How Did the Canada Child Benefit Affect Household Spending?

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Abstract

In this study, we assess how the increase in the Canada Child Benefit (CCB) in July 2016 affected household spending on various types of consumption. The increase was more than $2,000 per child per year for most recipient households, so it represented a very large increase in transfers to households with children. Further, because the CCB has a very low tax-back rate, the policy change was similar to raising the rate in a universal basic income scheme.

We assess the effect of the policy change on a measure of overall consumption, and its seven constituent categories: clothing, food, health care, household operation, recreation, shelter, and transportation. We focus on households whose income is below the median income (as this is the principal policy target), and evaluate effects for renters and owners separately. We find statistically significant effects of the policy change only for spending on clothing, food, and shelter, and these arise only for rental-tenure households.

We find that rental-tenure households with children below the median income increased their annual consumption by roughly $3,000 in response to the CCB increase of roughly $4,300 for these households. With average annual consumption around $30,000, this represents an increase in consumption of roughly 10% for these households. They increased spending on food by roughly $700, and on shelter by nearly $1,400. They increased spending on clothing by roughly $300, but only on children's clothing and not on adult clothing. We find mild evidence that households with more children responded differently to those with fewer children, in particular, that shelter spending rose by much more for the former households.

That the policy changed increased spending on the necessities of food, shelter, and clothing suggests that it achieved some measure of success in improving the consumption of lower-income households with children. The lion's share of the increased spending was allocated to increased shelter spending for rental-tenure households. We find weak evidence that these households moved after the policy change. But given the existing evidence that low-income housing supply may be quite inelastic, it is possible that rent price increases ate up this increased spending. More research on this question, using different data, is needed.

Introduction

In July 2016, the Canada Child Benefit (CCB) replaced a variety of federal transfer programs aimed at households with children, including the Canada Child Tax Benefit, the Supplemental Child Benefit, and the Universal Child Benefit. Besides replacing this hodgepodge of programs, it also substantially increased the benefit rate and decreased the implicit tax rate on the benefits. The increase in the overall child benefit was large: a typical household with children below the median of the income distribution saw an increase in transfers of roughly $2,300 per child per year (Government of Canada, 2015).

Using data from the Surveys of Household Spending (SHS) from 2014 to 2017, we consider how the increase in child benefits affected household spending. In particular, we estimate the effect of the change in the structure and size of child benefits on average household spending in seven categories: clothing, food, health care, household operation, recreation, shelter, and transportation. Essentially, we ask: how much food did the CCB put in the mouths of children? And, analogously for the other categories of consumption goods.

Relative to the literature on how child benefits affect spending in Canada, we use a feature of the SHS that has not previously been exploited: it is conducted in monthly waves with samples of roughly 1,000 households per month. Furthermore, it has information on the ages of all household members at the month level. Consequently, we can precisely identify which households received the increased child benefits, and when.

The Policy

The Canada Child Benefit is paid monthly, with the first payment distributed in July 2016. Payments are administered by Canada Revenue Agency (CRA) and are largely by direct-deposit mid-month (Government of Canada, CRA, 2017). A key feature of CRA-administered benefits is that for the vast majority of households, they are automated. Households did not need to sign up for the CCB; rather, CRA just changed the direct-deposit amounts going to households already receiving benefits in July 2016. Consequently, an uninformed adult in a household with children was “surprised” by CRA transfers into their accounts that were roughly $200 per child per month higher than they had previously been. Households with children under 18 had on average 1.8 children under 18, so this represents an average increase of roughly $4,300 per year for low-income child-benefit recipient households (Statistics Canada, 2020).

The CCB is paid to the parent who is “primarily” in charge of the care and upbringing of the child. Under tax rules, the primary caregiver is deemed to be the female parent when a male and a female parent live together in the same household as the child, unless the female parent notifies tax authorities otherwise (Government of Canada, CRA, 2019).

The CCB is much more generous than the systems it replaced (Government of Canada, 2016). The Government of Canada wrote in Budget 2016:

> The Canada Child Benefit will provide a maximum annual benefit of up to $6,400 per child under the age of 6 and up to $5,400 per child for those aged 6 through 17. Families

> with less than $30,000 in net income will receive the maximum benefit. Under the current system, families with $30,000 in net income and one child would have received $4,852 in child benefits, after tax, if their child is under the age of 6 and $3,916 if their child is aged 6 through 17. (Government of Canada, 2016)

Additionally, the implicit tax rate on CCB benefits is low, varying from 7% to 23% (Kesselman, 2019). As a result of the low tax-back rate combined with a high benefit, households receiving the CCB stretch far into the upper quantiles of the income distribution.

