Liquidity Constraints and Human Capital: The Impact of Child Allowances on Arab Families in Israel

Shay Tsur*

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Abstract

Liquidity constraints might lead to children quitting school, even if the long run returns to schooling justify the investment. This study empirically tests for the existence of such liquidity constraints, by evaluating a policy decision that increased child allowances paid to Arab families in Israel during the 1990s. The identification strategy is based on the fact that the increase in the allowance to households with 4 or more children was much larger than the increase for households with 3 children. Using difference-in-differences methodology, I find that the increased allowance significantly raised high-school attendance of girls with low socioeconomic status, due partly to a decrease in their employment: An increase of 1% in family income as a result of the increase in the allowance led to an increase of more than 2 percentage points in school attendance. This evidence suggests that direct cash transfers to poor families in advanced economies can enhance the human capital accumulation of their children, even if the transfers are not conditioned on education.

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1 Introduction

Growth theory and policy makers have embraced insufficient human capital accumulation as an important channel through which poverty negatively affects economic growth. This negative effect might be driven by liquidity constraints (Galor and Zeira, 1993), and both theoretical and empirical studies have emphsized the role of these constraints in poor countries. Baland and Robinson (2000) claim that liquidity constraints might make children work even if the long run returns to schooling are higher than the returns to work. Hazan and Berdugo (2002) show that in the early stages of a country's development, child labor reinforces the development trap, where fertility is high and output per capita is low. According to Hazan and Berdugo, compulsory schooling for a given period, and compensating parents for the forgone earnings of their children, expedites the transition process and generates a Pareto dominated outcome. Empirically, Edmonds (2006) found a large increase in school attendance and a decline in total hours worked among children in black South African families who became eligible for social pension income. Calero et al. (2009) found that remittances increase school enrollment and decrease the incidence of child labor, especially for girls in rural areas in Ecuador. In Mexico, a middle income country, Fernald et al. (2008) and Fernald et al. (2009) found that an improved environment for children (e.g., housing level) and a better ability to purchase goods could directly affect child development. They claim that improvements in child outcomes might also occur indirectly via the psychological well-being of family members, or via better ability to finance direct schooling costs.

While scholars agree that liquidity constraints are binding in poor countries – where family's decision in favor of child labor is enforced by the subsistence threshold – the importance of liquidity constraints to education decisions is controversial in the context of advanced economies. Moreover, the discussion in countries like the United States is focussed on tertiary education rather than on more basic education. Kane (1994) and Ellwood et al. (2000) claim to find direct empirical evidence of liquidity constraints in purchasing a college education. Furthermore, Card (2001) has argued that findings on higher return to schooling in instrumental variable regressions generally support the presence of liquidity constraints. However, at the same time, Cameron and Heckman (2001) and Carneiro and Heckman (2002), argue that those findings of liquidity constraint indicate long-term family background factors, rather than short-term liquidity constraints.

This paper examines the implication of liquidity constraints on basic human capital accumulation in Israel, a high income country.[1] The study exploits a natural experiment whereby child allowances paid to Arab families in Israel increased, and tests the causal effect of such transfer on children's high school attendance. The Israeli economy is well developed, but inequality and the poverty rate in Israel are higher than the OECD average. Particularly, the Arab population in Israel is much poorer compared to the general population: in 2014, 53 percent of Arab families were poor, compared to 19 percent in the general population.[2],[3] Therefore an important contribution of this study is to better understand the implication of large inequality in high income countries on liquidity constraints and on human capital accumulation. The study will also shed some light on the channels of action. This study, however, does not aim to test the overall effectiveness of child allowances, because child allowance might also increase birthrate and decrease parents' labor incentive.

Since the 1970s, large Israeli veterans' families – headed by a person who served in Israel's military – have received a supplementary child allowance. The vast majority of Israeli Arabs were not eligible for that allowance, since they generally do not serve in the military. The government decided in December 1992 to gradually increase, from the beginning of 1994, the allowances paid to non-veterans' families, and to equalize them with

[1] Israel's GDP per capita is $35,000 (2015), and the country is ranked 18 in the Human Development Index (HDI).

[2] The "post goverment intervetion" GINI in Israel is 0.36, compared to 0.31 of the OECD average, and the poverty rate is 19 percent, compared to 11 percent, on average, over OECD countries (the Bank of Israel Annual Report for 2015).

[3] See the National Insurance Institute of Israel Poverty Report for 2014.

the level of the child allowance that veterans' families received. The main beneficiaries of this decision were the Arab population.

My identification strategy is based on the fact that the allowance had been raised to all Arab families with at least 4 children aged 0-17 since 1994. This rule created a useful discontinuity point, allowing a comparison between the treated – children aged 16-17 from households with four or five children aged 0-17, and the untreated – children aged 16-17 from households with three children aged 0-17. Using Labor Force Survey (LFS) data before and after 1994, difference-in-differences regressions were designed in order to control for the basic differences between these two groups.

The evidence presented in this paper suggests that the extra allowance paid to Arab families with four or five eligible children (aged 0-17) increased school attendance of girls aged 16-17 from a low socioeconomic background. The extra allowance decreased the employment of the treated girls to a near-zero level, but most of the increase in school attendance was reflected in a decrease of the share of children neither studying nor working in paid jobs (idleness). The strongest effect is found among girls with young siblings, supporting a dominant mechanism of reducing home production and childcare participation of girls.

The results show that poverty might lead to a liquidity constraint among sub-populations in high income countries where poverty is not necessarily reflected in subsistence risk. The results support the effectiveness of welfare policy that directly transfers resources to poor families in easing liquidity constraints and enhancing human capital accumulation.

The remainder of this paper is organized as follows: Section 2 presents background on child allowances in Israel and on the policy studied here. Section 3 describes the data used in this paper, and explains the identification strategy. Section 4 discusses the results, and Section 5 concludes.

2 Child allowances in Israel

Child allowances have had a major role in redistribution policy in Israel over time. Since the early 1970s, child support had grown rapidly, favoring large Jewish families. Army Veteran families received a special additional child allowance, and ultra-Orthodox households headed by students in a "yeshiva" (religious studies institution) also enjoyed this supplementary allowance even though their head of household did not perform military service. Mayshar and Manski (2000) show – without claiming causality – that the total birthrate among Ashkenazi (Jews originating from Eastern Europe) ultra-Orthodox women increased sharply following the transition (1975–79). They relate these findings to the Malthusian hypothesis, which argues that child support increases the birthrate and decreases labor incentive, thus preserving poverty.

The child allowances that had been paid to other households, most of them Arab, increased gradually during the period 1994-97, following the decision in December 1992 that canceled the link between military service and the amount of child allowances.[4] Until then, in 1975–1993, households that were denied "army veteran" status received the local-currency equivalent of USD 65 per child monthly (Table 1) for each of their first two children and USD 82 for each additional child. In 1997, following the gradual equalization of eligibility, the child allowance for the fourth child increased by USD 117 while the monthly increase in the child allowance for the third child was only USD 16 during that period.

Child allowances constitute a significant proportion of the Arab population's income because Arab families have a relatively low per-capita income. Their low per-capita income traces to the low wage earned by men, the low workforce participation rate among women (primarily in the Muslim population), and the high number of children per household. Specifically, the child allowances for the fourth child constitute more than 10 percent of average Arab family income and 32 percent of the per-capita (private) consumption of

[4] "Yediot Achronot" newspaper, December, 9, 1992. The implementation was planned to take 3 years according to the original decision, but in practice it took 4 years.

