = 80, Specification test for CBPS p-val = .904						
Hours worked						
all households						
τ	1.437 (0.054)	1.348 (0.101)	1.128 (0.225)	1.310 (0.122)	0.947 (0.303)	1.350 (0.127)
N= 1664, N treated = 820, Specification test for CBPS p-val = .001						
youngest child younger than 6						
τ	0.927 (0.425)	0.563 (0.679)	0.228 (0.857)	0.719 (0.561)	0.767 (0.587)	0.861 (0.549)
N= 702, N treated = 408, Specification test for CBPS p-val = .872						
youngest child between 6 and 11						
τ	0.757 (0.534)	0.085 (0.948)	-0.320 (0.804)	-0.493 (0.692)	-0.228 (0.875)	-0.375 (0.795)
N= 575, N treated = 332, Specification test for CBPS p-val = .899						
youngest child older than 11						
τ	5.641** (0.002)	5.371** (0.004)	7.599 (0.091)	6.152** (0.004)	7.683*** (0.001)	5.785** (0.008)
N= 387, N treated = 80, Specification test for CBPS p-val = .904						

We control for the following set of covariates: (1), (2), (3), (4), (5) and labor status of the father – see table 1. p -values in parentheses. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Slightly different story can be told about hours worked. According to the estimates, the program affects positively hours worked by mothers of at least two children who are older than 11, i.e. they have finished the primary school or are about to do so. The average impact amounts to approximately 6-8 hours. This result is consistent with an intuition and theoretical predictions. Teenagers do not require full time maternal care. On the contrary, the market services might outperform informal care by means of the quality, as the needs of teenagers are different than those of younger children. Therefore, given a non-equivalent transfer mothers become able to afford additional after-school classes or other form of activities for a child gaining additional time to work. Provided the leisure plays a minor role in total utility, both mother and a child might

spend the time more productively. One should emphasize that this effect concerns adjustments of labor supply only at the intensive margin. Furthermore, it is worth mentioning that in the sample 21% and 13% of the untreated and treated mothers respectively decreased the number of working hours, whereas 14% and 25% of the untreated and treated respectively increased the number of hours worked. Therefore, the data background is different than in case of probability of being employed in this comparison. It supports the conclusion about the effect's significance. One should also notice that all but one estimates for hours worked by females with teenager as the youngest child are statistically significant. Only the estimate from the ACBPS model seems insignificant. Nevertheless, it is numerically close to the rest of effects. We interpret this results as a lack of precision. ACBPS is a conventional GMM estimator and therefore might be inefficient in small samples.

Table 3: Effects on father's who worked in T_0

	OLS		Abadie		Heckman	
	raw	with x	cbps	logit	cbps	logit
Probability of working in T_1						
all households						
τ	0.011 (0.059)	0.011 (0.084)	0.011 (0.251)	0.015 (0.445)	0.010 (0.198)	0.013 (0.087)
	N= 2355, N treated = 1255, Specification test for CBPS p-val = 0					
youngest child younger than 6						
τ	0.006 (0.492)	0.011 (0.242)	0.019 (0.330)	-0.078 (0.713)	0.014 (0.223)	0.007 (0.531)
	N= 1214, N treated = 754, Specification test for CBPS p-val = .251					
youngest child between 6 and 11						
τ	0.024* (0.040)	0.017 (0.122)	0.004 (0.593)	0.049* (0.040)	0.008 (0.556)	0.010 (0.503)
	N= 704, N treated = 406, Specification test for CBPS p-val = .344					
youngest child older than 12						
τ	0.020** (0.008)	0.011 (0.198)	0.010 (0.321)	0.010 (0.769)	0.015 (0.311)	0.015 (0.246)
	N= 437, N treated = 95, Specification test for CBPS p-val = .801					
Hours worked						
all households						
τ	1.218 (0.056)	1.408* (0.038)	1.728* (0.041)	1.737* (0.019)	1.690* (0.035)	1.856* (0.017)
	N= 2355, N treated = 1255, Specification test for CBPS p-val = 0					
youngest child younger than 6						
τ	1.764 (0.051)	2.097* (0.034)	3.215* (0.014)	3.512 (0.135)	2.590* (0.024)	2.152 (0.054)
	N= 1214, N treated = 754, Specification test for CBPS p-val = .251					
youngest child between 6 and 11						
τ	-0.299 (0.796)	0.020 (0.987)	-0.639 (0.644)	-0.812 (0.515)	-0.312 (0.822)	-0.741 (0.593)
	N= 704, N treated = 406, Specification test for CBPS p-val = .344					
youngest child older than 11						
τ	3.743* (0.033)	2.612 (0.158)	2.720 (0.332)	2.606 (0.172)	3.167 (0.174)	2.662 (0.207)
	N= 437, N treated = 95, Specification test for CBPS p-val = .801					