Due to its large size and low implicit tax rate, the CCB is essentially a basic income scheme for households with children. If a household has zero income, then they receive the CCB of roughly $6,000 per child annually regardless of their employment status. If that same household gains some market income, their CCB stays the same. If they gain market income beyond roughly $30,000 per year, then their CCB slowly reduces (is "clawed back") until it disappears at an income in excess of $150,000.

Literature

Jones et al. (2019) employ the public-use versions of the SHS 1997 through 2009 to investigate how changes in child benefits programming affected household spending. They did not exploit monthly variation in program timing or children's ages. They found that some benefit income is spent on direct education inputs, while some is spent on everyday items likely to improve the general conditions children face. Additionally, they found that some families reduce spending on risky behaviour items, including spending on alcohol and gambling. They write:

> In general, these studies find high sensitivity of household consumption to tax refunds, meaning that families are prompted to increase spending after the income shock … The proportion of the return or rebate spent on non-durable goods—spending that does not directly build wealth—has been found to depend on individuals' access to liquidity. More highly constrained individuals tend to spend their tax rebate income more quickly (or are less likely to save it or use it to pay down debt) and are more likely to spend it on non-durable necessities like gas or groceries. (Jones et al., 2019, p. 4)

This paper provides evidence in the spirit of Jones et al. (2019), but with cleaner identification made possible by use of the confidential files of the SHS available in the Research Data Centre.

The confidential data have two advantages over those used by Jones et al.: (a) they are comprised of repeated cross sections at a monthly frequency; and (b) they cover the very large increase in child benefits that occurred in July 2016. Consequently, the effects of the benefit increase can be more accurately gauged because both the calendar date and the child-benefit eligibility of residing children can be tracked at the month level rather just at than the year level. Here, the key gain is the ability to identify households who have births in a given month, and households whose children have aged out of eligibility in a given month. Another potential gain from the use of these data would be the ability to distinguish children below 6 years of age from

children aged 6 to 17. However, as we describe below, the estimated treatment effects do not differ between these two groups, possibly because the child benefits are quite similar in size across the two groups. Consequently, for this paper, we focus on all children (rather than children by age group).[1]

Data

The SHS is a nationally representative survey on household spending patterns. It integrates the older Canadian facilities surveys and family expenditure Surveys, collecting information on the demographic characteristics of households, the facilities present in households (e.g., washing machine, internet), and the allocation of household spending across roughly 200 spending categories. Its primary use is to provide the expenditure weights used in the calculation of the Consumer Price Index. An additional use is to investigate consumer demand behaviour.

The surveys are categorized into diary and interview components. The diary component derives from an in-depth diary keeping track of expenditure in extremely detailed categories. In contrast, the interview component uses recall data via an interviewer-enhanced questionnaire, with expenditure in fewer categories. However, the diary sample is only half as large as the interview sample. In this work, we exclusively use the interview data to investigate how consumers respond to the additional income arriving in the household as a result of the change in the child benefits structure.

The SHS has three features that enable us to evaluate how the policy change affected spending. First, each year of the SHS has roughly 12,000 observations of households, with roughly 1,000 sampled in each calendar month. Respondents fill out questionnaires detailing retrospective spending over the previous month (e.g., for food), previous 3 months (e.g., for clothing), and previous 12 months (e.g., for household furnishings). For this analysis, we observe repeated cross sections of households at the calendar-month level over 48 months from January 2014 to December 2017.

Second, the SHS has information on the birth month and year of every household member, so that we can exactly identify how many household members are eligible for the CCB in each household (given the month and year of the survey). We use these two data features together: for each household, we create a variable named *children* equal to the number of children aged less than 18 in the month before data collection. We create a variable named *policy_target* for households with any children who would be eligible for the credit, equal to 1 if *children>0*.

[1] The confidential SHS data also permit us to distinguish single-parent from dual-parent households. However, due to the pandemic-related closure of the Research Data Centre, we have not yet undertaken this investigation.

