The Effects of Child Allowances on Labor Outcomes

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Although prevalent in developed countries, research on the effect of universal child allowances on labor outcomes has been scarce. I aim to fill this gap by examining the effect on labor outcomes of a policy that drastically reduced child allowances in Israel in varying degrees of intensity, depending on parity. Employing a difference-in-difference design, I find that the policy increased the labor force participation of young women by 6.6% from baseline, whereas I do not find an effect on working hours; estimates for men are less reliable. I also find that younger and more educated women were more responsive.

Benefits to families with children are prevalent in developed countries. the benefits might be cash transfers (child allowances), tax benefits, services in kind, or other forms of assistance (Bingham, J. 2010, Bradshaw & Finch, 2002). In 2007, all 33 countries of the OECD offered some sort of cash transfers to families with children. These and other family benefits amounted to just over 2.4% of GDP on average across the OECD (OECD, 2011).

Although child allowances are prevalent in the West, there has been little research done about them in general, and specifically regarding their effect on labor outcomes. Previous research has mainly focused on one form of family benefits (childcare), whereas only a few papers focused on family benefits in the form of child allowances. Those that did, did so in specific contexts resulting in estimates that might not be generalizable for other, more commonplace forms of child allowances.

In this paper I aim to fill this gap by examining a change in policy that occurred in Israel where child allowances are universal and are generous in both eligibility and value. The program is one the largest social welfare programs in terms of exposure and in monetary terms. In 2015, it distributed allowances to around 3 million children (Israel's population is 8.5 million), in amounts worth about 1% of Israel's GDP (National Insurance Institute, 2015). Between 2002 and 2005, child allowances were drastically cut. For individuals of parity 1-3 the cuts was minimal, whereas for individuals of parity 4 and above the cuts were of significant. For example, an individual of parity 7, for which child allowances constituted about %50 of his income prior to the policy change, experienced a monthly reduction of $425 (-%43). The policy put many Israeli families, especially those of higher parity, below the poverty line at least in the short term, and yet, its causal effect on the labor force participation of these populations, which the policy aimed to increase, has not been established, nor estimated.

This paper is the first to estimate the causal effect of the policy. It also adds to a scarce literature on the effect of child allowances on labor outcomes by examining a

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policy that is more prevalent in other countries than those previously examined.

Using detailed individual level data from Israel's annual Labor Force Surveys for the years 1997-2009, I examine the effect of the policy on the labor force participation and working hours of Israeli men and women. I exploit a variation in treatment intensity that resulted from the varying reductions in child allowances by parity in a difference-in-difference model where I compare the evolution of labor outcomes before and after the policy change of individuals who were hardly affected by it (individuals of parities 1-3) to those who were greatly affected by it (individuals of parity 4 and above). Although these population groups differ on characteristics that might affect labor outcomes, I provide evidence that they followed similar pre-policy time trends with respect to labor outcome. Moreover, I am able to control for differences in observable characteristics. In addition, I show that these did not differentially evolve over time for the two groups. These identifying assumptions hold for women convincingly, but not for men, whose estimates should be considered with reservations.

The analysis suggests that the decreases in child allowances increased the labor force participation of young women but did not significantly affect their working hours. Although no significant effect is found in a sample of all working-age women, as I include younger women, the estimates become significant, and increase in magnitude, suggesting that either older women are less inclined to alter their labor behavior, or are less able to. Women of ages 45 and under are estimated to have increased their labor force participation by 2.2 percentage points following the policy change, a 6.6% increase from the pre-policy baseline. Women of ages 35 and under exhibit even stronger increases. Heterogeneous effect analysis also suggests that in general, education is associated with a stronger increases in labor force participation following the policy change. A specification that allows for varying changes in treatment intensity within the control and treated group produces similar results. I also find no evidence that the policy had a delayed effect. Finally, A placebo test confirms that the estimates are not the result of chance.

I. Literature Review

Two previous studies have looked at this specific policy change, both of which examined its effect on fertility only. Cohen, Dehejia & Romanov (2013) use confidential birth data to estimate the effect of child allowances on the probability of bearing another child. They estimate the effect at 0.99 percentage points per annum on average. The effect is strongest among Arab Muslims and weakest in the ultra-Orthodox Jewish population, for whom the effect is positive but small in magnitude and statistically significant only in some specifications. Toledano et al. (2009) find similar results; The effect on Muslims women is estimated to be the strongest at about 6% decrease in fertility, the effect on Ultra-Orthodox Jewish women is estimated to be modest at about 3% decrease in fertility, and the effect on other women is estimated to be insignificant.