We control for the following set of covariates: (1), (2), (3), (4), (5) and labor status of the mother – see table 1. p -values in parentheses. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

The evidence for an impact of the program on the previously employed men's hours worked is also

weak. One should not focus on the pooled sample estimates as they do not pass the specification test. However, the results for men who worked in T_0 and whose youngest child is below 6 are consistently positive and barely significant, especially in the specifications using CPBS approach. It would suggest that proper balancing allows us to uncover a weak effect. Even though an increase in labor supply by 2-3 hours weekly seems insignificant economically, it is interesting to look at these estimates taking into account the negative estimates for married woman in the same child age group. Holding all concerns about the size of the effect mentioned before, the child benefit program might cause an intra-household change of the social roles. Consider a household in which the mother quit the job in order to provide more maternal care. The amount of money received as a benefit is typically smaller than earnings lost as a result of job resignation. Therefore, it sounds reasonable that the father increases hours worked to smooth at least partially family's income.

Table 4: Effects on mother's who didn't work in T_0

	OLS		Abadie		Heckman	
	raw	with x	cbps	logit	cbps	logit
Probability of working in T_1						
all households						
τ	0.007 (0.789)	0.027 (0.345)	0.007 (0.806)	0.010 (0.722)	0.030 (0.427)	0.008 (0.825)
N= 906, N treated = 528, Specification test for CBPS p-val = .001						
youngest child younger than 6						
τ	-0.048 (0.207)	0.038 (0.328)	0.028 (0.403)	0.042 (0.196)	0.025 (0.621)	0.027 (0.598)
N= 599, N treated = 396, Specification test for CBPS p-val = .355						
youngest child older than 5						
τ	0.018 (0.616)	0.027 (0.454)	0.049 (0.212)	0.050 (0.124)	0.050 (0.332)	0.060 (0.223)
N= 307, N treated = 132, Specification test for CBPS p-val = .778						
Hours worked						
all households						
τ	0.343 (0.723)	1.217 (0.246)	0.582 (0.563)	0.723 (0.471)	1.309 (0.322)	0.683 (0.596)
N= 906, N treated = 528, Specification test for CBPS p-val = .001						
youngest child younger than 6						
τ	-1.479 (0.287)	1.455 (0.326)	1.051 (0.391)	1.439 (0.260)	0.991 (0.590)	0.987 (0.591)
N= 599, N treated = 396, Specification test for CBPS p-val = .355						
youngest child older than 5						
τ	0.835 (0.513)	0.859 (0.481)	1.920 (0.170)	1.800 (0.115)	1.499 (0.369)	1.954 (0.248)
N= 307, N treated = 132, Specification test for CBPS p-val = .661						

We control for the following set of covariates: (1), (2), (4), (5) and labor status of the father – see table 1. p -values in parentheses. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Tables 4 and 5 presents analogous effects of the program on the subsample of parents who did not work in T_0 . The results are identical regardless the specification. No estimate is significant either statistically or economically. However, this comparison scenario suffers more intensely from the lack of data. The subsamples of males and females who did not work in T_0 are substantially smaller than for the employed parents. It is especially visible in case of the fathers, as in general men are more likely to be employed.