Third, the SHS collects some data at the person- (as opposed to the household-) level. In particular, it collects 3-month recall expenditure on clothing and footwear.[2] We use the latter data to consider how the child benefit affects spending on children's clothing versus adult clothing.

It is important to note a few demerits of the SHS. First, it does not have contemporaneous income data. This means that we do not know which households actually receive the child benefit in any given month, nor the amount of that benefit. Consequently, the estimates of how treatment with an expanded child benefit affects behaviour are more like intent-to-treat estimates than like treatment estimates.

Second, the data we use are based entirely on individual recall of expenditures. Some expenditures, such as rent, are repeated and predictable, and so suffer from little recall error. Others, such as spending on food, are high variance and high frequency, and so suffer from more recall error. Others yet, such as tobacco, alcohol, and gambling, may be embarrassing to report, and thus suffer from biased recall. So recall data must be taken with a grain of salt.

Methodology

To estimate the treatment effect of the policy change on household spending, we use a difference-in-differences identification approach. Essentially, we identify the treatment effect for households with children by tracing out how their spending changes before and after the policy change. We control for the possibility of a correlated time trend by netting out the time trend for a control group defined as households without children.

We first create a variable named *policy_date* equal to 1 in months on or after August 2016. This dummy variable "turns on" the month *after* the first CCB payment because expenditure is retrospective. Let *X* be a vector of control variables: a constant, and dummies for calendar year (3), calendar month (11), province of residence (9), and city size (5).

Let *Y* be a vector of outcome variables, including total current consumption, and its seven constituent categories of spending. Those categories are: food (food and non-alcoholic beverages purchased from stores), shelter (minus mortgage payments), clothing, health, household operation (as an aggregate, and its subcomponent child care), transportation (minus vehicle purchases), and recreation (including reading and education). Additionally, we consider clothing separately for adults and for children; child care, which is a subcomponent of household operation; and rent, which is a subcomponent of shelter. This gives a total of 12 dependent variables in *Y*.

We regress *Y* on *X*, *policy_date*, *policy_target*, and *policy_date*policy_target*. The variable *X* picks up seasonality via month dummies. The variable *policy_date* picks up the post-policy time-trend for control households. The variable *policy_target* picks up the differential spending patterns of households with children. Finally, the coefficient on the interaction term *policy_date*policy_target* is the estimated average treatment effect of the policy change on the

[2] The SHS also collects annual income and annual registered retirement saving (along with the stock of registered retirement accounts) from CRA's T1 records at the person level for adult household members.

outcome variable *Y*. We also consider heterogeneous treatments, driven by the fact that the policy has larger benefits for households with more children.

Results

We consider three population subsamples, all of which include only households whose market income is less than the median household market income in the previous year and that have no elderly members: (a) all households with less than median annual income and that have no elderly members; (b) only those that are renters and are not observed in August, September, or October 2016; and (c) only those that are owner-occupiers and are not observed in August, September, or October 2016. The exclusion of households with elderly members is to account for the fact that elderly benefits (especially the Guaranteed Income Supplement) changed at the same time. The focus on rental-tenure households is to look at a population that can more quickly adjust their shelter spending. The exclusion of August to October 2016 is to avoid pollution from 3-month recall spending questions (notably clothing) with some months before the policy change.

Table 1 gives weighted means of the response variables *Y*, where the weights are Statistics Canada's inverse-probability sampling weights. Columns give means and standard deviations of several outcome variables. The current consumption variable is the sum of all seven top-level consumption categories. Subcategories are provided for clothing (adult and children's clothing), for household operation (child care) and for shelter (rent). Consumption is at the annual level, annualized from monthly recall (for food and shelter), quarterly recall (for clothing), and annual recall (for household operation, health, transportation, and recreation).

As noted above, the annual child benefit after July 2016 was roughly $6,000 per child, and that the increase between June 2016 and July 2016 was roughly $2,300 per child (or about $200 per child per month). Given that households with any children under 18 had on average 1.8 children under 18, this implies that the increase in child benefits was roughly $4,300 per year (1.8 children times $2,300 per child) for children below the clawback range. The average current consumption in Column 1, for all households with market income less than the median in their respective data year, is $31,758. Thus, both the increase in the child benefit and its overall level are large relative to average current consumption.