Table 1: Monthly child allowance paid to non-veteran households by child order (per child, 1993 and 1997, 1993 Constant prices)

The allowance in USD			The gap between 1993 to 1997	
Child order	1993	1997	In USD	% of per capita consumption
1	65	65	0	0%
2	65	65	0	0%
3	82	98	16	8%
4	82	199	117	32%
5	82	167	85	24%
6	82	184	102	28%
7	82	172	90	25%

Source: Based on table 1 from Frish (2008)

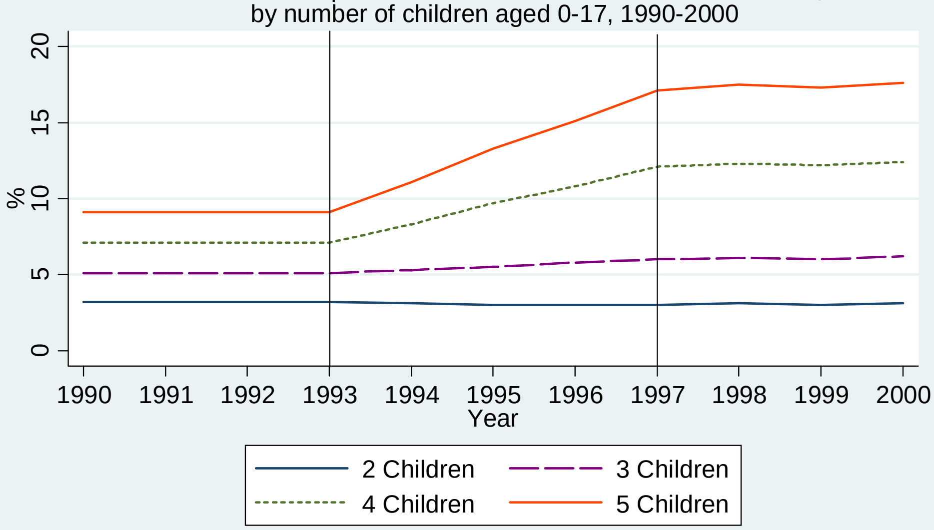
non-Jewish households (Table 1).

Frish (2008) found that the 1994-1997 increase in child allowances increased the completed fertility rate of Druze women but did not affect the fertility of Bedouins and Muslims. Toledano et al. (2011) found that child allowances increased the probability of a married Arab woman giving birth by about 6–7 percent. My paper is the first to test whether the child allowances increase has alleviated poverty among children, at least shortly after the change, and led them to acquire more education.

3 Sample description and identification strategy

To empirically test the existence of liquidity constraints in the purchasing of human capital, I evaluate the effect of a policy decision that increased the child allowance paid to Arab families in Israel during the 1990s. The decision made in 1993 emphasised the support for large families. Figure 1 shows that there was no increase in the allowance to Arab households with two children, there was a small increase in the allowance to households with three children, and a much higher increase to households with at least four children.

Figure 1
Total child allowance for Arab households,
1993 constant prices as a share of 1995 household income,
by number of children aged 0-17, 1990-2000



Source: Based on National Insurance Institute of Israel annual reports, and the 1995 CBS Census.

This structure of the change in the allowance created the treated group – children aged 16-17 from households with at least four children aged 0-17 (eligibility spectrum). Children aged 16-17 from households with three children aged 0-17 are a good comparison group, since the increase in the allowance for such households was negligible. The increase in the allowance for the fifth child was very similar to the increase in the allowance given for the fourth child, so the total increase in the allowance that was paid to these households was higher. I will exploite this variation as well.

The estimated equation is:

$$Y_{it} = \alpha + \beta_1 x_{it} + \beta_2 four_{it} + \beta_3 post94_{it} + \beta_4 four_{it} \times post94_{it} + \delta_t + \varepsilon_{it}$$

where Y_{it} is the outcome variable (school attendance, employment or idleness) for individual i in time t, x is a set of child characteristics, $four$ is a dummy variable for living in a household with 4 children aged 0-17, and $post94$ is a dummy variable for the years after 1994 – the years after the beginning of the implementation. The interaction variable that denotes the treatment on children from households with 4 children aged 0-17 after 1994 is $four \times post94$. The coefficient of interest is β_4 which captures the difference-in-differences estimate. δ is the year effect for each year in the sample. The same methodology was used when setting households with four or five children as a treatment group. A parallel methodology is used in the robustness and in the placebo tests.

I expect to find that the treatment has primarily affected children from low socioeconomic background facing a liquidity constraint. Therefore the focus will be on estimating difference-in-differences regressions using a sample of children with maximum 12 years of father's schooling. Since schooling in Israel was compulsory until age 15 ($10^{\underline{th}}$ grade), the study will focus on children aged 16-17.[5]

The data source for the primary tests presented in this paper is the LFS, comparing the post-treatment years 1994-1997 to the pre-treatment period 1989-1993. The top panel of Table 2 displays a balancing test of the characteristics of children aged 16-17 with maximum 12 years of father's schooling over the pre-treatment period (1989-1993). Average school attendance was higher in households with three children, whereas employment and idleness were lower, both for boys and for girls. As for the covariates, the girls' ratio is around half in all groups. There are some differences in parents' education: Mothers' years of schooling were slightly but significantly lower for treated girls, but fathers' years of schooling were higher for the group of girls from households with four children aged 0-17. The parents of the treated are significantly younger. Further exploring the background of the groups based on a snapshot from the 1995 census (in the bottom panel) suggests that the likelihood of being treated is not derived from differences in mother's total fertility rate.[6] Hence, the number of children aged 0-17 in the time of the treatment is either the result of incomplete fertility, or reflects families with older offsprings. Considering that the average age of the

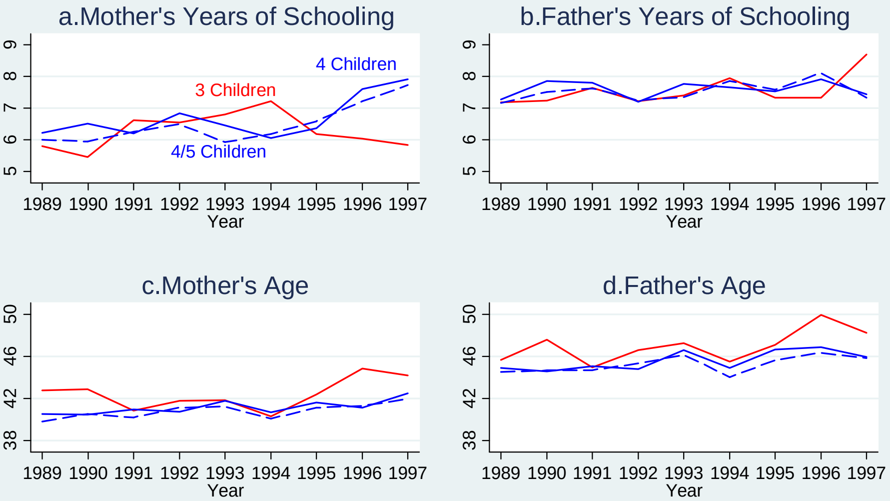
[5] Schooling was free until the end of high-school. At the present, schooling in Israel is compulsory and free until the end of high-school.

[6] Data from the 1995 census was used in order to have richer balancing tests. The covariates shown in the bottom panel of Table 2 are not available in the Labor Force Surveys, so they can not be used in the regressions.

mothers is 41, and that half of the mothers in the sample are above 40, it seems that both reasons are plausible. I will adress later the concerns that the case of incomplete fertility rate produces. Finally, the total income of all groups was not significantly different in 1995. Although some characteristics are significantly different between both groups, overall the balancing tests suggest that differences in the likelihood of being treated do not systematically derive from differences in family background, but mainly from the timing of the treatment in family life cycle. It seems that the background of the groups is close enough that potential differences between these two groups can be controlled by using difference-in-differences methodology between them.