Studies examining the effects of child benefits of all sorts on labor force participation have mainly focused on benefits in the form of services in kind (such as childcare), or tax credits, while only a few studies examined the effect of child benefits in the form of pure cash transfers, as I examine here, although cash transfers are prevalent in the developed world. A few examples of the former kind of studies are - Baker, Gruber & Milligan (2008) and who use a difference-in-difference approach to estimate the effect of an introduction of universally accessible child care in Quebec which was not introduced in other parts of Canada, and find that maternal labor supply increased significantly. Similarly Lefebvre & Merrigan (2008) estimate the effect of the reduction in the the prices of childcare in Quebec on labor supply of women, finding that it led to a significant increase in labor supply. In contrast, Havnes & Mogstad (2011) analyze a staged expansion of subsidized child care in Norway. Their difference-in-differences estimates reveal that there is little, if any, causal effect of subsidized child care on maternal employment, despite a strong correlation. Dave et al. (2015) study the effects of expansions in Medicaid eligibility for pregnant women in the late 1980s and the early 1990s on labor supply, and find that increase in Medicaid eligibility decreases labor supply; Montgomery & Navin (2000) reach similar results for the same time periods using place and time fixed effect models.

Other papers examine policies that are more closely related to the policy examined in this paper, as they involve cash transfers, even though most don't involve universal permanent child benefits specifically, or don't create only a pure income effect as with the policy I analyze. Gonzalez (2013) for example, uses a regression discontinuity design to estimate the impact of an introduction of a one-time cash payment at birth (whereas I study a permanent change in income); she finds that eligible mothers stayed out of the labor force longer after childbirth. Schirle (2015) uses a difference-in-difference approach that compares households eligible for a newly introduced child benefit to households who were not. The author finds that the introduction of the benefit reduced the labor supply of married women by about 3.2%, whereas the effect for men is small. Koebel & Schirle (2016) repeats essentially a similar analysis but focuses on married versus divorced women; finding that the effect on the latter is stronger. These papers are significantly different than the what is proposed in this paper, as will be explained in the next section.

Other papers examine changes in social cash transfers policies that are not related to child benefits, such as disability benefits. For example, Gruber (2000) utilizes a difference-in-difference design by comparing Quebec which raised disability benefits to the other provinces in Canada which did not; he finds that the increase in benefits increased non-employment significantly. Examining the effect of disability benefits as well, French & Song (2010) use an IV approach that exploits the random assignment of judges to disability insurance cases and find that benefit receipt reduces labor force participation significantly.

Additional papers examine the effect of policies such as tax reform and changes in duration of unemployment duration on labor force participation (for example, Bosch & Van Der Klaauw (2012) who study the effect of tex reform and Hagedorn et al. (2013) who study the effect of unemployment benefit duration). It must be noted that these studies examine a policy that entails both an income effect and a substitution effect, as well as the fact that it is not universal in a sense that it affects only the employed, although the incentives of those outside the labor force are altered as well.

A. Contribution

The contribution of this paper to the literature is potentially multifold. First, it is the first paper to examine the effect of the reductions in child allowances on the labor force participation of Israeli men and women; previously, only the effect on fertility was studied. Although allowances were reduced with the intent to increase the labor force participation of population groups with historically low participation rates, the effect of the policy has not been researched rigorously; although anecdotally, the link between the reduction in allowances and the increased labor force participation of several population groups was mentioned in different policy reports (for example, Kimhi, 2012), the causal effect of the reduction in allowances was not estimated, nor established. As mentioned above, child allowances are significant both in total value distributed, as well as the number of families affected; therefore, studying the effect of changes in this important social tool is essential.

Second, as detailed in the literature review, although prevalent in developed countries, only a handful studies examine the effect on labor force participation of child allowances in the form of cash transfers, while most deal with benefits in the form of services in kind (such as childcare) or benefits in the form of tax credits. In the ones that do examine a cash transfer child benefit system, the system examined is significantly different than the one present in Israel; in none of the studies is the program truly universal (not mean-tested) and involves a pure cash transfer. For example, in Canada, which two of the studies are about, the program is mean tested, the benefit is taxable, and the allowances are only given up to age 6. In Spain, which the other study is about, the program is just a one time post birth transfer. In addition, the years studied in these studies are only up to 3 years before and after the policy change, while I use a longer time series that include additional information on pre-levels and enables the examination of longer term responses.

Third, I haven't encountered papers where the identifying variation stems from different treatment intensity assigned to different groups by the parity, which results from the unique structure of the child allowance policy in Israel prior to the change. Although this poses challenges, an analysis of such a policy in trying to overcome the challenges could be applicable to other policies as well.

Fourth, due to their universal nature and the fact that there is practically a full take-up of child allowances in Israel (mothers provide their bank account during the first child hospitalization and allowances are automatically allocated in all subsequent births, as explained below) , I am able to assign the intensity of treatment in my main data source, even though the data does not contain actual allowance receipts. This is in contrast to many of the studies mentioned above, specifically those that examined cash transfer child allowances, since these studies assign treatment without being certain that a person is indeed being treated.

Fifth, the papers mentioned above that examine the effect of different child benefits, have looked at an introduction of a benefit or an increase in benefits, whereas none have looked at a decrease in benefits although it could be the case that individuals would respond differently to a negative income effect than they would to a positive income effect.