Table 5: Effects on father’s who did not work in T_0

	OLS		Abadie		Heckman	
	raw	with x	cbps	logit	cbps	logit
Probability of working in T_1						
all households						
τ	0.061	0.008	0.058	-0.042	-0.010	-0.012
	(0.317)	(0.910)	(0.487)	(0.712)	(0.919)	(0.871)
N= 215, N treated = 93, Specification test for CBPS p-val = .508						
Hours worked						
all households						
τ	2.426	0.440	2.398	-1.272	-0.425	-0.105
	(0.343)	(0.881)	(0.512)	(0.788)	(0.918)	(0.974)
N= 215, N treated = 93, Specification test for CBPS p-val = .508						

We control for the following set of covariates: (1), (2), (3), (4) and labor status of the mother – see table 1. p -values in parentheses. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

In conclusion, the program seems to have minor impact on labor status of parents both employed and unemployed in a pre-treatment period. Considering hours worked, the results indicate a moderate increase of 5-6 hours weekly worked by treated mothers in comparison with the non-participating, whose youngest child is a teenager. Moreover, regardless of the estimator applied the qualitative interpretation of this outcome remains the same. However, the small number of observations (80 treated mothers) undermines the credibility of the results. An interesting story can be told about households with both parents and youngest child below 6. There is weak evidence on a small decrease in probability of employment for women and moderate increase in father’s hours worked. We expect this effect to become more evident in the subsequent quarters as households will have more time to adjust their behavior. The program *Rodzina 500 Plus* does not differentiate the treated and untreated parents among the rest of comparison groups.

6.2 SINGLE PARTICIPATING MOTHERS AND INELIGIBLE FULL FAMILIES

In the next step we compare single mothers with two or more children with women from the full family which have only one child. Due to the relatively small number of single woman households we are not able to separate the effects with respect to the age of the youngest child. Moreover, estimation on the subsamples of sole males is infeasible due to lack of data. Fortunately, all specifications pass the over-identification test. The rationale for the comparisons between married and single females lies in larger employment elasticities for the latter (Connelly and Kimmel, 2003). They might react strongly for any disincentive to work. However, the data provide no evidence supporting this hypothesis. The program does not differentiate the probability of working in T_1 between sole participating mothers and matched untreated married females regardless whether they worked in T_0 or not.

Considering hours worked, we obtain slightly significant estimates from Heckman’s models on the employed in T_0 . These estimators utilize matching so are expected to outperform the rest of methods as the number of treated observations is dramatically low. At the same time, ACBPS

Table 6: Effects on lone mothers who worked in T_0

	OLS		Abadie		Heckman	
	raw	with x	cbps	logit	cbps	logit
Probability of working in T_1						
all households						
τ	0.004	0.025	0.085	0.039	0.034	0.031
	(0.870)	(0.267)	(0.261)	(0.110)	(0.083)	(0.114)
N= 903, N treated = 59, Specification test for CBPS p-val = .645						
Hours worked						
all households						
τ	0.906	2.013	3.243	1.759	2.855*	2.692*
	(0.538)	(0.157)	(0.209)	(0.257)	(0.020)	(0.028)
N= 903, N treated = 59, Specification test for CBPS p-val = .645						

We control for the following set of covariates: (1), (2), (3) – see table 1. p -values in parentheses. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

indicates an effect even stronger numerically, but (not surprisingly because of the GMM nature of the estimate) insignificant. Although one should not claim economic importance of the effect that amounts to 3 hours weekly, the estimate provide an average of all effects for various ages of the female’s youngest child. Therefore, we interpret it as a premise that some sole treated mothers work in T_1 significantly longer than their untreated counterpart. The positive sign of the estimate suggests that the program provides incentive to work for lone mothers of rather older children. In such a case the difference between the quality of informal and market care is not dramatic. Additional non-labor income allows those mothers to buy more market care and therefore work longer, whereas married woman might have used informal care provided by other members of a household in both periods. We shall emphasize that the positive effects concern only those who are employed and decide to work longer. There is no connection with changing the labor market status between unemployment and employment.