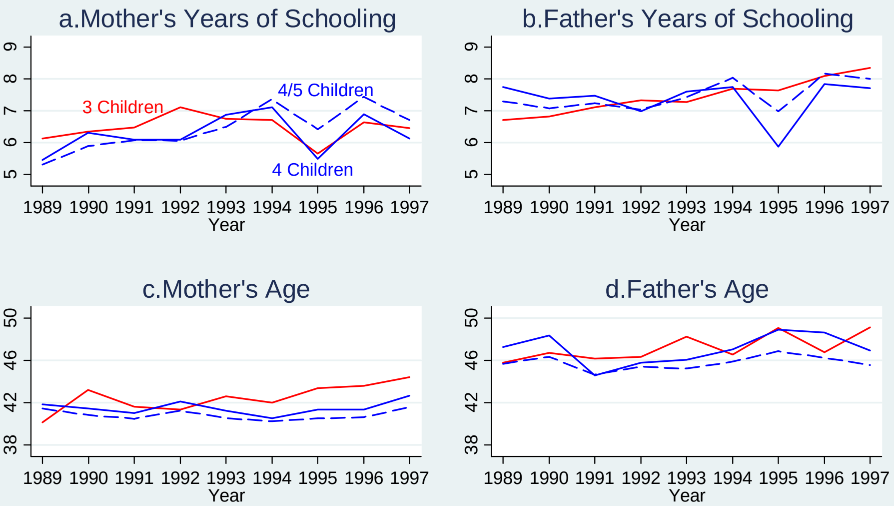
The sample being used in this study is repeated cross sections and not a panel of households. Therefore the composition of the groups is tested in order to reduce the possibility that a change in the composition of the groups affects the results. Figures 2 and 3 show the evolution of parents' age and education year by year between 1989-1997, for boys and girls respectively. Most of the covariates have changed similarly between the pre-treatment and the post-treatment period, and the difference-in-differences between them are usually not significant. Differential evolutions did appear in mothers' education for boys - mothers in the group of four or five children were relatively more educated in the later period (Figure 2.a). Mothers were also younger in the group of four or five children in the later period for girls (Figure 3.c). I conclude that the four or five children group's characteristics has slightly improved relative to households with three children. Although this improvement seems to be neglectad, this concern will be addressed further in Section 4.2.

Figure 2
Household background by number of children aged 0-17,
Boys, 1989-1997



Source: Based on Labor Force Surveys.

Figure 3
Household background by number of children aged 0-17,
Girls, 1989-1997



Source: Based on Labor Force Surveys.

Difference-in-differences methodology assumes that the trend in both groups explored would be the same if it were not for the intervention. Naturally, it is impossible to verify that assumption, but the pre-treatment similarity of the characteristics demonstrated in Table 2 contributes to the possibility that trends would have been similar. Similar historical trends might also support this assumption: Figure 4 shows that the trends of school attendance in all three groups (3, 4, and 5 children) were almost the same until 1993, preserving a constant gap between groups. The gap was closed during the years 1994 to 1997, the years of implementing the policy that increased large families' child allowances. Figures 5 and 6 show that the constant gap in school attendance (left side graphs) before 1994, and the narrowing of the gap after 1994 was emphasized much more among girls. A decrease in girls' employment can be seen during the years 1994-1997, without a similar decraese among girls from households with 3 children.

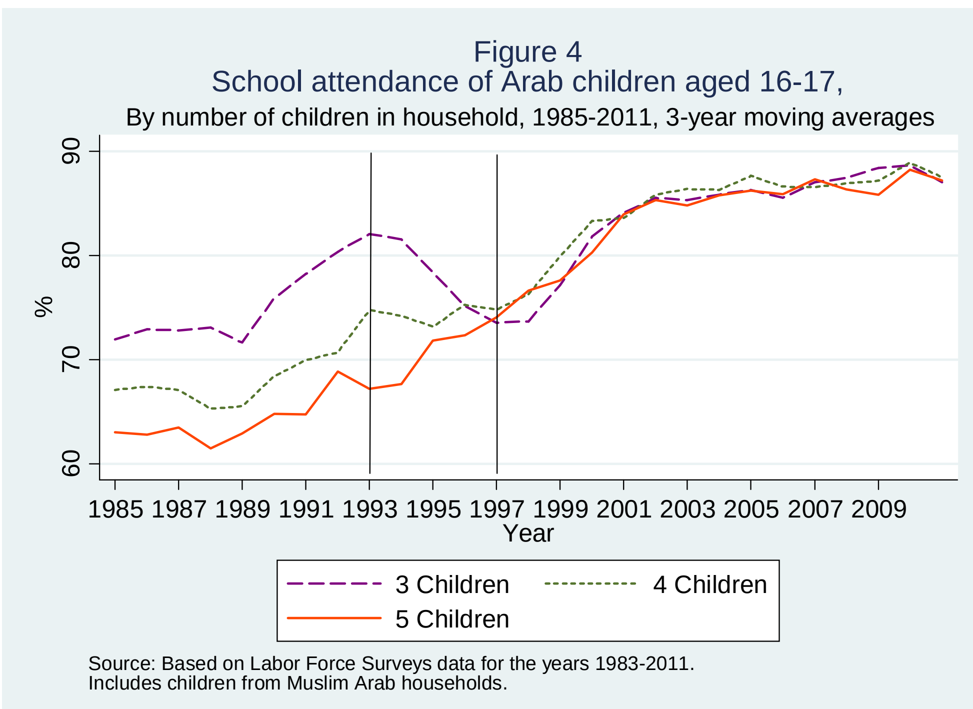
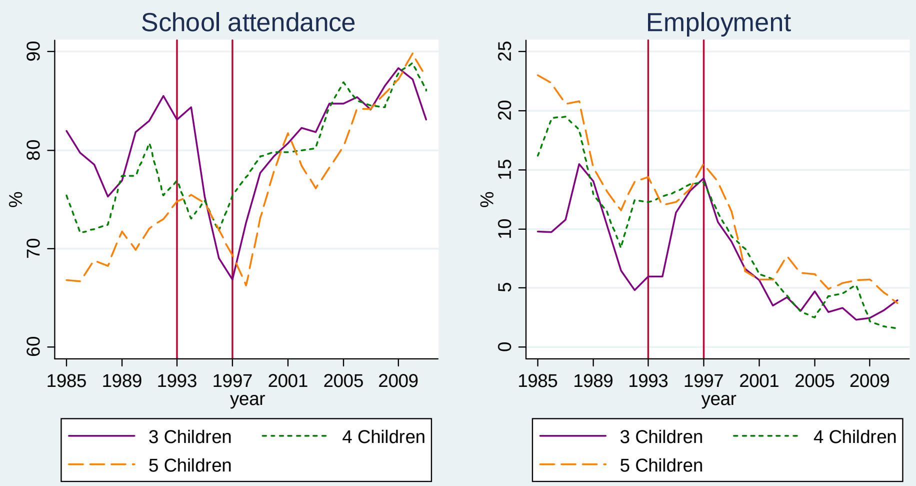
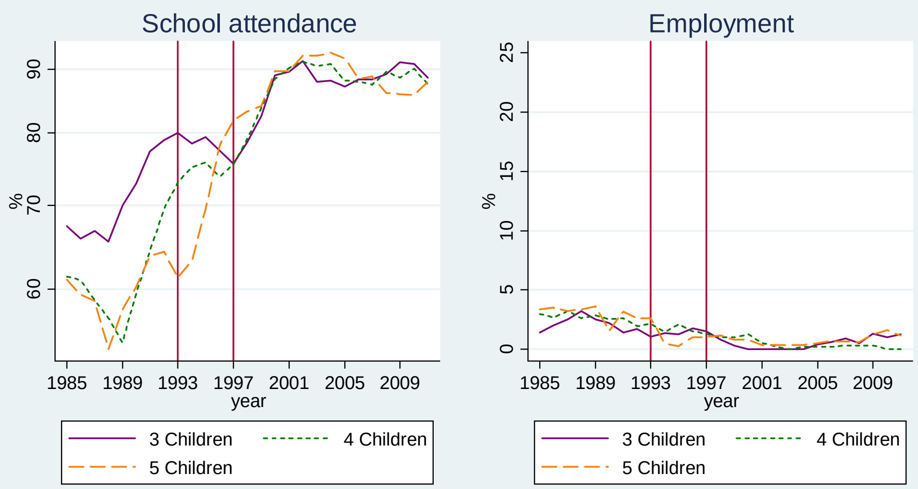