II. Child Allowances in Israel

Child allowances are paid to all families with children under the age of 18, according to the family's parity. The allowances are paid from birth until the age of 18. The allowances are universal (i.e. they do not depend on the family's income or any other criteria). The allowance are paid monthly and are nontaxable. In order to receive payments, the mother of the child provides her bank account to the hospital administration during her first birth hospitalization. The hospital then transfers the information to Israel's National Insurance Institute (NII), the equivalent of the United States Social Security Administration, which initiates payments. Initiation of payments for any subsequent children is automatic, as hospitals transfer birth record data to the NII. Except for providing a bank account at first birth, there is no claim to be filed in order to receive payments. Therefore, it is plausible to conclude that take-up is essentially full; according to Asiskovitz (2007) who compared the actual number of children under the age of 18 in Israel and the number of children who received the allowances, take-up for the years 1997-2002, for which the latest data is available, ranged between 98.9% and 99.7%. In families where the children live with both parents, the allowance is paid in the name of the mother, so that the NII will transfer the allowance to a bank account in her name alone, or a joint bank account in which she is one of the owners. In cases where the parents are divorced or separated, the allowances are paid to the legal guardian of the child, which is mostly the mother as well (National Insurance Institute, 2017a).

A. *The Allowances and the Policy Change*

Figure 1 (left) details the monthly allowance per child, in 2010 prices, from 1997 to 2009. The exchange rate between the Israeli currency (NIS) and the USD during this period ranged between 3.5NIS/USD and 4.5NIS/USD. The allowances are cumulative, such that a family of parity 3 will receive the allowances for the first, second and third child. Allowances above the fifth child are fixed at the fifth child level. During the years 1997-2001 the allowances were rather stable, except for an increase in the allowance of the fifth child and above that began in January 2001 following the passage of the "Helpart Law"; the law was revoked by February 2002 (Lau, 2016). In 2001 and 2002 Israel has experienced a minor recession, where GDP growth rates declined from 8.8% in 2000 to 0.1% and -0.2% in 2001 and 2002, respectively. In 2003, the GDP growth rate increased to 1.1%, and by 2004 GDP growth rates returned to their previous trend of 3+% growth (Central Bureau of Statistics, 2017a). Due to the fiscal strain following the recession, the budget of 2002 entailed a reduction in child allowances, as seen in the graph. In February 2003, a new government came to power which led to two unexpected occurrences; first, the party that represents the Ultra Orthodox Jews, whose main economic plea was to keep the allowances at their high levels in order to support its high fertility constituents, was not invited to join the coalition for the first time in 30 years; second, the prime minister unexpectedly appointed his rival, Benjamin Netanyahu as finance minister (Cohen, Dehijia & Romanov, 2013). Economically, Benjamin Netanyahu is a right wing conservative, who is in favor of small government. The absence of the Ultra Orthodox Jewish party from the coalition, as well as his small government ideals, enabled the finance minister to lead further reductions in child allowances which were drastically reduced in 2003 and 2004. In order to soften the blow for families who were already receiving child allowances for existing children, the reduction in child allowances for children who were born before May 31, 2003 (also referred to as "old children") was less drastic than the reduction in child allowances for children born after May 31, 2003 (also referred to as "new children"); furthermore, the reductions for "old children" were phased in over a longer period.

The line graph in Figure 1 (left) illustrates the allowances for "old children" while the connected dots graph illustrates the allowances for "new children". As seen in the graph, the allowance per child for "new children" is the same no matter the parity, while the allowances per child for "old children" differs by parity. In reality, families consist of a mixture of "old children" and "new children", and receive child allowances accordingly [1].

Since 2005 the child allowances experienced only small changes, mainly due to inflation adjustment as well as changes in coalitions and subsequent agreements with the different parties.The average monthly allowance per child dropped almost 50% between 2002 and 2004 and was rather steady before and after those years.

The policy affected families of varying parities differently. Before the reductions, child allowances for the first and second child were around $50; allowance for the third child was around $100, while the allowance for the fourth child and above was around $200. As Figure 1 (right) illustrates [2], prior to the reduction, a family of parity 2 received around $100 in child allowances, a family of parity 4 received around $400 in child allowances and a family of parity 7 received $1000 in child allowances. Prior to the reduction, child allowances were a significant source of income to families of high parities. Analyzing the Income Survey of 2001 [3] just prior to the policy change, child allowances were 3.7%, 16.5% and 48% of the household total net monthly income of families of parities 2,4 and 7, respectively. These differences are due to the fact that not only do child allowances increase significantly with each additional child, but families of higher parities are poorer on average, such that child allowances account for a higher share of their total income.

The reduction in child allowances varied according to parity both in absolute and rel-

[1] In 2004, the share of "new children" who received child allowances was 4.5%, which steadily rose to 29% by 2009.