Table 7: Effects on lone mothers who did not work in T_0

	OLS		Abadie		Heckman	
	raw	with x	cbps	logit	cbps	logit
Probability of working in T_1						
all households						
τ	-0.030	0.058	0.044	0.058	0.064	0.063
	(0.627)	(0.344)	(0.574)	(0.319)	(0.434)	(0.406)
N= 419, N treated = 41, Specification test for CBPS p-val = .256						
Hours worked						
all households						
τ	-0.140	2.503	2.477	2.562	2.658	2.734
	(0.953)	(0.278)	(0.443)	(0.249)	(0.380)	(0.335)
N= 419, N treated = 41, Specification test for CBPS p-val = .256						

We control for the following set of covariates: (1), (2) – see table 1. p -values in parentheses. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

7 CONCLUSION

In this paper we provide evidence that the introduction of a large scale child benefit program has a minor immediate impact on women’s labor supply and almost no effect on men’s labor supply. We found a small encouraging effect on treated woman’s hours worked on the subpopulation who worked in the pre-treatment period – both single and married. It refers rather to mothers whose youngest child is at school age. Additionally, there are some signals that married mothers of the youngest children who obtain benefits decrease their probability of employment in comparison with the untreated. Simultaneously, the effect for fathers’ hours worked in the same comparison group are positive and barely significant. It would suggest a program-induced change in the division of household duties between men and women. We expect this result to become more evident in subsequent quarters. Except for that, all other effects on both employment and hours worked equal zero. Each device, either simple as OLS regression or more sophisticated as CBPS-based DID estimators, mostly produces estimates that are statistically insignificant and economically unimportant. Results are strongly robust to the choice of estimation strategy. The labor supply of the treated follow the same path as the untreated regardless whether they take care of an infant, they worked in the pre-treatment period and are single mothers.

There are some possible explanations for such outcomes. First, as mentioned before we provide estimates of immediate effects of the program introduction. Households might not have enough time to adjust their behavior for the presence of an additional non-labor income. The process of quitting or finding a job is not immediate and may last over a quarter. Second, one should not expect any reaction in labor supply if households do not perceive the benefit as a long term increase in income. Third, the data utilized in the paper concern the third quarter which contains summer holidays period. It would not be rare if the families postpone any important decisions for subsequent quarters. Fourth, the precision of estimators suffer from the small size of sample as only one wave of the LFS might be utilized in estimation.

With the time and arrival of subsequent waves of data we will be able to verify if the effects intensify. The data from LFS is published quarterly. We expect a new wave (on 2016Q4) in early April. Additional data will double the sample size and provide information on household activities in periods more remote from the program announcement and introduction.

