Labeling vs Targeting: How did the Canada Child Benefit affect household bargaining and preferences?

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Abstract

Child benefits in Canada are sizeable, targetted to children and paid to custodial mothers, and so might be expected to affect household resource allocations and spousal bargaining. The introduction of the Canada Child Benefit in July 2016 more than doubled the amount of child benefits received by households with children. We estimate the effect of this change on the preferences and resource shares of adult males and females within dual parent households. Resource shares, defined as the fraction of household expenditure consumed by females vs males, are important parameters of collective household models. Using a difference-in-difference strategy within a structural collective household model, we find little evidence that preferences of either adult males or females changed in response to the increase in child benefits. However, we do find evidence that the policy increased female resource shares, but only among homeowner (and not renter) households. We provide possible explanations for this heterogeneous treatment effect across owners versus renters as resulting from differences in the females' bargaining power, or through differences in the marginal price of shelter.

JEL classification: D13, D63, C31

1 Introduction

The Canada Child Benefit (CCB) was established in 2016 replacing the existing combination of child benefits provided through the Universal Child Care Benefit, the Canada Child Tax Benefit, and the National Child Benefit. The CCB was introduced with increased amounts for benefits, a new label for the comprehensive benefit and was paid to primary caregivers,which is defaulted to be the female parent in dual parent households. We assess the impact of this policy change on resource shares, defined as the fraction of household spending consumed by different household members, and on preferences within the dual parent households. Using data from the Survey of Household Spending (SHS) from 2014 to 2019, we estimate changes in resource shares of the adult female (mother) and male (father) within the household, along with changes in their preference parameters using a collective model of the household.

Resource shares are influenced by bargaining power within the household, and are measures of the relative consumption of household members, therefore reflecting possible consumption inequality within households. Male versus female resource shares are important parameters of collective household models because they speak directly to gender inequality within households. Unfortunately, they are not directly observable, so we cannot directly compute the effect of expanding the child benefit on resource shares. Instead, we have to use a model.

We use difference-in-difference variation to identify the response of collective household model parameters to the change in child benefits. In our view, this approach harnesses the "best of both worlds" in terms of using quasi-experimental variation in child benefits (aka: the treatment) to cleanly identify how resource shares within a collective household model respond to child benefit increases (aka: the treatment effect). In our application, the treatment group is dual parent households with children aged 0 to 18 years who are eligible to receive the child benefit and the control group is couples without children residing within the household and thus, not eligible to receive the CCB. The treatment is an indicator variable for the treatment group post change in the policy in June 2016. The identifying assumption here is that expenditures within households in the treatment and control group follow parallel trends, conditional on covariates. Any changes in the observed time path of households with children after the introduction of the CCB can then be attributed to the policy change.

We use the model of Lechene et al. (2022) to identify preferences and resources shares from reduced-form estimates of linear Engel curves for a private assignable good. The outcome variable of the reduced form model is the "Engel curve", defined as fraction of total household expenditure spent on that good, at a fixed price vector. A private assignable good is one where the person level expenditure or consumption is observable. In this paper, we use clothing as the private assignable good since the data allows us to assign expenditure on clothing to adult men and women separately. Thus our reduced form regression is a regression of the fraction of household expenditures allocated to men's clothing and to women's clothing on the (logged) household budget and other covariates including, importantly, the treatment status of the household and the month in which the data were collected.

The Survey of Household Spending (SHS) is a national survey that gathers information on the spending habits of Canadians. We use data from the SHS from 2014-2017 because these surveys collect 3-month recall data on clothing expenditures and total household budgets for a different sample every month. Thus, in our 4 years of data, we have 48 cross-sections of roughly 1000 households each, implying that our difference-in-differences methodology can use month-level time variation to identify the effect of the policy change occurring in June 2016.

While a reduced form difference-in-difference estimation allows us to estimate the treatment effect on observable variables, such as the clothing Engel curve, expenditure and so on, this approach of blending difference-in difference methodology within the estimation of the structural model allows us to estimate the treatment effect on unobserved objects. Our unobserved objects of interest are: individual preferences for different goods and the resource shares of adult men and adult women within the household. The collective household model we use relates these unobserved structural parameters to the observed reduced form coefficients from these diff-in-diff regressions.

Our findings are easy to summarize. First, although the child benefit had the word "child" right in it, and the increase in benefits was very substantial more than doubling the child benefit received by most households, we see no evidence that the policy change affected preferences. Second, we find evidence that the policy increased women's resource shares, but only in owner-occupied households. We provide some speculation about why owners might be affected more than renters in the final section of the paper.

2 Past Research

Existing literature has studied how changes in child benefits affect behavior. Najjarrezaparast and Pendakur (2021) find that the increase in the child benefits increased overall consumption. Our approach allows for impacts on total consumption that are heterogeneous across households because we condition on the total household budget in our reduced form. They also suggest a possible effect of the change in the labeling as it increased expenditure on children's clothing, but not adults' clothing. We pursue this by identifying the impact on preferences, but find no evidence of such an effect. Finally, they find heterogeneous treatment effects across renters and homeowners, and so we allow for that in our work as well.

Kooreman (2000) uses exogenous income from child benefits in Netherlands and finds that the marginal propensity to consume child's clothing from child benefits is higher than from other income sources. As the result holds for both two parents and single parent households, it suggests that it is the labeling effect of the child benefit that drives the change in marginal propensity to consume for children, rather than the role of the recipients. The labeling of the benefit creates a moral obligation for the parents, mother or father regardless, to spend it on children's good. However, they do not separately allow for an effect on bargaining power.

With our model, we can distinguish between changes in behavior that stem from preference changes (due, e.g., to the moral obligations surrounding child benefits) and those that stem from changes in bargaining power. While our model does not estimate change in preferences for children's goods specifically, we do not find any overall changes in preferences towards adult's clothing arising from the introduction of the newly labeled CCB.

Bargaining power is often measured in the literature based on individuals' survey responses on questions about decision making within the household on reproduction, division of labor, health, social life, children's education and upbringing, finances and so on (Conference of European Statisticians Task Force, 2021). However, this may be erroneous due to differences in perception about contributions to decision making and may vary by contextual factors such as gender itself as illustrated in the findings by Acosta et al. (2020). Hence, our approach overcomes the issue of measurement error from unobservable biases by measuring intra-household bargaining power using structural estimates of resource shares within the household.

The CCB transferred money to the primary caregiver, that is, the mother in a dual parent household. Increasing the individual income of mothers may have increased their bargaining power with respect to fathers. Further, since child benefits follow children, and since mothers are more often custodial parents following divorce, this policy also enriched the outside option of married mothers. Therefore, one of our main focus is estimating the effect of this change on the resource shares of the mother and father within the household. We do find significant increases in resource shares for females within home-owning households. We provide two possible explanations for the heterogeneity in the treatment effect across homeowners and renters - the first explanation hinges on changes in the outside and inside option for women and the second explanation hinges on the difference in marginal price of shelter for homeowners and and that for renters.

To the best of our knowledge, our paper is one of the first to study the effect of policy reform in child benefits on adult's preferences and resource shares using a structural model of the household (García (2022) uses a similar methodology to study a wage subsidy in India.) Structural models to study the effect of child benefits and child care has previously mostly focused on models of fertility and women's labor supply(McNown and Ridao-cano, 2004; Ribar, 1995; Brink et al., 2007). Collective household models incorporating child benefits within the estimation has used exogenous income from the benefit to test the income pooling hypothesis and the effect of targeting transfers to women (Lundberg et al., 1997; Alderman et al., 1995). These studies reject the income pooling hypothesis and suggest that resources controlled by women generally benefit the children. We contribute to this literature of structural models by using the change in the child benefit policy to implement a difference-in-difference methodology within the collective household model. This allows us to estimate the treatment effect of a change in the policy on structural parameters defining adults' preferences within the household.

Our findings contribute to the vast literature on the effect of targeting resources towards women. Existing literature has shown that a shift in control of child allowance from fathers to mothers due to a policy change in the UK led to an increase in expenditure on women's and children's clothing (Lundberg et al., 1997), increased expenditure on food and a more nutritious diet (Armand et al., 2020) and women empowerment(Almås et al., 2018). Attanasio and Lechene (2014) uses the targeted cash transfers of PROGRESA, a welfare program in rural Mexico, as a distribution factor and shows that the collective model can be used to explain the impact of the program on the structure of food expenditure and also cannot reject efficient decision making within the household. Our findings align with the literature as we find that the targeted child benefit results in increased resource shares of women, except for the nuanced finding that this increase is significant only among homeowners. Given the previous finding in literature that resources controlled by women tend to benefit children, it suggests that the policy change can be beneficial for children. As our paper provides further insight that resource shares increase only among homeowners and not renters, it suggests further research is required on the heterogeneous effects of targeting transfers based on home ownership.

Our paper also contributes to the literature that analyzes the effect of changes in child benefit policies. A large body of literature focuses on how such policy changes affect expenditure within the household. Studies have found that changes in child benefit policies, such as increased amount of benefits, and changes in its structure increase expenditure on children or bring about improvement in the environment for children and thus their physical and mental health (Milligan and Stabile, 2009, 2011; Kooreman, 2000; Hener, 2017). In response to the CCB, Najjarrezaparast and Pendakur (2021) shows that rental-tenure households with below median household income increased their annual consumption by roughly \$3000 (\$1400 on shelter, \$700 on food and \$300 on clothing). Further, the paper finds mild evidence of households with more children increasing spending on shelter by much more than those with fewer children. Given these existing findings, our paper focuses on how the changes in the child benefit policy affect preferences and resource shares of the adult female and male within the household. This can be a potential mechanism driving the changes in expenditure on children found in the existing literature. We find little or no evidence of changes in preference from labeling of the child benefit, while there is a sizable increase in women's resource shares due to targeting the benefits towards females. The latter effect is specific to home-owning households and not renters. Our findings therefore suggest that firstly, the targeting of the policy as opposed to the labeling can have a more beneficial impact on children; secondly, the impact on children's welfare can vary based on home ownership which should be brought into consideration when making policy reforms.

2.1 Canada Child Benefit Policy

In July 2016, the Government of Canada introduced the Canada Child Benefit (CCB), a tax-free transfer to families with children conditional on income levels. Previously, there was a complex system of child benefits provided through the Universal Child Care Benefit, the Canada Child Tax Benefit, and the National Child Benefit. The introduction of the CCB resulted in all the benefits being combined under the single label of the Canada Child Benefit. Though the benefits are not required to be spent directly on the children, the labeling of the benefit as child benefit could lead to adults feeling morally obligated to direct the benefits received towards the child.

The CCB led to a significant rise in child benefits, the maximum benefit being $6,400 for children under six and $5,400 for children aged 6 to 17, payable to families with net incomes below $30,000. At higher family incomes, the benefit is reduced at claw-back rates that vary with the number of children and income bins. The increase in child benefits was large for the households below the median of the income distribution with them receiving an additional amount of approximately $2,300 per child per year (Government of Canada, 2016).

The CCB essentially plays the role of a basic income scheme for households with children. For instance, a household with zero income would receive around $6,000 per child annually regardless of their employment status under the CCB. When that same household starts earning some market income, the amount of benefits they receive remain the same unless the income exceeds $30,000 per year. After that, their CCB is "clawed back" based on their income levels until the household earns an income in excess of $150,000 after which they no longer receive benefits.

Furthermore, the CCB is paid to the parent who is considered the primary caregiver of the child. As per CRA (2019), if a household has two individuals of the opposite sex who are spouses or common law partners residing along with the child(ren), the female parent is considered the parent who is primarily responsible for the care of the children at home and the female parent receives the CCB unless notified otherwise. Hence, as we do not have data on exceptions of households where the male parent receives the CCB, in this paper, we assume that in a dual parent household with children, the female parent is the one receiving the benefits. If anything, this assumption underestimates our results of the effect of the CCB on bargaining power of the parents.

Therefore, given these features, the CCB can affect within household expenditure shares in at least three ways: (1) budget effect: due to the significantly increased amount of benefits, it will have a direct impact on the household budget; (2) labeling effect: as the entire amount is now labeled child benefit, it may directly shift preferences of parents regarding how they spend the transfer; (3) targeting effect: finally, since the benefit is paid to females in dual parent, male-female households, the CCB can have an effect on the intra-household bargaining power and resource shares.