[2] The figure details the total monthly child allowances for families consisting of "old children" only. In reality, after a 2004, a growing share of families include "new children" which would entail even bigger reductions in allowances, since "new children" child allowances are significantly lower than those for "old children"

[3] The surveys are conducted by Israel's Central Bureau of Statistics and are representative surveys of Israeli households of around 15,000 individuals aged 15+ (about a 0.3% sample).

ative terms. From its level in 2000 to its level in 2005, the allowance for the first and second child decreased by less than $20, each, or about 35% reduction; the allowance for the third child decreased by $60, a 58% reduction; the allowance for the fourth child decreased by $110, a 52% reduction, and the allowances for the fifth child and above decreased by $70, a 38% reduction. As illustrated in Figure 1 (right), for a family of parity 2, this entails a reduction of $40 in total monthly child allowances, or 35%. For a family of parity 4, this entails a reduction of $235, or 50% and for a family of parity 7, this entails a reduction of $425, or 43%. As mentioned before, as families of higher parities are poorer on average, the decrease in child allowances as share of their income is even higher.

III. Data and Empirical Framework

A. Data

The main dataset for the analysis are Israel's annual Labor Force Surveys for the years 1997 (5 years prior to the major reductions in allowances) until 2009 (4 years after the major reductions in allowances). The surveys are repeated cross sections that are conducted annually by Israel's Central Bureau of Statistics (CBS) and are representative surveys of Israeli households of around 110,000 individuals aged 15+ (about a 2% sample). The surveys contain the relevant outcome variables of interest - labor force participation and weekly working hours for each individual, as well as a rich demographic information on variables that might affect an individuals' labor outcomes, such as age, education, religion, immigrant status and marriage status. The surveys also detail the number of biological children under the age of 18 that each individual has, as well as their ages in bins. Although the receipt of child allowances is not detailed, these can be imputed, as described below.

The labor force surveys provide population weights for each observation which are used in all results in order to accurately account for the population structure. The main sample includes individuals aged 25-64; this definition of "working-age" individuals is used by Israel's CBS (CBS, 2015) as well as in the academic literature. Additional results are detailed using different age groups.

The main outcome variables analyzed are individual's labor force participation and weekly working hours. I follow the standard definitions employed by Israel's CBS (CBS, 2015) as well as the OECD and the US BLS (OECD, 2017, Bureau of Labor Statistics, 2015). *Member of the labor force* is an individual who is either employed (i.e. worked for at least one hour) or unemployed (but actively searched for a job) during the reference week. *Weekly working hours* refers to the hours the individuals worked in the reference week.

The data on the level of allowances (in current terms) was extracted from three sources; for the years 1997-2000, from two studies that document the history of child allowances in Israel (Lau, 2016, Asiskovitz, 2007) and for the years 2001-2009, from the yearly reports published by the NII (NII, different years). These reports detail the relevant allowance by parity, month and year. When the allowances changed several times during the year,the allowance imputed for the whole year was calculated as a weighted average. The allowances were then converted to 2010 constant prices, using the average consumer price indices for each year (Bank of Israel, 2017).

One of the estimated models requires each individual's receipt of child allowances. Although Labor Force Surveys do not contain information on child allowances, they do contain information on each individual's number of biological children grouped into age bins. Using the parity and the year enables me to impute each individual the child allowance received each year. The procedure is detailed in Appendix A.

Since the parity detailed in the labor force surveys are the individual's biological children (i.e. the ones for which she receives child allowances), and not the number of children in the household, the imputation of child allowances should be fairly accurate. Indeed, the total amount of allowances that were imputed to individuals in the survey each year, as well as the calculated number of children for which allowances were received, are fairly close to the actual amount of child allowances that the government transferred, and the actual number of children that received them, as detailed in Table 1. For example, in 2009, 2,417 Million children received allowances worth ₪5,802 Million; the weighted number of children in the survey was 2,399 Million for which ₪5,353 Million worth of allowances were imputed.

Table 2 provides summary statistics for working age women [4]. Statistics are provided for the period before the policy change (1997-2001) and the period following the policy change (2005-2009), for the control and treatment group under the main specification; the control group are individuals of parities 1-3, while the treatment group are individuals of parity 4 and above. In addition, statistics are provided for individuals of parity 0. Although the control and treatment group are dissimilar on many aspects, individuals of parity 0 are probably even more so; in the sample these include either older individuals who previously bore children who are no longer under the age of 18, or individuals of child-bearing age who have decided not to have children. In either case, individuals of parity 0 are drastically different on observables (specifically they are older and are less likely to be married, and more likely to be immigrants) but probably on unobservables as well, from the individuals of other parities. For that reason, I exclude them from the control group.