Figure 5
School attendance and employment of Arab boys aged 16-17,
by number of 0-17 children, 1985-2011, 3-year moving averages



Source: Based on Labor Force Surveys data for the years 1983-2011.
Includes children from Muslim Arab households.

Figure 6
School attendance and employment of Arab girls aged 16-17,
By number of 0-17 children, 1985-2011, 3-year moving averages



Source: Based on Labor Force Surveys data for the years 1983-2011.
Includes children from Muslim Arab households.

Table 2: Descriptive statistics and balancing tests for young Israeli Arabs in households with 3-5 children

(1989-1993, Maximum 12 years of father's schooling, Children aged 16-17)

	All					Boys					Girls				
0-17 Children	3	4	4/5	Diffs		3	4	4/5	Diffs		3	4	4/5	Diffs	
				(4)-(3)	(4/5)-(3)				(4)-(3)	(4/5)-(3)				(4)-(3)	(4/5)-(3)
Observations	563	656	1044			274	342	522			289	314	522		
School Attendance	79.8	73.4	70.3	-0.06***	-0.09***	79.3	74.9	73.7	-0.04	-0.06*	80.2	71.8	67.1	-0.08**	-0.13***
Employment	3.7	7.2	7.3	0.03***	0.04***	6.7	11.7	11.9	0.05**	0.05**	1.1	2.4	2.8	0.01	0.02*
Idleness	16.5	19.2	22.5	0.03	0.06***	14.0	13.4	14.4	-0.01	0.00	18.7	25.5	30.3	0.07**	0.12***
Female	53.0	48.5	50.8	-0.05	-0.02										
Mother's Schooling	6.5	6.4	6.0	-0.14	-0.44***	6.3	6.5	6.1	0.15	-0.20	6.6	6.2	6.0	-0.41**	-0.67***
Father's Schooling	7.2	7.5	7.3	0.28**	0.07	7.4	7.6	7.4	0.22	0.02	7.1	7.4	7.2	0.32*	0.11
Mother's Age	41.9	41.2	40.8	-0.72**	-1.13***	41.9	40.9	40.6	-1.02***	-1.27***	41.9	41.5	40.9	-0.40	-1.00***
Father's Age	46.7	45.7	45.2	-0.95***	-1.40***	46.5	45.1	45.1	-1.35***	-1.43***	46.8	46.3	45.4	-0.49	-1.37***
1995 Census															
Observations	492	559	1056			257	285	536			235	274	520		
Mother's TFR	6.6	6.5	6.7	-0.01	0.15	6.4	6.4	6.5	-0.08	0.08	6.7	6.7	6.9	0.06	0.22
Household's Income	6848	6535	6464	-313.06	-384.52	6720	6610	6479	-109.42	-240.27	6989	6458	6448	-531.76	-541.31

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1
The top panel reports averages based on pre-treatment (1989-1993) Labor Force Surveys data.
School attendance, employment, idleness, and sex represent shares of total in percents.
The bottom panel reports averages based on 1995 census. Household's Income in NIS, nominal 1995 prices.

The relatively low school attendance and the high employment rates among teenagers from larger households before the treatment (Table 2 and figures 4-6) can be related to their low per capita income and to liquidity constraints, since other characteristics of both groups were very similar. The narrowing gaps demonstrated in Figure 4 provide preliminary support to the possibility that the increase in the allowance paid to Arab families with more than 4 eligible children increased school attendance by easing the liquidity constraint (especially among girls, Figure 6). However, one may ask why the gap has closed at the same time that a decrease in school attendance among the comparison group took place. Figures 5 and 6 show that this dynamic was limited to boys, wherhes among girls most of the effect is the result of an increase in school attendance among the treated. A possible explanation for the dynamic among boys is that labor market macro conditions improved sharply in the years of treatment, resulting in an increase of the employment for the comparison group. Figure A1 shows indeed that general unemployment sharply decreased, and the option of working instead of studying generally became more attractive. These macro conditions acted as a contrarian force against the allowances for the treated group, but resulted in a clear increase of employment and a decrease in school attendance for the comparison group among boys.

4 Results

4.1 School attendance, employment and idleness

As already discussed in Section 3, it is likely that the treatment particularly affected children from low socioeconomic background, since they are more likely to face liquidity constraints. The treatment was also more likely to affect children aged 16-17 because compulsory schooling in Israel is binding until the age of 15 ($10^{\underline{th}}$ grade). Table 3 reports difference-in-differences estimations using a sample of children aged 16-17 with maximum 12 years of father's schooling. The results in Panel A indicate that the increase in the allowance paid to Arab families with 4 eligible children increased school attendance of children aged 16-17, by 8 percent. The estimates for girls (equations 3 and 4) are higher (11 percent) and significant. Considering that the increase in the allowance for the treatment group was equal to 4 percent of the average household's income, I conclude that a 1 percent increase in income due to the allowance led to a more than 2 percentage points increase in girls' school attendance. The estimates for boys (equations 5 and 6) are lower and not statistically significant. The estimates in the models with or without controls are very similar. This indicates that the treatment is unlikely to be correlated with the observables, and therefore lowers the possibility of potential bias due to unobservable relevant variables, or a change in the composition of the groups between years.

Panel B in Table 3 presents results of the specifications that used four or five children households as a treatment group, among whom the average increase in the allowance was 6 percent of family income. The post-1994 estimates for girls and for boys together, and for girls separately, are higher than in Panel A, preserving the elasticity with respect to income for girls as in Panel A.

After comparing groups of years - before and after treatment - it would be helpful to test for the timing and evolution of the effect as well. The results from models that regress year by year difference-in-differences (presented in the appendix - Tables A1 and A2) suffer from high standard errors and are very volatile, *inter alia* because the number of observations is low for each year individually.[7] The model for girls yields a positive and significant effect in 1997 - the year of the full implementation - (relative to the omitted years 1991-1992), both when using the group of only four children households as the treatment group (Table A1), or using the four and five children households joint group (Table A2). In order

[7] The regressions are represented by:

$$Y_{it} = \alpha + \beta_1 x_{it} + \beta_2 four_{it} + \beta_y four_{it} \times year + \delta_t + \varepsilon_{it}$$

β_y represents a specific estimate for each year interacted with the dummy for the treated, while omitting 1991 and 1992 from the regression, so the effect is relative to the gap between the two groups in those years.