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APPENDIX

A ESTIMATES USING DATA FOR BOTH 2016Q2 AND 2016Q3

Table 8: Effects on mother's who worked in T_0

	OLS		Abadie		Heckman	
	raw	with x	cbps	logit	cbps	logit
Probability of working in T_1						
all households						
τ	0.001 (0.933)	-0.002 (0.764)	-0.003 (0.733)	0.011 (0.448)	-0.004 (0.623)	-0.003 (0.749)
N= 2592, N treated = 1278, Specification test for CBPS p-val = 0						
youngest child younger than 5						
τ	-0.020 (0.111)	-0.032* (0.024)	-0.024* (0.032)	-0.069 (0.586)	-0.029 (0.051)	-0.026 (0.078)
N= 1067, N treated = 635, Specification test for CBPS p-val = .462						
youngest child between 6 and 11						
τ	0.021 (0.066)	0.017 (0.157)	0.018 (0.175)	0.048* (0.036)	0.020 (0.178)	0.021 (0.144)
N= 926, N treated = 527, Specification test for CBPS p-val = .403						
youngest child older than 11						
τ	0.031*** (0.000)	0.030** (0.001)	0.023 (0.108)	0.032 (0.122)	0.025 (0.061)	0.026* (0.037)
N= 599, N treated = 116, Specification test for CBPS p-val = .659						
Hours worked						
all households						
τ	1.048 (0.060)	0.779 (0.200)	0.491 (0.476)	0.730 (0.243)	0.458 (0.506)	0.725 (0.278)
N= 2592, N treated = 1278, Specification test for CBPS p-val = 0						
youngest child younger than 5						
τ	0.308 (0.733)	0.012 (0.991)	-0.082 (0.933)	-0.026 (0.979)	0.053 (0.962)	0.119 (0.916)
N= 1067, N treated = 635, Specification test for CBPS p-val = .462						
youngest child between 6 and 11						
τ	0.873 (0.329)	0.340 (0.721)	0.023 (0.981)	-0.035 (0.970)	0.158 (0.884)	0.008 (0.994)
N= 926, N treated = 527, Specification test for CBPS p-val = .403						
youngest child older than 11						
τ	4.513** (0.001)	4.598*** (0.001)	4.957 (0.080)	4.782** (0.002)	6.006*** (0.000)	4.605** (0.004)
N= 599, N treated = 116, Specification test for CBPS p-val = .659						

We control for the following set of covariates: (1), (2), (3), (4), (5) and labor status of the father – see table 1. p -values in parentheses. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table 9: Effects on father's who worked in T_0

	OLS		Abadie		Heckman	
	raw	with x	cbps	logit	cbps	logit
Probability of working in T_1						
all households						
τ	0.010*	0.010*	0.007	0.025*	0.008	0.009
	(0.031)	(0.026)	(0.272)	(0.034)	(0.165)	(0.115)
	N= 3676, N treated = 1945, Specification test for CBPS p-val = 0					
youngest child younger than 5						
τ	0.005	0.009	0.013	-0.055	0.009	0.006
	(0.441)	(0.169)	(0.372)	(0.726)	(0.295)	(0.442)
	N= 1857, N treated = 1150, Specification test for CBPS p-val = .001					
youngest child between 7 and 11						
τ	0.018*	0.011	0.002	0.033*	0.006	0.007
	(0.034)	(0.172)	(0.770)	(0.043)	(0.596)	(0.491)
	N= 1134, N treated = 652, Specification test for CBPS p-val = .001					
youngest child older than 12						
τ	0.015	0.012	-0.004	0.016	0.008	0.007
	(0.108)	(0.141)	(0.790)	(0.423)	(0.592)	(0.532)
	N= 685, N treated = 143, Specification test for CBPS p-val = .222					
Hours worked						
all households						
τ	1.032*	1.000*	1.220*	1.103*	1.150*	1.183*
	(0.029)	(0.047)	(0.045)	(0.043)	(0.048)	(0.037)
	N= 3676, N treated = 1945, Specification test for CBPS p-val = 0					
youngest child younger than 5						
τ	1.299	1.471*	2.340*	2.157	1.764*	1.467
	(0.055)	(0.045)	(0.018)	(0.065)	(0.039)	(0.075)
	N= 1857, N treated = 1150, Specification test for CBPS p-val = .001					
youngest child between 6 and 11						
τ	-0.284	-0.328	-0.144	-0.367	-0.147	-0.380
	(0.735)	(0.712)	(0.877)	(0.667)	(0.882)	(0.696)
	N= 1134, N treated = 652, Specification test for CBPS p-val = .001					
youngest child older than 11						
τ	2.620	1.691	0.530	1.160	1.535	1.115
	(0.061)	(0.237)	(0.795)	(0.442)	(0.387)	(0.485)
	N= 685, N treated = 143, Specification test for CBPS p-val = .222					

We control for the following set of covariates: (1), (2), (3), (4), (5) and labor status of the mother – see table 1. p -values in parentheses. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table 10: Effects on mother’s who didn’t work in T_0