The cutoff of 3 children was chosen for a few reasons. Analyzing the Income survey of 2001, just before the policy change, the child allowances were 0.2%, 3.7%, 7.3% and 16.5% of the household total net monthly income of families of parities 1,2,3 and 4, respectively. Therefore, child allowances were not a major source of income for families of parity 1 or 2, as well as the fact that in absolute value, the overall monthly reduction in child allowances was approximately \$20 only. For families of parity 3, child allowances were still less than 10% of their income, as well as their overall monthly reduction in child allowances was approximately \$70, a 35% reduction. For families of

[4] The summary statistics for men are available by request.

parity 4 however, the share of child allowances of their income more than doubled to 16.5%, as did the overall monthly reduction (approximately $200, a 45% reduction). Furthermore, the labor force participation patterns of individuals of parity 3, as detailed in Figure 2 (left), resemble the patterns of families of parities 1 and 2 both in absolute levels, and fluctuations, whereas the pattern of individuals of parity 4 are closer to those of higher parities. Other treatment and control group specifications also explored .

Women of parities 1-3 differ from those of parity 4 or above. In the pre-policy period, the former were older (38.5 vs. 36.7), were less likely to be married (0.85 vs. 0.95), were more educated (for example 0.77 were high-school graduates, vs. 0.55) and were more likely to be Jewish (0.84 vs, 0.56). The differences in observables between these groups that appear in Table 2 are all significant at 1% except for the share of immigrants. Nonetheless, I am able to control for all these observed differences through the inclusion of individual level controls. However, there is still fear that differences in observables might indicate differences in unobservables that could not be controlled for. This is mitigated by the fact that the two groups follow similar pre-policy time trends in labor force participation, which suggests that they behave similarly with respect to the outcome variable. Figure 3, which describes the labor participation rates before, during and after the policy change for the two groups, illustrates this. Thus, the control group, which has experienced the treatment minimally, could potentially be a viable counterfactual for the treatment group.

With respect to the variables of interest, Table 2 details the reduction in yearly child allowances. For women of parities 1-3, yearly child allowances decreased from ₪5,281 on average prior the policy change, to ₪3,579 after (-32%). For women of parity 4 or above, yearly child allowances decreased from ₪30,792 to ₪13,497 (-56%). With respect to the outcome variables - the labor force participation of the former increased from 69% prior to the policy change, to 74% after (+7.2%), while the labor force participation of the latter increased from 35% to 42% (+20%). With respect to average weekly working hours, it slightly decreased for both groups.

With respect to Men (not shown) the differences between men of parities 1-3 to those with 4 children or above still hold, except that these groups are more similar in average age and the share who are married compared to the differences between the women groups. Also, while women labor force participation rates rose after the policy change, the labor force participation rates of men declined, more so for the treatment group (74% to 72%) than for the control group (87% to 86%). Weekly working hours have also declined for both groups.

B. Descriptive Evidence on the Response of Labor Market Outcomes

I am interested in examining whether the drastic reduction in child allowances that started in 2002 coincided with an increase in labor force participation rates or working hours for the treated populations. I conjecture that the changes in labor outcomes would be starker the higher the treatment intensity. As noted previously, starting with parity 3, the higher the parity, the stronger the effect of the policy; although the percentage decline in child allowances does not differ significantly between individuals of different parities, the absolute decline is starker the higher the parity, and so is the decline as the share of the families' budget, as families with more children are poorer on average.

There are reasons to believe that the response to the change in child allowances would be delayed mainly because the families affected the most might believe that the changes in allowances are temporary, as governments in Israel change frequently. Also, families might first dip into savings or might use communal financing. In addition, entering the labor force might require training or other preparation which requires time. Furthermore, it could take individuals time to realize that the policy was enacted. I will deal with this by including lagged variables in the estimation. There are also reasons to believe the the response to the change in child allowances would be stronger for women than for men. Child allowances are transferred in the women's name to a bank account owned by her (either alone or jointly with her husband). If there is no family pooling of resources, then this means that the women would experience the income effect exclusively. In case there is some level of family pooling of resources, women are still likely to exhibit a stronger response, as it has previously documented in the literature that women's labor supply elasticities are substantially larger than men's, although they have fallen over time (Heim, 2007, Blau & Kahn, 2007). In a related paper, Cesarini et al. (2015) finds effects on the extensive margin following a lottery win that are stronger for the winning spouse, than the other spouse. In addition, the populations affected most by the policy change are conservative and patriarchal (mostly religious Arab and Jewish women), where a negative income effect could encourage husbands to "force" their wives into the labor market.