Table 3:
The effect of child allowance on school attendance of young Israeli Arabs
(1989-1997, Maximum 12 years of father's schooling, Children aged 16-17)

Panel A: Households with 4 children relative to households with 3 children						
	(1)	(2)	(3)	(4)	(5)	(6)
	All	All	Girls	Girls	Boys	Boys
4 children*post 1994	0.07	0.08*	0.10	0.11*	0.04	0.06
	(0.04)	(0.04)	(0.06)	(0.06)	(0.06)	(0.06)
Observations	1,991	1,991	956	956	1,035	1,035
Year effect	yes	yes	yes	yes	yes	yes
Control variables	no	yes	no	yes	no	yes
Panel B: Households with 4 or 5 children relative to households with 3 children						
	(1)	(2)	(3)	(4)	(5)	(6)
	All	All	Girls	Girls	Boys	Boys
4/5 children*post 1994	0.10**	0.09**	0.15***	0.13**	0.04	0.05
	(0.04)	(0.04)	(0.05)	(0.05)	(0.05)	(0.05)
Observations	2,695	2,695	1,334	1,334	1,361	1,361
Year effect	yes	yes	yes	yes	yes	yes
Control variables	no	yes	no	yes	no	yes

Robust standard errors in parentheses; *** p<0.01, ** p<0.05, * p<0.1
The table reports difference-in-differences for 1989-1997 Labor Force Surveys. The sample in Panel A includes only households with 3-4 children aged 0-17. The sample in Panel B includes only households with 3-5 children aged 0-17. The treatment variable is the interaction of four (four/five in Panel B) children with post-1994. Where indicated, a set of controls listed below are included: mother's education, father's education,mother's age, father's age, dummies for district of living, quarter of survey, age, and gender. Data is weighted using sampling weights.

to overcome the high standard errors problem and test if the evolution of the estimates before 1997 is consistent with the timing of the policy, every two years are combined and regressed relative to the omitted years 1991-1992. This way a much clearer evolution of the estimates is found, especially in the test of the four and five children household group in Table A2: the estimate for 1989-1990 is zero for girls (equation 4), suggesting that there was no differential trend before the treatment, the estimate for 1993-1994 is positive but not significant, and the estimate for 1995-1996 is higher and significant. The estimate for 1997 is the same estimate as the one obtained in model 3 - it is the highest and the most significant.

The strong results that were found using four or five children households as treatment groups are reflected in the estimations that explore the channels of the influence as well. The

Table 4:
The effect of child allowance on the alternatives for school of young Israeli Arabs, Households with 4 or 5 children relative to households with 3 children (1989-1997, Maximum 12 years of father's schooling, Children aged 16-17)

Panel A: Employment						
	(1)	(2)	(3)	(4)	(5)	(6)
	All	All	Girls	Girls	Boys	Boys
4/5 children*post 1994	-0.02	-0.01	-0.03*	-0.03*	0.01	0.01
	(0.02)	(0.02)	(0.02)	(0.02)	(0.04)	(0.04)
Observations	2,695	2,695	1,334	1,334	1,361	1,361
Year effect	yes	yes	yes	yes	yes	yes
Control variables	no	yes	no	yes	no	yes
Panel B: Idelness						
	(1)	(2)	(3)	(4)	(5)	(6)
	All	All	Girls	Girls	Boys	Boys
4/5 children*post 1994	-0.08**	-0.08**	-0.12**	-0.10*	-0.05	-0.06
	(0.03)	(0.03)	(0.05)	(0.05)	(0.04)	(0.04)
Observations	2,695	2,695	1,334	1,334	1,361	1,361
Year effect	yes	yes	yes	yes	yes	yes
Control variables	no	yes	no	yes	no	yes

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

The table reports difference-in-differences for 1989-1997 Labor Force Surveys. The sample includes only households with 3-5 children aged 0-17. The treatment variable is the interaction of four/five children households with post-1994. Where indicated, a set of controls listed below are included: mother's education, father's education, mother's age, father's age, dummies for district of living, quarter of survey, age, and gender. Data is weighted using sampling weights.

effect of the treatment on employment and on idleness (Table 4) are negative and significant for girls, unlike the effects on families with 4 eligible children (which are negative but not significant - not presented). The estimates of the effect of the treatment on employment (–0.03, Panel A, Equation 3 and 4) indicate that the rate of employment decreases from an average of 2.8 for the years before the policy change (Table 2), to a zero level after the policy change. However, only a fifth of the positive effect on school attendance of girls was driven by the decrease in the employment to that zero level. Most of the increase in school attendance was accompanied by a decrease of the share of children neither studying nor working (idleness, Panel B).

The large negative effect of the policy change on idleness might be related to reporting issues, if the decrease took place in unreported employment, *inter alia* in unreported employment in family business. A more substantive explanation might be that there was a decrease in the participation in home production and childcare. In order to test if it is likely that this was the channel, regressions for girls are run separately for girls from households with young children (younger than 9 years old), and for the rest of the girls (Table 5). I find strong support for the participation in home production and in childcare mechanism: girls in households with young children largely increased their school attendance, while the results for the rest of the girls are not significantly different from zero. It might be that the parents could afford other alternatives for the participation of girls in home production and in childcare, such as purchasing services or reducing their own employment hours.

The effect being limited to girls needs to be explained. The first possible explanation is that the return on education in 1995 according to a standard Mincer equation was only 4 percent for Arab men, while the return for Arab women was 10 percent. This gap might have made the schooling option less attractive for boys. The second possible explanation is that the allowance increase - 8 percent of total household income - was too low to compensate boys for not working: According to 1995 Census data, the average income of employed boys aged 16-17 was 30 percent of total household income, while average

Table 5:
The effect of child allowance on school attendance of young Israeli Arab girls in households with 4 or 5 children relative to households with 3 children, by having at least one sibling under the age of 9 or not
(1989-1997, Maximum 12 years of father's schooling, Children aged 16-17)

	(1) All	(2) All	(3) Sib'<=9	(4) Sib'<=9	(5) Sib'>9	(6) Sib'>9
4/5 children*post 1994	0.15***	0.13**	0.41**	0.35**	0.03	0.04
	(0.05)	(0.05)	(0.18)	(0.18)	(0.07)	(0.07)
Observations	1,334	1,334	402	402	932	932
Year effect	yes	yes	yes	yes	yes	yes
Control variables	no	yes	no	yes	no	yes

Robust standard errors in parentheses; *** p<0.01, ** p<0.05, * p<0.1
The table reports difference-in-differences for 1989-1997 Labor Force Surveys. The sample includes only households with 3-5 children aged 0-17. The treatment variable is the interaction of four/five children households with post-1994. Where indicated, a set of controls listed below are included: mother's education, father's education, mother's age, father's age, dummies for district of living, quarter of survey, age, and gender. Data is weighted using sampling weights.

income for girls was 20 percent. This difference might explain the lack of any effect of the allowance on the employment for boys, especially given the high share of employed boys (11 percent for the treated). The boost in construction during the treatment period might have further increased the outside option for boys, and weakened the potential effect of the allowance. These two explanations are complementary, in the sense that they incorporate a cost-effectiveness consideration into the decision making in the age of 16-17.

Since child allowances in Israel are generally paid to the mothers, the results here might be related to bargaining power within the households.[8,9] Lundberg et al. (1997) find that when a child allowance was provided to women, there was a rise in the expenditure on

[8] The National Insurance pays the allowance to the bank account of the mother or to a joint account.

[9] As oppose to "unitary" approach models of household's decision making (Becker, 1981), individual utility models of the households (see for example Lundberg and Pollak (1994)) distinguish between the effect of income received by the husband and the effect of income received by the wife on consumption and time allocation decisions.

childrens' clothing. Duflo (2003) finds that pensions received by women in South Africa had a large impact on the health of girls, but little effect on that of boys. Duflo's paper raised the hypothesis that grandmothers prefer girls, but this hypothesis has been left as an open question. Hence, it might be that paying the allowances to the mother in Israel has enhanced the effect on human capital accumulation, especially for girls.