Estimated Average Treatment Effects

Table 2 gives estimated treatment effects in each of the three samples (columns) for each of the 12 dependent variables (rows). Regressions are weighted with Statistics Canada's inverse probability weights, and standard errors are clustered at the community (aka census sub-division) level. Levels of statistical significance are indicated with asterisks: * indicates significance at the 10% level, ** at the 5% level and *** at the 1% level.

Table 1

Descriptive Statistics of Spending Across Categories

		Bottom half		Bottom half, drop Aug to Oct 2016			
				Renters		Owners	
Category	*Subcategory*	Mean	*Std dev*	Mean	*Std dev*	Mean	*Std dev*
Consumption		31,758	*20,612*	28,893	*18,085*	36,950	*23,460*
Clothing	*All*	1,879	*2,583*	1,760	*2,421*	2,068	*2,840*
	Adult clothing	1,446	*2,138*	1,356	*2,017*	1,592	*2,336*
	Children's clothing	433	*1,159*	404	*1,110*	477	*1,249*
Food		5,688	*3,763*	5,243	*3,474*	6,387	*4,081*
Health		1,609	*1,925*	1,262	*1,563*	2,125	*2,201*
Household operations	*All*	869	*2,305*	658	*1,338*	1,201	*3,314*
	Child care	130	*957*	121	*922*	150	*1,064*
Recreation		3,110	*5,951*	3132	*6,337*	3,139	*5,253*
Shelter	*All*	11,928	*7,479*	11,065	*5,113*	13,727	*9,749*
	Rent	6,032	*6,239*	10,154	*4,833*	0	*0*
Transportation		6,674	*10,335*	5,773	*9,311*	8,304	*11,915*

Table 2

Estimated Treatment Effects of the CCB on Household Consumption

		Bottom half		Bottom half, drop Aug to Oct 2016			
				Renters		Owners	
Category	*Subcategory*	Est	*Std err*	Est	*Std err*	Est	*Std err*
Consumption		1,898*	*1,135*	3,255***	*1,107*	−444	*2,542*
Clothing	*All*	134	*196*	348	*212*	−272	*312*
	Adult clothing	−49	*137*	95	*128*	−359	*251*
	Children's clothing	183*	*93*	252*	*129*	87	*149*
Food		377*	*215*	709**	*310*	231	*408*
Health		28	*105*	−21	*118*	178	*217*
Household operations	*All*	101	*132*	192**	*92*	−106	*314*
	Child care	25	*79*	151	*131*	−83	*123*
Recreation		−386	*284*	−208	*364*	−798	*685*
Shelter	*All*	1,313***	*478*	1,386***	*426*	1,396	*1,087*
	Rent	216	*327*	923**	*387*		
Transportation		331	*578*	848	*659*	−1,073	*1,202*

Statistics Canada does not permit unweighted estimation on its internal data files. Clustered standard errors accommodate the heteroskedasticity that may be induced by weighted estimation. Using hetero-robust, rather than clustered, standard errors does not change any reported finding.

There are five headline results from Table 2. First, the policy raised the consumption of households with children. The estimated average treatment effect for all households without elderly members and below the median income, shown in the leftmost column is nearly $2,000 (though it is only marginally statistically significant). Given that these households have average current consumption a bit more than $30,000, this suggests that the policy change raised the consumption of households with children below the median income by about 6%.

The middle columns present estimates for the subsample of households that is rental-tenure and excluding the months immediately after the policy change. Here, we see a much larger treatment effect of more than $3,200. This population also has somewhat lower current consumption, so this estimated treatment effect suggests that the policy change raised current consumption by about 11%.

In the columns on the right, we present results for the subsample of households that is non-rental-tenure and excluding the months immediately after the policy change. For this subsample, there is no statistically significant treatment effect that we can connect causally to the policy change.

Second, among the various types of expenditure, the largest estimated treatment effect is on shelter expenditures. The middle columns give estimated treatment effects for rental-tenure households that have less than the median household market income, and excludes households interviewed in the 3 months following the policy change. Here, we see a large and statistically significant treatment effect on shelter spending of $1,386. This finding is somewhat surprising, given that shelter expenditures are probably the most pre-committed of all the expenditure types. Yet, the result is unambiguous. Roughly half of the increase in consumption went to shelter.