	OLS		Abadie		Heckman	
	raw	with x	cbps	logit	cbps	logit
Probability of working in T_1						
all households						
τ	-0.007 (0.764)	0.006 (0.802)	-0.021 (0.426)	-0.025 (0.338)	-0.009 (0.762)	-0.041 (0.147)
N= 1418, N treated = 815, Specification test for CBPS p-val = 0						
youngest child younger than 6						
τ	-0.063* (0.044)	0.012 (0.712)	-0.032 (0.327)	-0.008 (0.804)	-0.027 (0.517)	-0.026 (0.523)
N= 921, N treated = 591, Specification test for CBPS p-val = .234						
youngest child older than 5						
τ	0.011 (0.696)	0.012 (0.693)	0.014 (0.692)	0.026 (0.396)	0.024 (0.527)	0.027 (0.466)
N= 497, N treated = 224, Specification test for CBPS p-val = .192						
Hours worked						
all households						
τ	-0.391 (0.626)	0.019 (0.983)	-0.949 (0.323)	-1.170 (0.237)	-0.509 (0.622)	-1.754 (0.082)
N= 1418, N treated = 815, Specification test for CBPS p-val = 0						
youngest child younger than 6						
τ	-2.236 (0.050)	0.238 (0.852)	-1.324 (0.284)	-0.738 (0.549)	-1.041 (0.494)	-1.300 (0.383)
N= 921, N treated = 591, Specification test for CBPS p-val = .234						
youngest child older than 5						
τ	0.314 (0.760)	0.081 (0.939)	0.741 (0.544)	0.622 (0.573)	0.379 (0.776)	0.450 (0.732)
N= 497, N treated = 224, Specification test for CBPS p-val = .303						

We control for the following set of covariates: (1), (2), (4), (5) and labor status of the father – see table 1. p -values in parentheses. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table 11: Effects on father’s who did not work in T_0

	OLS		Abadie		Heckman	
	raw	with x	cbps	logit	cbps	logit
Probability of working in T_1						
all households						
τ	0.063 (0.201)	-0.001 (0.992)	0.072 (0.319)	0.014 (0.832)	0.011 (0.877)	0.026 (0.685)
N= 334, N treated = 148, Specification test for CBPS p-val = .759						
Hours worked						
all households						
τ	2.541 (0.220)	0.177 (0.937)	3.044 (0.318)	0.955 (0.732)	0.216 (0.944)	1.308 (0.629)
N= 334, N treated = 148, Specification test for CBPS p-val = .759						

We control for the following set of covariates: (1), (2), (3), (4) and labor status of the mother – see table 1. p -values in parentheses. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table 12: Effects on lone mothers who worked in T_0

	OLS		Abadie		Heckman	
	raw	with x	cbps	logit	cbps	logit
Probability of working in T_1						
all households						
τ	-0.005	0.016	0.084	0.025	0.024	0.021
	(0.835)	(0.442)	(0.326)	(0.240)	(0.210)	(0.271)
N= 1391, N treated = 77, Specification test for CBPS p-val = .311						
Hours worked						
all households						
τ	-0.495	0.348	-0.503	0.149	0.438	0.294
	(0.688)	(0.774)	(0.843)	(0.908)	(0.710)	(0.802)
N= 1391, N treated = 77, Specification test for CBPS p-val = .311						

We control for the following set of covariates: (1), (2), (3) – see table 1. p -values in parentheses. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table 13: Effects on lone mothers who did not work in T_0

	OLS		Abadie		Heckman	
	raw	with x	cbps	logit	cbps	logit
Probability of working in T_1						
all households						
τ	-0.044	0.018	-0.005	0.023	0.008	0.024
	(0.404)	(0.738)	(0.939)	(0.649)	(0.896)	(0.691)
N= 664, N treated = 61, Specification test for CBPS p-val = .162						
Hours worked						
all households						
τ	-0.502	1.270	0.499	1.476	0.884	1.512
	(0.804)	(0.536)	(0.828)	(0.458)	(0.706)	(0.510)
N= 664, N treated = 61, Specification test for CBPS p-val = .162						

We control for the following set of covariates: (1), (2) – see table 1. p -values in parentheses. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$