Starting with labor force participation rates of women of working age (25-64) by parity, as illustrated in Figure 2 (left), the labor force participation of women of parities 1,2 and 3 behaved rather similarly over the period. Despite small fluctuations in certain years, the labor force participation of these women has increased gradually over the period, without any noticeable trajectory changes either before, during or after the policy change. The labor force participation of women of parities 4,5,6 and 7 was rather stable before the policy change (1997-2001), except for the year 1997, with a slight upward trend. Since the initial labor force participation rates of individuals of parity 4 and above are significantly lower than those of individuals of parity 3 or less, the relative changes are bigger for the former. It appears that the pre-policy time trends for individuals of different parities are not notably dissimilar. As opposed to individuals of parity 3 or less, individuals of parity 4 and above all experience a trajectory change where their labor force participation starts to increase significantly at different years following the commencement of the policy change; while the labor force participation of individuals of parity 6 and 7 starts to increase as soon as 2002, that of parity 5 starts increasing in 2005 and that individuals of parity 4 starts increasing in 2006. Once the labor force participation starts increasing, the rate of increase for these families is significantly higher than the rate of increase during the post period that individuals of parity 3 or less experienced. For individuals of parity 2, labor force participation rose from around 72% right before the policy (2001) to about 77% by 2009 (5 percentage points increase, +7%). For individuals of parity 4, labor force participation rose from around 44% right before the policy to about 55% by 2009 (11 percentage points increase, +25%), while For individuals of parity 7 and above, labor force participation rose from around 15% right before the policy to about 33% by 2009 (18 percentage points increase, +120%).

overall it appears that as treatment intensifies (for individuals of parity 4 and above), so does the labor participation response; furthermore, the timing of the response is earlier the higher the intensity of treatment.

With respect to men's labor force participation response, examining Panel B in Figure 4 (right) doesn't reveal specific patterns that emerge in response to the change in policy. The labor force participation of men of parity 4 and above is very volatile, which makes it difficult to reach any conclusions based on descriptive evidence, and undermines causal identification, as will be discussed further on.

The response to the policy could also occur at the intensive margin. Figure 2 (right) details the weekly working hours for working age women. For women of parity 3 or less it is not clearly evident that the trend of weekly working hours changed following the policy. For individuals of parity 4 and above, working hours are very volatile, mainly due to a smaller sample of employed women amongst these population groups. The trends for men are volatile and inconclusive. This makes it difficult to reach any conclusions based on the figure and undermines causal identification.

C. Empirical Framework

I utilize two specifications of a difference-in-difference model. In the first, I define the years 1997-2001 as the years prior to the policy change, the years 2002-2005 as the years during the policy change and the years 2006-2009 as the years after the policy change. In the main specification the treated group are individuals of parity 4 and above and the control group are individuals of parities 1-3. The estimating equation is:

$$(1)\ Labor\_Outcome_{ipt} = \lambda_t + \gamma_p + \beta_1(Post_t * Treat_p) + \beta_2(Between_t * Treat_p) + \delta \mathrm{X}_{ipt} + \epsilon_{ipt},$$

where *i* indexes individuals, *p* indexes parity and *t* indexes year. $Labor\_Outcome_{ipt}$ is either labor force participation (1 for being a member of the labor force), in which a linear probability model is estimated or weekly working hours (conditional on working). I include year fixed effects to account for factors that vary similarly over time across parities, such as common shocks, and parity fixed effects which hold constant differences across parities that are time invariant. The variable of interest is the interaction $Post_t * Treat_p$ which takes on a value of 1 for individuals of parity 4 or more after the policy change; its coefficient will measure the effect of the policy on labor outcomes after it's been fully implemented. The variable $Between_t * Treat_p$ takes on a value of 1 for individuals of parity 4 or more during the polcy change period, prior to its full implementation; its coefficient will measure the effect of the policy during the transition period. In addition, a full set of individual level controls X_{ipt} are included in order to control for changes in the demographic composition of each parity group over time that could be producing a spurious effect, as well as in order to increase precision. These include marriage status, age, age squared, education, religion, whether the individual is an immigrant, and the number of small children the individual raises[5]

As I control for year and parity group FE, the average treatment effect of the policy change on the treated individuals is identified as the change in labor outcomes for them, relative to the control group, in 2006 or later relative to 2001 or earlier.

The second specification utilizes the continuous measure of treatment - the yearly child allowances each individual receives. The estimating equation is:

$$(2) \qquad Labor\_Outcome_{ipt} = \lambda_t + \gamma_p + \beta Year\_Allowance_{ipt} + \delta \mathrm{X}_{ipt} + \epsilon_{ipt},$$

where now the variable of interest is *Year_Allowance*$_{ipt}$; its coefficient is an estimate of the effect of the decrease of one unit in child allowances on labor outcomes, whereas the previous specification estimated the total effect of the policy on labor outcomes. The effect of policy is identified by intra-group variations in yearly allowances across time, relative to the corresponding changes in other groups. This specification captures the policy change more comprehensively, as it allows for variation of treatment within the treated group, as for example, individuals of parity 4 experienced different reductions in child allowances than those of parity 5 (which is not captured in the previous specification where all treated are assigned a single value). Nonetheless, its interpretation is less clear, as there is no clear control and treatment groups who are compared before and after the policy; furthermore, it relies on the yearly child allowances measure which might have been imputed imperfectly, as described above, which introduces measurement error. Moreover, it is plausible that the drastic decline in child allowances spurred a trajectory change in labor force participation trends for women, that continued after child allowances stabilized, such that labor force participation continued to rise more drastically for the treated groups then the non-treated groups although child allowances remained the same. This would attenuate the estimate of the true longer-term effect of the policy change, which is captured in the first specification.