In the next section, we introduce the structural model that allows us to decompose the treatment effect into these three separate channels - budget, preferences and resource shares. Section 4 describes the dataset used for the the empirical analysis. We then provide an analysis of the pre-trends in Section 5 for ensuring a valid comparison group for implementing the difference in difference methodology, followed by the estimation results in Section 6. Finally, we discuss potential explanations for the findings in Section 7 and conclude in Section 8.

3 Model

We use the efficient collective household model of Browning et al. (2013) (which we will refer to as BCL) on which we impose the identifying restrictions of Dunbar et al. (2013) (referred to as DLP from hereon) and use the linear estimator from Lechene et al. (2022) (hereafter referred to as LPW). In an *efficient* collective household model, the members of the household are assumed to reach the Pareto frontier, and therefore the household optimization may be represented as the sum of a set of decentralized decisions, just like in general equilibrium theory. We may therefore think of the household as creating budget constraints for each household member, and each individual making decisions against their personal constraint.

In BCL's model, these shadow constraints are characterized by individual-level shadow budgets and a household-level shadow price vector. The *resource share* of a person is defined as their shadow budget divided by the full household budget, that is, their share of the household budget.

Given these shadow budget constraints, each individual decides what quantities to demand, and the household satisfies all these demands by buying a sufficient set of goods in the market. The shadow price vector differs from the market prices because some goods, like transportation and shelter, are shareable and therefore their shadow prices are lower than their market prices. Other goods, like clothing, are not shareable and therefore their shadow prices are the same as their market prices. When the household goes to the market, it has to buy the sum of what individual members want for non-shareable goods. But, they the household need not buy the sum for shareable goods; it can satisfy all individual demands with less than that because individuals share consumption (for further detail, see LPW). The identification and estimation methodologies of DLP and LPW allow for any shadow price vector, and therefore any degree of sharing for any good, but they do not identify the shadow price vector within households.

In contrast, the methodologies of DLP and LPW do identify the resource shares of each household member. These are not equal across household members due to differences in bargaining power, and have a one-to-one correspondence with Pareto weights on individual utilities in the household's primal maximization problem.

DLP shows how to identify the resource shares of each household member using Engel curves for *assignable private goods*, defined as goods that are shared and whose quantities are observed for each household member, such as clothing for male and female. DLP shows that resource shares are identified by such data if resource shares do not vary with total expenditure and preferences are similar—but not identical— across people (SAP). LPW shows that if preferences are characterised by linear Engel curves, then the entire model has a linear reduced form where resource shares are given by nonlinear functions of reduced form regression coefficients on (logged) budgets.

An important difference between our work and that of LPW (and DLP) is that they identify children's resource shares as well as those of adults. In contrast, in this work, we focus on the resource shares of adult men and women. We do not identify children's resource shares. Instead, we focus on the fraction of adult consumption that goes to adult women, which is a measure of gender disparity in within-household consumption.

The reason we pursue this target—the woman's share of adult resources—is data driven: observed Engel curves for children's clothing violate the identifying restriction used by LPW. In particular, whereas clothing is a luxury for adults, it is a necessity for children, so the methodology of LPW to identify their resource shares cannot be applied. Thus, although the model has resources that are allocated to children, we do not identify the size of the children's resource share.

3.1 Structural Model

This section details the notation and setup of LPW, adapted to our difference-in-difference setting. Let $i = m, f$ index adults (male and female respectively) within the household. Let $N = \sum_i N_i + N_c$ be the total number of individuals in a household where N_c is the number of children within the household.

We assume that decision making is carried out by adults, and children are considered as attributes of the household, or, equivalently, spending on children is a non-assignable private good. We choose this specification because the model requires that the Engel curves of all individual types have slopes in the same direction. But, in the population we are studying (Canadian households), the Engel curves for clothing have slopes with same signs for adult male and female, but the opposite sign for children's clothing. In particular, clothing is a luxury for adults and necessity for children. Our two-person model model is a world where adults share household resources, and adults spend some of their resources on their children. Children in this model do not have decision power. [1]

Let y denote the observed household budget. The share of household budget allocated to adult i is denoted η_i. These resource shares add up to 1, so that $\sum_i \eta_i = 1$. They can depend on household budgets, prices and other factors. Following DLP, we assume that the resource shares do not depend on the budget[2], that is, $\eta_i(y) = \eta_i$. Furthermore, we estimate the resource shares at a fixed price vector $\mathbf{p}$ as in DLP and LPW[3]. Each adult, $i = \{m, f\}$, within the household gets a personal budget equal to $\eta_i \cdot y$ which is an unobserved shadow budget based on their resource share and the total household budget[4].

Assignable goods are those for which we can observe the expenditure on or the quantity consumed of, by each type of individual. In this paper, we use clothing as an assignable good where expenditure on clothing for males and females is separately observed. Let ρ indicate the price of person i's assignable good and q_i indicate the quantity purchased of person i's assignable good. Here, we assume that the price of each person's assignable good is the same, but that assumption can be relaxed (see Lechene et al 2022). In the data, we observe household level expenditure on assignable goods, ρq_i, and household-level total expenditure, y. Let $W_i = \rho q_i / y$ be the observed household-level Engel curve for clothing of adult i.

[1] An alternative version of the model, also consistent with the estimator, is a world in which children consume an unidentified fraction of the households resources, leaving the rest for adults to divide according to the estimated male and female resource shares. We would still interpret the estimated resource shares as revealing gender inequality (amongst adults) within the household. See Blundell, Karlajeinen, Lechene and Pendakur (2024) for details.

[2] There is some empirical evidence in the literature that supports this assumption (Cherchye et al., 2015; Menon et al., 2012). Note that we allow the resource shares to depend on other variables - preference shifters and distribution factors. Since we can condition on these variables, we suppress the conditioning here for simplicity.

[3] We do not observe market prices, and are thus unable to estimate shadow prices, that is, the within-household prices of consumption that accounts for economies of scale.

[4] Our estimation is restricted to households with one adult male and one adult female and thus, the shadow budget does not have to be adjusted for number of individuals of each type $i = \{m, f\}$

Let $w_i(y)$ be the Engel curve function of adult i for clothing. This is the unobserved function determining what an individual's Engel curve would be if they faced a budget constraint defined by their personal budget and the household shadow price vector. BCL show that

$$W_i = \eta_i(y) w_i(\eta_i(y) y) = \eta_i w_i(\eta_i y)$$

where the first equality is from BCL under the assumption that shadow prices are linear market prices and the second equality follows from the assumption that resource shares don't depend on the household budget y.

Since our objective is to see how resource shares respond to the child benefit change (among other things), we now add covariates other than the budget y to the model. Define $\mathbf{z} = [\mathbf{s}\ \mathbf{B}]$ as a vector of preference shifters where $\boldsymbol{s}$ is a vector that include demographics and other factors that affect both preferences and resource shares and $\mathbf{B}$ is a vector of difference-in-difference regressors. As mentioned earlier, as Najjarrezaparast and Pendakur (2021) finds heterogeneous treatment effects across homeowners and renters, so define a Renter dummy R included in the preference shifters $\mathbf{s}$. We have $\mathbf{B} = [K \quad P \quad T \quad T \times R]$ where K is an indicator variable for having children (kids) eligible for the child benefit, P is a dummy indicating calendar time following the change in the child benefit policy (post-treatment), T is an interaction term between K and P, and $T \times R$ is an interaction of that treatment effect with the renter dummy (R).

Dual parent households that do not receive the child benefit policy include households without children and act as the control group ($K = 0$). Couples with children eligible for the child benefit policy make up the treatment group ($K = 1$) such that for this group, T is equal to zero in the period before the policy change and is equal to 1 after the policy change. The dollar value of the CCB received by each family depends on the number of children and income levels of the household. Its dependence on the age of children is relatively small. In contrast, the CCB is roughly linear in the number of children (that is, its value for a household with 2 children is twice that of a household with 1 child). We generally pool all households with children. We do some robustness checks to show that treatment effects are not very heterogeneous with respect to the number of children. However, they are quite heterogeneous with respect to homeowner versus renter status. The interaction term ($T \times R$) allows identification of the treatment effect on renters [5].

DLP and LPW let the individual Engel curve functions for individuals i be in the PIGLOG class (Muellbauer 1975) and add the argument z to the Engel curve function so that individual Engel curve functions are linear in the log of the household budget for every z: $w_i(y, \mathbf{z}) = \alpha_i(\mathbf{z}) + \beta_i(\mathbf{z}) \ln y$. They further add the similar-across-people restriction on preferences (see DLP) resulting in $\beta_i(\mathbf{z}) = \beta(\mathbf{z})$.

Here, we will be slightly more restrictive and let these Engel curve functions be given by the Almost Ideal demand system of Deaton and Muellbauer (1980), so that the slope term β_i does not depend on demographics z. This gives

$$w_i(y, \mathbf{z}) = \alpha_i(\mathbf{z}) + \beta \ln y.$$

Here, the levels of Engel curves are different for males and females i and depend on demographics z, but the budget response β is the same for all people i in all households. Substituting in, we get the structural equation

$$W_i = \eta_i(\mathbf{z})[\alpha_i(\mathbf{z}) + \beta(\ln y + \ln \eta_i(\mathbf{z}) - \ln N_i)] \tag{1}$$

where $\eta_i(\mathbf{z}) = \eta_i(p, \mathbf{z})$ is the resource shares at fixed prices p written to depend on demo-

[5] We do not include interaction terms of the renter dummy with indicator for households with children ($K \times R$) and indicator variable for calendar time post policy change ($P \times R$). This is because we test for joint significance of the coefficients of these terms in our model and get a chi-square statistic such that we cannot reject the null hypothesis that the terms are jointly not significantly different from zero (test statistics provided in Table B21). As a robustness check, we also provide results including these interaction terms in the model (results in Appendix B.8). There is still a positive significant treatment effect on the bargaining power of females among homeowners, but the difference in the treatment effect between homeowners and renters becomes insignificant. The treatment effect on the preference parameters remain qualitatively similar.

graphic controls $\mathbf{z}$. We call this structural equation because all of its parameters are elements of the structural collective household model.

3.2 Linear Reduced Form

LPW show that this model has a linear reduced form, which we now describe. Although the model is identified with all shifters $\mathbf{z}$ affecting both preferences and resource shares, it is helpful to reduce collinearity by breaking $\mathbf{z}$ into three pieces. Let $\mathbf{z} = [\mathbf{s}\ \mathbf{B}] = [\mathbf{z_c}\ \mathbf{z_s}\ \mathbf{B}]$ such that preference shifters $\mathbf{s}$ are distinguished as $\mathbf{z_c}$ and $\mathbf{z_s}$. The vector $\mathbf{z_s}$ includes preference shifters that affect both preferences and resource shares. In this paper, this includes ages of the household members, household size and an indicator for being a renter. The other preference shifters ($\mathbf{z_c}$) only affect preferences and not resource shares. These include control variables for year, month, province of residence and city size which may be plausibly excluded from resource shares. We provide tests[6] to show that variables in $\mathbf{z_c}$ indeed do not have any effect on the budget shares through the household budget[7].

We add an error term ε_i to equation (1) and express it as a linear reduced form:

$$W_i(y, \mathbf{z}) = a_i(\mathbf{z_c}\ \mathbf{z_s}\ \mathbf{B}) + b_i(\mathbf{z_s}\ \mathbf{B}) \ln y + \varepsilon_i \tag{2}$$

where

$$a_i(\mathbf{z}) = \eta_i(\mathbf{z_s}\ \mathbf{B})[\alpha_i(\mathbf{z_c}\ \mathbf{z_s}\ \mathbf{B}) + \beta \ln \eta_i(\mathbf{z_s}\ \mathbf{B}) - \beta \ln N_i]$$

and

$$b_i(\mathbf{z_s}\ \mathbf{B}) = \eta_i(\mathbf{z_s}\ \mathbf{B})\beta.$$

We call this the linear reduced form because it is the relationship between observed objects

[6]We replicate the estimation including these preference shifters in the slope term, that is, $\mathbf{z_s}$, and show that the estimated coefficients of these variables are jointly insignificant.

[7]Note that these restrictions are not required for identification of the parameters in the model and are only imposed for simplicity in estimation.

(log-budgets, y, covariates z and budgets shares W) that can be recovered via linear regression methods, e.g., OLS, 2SLS or GMM regression of budget shares of covariates, log-budgets and covariates interacted with log-budgets.