Since it is reasonable to assume a continuous effect, I tested specifications that also included the years 1998-2000 - the years after completing the implementation (not presented).[10] Most of the results are similar to the results in Tables 3 and 4. The estimates for boys are higher and almost significant. It might be that the effect was delayed for boys, because they had a more important part in creating income for the household, especially during the construction boom.

The same models were estimated without restricting the sample to children with maximum 12 years of father's schooling, and among children aged 15 (not presented). Although positive, none of these models yielded a significant estimate. As assumed, the policy has especially affected children aged 16-17 from low socioeconomic background, who are more likely to face a liquidity constraint.

Some Jewish families that were not headed by an army Veteran benefited from the decision to equalize the allowances. However, most of the Jewish population serves in the army and already received the larger allowance before 1994. Therefore I excpect to find the effect on Jewish children to be mild, if at all. Table 6 presents the results for Jews: Some of the estimates without controls are significant, but most of them are not, and they are much weaker than the estimates for Arabs. The results for the specifications using only households with four children as a treatment group, or jointly using households with four and five children as a treatment group (presented in Table 6), are essentially the same for Jews, whereas the latter specifications produced much larger and much more

[10] I limited the period until 2000, because in 2001 the government decided on another policy change - an increase in child allowances for all large families. Shortly after, in 2003, it decided to cut them to a very low amount, in a gradual process through 2003-2007.

Table 6:
The effect of child allowance on school attendance of young Israeli Jews
in households with 4 or 5 children relative to households with 3 children
(1989-1997, Maximum 12 years of father's schooling, Children aged 16-17)

	(1)	(2)	(3)	(4)	(5)	(6)
	All	All	Girls	Girls	Boys	Boys
4/5 children*post 1994	0.04**	0.03**	0.04*	0.03	0.04	0.04
	(0.02)	(0.02)	(0.02)	(0.02)	(0.03)	(0.03)
Observations	5,216	5,216	2,711	2,711	2,505	2,505
Year effect	yes	yes	yes	yes	yes	yes
Control variables	no	yes	no	yes	no	yes

Robust standard errors in parentheses; *** p<0.01, ** p<0.05, * p<0.1
The table reports difference-in-differences for 1989-1997 Labor Force Surveys. The sample includes only households with 3-5 children aged 0-17. The treatment variable is the interaction of four/five children households with post-1994. Where indicated, a set of controls listed below are included: mother's education, father's education, mother's age, father's age, dummies for district of living, quarter of survey, age, and gender. Data is weighted using sampling weights.

significant estimates for Arabs (Panel B in Table 3). The weaker and mostly not significant results (although the number of observations is much larger) support the conclusion that the narrowed gap between 3 children and 4 or 5 children Arab households after 1994 is a result of the larger allowances given mostly to Arabs.

4.2 Robustness tests and potential threats to the identification

To ensure that the results are robust to the choice of the first year of the sample, the models were tested by using two more samples (not presented): a sample for the years 1988-1997 (beginning one year earlier), and for the years 1990-1997 (beginning one year later). The estimates obtained in those tests are similar to the baseline results, although the effects for the shorter sample are only barely significant on families with 4 eligible children. Estimates using households with four or five children as a treatment group are significant in the shorter sample as well.

Both the income effect and the substitution effect of the child allowances point toward having another child in order to enjoy the larger allowance for the fourth child. The direction of the bias in cases where families had indeed made this decision is ambiguous: The estimates are downward biased if the dominant channel would be that the larger allowance is financing another child, instead of increasing per-capita income and easing liquidity constraints. In contrast, families that otherwise would have less children might be from better backgrounds and might bias the estimates upward. Therefore a test was conducted limited to children whose mother's age was 43 or more in 1993, and therefore had a low chance for having another child. Results in Table 7 indicate that the estimates are higher in these sub-sample regressions, specifically for girls, implying that the main results might be downward biased. However, their large size might simply reflect the fact that children with older mothers live in more traditional households, from lower socioeconomic background, facing tighter liquidity constraints. Therefore, instead of limiting the age of the mothers, I conducted a test limiting the sample to children whose mother did not give birth in the years 1993-1997 (not presented[11]). The results indicate that the estimates are very similar to the baseline estimates (from Table 3). This test indicates that the results in the paper were not obtained due to families with 3 children in 1993 from strong socioeconomic background that moved to the 4 or 5 children group as a result of the policy decision and biased the estimates upward.

One may wonder whether the effect found among treated children can be attributed to the intervention or whether it is the result of other shocks or convergence that have generally narrowed the gaps between children. To address this, two kinds of "placebo" tests were performed:

The first, presented in Panel A of Table 8, refers to children from households with three children aged 0-17 as the treated group and children from households with two children

[11] It should be noted that these estimates might be biased due to the fact that they are based on a selective sub-sample that didn't react to the policy by having another child.

Table 7:
The effect of child allowance on school attendance of young Israeli Arabs in households with 4 or 5 children relative to households with 3 children
Only mothers aged 43+ in 1993
(1989-1997, Maximum 12 years of father's schooling, Children aged 16-17)

	(1)	(2)	(3)	(4)	(5)	(6)
	All	All	Girls	Girls	Boys	Boys
4/5 children*post 1994	0.12**	0.13**	0.18**	0.21***	0.05	0.09
	(0.05)	(0.05)	(0.08)	(0.07)	(0.08)	(0.08)
Observations	1,186	1,186	577	577	609	609
Year effect	yes	yes	yes	yes	yes	yes
Control variables	no	yes	no	yes	no	yes

Robust standard errors in parentheses; *** p<0.01, ** p<0.05, * p<0.1
The table reports difference-in-differences for 1989-1997 Labor Force Surveys. The sample includes only households with 3-5 children aged 0-17. The treatment variable is the interaction of four/five children households with post-1994. Where indicated, a set of controls listed below are included: mother's education, father's education, mother's age, father's age, dummies for district of living, quarter of survey, age, and gender. Data is weighted using sampling weights.

as the control group. The estimates where the dependent variable is school attendance are negative and none of them are significant. These results reinforce the interpretation that the narrowing of the gap between a 3 child and a 4 child household after 1994 is the result of the increase in child allowance given for the 4th child, and not a result of other shocks that have generally narrowed the gaps between children from various household types.

The second placebo presented in Panel B of Table 8 tested a fake treatment in which the sample and the treatment were shifted five years back. If a general convergence had taken place we would expect the estimates to be in the same direction as in Tables 3 and 4, but none of them are significant, again reinforcing the conclusion that the narrowed gap between 3-children households and 4 or 5 children households after 1994 is a result of the large allowance given to the 4th child.