Shelter expenditures include utilities, repairs, and other non-rent items. We also provide the estimated treatment effect on the rent subcategory of shelter expenditures. Of course, this is relevant only for rental-tenure households. We see that the lion's share of the increase in shelter expenditures was allocated to rent, with an estimated treatment effect on rental spending of roughly $900. Spending on rent can rise for a given accommodation if prices go up; alternatively households could shift address to higher-rent accommodation. These two channels are not welfare-equivalent: landlords benefit from the price increases; tenants benefit from quantity increases. We consider whether or not households move in response to the policy in Table 4.

In the rightmost columns, we give the estimated treatment effects on non-rental-tenure households. Here, there are no statistically significant effects on the consumption of any component of consumption. This means that the null result for total current consumption for these households is not driven by offsetting increases and decreases in different consumption categories. Rather, it is driven by the lack of a statistically significantly treatment effect on any category. This lack of statistical significance is not just the result of imprecision. The estimated

standard errors in Column 3 are about 50% larger than those reported in Column 2. But, additionally, many of the point estimates in Column 3 are smaller in magnitude than those in Column 2.

Third, the increased child benefit put food on the table in households with children. The estimated average treatment effect on food consumption reported in the leftmost columns (all below median households) is $377. For rental-tenure households, in the middle columns, the estimated treatment effect is roughly $700, representing an increase in food consumption for these households of almost 15%. As with overall consumption, there is no statistically significant effect discerned for households living in owned accommodation.

Fourth, the increased child benefit increased spending on clothing, but entirely through increased spending on children's clothing and not through increased spending on adult clothing. The estimated treatment effect shown in the leftmost columns is $183 for children's clothing, and is marginally statistically significant. In rental-tenure households, shown middle columns, the estimated average treatment effect on children's clothing expenditures is roughly $250 and is marginally statistically significant. But, in those same population subsamples, the point-estimate of the average treatment effect on adult clothing is much smaller (or negative) and not statistically significant. That households respond to an increased child benefit by increasing spending on children's clothing but not adult clothing suggests that either or both of two explanations. A preference-based (aka behavioural) approach would argue that preferences respond to the labelling of the child benefit, as in Kooreman (2000). A bargaining-based approach would argue the increase in female bargaining power in male-female households arising from the fact that benefits are paid to females shifts household spending toward female preferences, which favour children.

Fifth, we do not see any response in spending on child care, a subcategory of household operation. This could be because child-care expenditures are too small to pick up a treatment effect (they average roughly $150 per year in our samples). Alternatively, it could be that there really is no effect on the allocation to child care. Given the salience of child-care expenditures in the public debate on public expenditures and public transfers, the finding that child-care expenditures do not respond to a very large increase in transfers to households with children raises the eyebrow. There are two possibilities here. First, it may be that that households with children do not consider additional child-care spending to be the first priority when household budgets increase. Or, second, it may be that child care has a minimum purchase level so large that even with the large increase in the child benefit, the minimum purchase of child care is still unaffordable.

Heterogeneous Treatment Effects: Intensity of Treatment

The Canada Child Benefit delivers money to households in a linear fashion as the number of children rises. That is, the benefit for two children is twice that of the benefit for one child, and this is the case up to the seventh child. In order to investigate whether or not consumption responses differ depending on the intensity of treatment, we evaluate the treatment effect separately (heterogeneously) for households with one, two, or three or more

children. To implement this, we drop the dummy variable indicating the presence of children under 18 (and its interactions), and include as additional dummy variable regressors indicators that the household has one, two, or three or more children plus interactions of these three variables with *policy_date*. The estimated coefficients on these three interaction terms may be interpreted as estimated heterogeneous treatment effects for these three household types.

Table 3 presents estimated heterogeneous treatment effects for a subset of consumption categories: current consumption, child care, food, clothing, and rent. We investigate this subset of categories because these are the only ones where statistically significant effects were observed in Table 2 for the overall treatment effect on households with (any number of) children.

At the broadest level for overall consumption, given that the number of children drives the intensity of treatment and does so linearly, we would expect to see the largest treatment effects for households with three or more children and the smallest for households with just one child. However, this pattern is not evident, either in magnitudes or in terms of statistically significant point estimates in Table 3. The basic problem is that there are too few households of any one size to get a good estimate of the treatment effect for that household size.