Since $\Sigma_i \eta_i(\mathbf{z_s}\ \mathbf{B}) = 1$, we have

$$\beta = \Sigma_i b_i(\mathbf{z_s}\ \mathbf{B})$$

So, we can rearrange to get

$$\eta_i(\mathbf{z_s}\ \mathbf{B}) = \frac{b_i(\mathbf{z_s}\ \mathbf{B})}{\beta} = \frac{b_i(\mathbf{z_s}\ \mathbf{B})}{\sum_i b_i(\mathbf{z_s}\ \mathbf{B})} \qquad i = \{m, f\} \tag{3}$$

The intuition for the identification of resource shares in this model is the following. The reduced form parameter $b_m(\mathbf{z_s}\ \mathbf{B})$ gives the response of household spending on the man's assignable good to a change in the household budget, and $b_f(\mathbf{z_s}\ \mathbf{B})$ gives the response of household spending on the woman's assignable good. The reduced form parameters depend on the resource share of the person and the preference parameter β. If the response of household spending is greater for the man's assignable good, since their (unobserved) preference parameters are the same, it must be because he has the higher resource share. Thus, the relative responses to changes in the household budget of household spending on assignable goods for different people identify the resource shares of those people.

We approximate the model by letting

$$a_i(\mathbf{z}) = a_i(\mathbf{z_c}\ \mathbf{z_s}\ \mathbf{B}) = a_{i0} + a_{iK}K + a_{iP}P + a_{iT}T + a_{iz_c}\mathbf{z_c} + a_{iz_s}\mathbf{z_s} \tag{4}$$

and

$$b_i(\mathbf{z_s}\ \mathbf{B}) = b_{i0} + b_{iK}K + b_{iP}P + b_{iT}T + b_{iz_s}\mathbf{z_s} \tag{5}$$

Since the structural parameter β is independent of $\mathbf{z}$ we impose the following linear restrictions:

$$\sum_i b_{iT} = \sum_i b_{iK} = \sum_i b_{iP} = \sum_i b_{iz_s} = 0 \tag{6}$$

These restrictions imply that the preference parameter governing the budget response of expenditure on clothing share of individuals does not vary with the preference shifters. We impose this restriction for two reasons. First, since the resource shares are estimated from Equation (3), the resource share would be undefined if β, the denominator came too close to zero. This restriction reduces the possibility of the denominator $(b_{m0} + b_{f0})$ being close to zero. Furthermore, the marginal effect of a covariate on the resource share does not depend on values of the covariates ($\mathbf{z_s}$ and $\mathbf{B}$). For robustness check, we provide the estimation results without imposing these restrictions in Appendix B.3 which show that estimates do not differ much and the results hold qualitatively.[8].

Given these linear restrictions Appendix B.3, we have $\sum_i b_i(\mathbf{z_s}\ \mathbf{B}) = b_{m0} + b_{f0}$, implying the following parametric structure for resource shares which is linear in the variables:

$$\eta_i(\mathbf{z_s}\ \mathbf{B}) = \frac{(b_{i0} + b_{iK}K + b_{iP}P + b_{iT}T + b_{iTR}T \times R + b_{iz_s}\mathbf{z_s})}{(b_{m0} + b_{f0})}. \tag{7}$$

b_{iT} identifies the treatment effect on the resource shares for owners :

$$\frac{\partial \eta_i(\mathbf{z_s}\ \mathbf{B})}{\partial T} = \frac{b_{iT}}{(b_{m0} + b_{f0})} \tag{8}$$

and for renters

$$\frac{\partial \eta_i(\mathbf{z_s}\ \mathbf{B})}{\partial T} = \frac{b_{iT} + b_{iTR}}{(b_{m0} + b_{f0})}$$

Since, by assumption, β does not respond to the treatment, the only preference effect is

[8]We find no significant treatment effect on $\beta(\mathbf{z_s}\ \mathbf{B})$ when we estimate the model without imposing these linear restrictions from Equation (6) further providing justification for imposing these linear restrictions. Estimates of β and treatment effect on β are provided in Table B8 and Table B11.

through α_i. We solve for α_i as follows:

$$\alpha_i(\mathbf{z}) = a_i(\mathbf{z})/\eta_i(\mathbf{z_s}\ \mathbf{B}) - \beta \ln \eta_i(\mathbf{z_s}\ \mathbf{B})$$

and we identify the treatment effect on preferences by computing the following difference:

$$\alpha_i(T = 1, P = 1, K = 1, \mathbf{z_c}, \mathbf{z_s}) - \alpha_i(T = 0, P = 1, K = 1, \mathbf{z_c}, \mathbf{z_s}) \tag{9}$$

We use Hansen (1982)'s generalized method of moments (GMM) to estimate the system of equations for budget shares of the adults within couples' households, that is, Equation (2) for $i = \{m, f\}$. The model can also be estimated using equation-by-equation ordinary least squares (OLS) or seemingly unrelated regression (SUR). However, we choose to use GMM over SUR since given the restrictions imposed by equation (6), SUR would be exactly identified whereas GMM is overidentified. Thus, using GMM, we can test the validity of the overidentifying restrictions in (6) by computing the Hansen's J statistic. Furthermore, if we expect the household budget to be endogenous and choose to use instrumental variables, the GMM estimator has the same number of degrees of freedom when using exogenous and endogenous regressors. This allows us to compare the two scenarios to determine if instrumenting is necessary by using the Hausman test.

All reported standard errors are clustered by province, the number of children, year and month. This is because firstly, Jones et al. (2019) suggests that since the child benefit policy in Canada not only vary by province, but also by the family size, standard errors should be clustered by province times number of children. Furthermore, seasonal changes usually affect clothing expenditure. So, we further cluster by year and month. This happily has the side effect of circumventing the issue of few clusters (Bertrand et al., 2004) which could otherwise lead to an underestimation of cluster adjusted standard errors.

The linear reduced form (2) illuminates how household Engel curves for clothing vary across the individuals within the household, and in particular identifies the budget response (5) of the household Engel curve for each individual's clothing. The structural model shows that, because we assume similarity across people in the budget response, the resource share is identified by equation (3). This equation says that the resource shares—parameters of the structural collective household model— are identified from the *relative* magnitude of these budget responses. Under the model, if the budget response of the household Engel curve for male's clothing, $b_m(\mathbf{z_s}\ \mathbf{B})$, is larger than that for female's clothing, $b_f(\mathbf{z_s}\ \mathbf{B})$, then the male's resource share is larger. This is true even if the Engel curve for female's clothing lies completely above that for male's clothing, which would occur for example if the level term for the household Engel curve for female's clothing, $a_f(\mathbf{z_s}\ \mathbf{B})$, was much higher than that for male's clothing, $a_f(\mathbf{z_s}\ \mathbf{B})$.

4 Data

We use the Survey of Household Spending (SHS), a national monthly survey with data on household spending, from 2014 to 2019. The survey collects data on household characteristics, spending and savings, housing and dwelling characteristics, income, pensions, spending and wealth. It is primarily used for deriving expenditure weights used in calculating the Consumer Price Index and additionally used for investigating consumer demand behavior. The data are collected using both a questionnaire (interview) and an expenditure diary. The questionnaire is generally used to collect expenditures for more expensive, and less frequently purchased goods and services. The diary is used to collect expenditures for smaller, less valuable items that are purchased more frequently and could be more difficult to recall. However, the diary sample is much smaller and thus, this paper uses data from the interview only.

As described in Najjarrezaparast and Pendakur (2021), there are three features of the SHS that allow us to evaluate how the policy change affected spending. These three features are: (i) they are monthly cross-sections; (ii) they have the exact age of each household member; and (iii) they record spending on clothing for each person as well as total spending of all people on all goods.

To elaborate on these, first of all, each year of the SHS has around 12,000 observations of households, with roughly 1,000 sampled in each calendar month. Thus, we observe repeated cross sections of households at the calendar-month level over 48 months from January 2014 to December 2017. These cross-sections cover 30 pre-treatment months and 18 post-treatment months. Secondly, using SHS information on the birth month and year of every household member, we exactly identify the age of each household member given the month and year of survey. This allows us to identify households eligible for CCB by calculating the number of children aged less than 18 in the month prior to the survey date. Finally, detailed retrospective spending for different expenditure categories is collected. This includes person level spending in the previous 3 months for clothing and footwear plus household-level spending in the past month for consumables like food and in the past year for semi-durables such as household furnishings. We use the person level expenditure data on clothing and footwear as the assignable good. Dividing these spending amounts by the total household expenditure yields Engel curves for each person's assignable clothing (including footwear).

We restrict our analysis to households with one male adult and one female adult (that is, $N_m = 1$ and $N_f = 1$) with a maximum age of 65 years of either adult. The sample comprises of adult couple households with no eligible children (control households) and two-parent households with 1-3 eligible children. We drop households with adult children present. We also drop a small number of households [9] where the number of children one month prior to the survey is not the same as number of children three months prior to survey. The eligibility or the amount received from CCB during the clothing recall period would change for these

[9] The number of households dropped is less than 1% of the sample. We don't report the exact number due to confidentiality requirements of the SHS data agreement.

households.

Total household expenditure is measured as the total of expenditure on food, shelter, transport, health, recreation and other household operating expenses, excluding any form of investment expenditure. Since these variables have different recall periods, we annualize all expenditure items to the annual level. Our static consumer demand model does not allow for savings and investment, so we treat everything as a consumption flow. We exclude transportation investment expenditure on purchase of recreational and all terrain vehicles, automobiles, sports utility vehicles, vans and trucks. We exclude investment expenditure on shelter in the form of mortgage paid on owned principle residence.

For renters, we may equate rental expenditures with the shelter consumption flow. However, the rental flow from consumption is not available for homeowners. Hence, we impute the rental flow from consumption for all households. The imputed rent for a household is the predicted value from a linear regression of rents (for rental-tenure households) on dwelling characteristics. These characteristics are: the number of bedrooms, bathrooms, repairs required, how crowded the dwelling is and the period the dwelling was constructed in, plus year and province dummies. In the main specification, we use imputed rent for both renters and homeowners to ensure that systematic measurement error is not arising from the imputation. However, we provide robustness checks using imputed rent for only homeowners and actual reported rent for renters[10].

Potential endogeneity concerns arise as measures of household expenditure often have measurement error (say, due to recall inconsistency). Additionally, our measure of total household expenditure includes imputed rent for all households which could accentuate this measurement error. Finally, because our dependent variable is $W_i = \rho q_i / y$ is linear in $\ln y$, we

[10]We also ran the GMM estimation without included shelter expenses in the household expenditure to reduce possible measurement error from imputing rent. The reduced form estimates (provided in Table B5) show larger standard errors which suggests that including shelter does not increase measurement error. Furthermore, as expenditure on shelter comprises a large portion of expenditure for Canadian households, we choose to include shelter expenses in our main text specifications.

have that y is on both sides of the equation, which induces endogeneity by construction. To address these endogeneity concerns, we instrument household expenditure with total household income. Household income is less likely to have measurement error, in part because SHS enumerators merge their records with income data from Canada Revenue Agency. We provide the results from Hausman test to evaluate the consistency of the efficient OLS estimator by comparing results with the consistent, less efficient estimates when instrumenting household budget. We drop observations in the bottom and top 1% of the expenditure and income distribution to exclude possible outliers from the sample.

Recall that our demographic shifters are divided into 3 groups. $\mathbf{z_s}$ affect both preferences and resource shares and are: the ages of the man, the woman and the average age of eligible children within the household; an indicator if the household is a renter as opposed to an owner; and, the number of children in the household. $\mathbf{z_c}$ affect only preferences and are: year and month dummies. B is a vector of treatment dummies, and we will generally report "treatment effect" coefficients on the interaction of the "eligible children present" dummy and the "post policy change" dummy and its interaction with the "renter" dummy. The former will be interpreted as the treatment effect on owners, and the latter will be interpreted as the additional treatment effect for renters.