Figure A2 adresses the concern that a differential trend before 1994 between the treated

Table 8:
Placebo effect of child allowance on school attendance of young Israeli Arabs

Panel A: Households with 3 children relative to households with 2 children (1989-1997, Maximum 12 years of father's schooling, Children aged 16-17)						
	(1)	(2)	(3)	(4)	(5)	(6)
	All	All	Girls	Girls	Boys	Boys
3 children*post1994	-0.03	-0.05	-0.01	-0.03	-0.05	-0.07
	(0.04)	(0.04)	(0.06)	(0.06)	(0.06)	(0.06)
Observations	1,660	1,660	779	779	881	881
Year effect	yes	yes	yes	yes	yes	yes
Control variables	no	yes	no	yes	no	yes

Panel B: Households with 4 or 5 children relative to households with 3 children (1985-1993, Maximum 12 years of father's schooling, Children aged 16-17)						
	(1)	(2)	(3)	(4)	(5)	(6)
	All	All	Girls	Girls	Boys	Boys
4/5 children*post1990	-0.00	0.00	-0.00	0.02	0.01	-0.01
	(0.04)	(0.04)	(0.06)	(0.05)	(0.05)	(0.05)
Observations	2,477	2,477	1,220	1,220	1,257	1,257
Year effect	yes	yes	yes	yes	yes	yes
Control variables	no	yes	no	yes	no	yes

The table reports difference-in-differences for 1985-1997 Labor Force Surveys. The sample in Panel A includes only households with 2-3 children aged 0-17. The sample in Panel B includes only households with 3-5 children aged 0-17. The treatment variable is the interaction of three (four/five in Panel B) children with post-1994 (post-1990 in Panel B. Where indicated, a set of controls listed below are included: mother's education, father's education, mother's age, father's age, dummies for district of living, quarter of survey, age, and gender. Data is weighted using sampling weights.

and the untreated has driven the results (see formal specification in the appendix). Naturally it is expected that the treatment has affected the observable trend during the period 1985-1997, but I expect that the change in the trend will be the strongest around the policy change. Figure A2 presents the estimates for the differential trend, year by year between 1988 to 1995, for the girls specification, using households with four or five children as a treatment group. The results in the figure are consistent with my interpretation of the other results: There is no significant change in the trend of the treatment group before 1991, and the largest and most significant estimate is for the differential trend since 1993 – the year of the policy decision and announcement.

Generally, the low number of observations each individual year separately makes the high frequency analysis challenging. It is encouraging that the results of the specifications on which I expect to get stronger results – using households with four or five children as a treatment group – are indeed more convincing when testing the year by year dynamic. It is also encouraging to find out that when combining two years together, the results become much clearer.

A concern that was already discussed in Section 3 is a change in the composition of the groups during the years. The composition of the four or five children joint group has relatively improved for girls, which is a concern regarding the identification using this specification. The risk for a potential bias in that specification is weakened by the finding that the relative improvement in mothers' background for households with 4 or 5 children (they are younger and more educated) occurs especially in 1994-1996 (Figures 3.a and 3.c), whereas the increase in school attendance is mostly high and significant in 1997, the year in which the gradual increase in the allowances was completed. Furthermore, the robustness of the estimates to the addition of controls reduces the concern of potential bias due to unobservable relevant variables, and specifically due to improvement in unobservable characteristics of the treatment group.

The fact that the results are exclusive for girls might raise the possibility of a specific bias related to girls. The effect of the treatment on the marriage market is one option. A household loses the eligibility for a child allowance if the child gets married. If so, the policy change might make a family decide to postpone their offspring's marriage and enjoy several years of payment for this offspring. It is more likely that an effect like this will take place among girls, since teen marriage is more common among them: According to the

1995 census, 6 percent of Arab girls aged 16-17 are married, whereas the number for boys is only 2 percent. The expected effect of this on the composition of the treatment group is a decrease in the quality of the girls in the group: I observe only girls that actually live in the household, and since it is reasonable to assume that teen marriage is a characteristic of girls from low socioeconomic background, I will observe more girls of that type after the treatment. Those girls' attendance in school is lower than the average, and therefore, this change in the composition biases my estimates downward. The size of the potential bias is limited, since the effect on marriage should have been extreme in order to get a meaningful effect relative to my estimates, considering that the share of married girls in 1995 was only 6 percent.

Finally, there may be some general equilibrium effects at work. For example, if labor supply of individuals coming from 4 children families declined, wages could have increased and would draw individuals from households with only 3 children. The sign of this potential indirect effect of the treatment on the comparison group would be the opposite from the sign of the direct effect on the treated, and might therefore bias the estimates upward. It is hard to fully address this risk, but its potential threat is only on the quantitative identification rather than on the existence of the direct effect of the allowances that was explored. If the allowances had no direct effect in the first place, there was no indirect effect either way.

5 Conclusions

This study empirically considered the impact of child allowances given to low income families on human capital accumulation, by evaluating a policy decision that increased the child allowances paid to Arab families in Israel. Using the structure of the change in the allowance, which provides a much larger increase to the fourth and to the fifth child than to the third, I estimated a difference-in-differences model, distinguishing between the pre-treatment period (before 1994) and the post-treatment period. The evidence suggests that the large increase in child allowances paid to Arab families with more than 4 eligible children significantly increased school attendance of girls aged 16-17, partly as a result of a decrease in their employment. The increase in school attendance was found among girls from low socioeconomic families, and it was stronger among households with a relatively old mother. The strongest increase in school attendance was found among girls with young siblings, supporting a dominant mechanism of reducing home production and childcare participation of girls.

The results are in line with the theoretical literature claiming that liquidity constraints might negatively effect human capital accumulation. The evidence suggest that direct and unconditional cash transfer payments to poor families might help in alleviating poverty, and in enhancing human capital accumulation in advanced economies.

The results herein do not reflect a full equilibrium result: they are limited to the effect of the child allowances on schooling decisions of children aged 16-17 shortly after the change. Specifically, the results here cannot contradict the Malthusian hypothesis according to which child allowances increase birthrate and decrease parents' labor incentive, therefore preserving poverty. However, the evidence does suggest that policies to reduce poverty among households with children can be useful if they take into account undesired incentives. For example, earned income tax credit (EITC) programs for parents, decreasing with the number of children, may provide the advantage of child allowances raised here, without decreasing parent's labor incentive and with only a limited birth incentive.