Turning to the consumption categories, and focusing on the estimates for rental-tenure households in the middle block of the table, we see that the basic results from Table 2 are not overturned when we consider heterogeneous effects. Any increase in clothing spending is driven by children's clothing, rather than adult clothing, and it is statistically significant for households with two children. This again suggests that the naming of the child benefit or the identity of the benefit recipient may be important to the incidence of spending.

The estimated treatment effect on food spending is larger for households with one or two children and is roughly $1,000 for these two household sizes. That we do not see a statistically significant effect for households with three or more children (and that the point estimate is negative) suggests that the larger child benefits received by households with more children do not result in more food spending. As food is a non-shareable good, this is somewhat surprising.

The only other statistically significant effect we see for rental-tenure households is in shelter spending, and specifically on rent. The point estimates are consistent with larger treatment effects as the intensity of treatment rises, that is, larger households increase shelter spending more than do smaller households. Indeed, the estimated causal impact of the increase in the CCB on shelter spending for households with three or more children is roughly $2,500, about double the average effect reported in Table 2. Not surprisingly, for rental-tenure households, the increase in shelter spending is dominated by an increase in rental spending of nearly $2,000.

In terms of the incentives facing households, food is non-shareable and so is a relatively expensive way to deliver well-being to household members. That is, to increase the well-being of all members, the household must buy more for each of them. In contrast, shelter is shareable, and so is relatively cheap as a mechanism to deliver well-being to household members. That is, if the household spends more on shelter, all members in principle benefit. This contrast between food and shelter is stronger in larger households. So, in a household with three members, it is in

Table 3

Estimated Treatment Effects on Household Consumption ($), by Number of Children

			Bottom half		Bottom half, drop Aug to Oct 2016			
					Renters		Owners	
Category	*Subcategory*	Children	Est	*Std err*	Est	*Std err*	Est	*Std err*
Consumption		1 child	2,185	*1,516*	1,233	*1,851*	−1,054	*3,196*
		2 children	1,374	*1,845*	4,402**	*1,722*	−1,529	*3,496*
		3+ children	1,228	*2,310*	2,080	*2,314*	4,652	*5,252*
Clothing	*All*	1 child	12	*321*	424	*308*	−991**	*439*
		2 children	450*	*258*	385	*274*	407	*473*
		3+ children	−217	*358*	−174	*495*	91	*681*
	Adult	1 child	−61	*259*	335	*215*	−961***	*366*
		2 children	201	*174*	58	*167*	328	*382*
		3+ children	−311*	*177*	−22	*225*	−486	*335*
	Children	1 child	73	*90*	89	*118*	−30	*157*
		2 children	250*	*151*	326**	*151*	79	*222*
		3+ children	94	*263*	−152	*340*	577	*489*
Food		1 child	676**	*323*	1,076**	*421*	296	*578*
		2 children	558*	*319*	821***	*316*	515	*559*
		3+ children	−806	*633*	−1,045	*695*	83	*1,177*
Household operations	*Child care*	1 child	0	*95*	36	*122*	−27	*152*
		2 children	−77	*165*	100	*286*	−245	*229*
		3+ children	180	*158*	292	*229*	109	*226*
Shelter	*All*	1 child	1,467**	*712*	471	*501*	1,038	*1,480*
		2 children	873	*856*	994	*820*	1,891	*1,537*
		3+ children	1,443*	*805*	2,458***	*622*	1,278	*2,129*
	Rent	1 child	−354	*483*	322	*440*		
		2 children	−462	*469*	458	*777*		
		3+ children	2,178***	*725*	1,905***	*554*		

some sense three times costlier to increase the well-being of household members through spending on food than through spending shelter. In contrast, in a household with five members, it five times costlier. A natural response to this would be for larger households to be more inclined to put their new money into shelter.

Did Households Move in Response to the Policy Change?

The CCB clearly increased shelter spending in households with children, especially rental-tenure households with many children. This could have taken many forms: (a) shifting

address to more expensive rental accommodation; (b) spending more on ancillary shelter consumption, including housing utilities; or (c) paying more in rent for the same accommodation. We cannot directly investigate any of these channels with the SHS data. However, we can shed a little light on whether or not the first channel was very large. The SHS has data on the date that a family took residence in their current household, recorded at the month/year level. Consequently, we can assess whether or not there was any statistically significant increase in the probability of moving induced by the increase in the CCB. We estimate the linear probability model for whether or not a household moved since August 2016 for households observed in August 2016 to December 2017, with the same controls as in Tables 2 and 3, and with an indicator for presence (or number) of children. Households with children generally move less than households without children.