Table 1: Summary Statistics

	All		Treated		Untreated		Treated vs untreated
	Mean	Standard Deviation	Mean	Standard Deviation	Mean	Standard Deviation	Difference
Demographics							
Age: Male	43.51	11.19	40.14	7.36	46.76	13.11	-6.62***
Age: Female	41.29	11.12	37.60	6.75	44.85	13.16	-7.26***
Average age of children	3.57	4.91	7.27	4.72			7.27***
Number of children	0.91	1.04	1.85	0.68			1.85***
Proportion of renter (Renter dummy)	0.24	0.43	0.23	0.42	0.26	0.44	-0.03***
Proportion of households with children	0.49	0.50					
Expenditure in dollar amounts							
Total household expenditure	40,990	14,573	44,503	15,124	37,605	13,163	6898.23***
Expenditure on:							
Food	7,986	3,967	9,240	4,216	6,777	3,287	2462.86***
Household operations	1,646	2,846	1,646	2,842	1,645	2,850	1.30
Clothing	3,035	2,883	3,787	3,053	2,311	2,505	1476.05***
Transportation	11,845	14,647	12,444	14,751	11,268	14,523	1176.09***
Health	2,794	2,586	2,790	2,563	2,799	2,607	-8.71
Recreation	5,044	6,796	5,546	6,908	4,560	6,651	986.22***
Shelter (Imputed rent expenditure)	40,990	14,573	44,503	15,124	37,605	13,163	1383.65***
Total household income	104,842	60,676	108,229	59,642	101,580	61,483	6649.17***
Share in total household expenditure of:							
Adult clothing: Male	0.020	0.024	0.017	0.019	0.022	0.027	-0.005***
Adult clothing: Female	0.031	0.030	0.026	0.025	0.036	0.034	-0.010***
Children's clothing	0.019	0.029	0.038	0.031			

Summary statistics (weighted by the population weights) is provided in Table 1. Columns (1) and (2) report the mean and standard deviation of the variables for the total sample, columns (3) and (4) for the treated population, that is, households eligible for CCB and columns (5) and (6) for the households without children. Columns (7) and (8) provide a t-test of the significance of the difference in these variables across the treated and untreated population.

In the overall sample, average age of males and females is around 44 and 41 years respectively. Average age of children within the treated population is around 7 years and number of children is around 2. The proportion of renters in the total sample, as well as the treated and untreated population is around 23 to 26%. The proportion of households with children, that is, treated population, is around 49%[11]. Columns (7) and (8) show that these demographic characteristics vary significantly across the treated and the untreated population and therefore we ensure controlling for these variables, along with total household expenditure. We also report the breakdown of household expenditure across different sub-categories. Finally, the table presents the share of adult and children clothing in total household expenditure which also varies significantly across the treated and untreated population. This is somewhat expected given the household composition since treated households are likely to direct some spending towards their children away from adult clothing.

5 Pre-trend

In this section, we provide the test for pre-trend, and provide some evidence to support the difference-in-difference strategy. We test whether couples with children eligible to receive the benefits followed the same time trend as couples who are not eligible for the benefits. This test aims to show that our control group serves to identify an appropriate counterfactual time-trend for the treatment group, so that we can estimate the treatment effect of the CCB on spending patterns by comparing the time-trends of treated to untreated households. If we fail parallel trends in the pre-treatment period, it would suggest that parallel trends post-treatment is not reasonable.

For the pre-trend test, first, we restrict the sample to the period prior to the policy change, that is, from January 2014 to July 2016. We then estimate equation (2) using our main estimation strategy, that is - we include imputed rent for all households when measuring household expenditure; cluster standard errors at province, number of children, year and month; and impose summation restrictions on the slope coefficients (Equation 6). We then include interaction terms between indicator variables for year and month. Finally, we include

[11]The unweighted number of households in the sample and the sub-categories of treated and untreated population cannot be disclosed due to confidentiality requirements of the Statistics Canada Research Data Center.

interaction terms between dummy variables for year and month and the indicator variable for being in the treatment group (K). The test for parallel trends is undertaken through a joint test of significance of the coefficient estimates of these latter interaction terms. We include the interaction terms within both the slope and the levels of the budget share equations. The coefficients on these terms represent time trends within the relevant parameters of couples with eligible children relative to the control group.

The test for significance of these coefficients jointly in *both the slope and the level term* gives a sample value of the chi-square test statistic of 211.05 with a p-value of 0.00 which means we can reject the null hypothesis that these interaction terms are jointly zero. This is mostly driven by differencew in coefficients *in the level term*. The sample value of the chi-square test statistic for the hypothesis that the levels follow parallel trends is of 83.24 and has a p-value of 0.025. This suggests that the pre-trend of the level of the Engel curves may not follow parallel trends. Hence, the treatment effect on α_i should be interpreted with caution.

In contrast, the sample value of the chi-square test statistic for the hypothesis that the slope coefficients follow parallel trends is 72.06 with a p-value of 0.14. Hence, we fail to reject the hypothesis that the coefficients of the interaction terms in the slope of the Engel curve are jointly equal to zero suggesting that the treatment group and the control groups have slopes of Engel curves that follow parallel time trends[12]. Thus, we are comfortable using a difference-in-difference methodology to identify treatment effects on the slopes of Engel curves for assignable goods, and then using those slopes to identify the treatment effects of the policy change on resource shares.

[12]Results are similar when the specification does not instrument for household expenditure. When using robust standard errors, for both with and without instruments, we always fail to reject that coefficients of the treatment variable interacted with year and time dummy is jointly equal to zero, for both the slope and the level terms. This provides evidence for parallel trends in the Engel curves of the treatment and control group. Results provided in Table A1

6 Results

6.1 Reduced form estimates

Before discussing the results from the GMM estimation, we first look at the treatment effect of the policy change on log of household budget using an OLS regression (shown in Table 2). The point estimates for the treatment effect on household budget is not significant. While this suggests no increase in total consumption from the additional benefits, it does not say much about possible shift in spending patterns within the household. Potential reasons for no effect on total household expenditure could be that the additional funds are not going towards consumption and instead being used for savings (say, for future expenses of the children) or for other investments (say, upgrades in housing, mortgage payments and so on).

This finding is in contrast to Najjarrezaparast and Pendakur (2021) (referred to as NP hereon), who find a positive significant treatment effect on total household budget among renters and within the total sample, but no significant effect on owners. The difference in our findings can arise for a multitude of reasons. First of all, our measure of household expenditure includes imputed rent while theirs does not. Additionally, our sample is restricted to households with one adult male and one adult female, with or without children. The sample in NP includes households with 1 to 4 adults, with or without children. NP also restricts their sample to those below median income. If we do the same, we similarly see a significant positive treatment effect on the household budgets of renters. In our sample, only 27% are renters (compared to 53% of the sample in NP) which reduces the overall treatment effect on total expenditure. However, unlike NP who seek to identify the treatment effect on total expenditures, we seek to identify the treatment effect on Engel curves conditional on total expenditures, and from there, the treatment effect on resource shares.

Table 2: Treatment effect of CCB on household budget

	Total sample			Below median income		
	(1) Overall	(2) Renters	(3) Owners	(4) Overall	(5) Renters	(6) Owners
Treatment effect on log of household budget	0.001	0.021	-0.003	0.006	0.048**	-0.010
	(0.012)	(0.027)	(0.013)	(0.009)	(0.019)	(0.011)

Standard errors clustered at province, the number of children, year and month in parentheses
*** p<0.01, ** p<0.05, * p<0.1.
Household budget includes imputed rent for homeowners as in out main specification

We now present the results from the GMM estimation of the system of equations comprised of adults' budget shares within the households (Equation (2) for $i = \{m, f\}$). As mentioned in previous sections, our main specification uses imputed rent for both owners and renters[13]. A renter dummy and an interaction term between the renter dummy and indicator for treatment is included to allow for heterogeneous treatment effects between homeowners and renters[14]. Our specification also uses log of income as an instrument for log of household expenditure[15]. We present results for both specifications - with and without instruments along with Hausman test results for parameter estimates. Our main specification clusters standard errors by province, number of children and year-month[16]. Finally, we impose the linear restrictions from Equation (6) on the slope term[17]. The reference group for the estimation, that is, when all covariates in $\mathbf{z}$ are equal to zero, refers to households in Ontario, in a population center of 100,000 or over, in June 2016 with two children where the children's average age is 10 and adult's age is 40[18].

[13]Results using imputed rent for only owners and actual rent for renters remain qualitatively the same (provided in Appendix B.7).

[14]Results excluding the renter dummy and interaction term is provided in Appendix B.1.
We also provide the results when additionally including interaction terms of the renter dummy with indicator variables for households with children, and months post policy change in Appendix B.8. The treatment effect on the preference parameters and the bargaining power of homeowners is still robust across specifications. However, the difference in treatment effect between owners and renters is not robust across different specifications when we include these interaction terms.

[15]Results from using squared log of income as instruments for household expenditure are provided in Appendix B.6.

[16]Results using only robust standard errors are qualitatively similar and provided in Appendix B.4 and Appendix B.5

[17]Results from relaxing this restriction are provided in Appendix B.3.

[18]For simplicity, we refer to this as $\mathbf{z} = \mathbf{0}$ without making the distinction between $\mathbf{z_c}$ and $\mathbf{z_s}$.

Table 3: Reduced form estimates of constant and slope of budget share

	IV estimates		OLS estimates	
	(1) female	(2) male	(3) female	(4) male
a($\mathbf{z} = 0$)	0.020***	0.015***	0.023***	0.016***
	(0.002)	(0.002)	(0.002)	(0.001)
b($\mathbf{z} = 0$)	0.034***	0.012***	0.023***	0.013***
	(0.005)	(0.004)	(0.002)	(0.002)
Instrument for log of budget	Yes (with log of income)		No	

Standard errors clustered at province, the number of children, year and month in parentheses
*** p<0.01, ** p<0.05, * p<0.1.
IV estimates refer to GMM estimation instrumenting household budget/expenditure with income. OLS estimates refer to GMM estimation without instrument for household budget.

We first present the reduced form GMM estimates in Table 3. The coefficients in the system of equations of the Engel curves for $i = \{m, f\}$ are evaluated for the reference group. The constant term (a_i), that is, the level of the Engel curve is significant for both male and female. The slope of the Engel curve (b_i), is positive and significant at the 1% level for both adults. This suggests that clothing is a normal good for adults in Canadian households. For the identification of resource shares, we require the Engel curve to have non-zero slopes in the same direction for both adults. For our sample, among the reference group, we have positive slopes for both adults and so this condition is satisfied. Further, for estimation of the resource shares, we need the sum of b_m and b_w (that is, β) to be significantly different from zero as can be seen from Equation (7). This condition is also satisfied as the sum of the two coefficients is positive and statistically significant at the 1% level. This gives us reassurance that our model of resource shares is identified.

Next, we look at the coefficient estimates of the treatment effect from the reduced form regression (Table 4). Columns (1)-(3) provide results for the specification including instruments for log of household budget and columns (4)-(6) provides the results without instrumenting. The Hausman test statistic, which tests the consistency of the estimator without instrumenting for household expenditure against the less efficient estimator which uses the instrument is reported in column (7). The Hausman test statistic for the coefficient of the treatment effect on both the level and the slope for homeowners is such that we reject the null hypothesis at the 5% significance level. In other words, we reject the null hypothesis that both these estimators are consistent. Therefore, we lean towards using the specification instrumenting for household expenditure as our main specification and discuss those results.

Table 4: Reduced form estimates: Treatment effect

	IV estimates			OLS estimates			
	(1) female	(2) male	(3) diff	(4) female	(5) male	(6) diff	(7) H-stat
Homeowner: Treatment effect on level (a)	0.002 (0.001)	-0.001 (0.001)	0.003** (0.001)	0.001 (0.001)	0.000 (0.001)	0.001 (0.001)	6.812
Renter: : Treatment effect on level (a)	0.001 (0.002)	0.000 (0.002)	0.001 (0.002)	0.001 (0.002)	-0.001 (0.001)	0.002 (0.002)	0.901
Homeowner: Treatment effect on slope (b)	0.011*** (0.004)	-0.011*** (0.004)		0.004* (0.002)	-0.004* (0.002)		11.377
Renter: Treatment effect on slope (b)	-0.013** (0.006)	0.013** (0.006)		-0.007*** (0.003)	0.007*** (0.003)		0.995
Instrument for log of budget		Yes (with log of income)			No		

Standard errors clustered at province, the number of children, year and month in parentheses *** $p<0.01$, ** $p<0.05$, * $p<0.1$.
IV estimates refer to GMM estimation instrumenting household budget/expenditure with income. OLS estimates refer to GMM estimation without instrument for household budget.