In both specifications, robust standard errors clustered at the locality level are used, to allow for the correlation of observations within a locality.

DID identification rests on two major assumptions; parallel trends and that no other factors (such as another policy change) occurred in concurrence with the discussed policy, that might have affected the labor outcomes of the groups differently. With respect to the first, Figure 3 illustrates that the parallel trends assumption holds quite solidly for women, as detailed above, and less so with respect to men which will weaken the validity of the estimates for men. To rigorously examine that the treatment and control groups of women were on similar pre-policy time trends with respect to labor force participation I utilize an event-study type model; I run a version of regression specification (1) with year dummies, as well as the interaction of each of them with the the

[5]Marriage status are dummies for married, married but living separately, divorced, widowed and single. Education are dummies for less than high-school graduate, high-school graduate, some college, college graduate and above college graduate. Religion is either Jewish, Muslim, Christian, Druze or other. Immigrant takes on a value of 1 if the individual was not born in Israel. Small children is the number of children aged 0-4 (pre-kindergarden) the individuals raises.

treatment variable [6]. Results are reported in Table 3; estimates for the interaction of treatment with each of the pre-policy years (1997-2001) are all small and insignificant. This suggests that the control and treatment groups were on a similar time trend of labor force participation in the years prior to the policy change.

Examining Figures 2 which depict the weekly working hours trends reveals that the parallel trends assumption is tenuous with respect to weekly working hours which will undermine the validity of any results. Therefore, I will focus the discussion on results concerning the the labor force participation of women. With respect to the second assumption, the reduction in child allowances occurred concurrently with a reduction in income support benefits, but this probably affected the estimates only minimally [7].

Another concern is that factors that might affect labor outcomes (such as education) are varying differentially over time for the treatment and control group. Then, the estimates might pick up differential trends in those factors, and not the effect of the policy.

To address this concern, I examine whether treatment is associated with changes in observable characteristics over time. Lack of differential trends in observables would suggest the lack of trends in unobservables as well. I run regression specification (1) and replace the dependent variable with an observable characteristic. The regression is run separately for each observable characteristic. The variable of interest is the interaction $Post_t * Treat_p$; its coefficient estimates whether treatment is associated with differential trends in each of the observable characteristics. The results are reported in Table 4. The estimates are small and insignificant for four out of the five observables characteristics. The estimate for age is significant at 5% but is very small in magnitude given the the average baseline age of women aged 45 and below (35.3). These provide evidence that treatment is not associated with differential trends in observable characteristics that might affect labor outcomes.

IV. Results

A. Main Results

Table 5 provides the main regression results for the first specification (equation (1)). The coefficient on the variable of interest $Post_t * Treat_p$ is reported from separate regressions where the outcome variable is either labor force participation or weekly working hours, run for women and men separately. The specification in column (1) includes only year and parity fixed effect; the specification in column (2) adds controls and the specification in column (3) adds parity-specific linear time trends. The sample

[6] I run the regression $Labor\_Outcome_{ipt} = \lambda_t + \gamma_p + \beta_1 Treat_p + \beta_2(Yeart * Treat_p) + \delta \mathrm{X}_{ipt} + \epsilon_{ipt}$. Year 2001 is omitted; results are similar for other omitted years.

[7] Income support benefits are mostly distributed to individuals who are members of the labor force who are unemployed and have passed their unemployment benefits eligibility period. Overall, it affected between 80,000 and 140,000 beneficiaries annually (only a fraction of those affected by child allowances). As most beneficiaries are members of the labor force already, even if the reduction in income support could incentivize individuals to work more, most are already members of the labor force, so that this overlapping policy would not affect the estimates on the increase in labor force participation directly, although it could affect the estimate on the effect on weekly working hours although probably minimally

includes all working-age individuals. For women, only specification (1) produces statistically significant estimates (at 1%), in which the policy is estimated to have increased their labor force participation by 4.3 percentage points. Adding demographic controls (specification (2)) decreased the magnitude of the effect and voided significance, suggesting that the increase in the labor force participation of treated women partially stemmed from the changing demographics of that group (for example, Table 2 reveals that their education levels did increase at a faster pace than those of the control group). Although not captured as part of the effect of the policy change, it is likely that the policy itself is partly responsible for the increase in the education levels of treated women which would suggest that the estimates are downward biased.

For men, only specification (2) produces statistically significant estimates (at 5%), in which the policy is estimated to have decreased their labor force participation by 2.0 percentage points.