We then construct a difference-in-differences estimator by differencing out the estimate from a linear probability model on whether or not a household moved since August 2014 for households observed in August 2014 to December 2015. The identifying assumption for this treatment effect estimator is that the difference between the probabilities of moving between households with and without children would have been the same in 2014/15 and 2016/17 if the CCB had not changed. Table 4 presents these estimates.

Table 4

Estimated Treatment Effects the Probability of Moving

	Bottom half		**Bottom half**			
			Renters		Owners	
Children	Est	*Std err*	Est	*Std err*	Est	*Std err*
Any children	−0.001	*0.025*	0.035	*0.041*	−0.048	*0.030*
1 child	−0.031	*0.034*	0.029	*0.059*	−0.063	*0.039*
2 children	0.004	*0.030*	0.062	*0.061*	−0.058	*0.036*
3+ children	0.051	*0.037*	0.062	*0.053*	0.005	*0.052*

The absence of stars in Table 4 indicates that no individual estimated coefficient is statistically significant. Thus, nothing very reliable can be gained from these estimates. However, the point estimates are suggestive. In the upper left cell, we see a point estimate of the overall treatment effect of roughly zero when we consider all households. Looking across the row, it is clear that this is due to the fact that the point-estimate for renters is positive and that for owners is negative. Thus, if anything, the causal effect of the increase in the CCB was to increase the probability that renters with children moved and decrease the probability that owners with children moved.

Looking down the middle panel, we see mild evidence that the induced mobility was greater for households with many children than those with one child. In particular, the estimated effect for rental-tenure households with two or more children is 6.2 percentage points. Although,

not statistically significant, the point-estimate suggests that these households were more likely to move than households without children after the CCB came into effect.

Discussion and Policy

The empirical work suggests that the very large increase in CCB rates that occurred in July 2016 raised overall consumption of rental-tenure households with children, mainly through increased rental spending, food spending and children's clothing spending. This pattern highlights several patterns, some previously noted in the consumption literature. First, the increase in benefits raised consumption. If the ultimate goal of income security programs is to improve the well-being of people in lower-income households, and we believe that increased consumption accomplishes this, then the program "worked." Beatty and Tuttle (2014); Smith et al. (2016); and Hastings and Shapiro (2018) note a similar pattern in the United States.

Second, the labelling of the benefit as a child benefit may be salient, because we see an increase in children's but not adult clothing spending. This is akin to the finding of Beatty et al. (2014), wherein a cash transfer that was named a "fuel program" increased fuel spending but no other types of spending. (This program was a pure cash transfer and did not change the relative price of fuel.) It is well known that childhood poverty and consumption deprivation yields long-term negative consequences in productivity and earnings. This suggests that labelling the program as a "child benefit" as opposed to, for example, an "income security" program may deliver benefits in the long term.

Third, that the largest effect is on rent is both heartening and discouraging. It is heartening because shelter is a shareable good that benefits all household members, and so greater spending here yields a priori more aggregate welfare than spending on private goods. Further encouragement comes from the suggestive but not very persuasive evidence presented in Table 4 that households did shift address in response to the policy change. However, it is discouraging because a substantial literature suggests that rental markets, especially those of low-income households, have very steep supply curves (see, e.g., Susin, 2002). That is, the increase in incomes drives up demand, which increases the price of shelter. Susin (2002) found that roughly one-third of the spending increase result from a housing voucher program was eaten by increased prices. Other estimates range from one-third to two-thirds (see, e.g, LaFerrère and LeBlanc, 2004; Fack, 2006; Kangasharju, 2010; and Viren, 2013). A recent paper by Eriksen and Ross (2016) challenges this interpretation, arguing that nominal price increases mask quality improvements, so that *real* rent prices do not respond that much.

The bottom line here is that income support policy probably needs to be accompanied by housing policy that ensures a sufficient supply response to the increased budgets of low-income households. This would be a big lift; such housing policies include direct public production of low-rent accommodation, subsidy of private production and/or management of low-rent accommodation, and subsidies to existing low-rent housing stock. All these policies are expensive, and have been without substantial federal leadership for several decades.

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