The upper left panel of Table 4 shows that the estimated treatment effect on the level term for both male and female Engel curves in not significantly different from zero for any household. The column labelled "difference" gives the difference between estimated treatment effects on male clothing and female clothing Engel curves. Here, we see that the treatment effect on the level term is significantly higher for the female's Engel curve as opposed to the male in home-owning households. The lower left panel gives the estimate treatment effects on the slope of Engel curves. Here, we see that treatment effect on the slope term of the female's Engel curve is positive and significant for homeowners while it is negative and significant for renters. Given the linear restriction (6), the treatment effect is exactly the reverse for the males' Engel curves, so we do not report estimated differences. These results are evident for both instrumented and non-instrumented (aka: OLS) specifications given in the right hand panels. The change in CCB policy resulting in changes in the slopes of the Engel curves suggests that it may have affected resource shares.

6.2 Estimates of Structural Parameters

We now move on to the estimates of the structural parameters: preference parameters and resource shares, and the treatment effect on them, as illustrated in Table 5. Once again, columns (1)-(3) presents results from our main specification, instrumenting for household expenditure and columns (4)-(6) present results without the instrument. Prior to discussing the parameter estimates, consider the performance of the IV estimates over the OLS estimates. First, column (7) presents the Hausman test statistic for each parameter which tests whether the OLS estimates of the parameters are consistent in comparison to the relatively less efficient IV estimates. The H-stat suggests, particularly for the estimates of resource shares and the treatment effect on resource shares among homeowners, that the exogenous specification (OLS) may not be consistent. We therefore use the log of income as an instrument for the log of household budget. Furthermore, the bottom rows of Table 5 presents the Hansen's J-statistic for testing the validity of the overidentifying restrictions. For the instrumented GMM estimates, we fail to reject the null hypothesis that all the overidentifying restrictions are jointly valid. For the exogenous GMM estimates (where we use the observed budget as an instrument for itself), we still have overidentifying restrictions due to the linear restriction imposed in (6), but we reject the null hypothesis at 5% significance level that the restrictions are jointly valid. Together, we take from this that dealing with endogeneity is important and that household income is a tolerably good instrument for observed household spending.

We also test whether the coefficient of the variables (year, month, province and city size) excluded from the slope term ($\mathbf{z_c}$) is jointly zero had they not been excluded. We fail to reject the null hypothesis which provides justification for excluding certain preference shifters from the slope term as they do not affect resource shares, but only preferences. Finally, we also test for the linear restrictions imposed in (6) by testing the null hypothesis that the coefficients of the covariates in the female's Engel curve is jointly equal to that in the male's Engel curves, and once again, fail to reject this hypothesis when using IV estimates. This gives us confidence in imposing these linear restrictions to enable us to estimate well behaved resource shares[19].

Focusing first on the preference parameter (α_i), for both homeowners and renters the parameter estimates are significant and positive for both male and female. The difference in the parameter estimates across male and female within household is not significantly different from zero. This suggests that the male clothing Engel curves and female clothing Engel curves have similar levels. For renter households, the policy change does not affect the preference parameter α_i. For home-owning households, the policy change results in a decrease for the female and increase for the male, both significant at 1% confidence level. The decrease in α_f relative to the increase α_m is also significantly higher, which may be indicative of a preference shift of the mother towards other expenditures (potentially children's goods) related the labeling aspect of the Canada Child Benefit policy. These results are however not robust across the different specifications as can be seen in Appendix B. But, the combined effect of the change in the preferences ($\alpha_m + \alpha_f$) is 0.023 and is not significantly different from zero. Overall, we do not find strong evidence of any overall effect on the preferences of the parents, suggesting that the new label of the benefit did not shift preferences away from adult's clothing significantly.

A possible reason could be that even though the CCB is an umbrella label for child benefits,

[19]Note that given the linear restriction in (6), we will not be observing any treatment effect on β. For robustness check, we relax this restriction and report the results for all parameters (α_i, β and η_i) in Table B8. We find no significant effect on β further increasing our confidence in the specification imposing the restriction.

Table 5: Parameter estimates

	IV estimates			OLS estimates			
	(1) female	(2) male	(3) diff	(4) female	(5) male	(6) diff	(7) H-stat
Homeowner: α_i (at $z = 0$)	0.074***	0.054***	0.020	0.050***	0.071***	-0.021	2.025
	(0.013)	(0.009)	(0.022)	(0.006)	(0.009)	(0.014)	
Renter: α_i (at $z = 0$)	0.086***	0.062***	0.024	0.054***	0.085***	-0.031*	1.347
	(0.021)	(0.014)	(0.034)	(0.007)	(0.013)	(0.018)	
Homeowner: Treatment Effect on α_i (at z=0)	-0.036***	0.059***	-0.096***	-0.009	0.021*	-0.030*	4.873
	(0.014)	(0.020)	(0.031)	(0.006)	(0.012)	(0.018)	
Renter: Treatment Effect on α_i (at z=0)	0.010	-0.005	0.015	0.017*	-0.021	0.038*	0.057
	(0.033)	(0.019)	(0.051)	(0.010)	(0.014)	(0.023)	
Homeowner: η_i	0.462***	0.538***	-0.077	0.579***	0.421***	0.158	6.518
	(0.067)	(0.067)	(0.134)	(0.049)	(0.049)	(0.097)	
Renter: η_i	0.450***	0.550***	-0.100	0.605***	0.395***	0.209*	3.827
	(0.098)	(0.098)	(0.196)	(0.058)	(0.058)	(0.117)	
Homeowner:Treatment Effect on η_i	0.247***			0.098*			7.019
	(0.080)			(0.056)			
Renters:Treatment Effect on η_i	-0.029			-0.103			0.346
	(0.147)			(0.076)			
Treatment Effect on η_i: Homeowner vs Renters	0.276**			0.201***			
	(0.138)			(0.071)			
Hansen's J chi2 (dof=9)		13.978			20.503		
p-value		*0.123*			*0.015*		
Test for exclusion on slope		36.216			23.976		
p-value		*0.167*			*0.730*		
Test for linear restriction		13.085			21.417		
p-value		*0.159*			*0.011*		
Instrument for log of budget		Yes (with log of income)			No		

Robust standard errors clustered at province, the number of children, year and month in parentheses *** p<0.01, ** p<0.05, * p<0.1.
IV estimates refer to GMM estimation instrumenting household budget/expenditure with income. OLS estimates refer to GMM estimation without instrument for household budget.

the previous child benefits (Universal Child Care Benefit, Canada Child Tax Benefit and the National Child Benefit) all still included the phrase *"child benefit"*. So perhaps this change in label was not very salient or important. Thus, the policy change did not shift preferences away from adult's clothing and towards children's clothing through the labeling channel.

Moving our focus to the estimates of the structural parameters for resource shares (η_i), the rows labeled "homeowner η" and "renter η" give the estimates of resource shares for households with children prior to treatment. The point estimates show that females have a resource share of 46% (45%) in home-owning households (renter households) while males have a higher share of 54% (55% in renter households). These resource shares are not very precisely estimated: we cannot reject the hypothesis that resource shares are equal (50%) for males and females. The estimates of the resource share is similar to what has been found in the literature regarding resource shares of female adults in developed countries (Lise and Seitz, 2011; Bargain and Donni, 2012; Bargain et al., 2022).

The rows labeled "homeowner treatment effect" and "renter treatment effect" give the causal estimate of the policy change on resource shares. We find a significant and sizable increase of around 25% in the resource shares of females due to the introduction of the CCB. The magnitude is quite large and would lead to female adults consuming 70% of the resources post treatment.

The OLS estimates are of a smaller magnitude of around 10%, and much more precisely estimated. The Hausman tests reject sharply for the overall treatment effect, but not so much for the interaction of that treatment effect with renter status. Recent work by Hansen (2017) suggests a way to combine the instrumented and uninstrumented estimates. Given that the H-statistic is not too large, the true magnitude of the treatment effect on resource shares is likely somewhere within the confidence sets of the IV and OLS estimates. Using the Stein-like 2SLS estimator of Hansen (2017), we estimate the shrinkage estimator for the treatment effect as a weighted average of the OLS and IV estimate, with the weight being inversely proportional to the Hausman test statistic for exogeneity. We find that the Stein-like estimate[20] is equal to 10%. Even then, the magnitude of the effect is quite large showing that the targeting aspect of the CCB did play a major role in reallocation of resources between adults within the household.

On the other hand, using the coefficient on the interaction term between the renter dummy and the treatment, we find no significant treatment effect on the resource shares for renter households. Furthermore, we compare the treatment effect on resource shares between homeowners and renters and find that the difference is significant at the 5% confidence level. These results qualitatively hold true for the specification without instrumenting and for all the different specifications used for robustness checks in Appendix B. This suggests that the introduction of the CCB increased bargaining power of females, but only within households which are homeowners and this effect was significantly different than the negative, but insignificant treatment effect on the resource shares among renters. In the next section, we discuss possible explanations for this heterogeneity in the treatment effect on resource shares.

7 Discussion

In this section, we discuss the possible reasons driving the effect of the child benefit policy on the parameters. The treatment effect we observe is on the resource shares with the main distinction being that women's resource share increases in homeowner households, while we see no significant effect on resource shares in renter households. An interesting

[20] Hansen (2017) computes the Stein-like estimator as follows:

$$\hat{\beta}^* = w\hat{\beta}_{OLS} + (1-w)\hat{\beta}_{2SLS} \tag{10}$$

where

$$w = \begin{cases} \frac{\tau}{H_n} & \text{if } H_n \geq \tau \\ 1 & \text{if } H_n < \tau \end{cases} \tag{11}$$

and τ is equal to the number of endogenous regressors (m) minus 2 if $m > 2$, is 1 if $m = 2$, and is 0.25 if $m = 1$.

observation is that when we do not make the distinction between homeowners and renters, the significant treatment effect we observe becomes statistically insignificant (as shown in the tables in Appendix B.1). Thus, in making the distinction between homeowners and renters, our paper provides useful insight into the possibility of heterogeneity in treatment effect of policy changes that can be crucial to keep in mind when introducing new policies.

Given the treatment effect hinges on home ownership, we analyze whether the change in the CCB has any effect on the probability of the households moving (or changing their location of residence). We use a difference in difference methodology in a linear probability model on the likelihood of a household moving within the months of August 2014 to December 2017[21]. The identifying assumption here is that the probability of moving between treatment and control group before and after the treatment would follow the same trend had there not been a policy change. As in Najjarrezaparast and Pendakur (2021)[22], we find that after the change in the CCB, relative to households without children, homeowners with children are less likely to move whereas renters with children are significantly more likely to move. Further, after the introduction of the CCB, renters with children are also significantly more likely to move relative to homeowners with children. These results are illustrated in columns (1) and (2) of Table 6.[23]

[21]We exclude the months prior to August 2014 such that the treated months (August 2016-December 2017) coincide with the months before the policy change (August 2014-December 2015) as the probability of moving can vary highly with the time of the year.

[22]The estimates slightly differ between our paper and Najjarrezaparast and Pendakur (2021) due to differences in sample and a slight coding error in the latter paper's estimation. The results are qualitatively similar.

[23]We note that these findings are *not* robust to adding controls for renter dummy interacted with the indicator variable for households with children and the months post policy change (columns (3) and (4) of Table 6). That is, if we run the analysis separately for a sample of homeowners and renters, we observe no significant effect of the policy change on the probability of moving for either homeowners or renters. On the other hand, we cannot reject the restriction that renter dummy interacted with the indicator variable for households with children and the months post policy change don't belong in the model. This suggests that our analysis of moving is underpowered, due to the fact that the majority of our sample is homeowners. So, we take our discussion of mechanisms as suggestive rather than definitive.