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6 Appendix

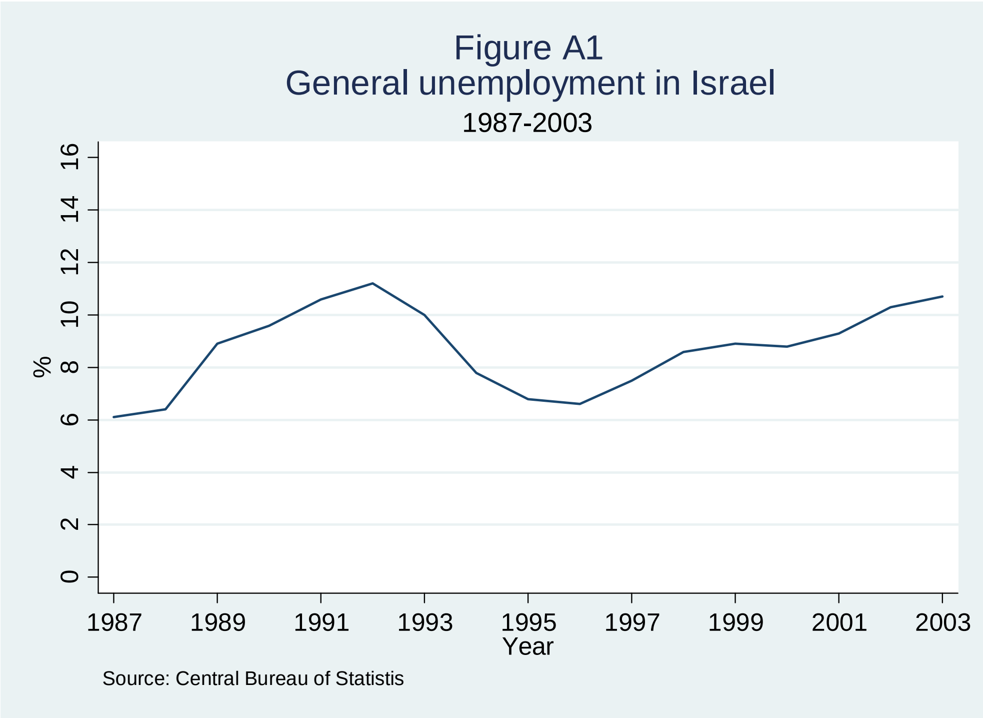
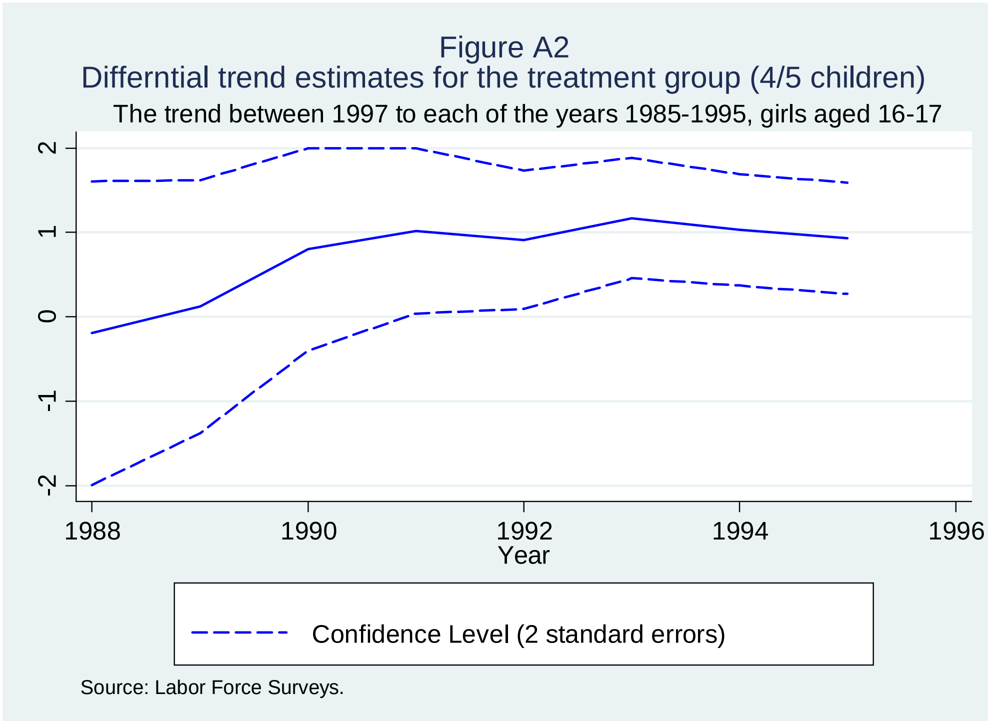


Figure A1
General unemployment in Israel
1987-2003
%
0
2
4
6
8
10
12
14
16
1987
1989
1991
1993
1995
1997
1999
2001
2003
Year
Source: Central Bureau of Statistis



Notes: The sample for the test in Figure A2 includes the years 1985-1997. The specification of the model represented by:

$$Y_{it} = \alpha + \beta_1 x_{it} + \delta_t + \beta_7 f_{it} t + \beta_8 f_{it} t y + \varepsilon_{it}$$

The model assumes that there is a general year effect (δ_t), a specific trend for the treatment group throughout the period ($f_{it}t$), and a different specific trend for the treatment group ($f_{it}ty$) since year y, that can be one of the years 1988-1995. Each value in figure A2 represents β_8 from a specific (1 out of 8) regression where y is one of the years 1988-1995.

Table A1:
Yearly estimates of the effect of child allowance on school attendance of young Israeli Arabs in households with 4 children relative to households with 3 children (1989-1997, Maximum 12 years of father's schooling, Children aged 16-17)

	(1)	(2)	(3)	(4)	(5)	(6)
	All	All	Girls	Girls	Boys	Boys
4 children*1989	-0.09		-0.12		-0.11	
	(0.08)		(0.12)		(0.10)	
4 children*1990	-0.07		0.01		-0.15*	
	(0.06)		(0.09)		(0.08)	
4 children*1993	0.12*		0.13		0.09	
	(0.06)		(0.09)		(0.09)	
4 children*1994	-0.01		0.10		-0.12	
	(0.06)		(0.10)		(0.09)	
4 children*1995	0.14*		0.14		0.15	
	(0.09)		(0.11)		(0.12)	
4 children*1996	0.09		0.05		0.12	
	(0.08)		(0.10)		(0.12)	
4 children*1997	0.14*		0.22*		0.08	
	(0.07)		(0.12)		(0.09)	
4 children*1989/90		-0.08		-0.04		-0.13*
		(0.05)		(0.08)		(0.07)
4 children*1993/94		0.06		0.12		-0.02
		(0.05)		(0.07)		(0.07)
4 children*1995/96		0.12*		0.10		0.13
		(0.07)		(0.08)		(0.10)
4 children*1997		0.14*		0.22*		0.08
		(0.07)		(0.12)		(0.09)
Observations	1,991	1,991	956	956	1,035	1,035
Year effect	yes	yes	yes	yes	yes	yes
Control variables	yes	yes	yes	yes	yes	yes

Robust standard errors in parentheses; *** p<0.01, ** p<0.05, * p<0.1
The table reports difference-in-differences for 1989-1997 Labor Force Surveys. The sample includes only households with 3-4 children aged 0-17. The treatment variable is the interaction of four children households with year or couple of years, omitting 1991/92. A set of controls listed below are included: mother's education, father's education, mother's age, father's age, dummies for district of living, quarter of survey, age, and gender. Data is weighted using sampling weights.

Table A2:
Yearly estimates of the effect of child allowance on school attendance of young Israeli Arabs in households with 4 or 5 children relative to households with 3 children (1989-1997, Maximum 12 years of father's schooling, Children aged 16-17)

	(1)	(2)	(3)	(4)	(5)	(6)
	All	All	Girls	Girls	Boys	Boys
4/5 children*1989	-0.05		-0.05		-0.05	
	(0.07)		(0.10)		(0.09)	
4/5 children*1990	-0.06		0.04		-0.18**	
	(0.05)		(0.08)		(0.07)	
4/5 children*1993	0.09		0.08		0.08	
	(0.06)		(0.09)		(0.09)	
4/5 children*1994	-0.01		0.09		-0.11	
	(0.06)		(0.09)		(0.08)	
4/5 children*1995	0.22***		0.20**		0.22**	
	(0.08)		(0.10)		(0.11)	
4/5 children*1996	0.10		0.10		0.06	
	(0.07)		(0.09)		(0.11)	
4/5 children*1997	0.11		0.22**		0.00	
	(0.07)		(0.11)		(0.09)	
4/5 children*1989/90		-0.06		-0.00		-0.13**
		(0.05)		(0.07)		(0.06)
4/5 children*1993/94		0.04		0.09		-0.02
		(0.05)		(0.07)		(0.07)
4/5 children*1995/96		0.16***		0.15**		0.13
		(0.06)		(0.08)		(0.09)
4/5 children*1997		0.11		0.22**		0.00
		(0.07)		(0.11)		(0.09)
Observations	2,695	2,695	1,334	1,334	1,361	1,361
Year effect	yes	yes	yes	yes	yes	yes
Control variables	yes	yes	yes	yes	yes	yes

Robust standard errors in parentheses
*** $p<0.01$, ** $p<0.05$, * $p<0.1$
The table reports difference-in-differences for 1989-1997 Labor Force Surveys. The sample includes only households with 3-5 children aged 0-17. The treatment variable is the interaction of four or five children households with year or couple of years, omitting 1991/92. A set of controls listed below are included: mother's education, father's education, mother's age, father's age, dummies for district of living, quarter of survey, age, and gender. Data is weighted using sampling weights.