Table 6: Treatment effect on probability of moving location of residence

	Indicator for moving residence		Indicator for moving residence	
	(1)	(2)	(3)	(4)
Homeowners: Treatment effect	-0.026***	-0.005	0.002	-0.006
	(0.009)	-0.010	(0.009)	-0.010
Renters: Treatment effect	0.103***		-0.044	
	(0.023)		(0.037)	
Treatment effect: Homeowners vs renters	0.129***		-0.046	
	(0.026)		(0.040)	
Renter dummy	Yes	No	Yes	No
$K \times R$ interaction term	No	No	Yes	Yes
$P \times R$ interaction term	No	No	Yes	Yes

Robust standard errors clustered at province, the number of children, year and month in parentheses *** p<0.01, ** p<0.05, * p<0.1.
$K \times R$ denotes renter dummy interacted with indicator for households with children
$P \times R$ denotes renter dummy interacted with indicator for post policy change time period

Based on these results, there are two possible reasons driving the heterogeneous treatment effect on the resource shares. The first possibility is related to the limited commitment intertemporal collective household, wherein resource shares change only if the value of remaining in marriage relative to exit changes (Chiappori and Mazzocco, 2017). The change in the CCB, because it raised the benefit and targets females, improves the outside option for females in households with children—those who exit marriage take the child benefit (and the children) with them. However, given that shelter is a shareable good, the increased budget from the CCB can also be used to improve the value of being in the household by improving shelter. Homeowners are constrained here due to their inability to move as easily as renters whereas renters can upgrade their shelter. Indeed, we observed above that renters do improve their shelter but homeowners may not. Thus, while the outside option for females increases in all households, the value of continuing to be in the household also increases for the female among renter households. On the other hand, as shelter cannot be upgraded by homeowners since homeowners are less mobile in terms of residence, only the outside option of the females improve which results in an increase in their resource shares from the changes in the CCB. Consequently, the increase in the outside option affects bargaining power–and thus resource shares—more for women in owner-occupier households.

An alternative reason for the differential treatment effect on resource shares runs through the marginal cost of shelter. The fact that homeowners are less likely to buy more shelter (through changing location of residence) than renters after the policy change implies that the marginal price of shelter (or shelter quality) is higher for owners than for renters. As a result, even with the additional funds from the CCB, the owners do not buy more shelter (say, by upgrading to better housing by changing location of residence). Thus, the optimal choice is to use the CCB funds to non-shelter expenditures. If women value non-shelter spending more than men, then the optimal use of funds will favour women's spending more than men, and the woman's resource share will rise (even though their weight in the household welfare function is unchanged).

Both of these possibilities could plausibly drive the heterogeneity in the treatment effect observed between homeowners and renters. Future studies might thus focus on clarifying these mechanisms.

8 Conclusion

Our study is the first step to identifying whether and how changes in the Canada Child Benefit policy affects preferences and resource allocation within the household. Our findings reflect possible changes that may occur within households beyond the ones intended by a change in the policy. In the paper, we first present a collective model of the household depicted from LPW and incorporate a difference in difference strategy in the structural estimation of the model to estimate the treatment effect of the policy change on preferences and resource shares.

Using GMM to estimate the model, we find no evidence of any significant change in the overall preference of the adult female and male (mother and father) within the household.

Individually, we find mild evidence of a decrease in the level of the Engel curve of females accompanied with an increase in that of males within home-owning households. This might suggest that while there is no overall change in preferences within the household, there may be some individual level changes in preferences arising from the new labeling of the child benefit. However, this result is not robust across all specifications.

Our results do suggest significant shifts in the resource shares of adults due to the policy change which are heterogeneous across homeowners and renters. We find that the resource shares for females significantly increase within homeowners, which can be expected given the CCB targets the payments towards the females in dual parent households. However, we do not observe an analogous treatment effect among renters where there is no significant change in the resource shares due to the policy change.

Given the heterogeneity in the treatment effect arises through home ownership, we provide two possible explanations. The first reason drives the change through the constraint faced by homeowners in moving. The policy change improves the outside option for females in all households. However, this is balanced out by an improvement in the female's inside option in renter households as the increased benefits/cash can be used to upgrade shelter. On the other hand, as homeowners are unable to move, the better outside option and no change in inside option leads to an increase in the females' resource shares. A second possibility suggested by the treatment effect on probability of moving is that homeowners face a higher marginal price of shelter. Thus, they choose not to purchase better shelter and instead, the recipient of the fund (females) are compensated by increased spending on their non-shelter goods. Further research on marginal pricing of shelter faced by homeowners and renters, as well as the effect on their outside option can allow identifying which of the two explanations are at play. However, we note that these findings about mobility are not very strong, and indeed not robust to some changes in model specification. So, further research on the mechanisms driving the heterogeneous responses of homeowners vs renters would be desirable.

A subsequent area for research involves estimating how our findings affected expenditure on children. The increase in expenditure on children's clothing due to the changes in the child benefit policy was more prominent among renter households as found in Najjarrezaparast and Pendakur (2021). This, along with our findings, suggests that the increased bargaining power of the female may not be the channel that led to increased spending on children. No overall effect on preference parameters of the adults also suggest that the increased spending was not driven by a shift in preference of the male and female towards children's clothing due to the label of the benefit. Hence the effect may solely be running through the increase in budget which raises the question of whether we would see similar effects from a cash transfer. Future work could thus focus on explicitly decomposing how much of the change in expenditure on children arises from the change in resource shares, budget and preferences.

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A Pre-trend

Table A1: Reduced form estimates

	(1)	(2)	(3)	(4)
Level term (a_m and a_f)				
Chi-square test statistic	83.24	101.06	55.11	70.12
p-value	*0.0252*	*0.0007*	*0.6545*	*0.1745*
Slope term (b_m and b_f)				
Chi-square test statistic	72.06	61.77	48.89	44.69
p-value	*0.1368*	*0.4128*	*0.8468*	*0.9300*
Errors	Cluster	Cluster	Robust	Robust
Instrument of log household expenditure	Yes	No	Yes	No

Linear restrictions on the slope term do not affect the pre-trend test statistics

B Robustness checks

B.1 Estimates from specification excluding renter dummy

Table B2: Reduced form estimates

	(1) female	(2) male	(3) female	(4) male	(5) female	(6) male	(7) female	(8) male
a($\mathbf{z} = 0$)	0.025***	0.017***	0.026***	0.018***	0.025***	0.017***	0.026***	0.018***
	(0.002)	(0.001)	(0.002)	(0.001)	(0.002)	(0.001)	(0.002)	(0.001)
b($\mathbf{z} = 0$)	0.023***	0.011	0.018***	0.008	0.023***	0.011	0.018***	0.008
	(0.002)	(0.002)	(0.004)	(0.003)	(0.002)	(0.002)	(0.004)	(0.003)
Errors	Clustered		Clustered		Robust		Robust	
Summation restriction on slope	Yes		No		Yes		No	

Standard errors (robust or clustered at province, the number of children, year and month in parentheses) *** p<0.01, ** p<0.05, * p<0.1.
All specifications include use log of income as an instrument for household budget.

Table B3: Coefficient of treatment effect

	(1) female	(2) male	(3) diff	(4) female	(5) male	(6) diff	(7) female	(8) male	(9) diff	(10) female	(11) male	(12) diff
Treatment effect on level (a)	0.002	-0.002		0.001	-0.002	0.003	0.002	-0.002	0.000	0.001	-0.002	0.003
	(0.002)	(0.002)		(0.003)	(0.003)	(0.004)	(0.002)	(0.002)	0.000	(0.004)	(0.003)	(0.004)
Treatment effect on slope (b)	0.001	-0.001	0.002	0.001	0.000	0.002	0.001	-0.001	0.002	0.001	0.000	0.002
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Errors		Clustered			Clustered			Robust			Robust	
Summation restriction on slope		Yes			No			Yes			No	

Standard errors (robust or clustered at province, the number of children, year and month in parentheses) *** p<0.01, ** p<0.05, * p<0.1.

Table B4: Parameter estimates

	(1) female	(2) male	(3) diff	(4) female	(5) male	(6) diff	(7) female	(8) male	(9) diff	(10) female	(11) male	(12) diff
α_i (at $z = 0$)	0.590***	0.410***	0.180*	0.589***	0.411***	0.178	0.590***	0.410***	0.180*	0.589***	0.411***	0.178
	(0.052)	(0.052)	(0.104)	(0.058)	(0.058)	(0.117)	(0.051)	(0.051)	(0.102)	(0.057)	(0.057)	(0.114)
Treatment Effect on α_i (at z=0)	0.050	0.000	0.000	0.057	0.000	0.000	0.050	0.000	0.000	0.057	0.000	0.000
	(0.057)	0.000	0.000	(0.064)	0.000	0.000	(0.055)	0.000	0.000	(0.063)	0.000	0.000
η_i	0.049***	0.073***	-0.024	0.048***	0.071***	-0.022	0.049***	0.073***	-0.024	0.048***	0.071***	-0.022
	(0.006)	(0.010)	(0.015)	(0.007)	(0.011)	(0.016)	(0.006)	(0.010)	(0.015)	(0.007)	(0.012)	(0.016)
Treatment Effect on η_i	-0.003	0.009	-0.012	-0.004	0.010	-0.014	-0.003	0.009	-0.012	-0.004	0.010	-0.014
	(0.006)	(0.011)	(0.017)	(0.007)	(0.013)	(0.018)	(0.006)	(0.011)	(0.016)	(0.007)	(0.013)	(0.018)
Hansen's J chi2 (dof=7)		16.116						16.116				
p-value		*0.024*						*0.024*				
Test for exclusion on slope		23.971			56.866			24.029			53.897	
p-value		*0.730*			*0.518*			*0.728*			*0.629*	
Test for linear restriction		18.173						17.406				
p-value		*0.011*						*0.015*				
Errors		Clustered			Clustered			Robust			Robust	
Summation restriction on slope		Yes			No			Yes			No	

Standard errors (robust or clustered at province, the number of children, year and month in parentheses) *** p<0.01, ** p<0.05, * p<0.1.

B.2 Estimates using total household expenditure excluding shelter expenses

Table B5: Reduced form estimates

	(1) female	(2) male	(3) female	(4) male
a($\mathbf{z} = 0$)	0.043***	0.029***	0.043***	0.029***
	(0.003)	(0.002)	(0.003)	(0.002)
b($\mathbf{z} = 0$)	0.013***	0.007***	0.013***	0.007***
	(0.003)	(0.003)	(0.003)	(0.003)
Instrument for log of budget	Yes (with log of income)		Yes	
Errors	Clustered		Robust	

Standard errors in parentheses *** p<0.01, ** p<0.05, * p<0.1.
All specifications include renter dummy and treatment indicator interacted with renter dummy.

B.3 Relaxing linear restriction on slope coefficients

Table B6: Reduced form estimates

	(1) female	(2) male	(3) female	(4) male
a($\mathbf{z} = 0$)	0.021***	0.016***	0.024***	0.017***
	(0.002)	(0.002)	(0.002)	(0.001)
b($\mathbf{z} = 0$)	0.032***	0.011	0.018***	0.009
	(0.009)	(0.007)	(0.004)	(0.003)
Instrument for log of budget	Yes (with log of income)		No	

Robust standard errors clustered at province, the number of children, year and month in parentheses *** p<0.01, ** p<0.05, * p<0.1.

Table B7: Coefficient of treatment effect (no summation restriction)

	(1) female	(2) male	(3) diff	(4) female	(5) male	(6) diff	(7) H-stat
Homeowner: Treatment effect on level (a)	0.002*	-0.001	0.003**	0.001	0.000	0.001	6.788
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	
Renter: : Treatment effect on level (a)	0.002	0.001	0.001	0.000	-0.001	0.002	0.072
	(0.002)	(0.002)	(0.002)	(0.002)	(0.001)	(0.002)	
Homeowner: Treatment effect on slope (b)	0.018***	-0.007		0.003	-0.004		5.301
	(0.007)	(0.005)		(0.004)	(0.003)		
Renter: Treatment effect on slope (b)	-0.004	0.019**		-0.007	0.007*		0.943
	(0.013)	(0.010)		(0.005)	(0.004)		
Instrument for log of budget		Yes (with log of income)			No		

Robust standard errors clustered at province, the number of children, year and month in parentheses *** p<0.01, ** p<0.05, * p<0.1.

Table B8: Parameter estimates (no summation restriction)

	(1) female	(2) male	(3) diff	(4) female	(5) male	(6) diff	(7) H-stat
Homeowner: α_i (at $z = 0$)	0.077***	0.037***	0.040	0.049***	0.068***	-0.019	1.656
	(0.025)	(0.013)	(0.036)	(0.007)	(0.010)	(0.015)	
Renter: α_i (at $z = 0$)	0.113	0.032	0.082	0.054***	0.085***	-0.031	0.184
	(0.125)	(0.022)	(0.146)	(0.008)	(0.015)	(0.020)	
Homeowner: Treatment Effect on α_i (at $z = 0$)	-0.041	0.077***	-0.118***	-0.010	0.024*	-0.034*	1.642
	(0.026)	(0.023)	(0.043)	(0.008)	(0.014)	(0.019)	
Renter: Treatment Effect on α_i (at $z = 0$)	-0.017	0.022	-0.039	0.014	-0.025	0.040	0.059
	(0.129)	(0.026)	(0.152)	(0.012)	(0.016)	(0.024)	
Homeowner: β (at $z = 0$)	0.032***	0.000	0.000	0.032***	0.000	0.000	0.001
	(0.008)	0.000	0.000	(0.005)	0.000	0.000	
Renter: β (at $z = 0$)	0.022*	0.000	0.000	0.035***	0.000	0.000	1.209
	(0.013)	0.000	0.000	(0.006)	0.000	0.000	
Homeowner:Treatment Effect on β (at $z = 0$)	0.011			-0.001			2.225
	(0.010)			(0.005)			
Renters:Treatment Effect on β (at $z = 0$)	0.015			0.000			0.701
	(0.019)			(0.007)			
Homeowner: η_i	0.400***	0.600***	-0.200	0.576***	0.424***	0.153	3.381
	(0.110)	(0.110)	(0.221)	(0.055)	(0.055)	(0.109)	
Renter: η_i	0.291	0.709**	-0.418	0.605***	0.395***	0.210*	1.249
	(0.287)	(0.287)	(0.575)	(0.061)	(0.061)	(0.122)	
Homeowner:Treatment Effect on η_i	0.316***			0.112*			4.127
	(0.120)			(0.064)			
Renters:Treatment Effect on η_i	0.138			-0.109			0.680
	(0.310)			(0.080)			
Treatment Effect on η_i: Homeowner vs Renters	0.178			0.221***			
	(0.258)			(0.076)			
Test for exclusion on slope		74.953			59.938		
p-value		*0.066*			*0.405*		
Instrument for log of budget		Yes (with log of income)			No		

Robust standard errors clustered at province, the number of children, year and month in parentheses *** p<0.01, ** p<0.05, * p<0.1.

B.4 Robust standard errors with linear restriction on slope coefficients

Table B9: Reduced form estimates

	(1) female	(2) male	(3) female	(4) male
$a(\mathbf{z}=0)$	0.020***	0.015***	0.023***	0.016***
	(0.002)	(0.002)	(0.002)	(0.001)
$b(\mathbf{z}=0)$	0.034***	0.012	0.023***	0.013
	(0.005)	(0.005)	(0.002)	(0.002)
Instrument for log of budget	Yes (with log of income)		No	

Robust standard errors in parentheses *** p<0.01, ** p<0.05, * p<0.1.

Table B10: Coefficient of treatment effect

	(1) female	(2) male	(3) diff	(4) female	(5) male	(6) diff	(7) H-stat
Homeowner: Treatment effect on level (a)	0.002	-0.001	0.003**	0.001	0.000	0.001	6.605
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	
Renter: : Treatment effect on level (a)	0.001	0.000	0.001	0.001	-0.001	0.002	0.749
	(0.002)	(0.002)	(0.002)	(0.002)	(0.001)	(0.002)	
Homeowner: Treatment effect on slope (b)	0.011***	-0.011***		0.004*	-0.004*		9.836
	(0.004)	(0.004)		(0.002)	(0.002)		
Renter: Treatment effect on slope (b)	-0.013*	0.013*		-0.007***	0.007***		0.797
	(0.007)	(0.007)		(0.003)	(0.003)		
Instrument for log of budget		Yes (with log of income)			No		

Robust standard errors in parentheses *** p<0.01, ** p<0.05, * p<0.1.

Table B11: Parameter estimates

	(1) female	(2) male	(3) diff	(4) female	(5) male	(6) diff	(7) H-stat
Homeowner: α_i (at $z = 0$)	0.074***	0.054***	0.020	0.050***	0.071***	-0.021	2.006
	(0.014)	(0.009)	(0.022)	(0.006)	(0.009)	(0.014)	
Renter: α_i (at $z = 0$)	0.086***	0.062***	0.024	0.054***	0.085***	-0.031	1.438
	(0.021)	(0.013)	(0.033)	(0.007)	(0.013)	(0.019)	
Homeowner: Treatment Effect on α_i (at $z = 0$)	-0.036***	0.059***	-0.096***	-0.009	0.021*	-0.030*	4.903
	(0.014)	(0.020)	(0.030)	(0.006)	(0.012)	(0.017)	
Renter: Treatment Effect on α_i (at $z = 0$)	0.010	-0.005	0.015	0.017*	-0.021	0.038*	0.050
	(0.035)	(0.019)	(0.054)	(0.010)	(0.014)	(0.023)	
Homeowner: η_i	0.462***	0.538***	-0.077	0.579***	0.421***	0.158*	6.151
	(0.067)	(0.067)	(0.134)	(0.047)	(0.047)	(0.095)	
Renter: η_i	0.450***	0.550***	-0.100	0.605***	0.395***	0.209*	4.195
	(0.097)	(0.097)	(0.193)	(0.060)	(0.060)	(0.121)	
Homeowner:Treatment Effect on η_i	0.247***			0.098*			6.498
	(0.080)			(0.054)			
Renters:Treatment Effect on η_i	-0.029			-0.103			0.299
	(0.156)			(0.077)			
Treatment Effect on η_i: Homeowner vs Renters	0.276*			0.201***			
	(0.148)			(0.070)			
Hansen's J chi2 (dof=9)		13.978			20.503		
p-value		*0.123*			*0.015*		
Test for exclusion on slope		31.833			24.152		
p-value		*0.327*			*0.721*		
Test for linear restriction		12.766			20.532		
p-value		*0.173*			*0.015*		
Instrument for log of budget		Yes (with log of income)			No		

Robust standard errors in parentheses *** p<0.01, ** p<0.05, * p<0.1.

B.5 Robust standard errors relaxing linear restriction on slope coefficients

Table B12: Reduced form estimates (no summation restriction)

	(1) female	(2) male	(3) female	(4) male
$a(\mathbf{z}=0)$	0.021***	0.016***	0.024***	0.017***
	(0.002)	(0.002)	(0.002)	(0.001)
$b(\mathbf{z}=0)$	0.032***	0.011	0.018***	0.009
	(0.009)	(0.007)	(0.004)	(0.003)
Instrument for log of budget	Yes (with log of income)		No	

Robust standard errors in parentheses *** p<0.01, ** p<0.05, * p<0.1.

Table B13: Coefficient of treatment effect (no summation restriction)

	(1) female	(2) male	(3) diff	(4) female	(5) male	(6) diff	(7) H-stat
Homeowner: Treatment effect on level (a)	0.002*	-0.001	0.003**	0.001	0.000	0.001	6.166
	(0.001)	(0.001)	(0.002)	(0.001)	(0.001)	(0.001)	
Renter: : Treatment effect on level (a)	0.002	0.001	0.001	0.000	-0.001	0.002	0.081
	(0.002)	(0.002)	(0.002)	(0.002)	(0.001)	(0.002)	
Homeowner: Treatment effect on slope (b)	0.018***	-0.007		0.003	-0.004		5.295
	(0.007)	(0.005)		(0.004)	(0.003)		
Renter: Treatment effect on slope (b)	-0.004	0.019*		-0.007	0.007*		0.900
	(0.013)	(0.010)		(0.005)	(0.004)		
Instrument for log of budget		Yes (with log of income)			No		

Robust standard errors in parentheses *** p<0.01, ** p<0.05, * p<0.1.

Table B14: Parameter estimates (no summation restriction)

	(1) female	(2) male	(3) diff	(4) female	(5) male	(6) diff	(7) H-stat
Homeowner: α_i (at $z = 0$)	0.077***	0.037***	0.040	0.049***	0.068***	-0.019	1.633
	(0.025)	(0.013)	(0.037)	(0.007)	(0.011)	(0.015)	
Renter: α_i (at $z = 0$)	0.113	0.032	0.082	0.054***	0.085***	-0.031	0.188
	(0.123)	(0.022)	(0.144)	(0.008)	(0.016)	(0.021)	
Homeowner: Treatment Effect on α_i (at $z = 0$)	-0.041	0.077***	-0.118***	-0.010	0.024*	-0.034*	1.631
	(0.025)	(0.023)	(0.043)	(0.007)	(0.014)	(0.019)	
Renter: Treatment Effect on α_i (at $z = 0$)	-0.017	0.022	-0.039	0.014	-0.025	0.040	0.061
	(0.127)	(0.027)	(0.150)	(0.012)	(0.017)	(0.025)	
Homeowner: β (at $z = 0$)	0.032***	0.000	0.000	0.032***	0.000	0.000	
	(0.008)	0.000	0.000	(0.005)	0.000	0.000	
Renter: β (at $z = 0$)	0.022*	0.000	0.000	0.035***	0.000	0.000	
	(0.012)	0.000	0.000	(0.006)	0.000	0.000	
Homeowner:Treatment Effect on β (at $z = 0$)	0.011			-0.001			
	(0.010)			(0.005)			
Renters:Treatment Effect on β (at $z = 0$)	0.015			0.000			
	(0.019)			(0.007)			
Homeowner: η_i	0.400***	0.600***	-0.200	0.576***	0.424***	0.153	3.119
	(0.113)	(0.113)	(0.226)	(0.053)	(0.053)	(0.107)	
Renter: η_i	0.291	0.709**	-0.418	0.605***	0.395***	0.210*	1.253
	(0.287)	(0.287)	(0.575)	(0.063)	(0.063)	(0.126)	
Homeowner:Treatment Effect on η_i	0.316***			0.112*			3.769
	(0.122)			(0.062)			
Renters:Treatment Effect on η_i	0.138			-0.109			0.679
	(0.311)			(0.081)			
Treatment Effect on η_i: Homeowner vs Renters	0.178			0.221***			
	(0.257)			(0.075)			
Test for exclusion on slope		56.230			57.492		
p-value		*0.541*			*0.494*		
Instrument for log of budget		Yes (with log of income)			No		

Robust standard errors in parentheses *** p<0.01, ** p<0.05, * p<0.1.

B.6 Instrument with square of log income

Table B15: Reduced form estimates

	(1) female	(2) male	(3) female	(4) male	(5) female	(6) male	(7) female	(8) male
a($\mathbf{z} = 0$)	0.021***	0.015***	0.022***	0.016***	0.021***	0.015***	0.022***	0.016***
	(0.002)	(0.002)	0.000	0.000	(0.002)	(0.002)	(0.002)	(0.002)
b($\mathbf{z} = 0$)	0.035***	0.010	0.031***	0.007	0.035***	0.010	0.031***	0.007
	(0.005)	(0.004)	(0.007)	(0.010)	(0.005)	(0.005)	(0.008)	(0.006)
Errors	Clustered		Clustered		Robust		Robust	
Summation restriction on slope	Yes		No		Yes		No	

Standard errors (robust or clustered at province, the number of children, year and month in parentheses) *** p<0.01, ** p<0.05, * p<0.1.
All specifications include renter dummy and treatment indicator interacted with renter dummy.

Table B16: Coefficient of treatment effect

	(1) female	(2) male	(3) diff	(4) female	(5) male	(6) diff	(7) female	(8) male	(9) diff	(10) female	(11) male	(12) diff
Homeowner: Treatment effect on level (a)	0.002	-0.001	0.003*	0.002*	-0.001	0.003**	0.002	-0.001	0.003*	0.002*	-0.001	0.003**
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.002)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.002)
Renter: : Treatment effect on level (a)	0.000	0.000	0.000	0.001	0.001	0.001	0.000	0.000	0.000	0.001	0.001	0.001
	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)
Homeowner: Treatment effect on slope (b)	0.014***	-0.014***		0.021***	-0.008	0.030***	0.014***	-0.014***		0.021***	-0.008	0.030***
	(0.004)	(0.004)		(0.007)	(0.005)	(0.008)	(0.004)	(0.004)		(0.007)	(0.005)	(0.008)
Renter: Treatment effect on slope (b)	-0.015**	0.015**		-0.010	0.018*	-0.027**	-0.015**	0.015**		-0.010	0.018*	-0.027**
	(0.006)	(0.006)		(0.012)	(0.009)	(0.013)	(0.007)	(0.007)		(0.012)	(0.010)	(0.013)
Errors		Clustered			Clustered			Robust			Robust	
Summation restriction on slope		Yes			No			Yes			No	

Standard errors (robust or clustered at province, the number of children, year and month in parentheses) *** p<0.01, ** p<0.05, * p<0.1.
All specifications include renter dummy and treatment indicator interacted with renter dummy.

Table B17: Parameter estimates

	(1) female	(2) male	(3) diff	(4) female	(5) male	(6) diff	(7) female	(8) male	(9) diff	(10) female	(11) male	(12) diff
Homeowner: α_i (at $z=0$)	0.050***	0.071***	-0.021	0.049***	0.068***	-0.019	0.050***	0.071***	-0.021	0.049***	0.068***	-0.019
	(0.006)	(0.009)	(0.014)	(0.007)	(0.010)	(0.015)	(0.006)	(0.009)	(0.014)	(0.007)	(0.011)	(0.015)
Renter: α_i (at $z=0$)	0.054***	0.085***	-0.031*	0.054***	0.085***	-0.031	0.054***	0.085***	-0.031	0.054***	0.085***	-0.031
	(0.007)	(0.013)	(0.018)	(0.008)	(0.015)	(0.020)	(0.007)	(0.013)	(0.019)	(0.008)	(0.016)	(0.021)
Homeowner: Treatment Effect on α_i (at $z=0$)	-0.009	0.021*	-0.030*	-0.010	0.024*	-0.034*	-0.009	0.021*	-0.030*	-0.010	0.024*	-0.034*
	(0.006)	(0.012)	(0.018)	(0.008)	(0.014)	(0.019)	(0.006)	(0.012)	(0.017)	(0.007)	(0.014)	(0.019)
Renter: Treatment Effect on α_i (at $z=0$)	0.017*	-0.021	0.038*	0.014	-0.025	0.040	0.017*	-0.021	0.038*	0.014	-0.025	0.040
	(0.010)	(0.014)	(0.023)	(0.012)	(0.016)	(0.024)	(0.010)	(0.014)	(0.023)	(0.012)	(0.017)	(0.025)
Homeowner: η_i	0.415***	0.585***	-0.170	0.319**	0.681***	-0.363	0.415***	0.585***	-0.170	0.319**	0.681***	-0.363
	(0.070)	(0.070)	(0.141)	(0.130)	(0.130)	(0.261)	(0.069)	(0.069)	(0.138)	(0.131)	(0.131)	(0.263)
Renter: η_i	0.426***	0.574***	-0.149	0.228	0.772**	-0.543	0.426***	0.574***	-0.149	0.228	0.772**	-0.543
	(0.103)	(0.103)	(0.205)	(0.313)	(0.313)	(0.626)	(0.098)	(0.098)	(0.195)	(0.308)	(0.308)	(0.616)
Home-owner:Treatment Effect on η_i	0.299***			0.403***			0.299***			0.403***		
	(0.083)			(0.139)			(0.082)			(0.140)		
Renters:Treatment Effect on η_i	-0.021			0.161			-0.021			0.161		
	(0.145)			(0.338)			(0.153)			(0.335)		
Treatment Effect on η_i: Homeowner vs Renters	0.321**			0.242			0.321**			0.242		
	(0.136)			(0.275)			(0.146)			(0.273)		
Summation restriction on slope		Yes			No			Yes			No	

Standard errors (robust or clustered at province, the number of children, year and month in parentheses) *** p<0.01, ** p<0.05, * p<0.1.
All specifications include renter dummy and treatment indicator interacted with renter dummy.

B.7 Results using imputed rents for only owners and actual rent for renters

Table B18: Reduced form estimates

	IV estimates		OLS estimates	
	(1) female	(2) male	(3) female	(4) male
a($\mathbf{z} = 0$)	0.020***	0.015***	0.023***	0.016***
	(0.002)	(0.002)	(0.002)	(0.001)
b($\mathbf{z} = 0$)	0.033***	0.011	0.022***	0.012
	(0.005)	(0.004)	(0.002)	(0.002)
Instrument for log of budget	Yes (with log of income)		No	

Standard errors clustered at province, the number of children, year and month in parentheses *** p<0.01, ** p<0.05, * p<0.1.
IV estimates refer to GMM estimation instrumenting household budget/expenditure with income.
OLS estimates refer to GMM estimation without instrument for household budget.

Table B19: Reduced form estimates: Treatment effect

	IV estimates			OLS estimates			
	(1) female	(2) male	(3) diff	(4) female	(5) male	(6) diff	(7) H-stat
Homeowner: Treatment effect on level (a)	0.001	-0.001	0.003*	0.000	0.000	0.000	6.422
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	
Renter: : Treatment effect on level (a)	0.001	0.000	0.001	0.001	-0.001	0.002	0.620
	(0.002)	(0.001)	(0.002)	(0.002)	(0.001)	(0.002)	
Homeowner: Treatment effect on slope (b)	0.011***	-0.011***		0.003	-0.003		12.268
	(0.004)	(0.004)		(0.002)	(0.002)		
Renter: Treatment effect on slope (b)	-0.012**	0.012**		-0.008***	0.008***		0.713
	(0.006)	(0.006)		(0.002)	(0.002)		
Instrument for log of budget		Yes (with log of income)			No		

Standard errors clustered at province, the number of children, year and month in parentheses *** p<0.01, ** p<0.05, * p<0.1.
IV estimates refer to GMM estimation instrumenting household budget/expenditure with income. OLS estimates refer to GMM estimation without instrument for household budget.

Table B20: Parameter estimates

	IV estimates			OLS estimates			
	(1) female	(2) male	(3) diff	(4) female	(5) male	(6) diff	(7) H-stat
Homeowner: α_i (at $z=0$)	0.071*** (0.014)	0.054*** (0.010)	0.017 (0.023)	0.048*** (0.006)	0.072*** (0.010)	-0.024 (0.015)	
Renter: α_i (at $z=0$)	0.078*** (0.019)	0.061*** (0.014)	0.017 (0.032)	0.050*** (0.007)	0.084*** (0.013)	-0.034* (0.019)	
Homeowner: Treatment Effect on α_i (at $z=0$)	-0.035** (0.014)	0.058*** (0.021)	-0.093*** (0.032)	-0.008 (0.006)	0.019 (0.013)	-0.027 (0.018)	
Renter: Treatment Effect on α_i (at $z=0$)	0.008 (0.028)	-0.005 (0.018)	0.013 (0.045)	0.021** (0.010)	-0.026* (0.014)	0.048** (0.023)	
Homeowner: η_i	0.472*** (0.072)	0.528*** (0.072)	-0.056 (0.144)	0.593*** (0.052)	0.407*** (0.052)	0.187* (0.105)	6.118
Renter: η_i	0.466*** (0.101)	0.534*** (0.101)	-0.068 (0.201)	0.620*** (0.060)	0.380*** (0.060)	0.240** (0.120)	3.618
Homeowner:Treatment Effect on η_i	0.243*** (0.084)			0.087 (0.060)			6.872
Renters:Treatment Effect on η_i	-0.028 (0.145)			-0.139* (0.077)			0.811
Treatment Effect on η_i: Homeowner vs Renters	0.271** (0.134)			0.226*** (0.071)			
Hansen's J statistic (dof=9) p-value		25.789 *0.002*			21.790 *0.010*		
Test for exclusion on slope p-value		37.501 *0.134*			21.624 *0.835*		
Test for linear restriction p-value		22.731 *0.007*			25.192 *0.003*		
Instrument for log of budget		Yes (with log of income)			No		

Robust standard errors clustered at province, the number of children, year and month in parentheses *** p<0.01, ** p<0.05, * p<0.1.
IV estimates refer to GMM estimation instrumenting household budget/expenditure with income. OLS estimates refer to GMM estimation without instrument for household budget.

B.8 Results including interaction terms of renter dummy with indicators for households with children and post-policy

Table B21: Joint test of significance of coefficients of interaction terms

	(1)	(2)
Slope and level term (a_m, a_f and b_f)		
Chi-square test statistic	6.76	6.87
p-value	*0.34*	*0.33*
Errors	Cluster	Cluster
Instrument of log household expenditure	Yes	No

Table B22: Reduced form estimates of constant and slope of budget share

	IV estimates		OLS estimates	
	(1) female	(2) male	(3) female	(4) male
a($\mathbf{z} = 0$)	0.020***	0.015***	0.023***	0.016***
	(0.002)	(0.002)	(0.002)	(0.001)
b($\mathbf{z} = 0$)	0.036***	0.011	0.024***	0.012***
	(0.005)	(0.005)	(0.002)	(0.002)
Instrument for log of budget	Yes (with log of income)		No	

Standard errors clustered at province, the number of children, year and month in parentheses
*** p<0.01, ** p<0.05, * p<0.1.
IV estimates refer to GMM estimation instrumenting household budget/expenditure with income. OLS estimates refer to GMM estimation without instrument for household budget.

Table B23: Reduced form estimates: Treatment effect

	IV estimates			OLS estimates			
	(1) female	(2) male	(3) diff	(4) female	(5) male	(6) diff	(7) H-stat
Homeowner: Treatment effect on level (a)	0.002*	-0.001	0.003*	0.001	0.000	0.001	3.926
	(0.001)	(0.001)	(0.002)	(0.001)	(0.001)	(0.001)	
Renter: : Treatment effect on level (a)	-0.001	-0.003	0.002	-0.005	0.000	-0.005	4.664
	(0.004)	(0.003)	(0.006)	(0.004)	(0.003)	(0.004)	
Homeowner: Treatment effect on slope (b)	0.011***	-0.011***		0.005**	-0.005**		2.963
	(0.004)	(0.004)		(0.002)	(0.002)		
Renter: Treatment effect on slope (b)	-0.009	0.009		-0.017***	0.017***		0.496
	(0.012)	(0.012)		(0.005)	(0.005)		
Instrument for log of budget		Yes (with log of income)			No		

Standard errors clustered at province, the number of children, year and month in parentheses *** p<0.01, ** p<0.05, * p<0.1. IV estimates refer to GMM estimation instrumenting household budget/expenditure with income. OLS estimates refer to GMM estimation without instrument for household budget.

Table B24: Parameter estimates

	IV estimates			OLS estimates			
	(1) female	(2) male	(3) diff	(4) female	(5) male	(6) diff	(7) H-stat
Homeowner: α_i (at $z = 0$)	0.062***	0.063***	-0.001	0.054***	0.064***	-0.010	0.767
	(0.012)	(0.012)	(0.023)	(0.008)	(0.009)	(0.015)	
Renter: α_i (at $z = 0$)	0.096*	0.063**	0.033	0.038***	0.178*	-0.139	1.266
	(0.052)	(0.029)	(0.080)	(0.008)	(0.101)	(0.108)	
Homeowner: Treatment Effect on α_i (at z=0)	-0.024**	0.051**	-0.076**	-0.013*	0.029**	-0.041**	1.423
	(0.012)	(0.020)	(0.030)	(0.007)	(0.012)	(0.019)	
Renter: Treatment Effect on α_i (at z=0)	-0.006	-0.002	-0.004	0.033***	-0.114	0.146	0.466
	(0.057)	(0.032)	(0.089)	(0.012)	(0.100)	(0.109)	
Homeowner: η_i	0.481***	0.519***	-0.037	0.536***	0.464***	0.073	1.196
	(0.075)	(0.075)	(0.150)	(0.056)	(0.056)	(0.111)	
Renter: η_i	0.396*	0.604***	-0.209	0.833***	0.167	0.666***	5.086
	(0.227)	(0.227)	(0.454)	(0.118)	(0.118)	(0.236)	
Homeowner:Treatment Effect on η_i	0.232***			0.141**			2.387
	(0.086)			(0.062)			
Renters:Treatment Effect on η_i	0.033			-0.331***			2.855
	(0.251)			(0.128)			
Treatment Effect on η_i: Homeowner vs Renters	0.199			0.472***			
	(0.267)			(0.146)			
Hansen's J statistic (dof=9)		15.333			36.072		
p-value		*0.168*			*0.000*		
Test for exclusion on slope		35.967			23.793		
p-value		*0.175*			*0.739*		
Test for linear restriction		15.550			26.238		
p-value		*0.159*			*0.006*		
Instrument for log of budget		Yes (with log of income)			No		

Robust standard errors clustered at province, the number of children, year and month in parentheses *** p<0.01, ** p<0.05, * p<0.1.
IV estimates refer to GMM estimation instrumenting household budget/expenditure with income. OLS estimates refer to GMM estimation without instrument for household budget.