Adding the linear time trends (specification (3)) results in non-significant estimates for both men and women. On the one hand, this could mean parity-specific time trends existed even prior to the policy. On the other hand, in case treatment effects emerge gradually, estimates that include parity-specific time trends would fail to distinguish treatment effects from differential trends, thus leading to imprecise estimates, even though an effect exists.

With respect to the other outcome variable - weekly working hours, the results for women and men are small and insignificant under all specifications.

I conjecture that older individuals will be less responsive to the policy change since they have fewer years left until retirement [8]. I test this by running the main specification (equation (1)) with controls and parity FE separately for individuals aged 55 and under, 45 and under and 35 and under, for women and men separately. The results are reported in columns (4) - (6). For both women and men, the magnitude of the effect of the policy change increases as age decreases. The estimates are significant for all regressions. When only including women aged 55 and under, the estimate of the effect of the policy change on labor force participation is +1.9 percentage points; when only including women aged 45 and under the estimate is +2.2 percentage points, and when only including women aged 35 and under the estimate increases to +3.8 percentage points. The results for men follow similar patterns. Table 6 provides the full regression results for women and men aged 45 and under. The signs and significance of the coefficients on all covariates are as expected.

Considering the average labor force participation rates of treated women and men under 45 prior to the policy (35.1% and 75.3%, respectively), the policy change led to a relative increase in labor force participation of women of 6.6% and a relative decrease of 3.3% for men from baseline levels. Although the decline for men is larger then the increase for women in absolute terms, the estimated relative change for men is smaller.

Figure 5 (left) depicts the coefficient plots the regressions run separately for women

[8] Older individuals might not find it beneficial to enter the labor force. They might suffer from discrimination which could preclude them from joining the labor force, or could just be set in their ways in a way that is hard to change later in life. Additionally, older individuals might be less ambitious then younger individuals.

aged 25-34, 35-44, 45-54 and 55-64. As the figure shows, as age increases the magnitude of the effect gets smaller and more insignificant; results for men similarly are stronger in magnitude as age decreases.

In the following regressions I restrict the sample to individuals aged 45 and under since they are the ones who respond to the policy and drive the overall results.

The results for man are puzzling. Although the estimated relative effect (from baseline) for women is stronger than for men, in line with the literature (as detailed in the descriptive statistics section), the sign of the estimate is surprising. Considering the fact that almost all treated men are married, presumably sustaining a joint budget, a significant negative income effect on the household budget should not theoretically lead to the increased labor force participation of men.

The are strong reasons to doubt the validity of the estimates obtained for men. Examining the Figure 4 (left), which plots the labor force participation of the control and treatment group over time reveals that the control and treatment group followed parallel trends only until 2000. In 2001, one year prior to the commencement of the policy change, there is a sharp drop in the labor force participation of the treatment group that does not occur for the control group (whose labor force participation actually rises in that year). A sharp decline in labor force participation that occurs prior to the policy exclusively in the treatment group could produce a spurious effect; the effect captured by the estimate will actually pick up differential trends and not the true effect of the policy. In other words, the decline in labor force participation of the treatment group could have been a continuation of the trend that began prior to the policy, and could have existed without the policy as well. The fact that the labor force participation of men during most years prior to the policy change (and prior to the sharp drop in 2001) were significantly higher than in the post-period cemented the significance of the estimates and negated the effect on the estimates of the sharp drop that lowered the labor force participation of men only for a single year, resulting in false inference. This casts doubt that the policy change was responsible for the decline in the labor force participation of men.

The pre-policy period includes the years 1997 - 2001. When I run the main regression specification that includes only the years 2001, in which the sharp decline in labor force participation occurred, and after (i.e., including only the year 2001 as a pre-policy period) the estimate for men is very small and insignificant (-.006 (.017)), whereas for women, it remains large, significant and close to the main estimate (.026 (.012)). When including only the years 2000 and after, the results are the same. Only when including 1999 and after, do the results for men become significant, whereas the results for women remain significant and close to the main estimates no matter the number of years included in the pre-policy period. Therefore, the significance of the estimate for men relies on minimization of the effect of the sharp decline in labor force participation that occurred in 2001, suggesting the estimate does not pick up an effect of the policy convincingly.

If the estimates obtained for men are valid, one possible explanation for the puzzle could be that the rise in labor force participation rate that women experienced after the policy change led to men substituting these women in the house, by decreasing their participation in the labor market. This is a a sensible conjecture given the specific context of the paper. Treated women are of higher parities (4 or more); if these women enter the labor force, they are unable to take care of the children during the day, so their partners might be substituting them. Moreover, as treated women are poorer than average women, they might not be able to afford child care for a large number of children. However, it is unclear how it would be "wise" to substitute the income of men who are exiting the labor market with those of women, most of which are entering the labor market for the first time, whose income must be lower than the former income of their partners, such that the overall income of the household would probably decrease. This hypothesis could have been tested if unique household identifiers were available; these would have enabled the examination of the labor supply of a whole household; unfortunately, unique household identifiers are missing from most of the data. Nonetheless, an indication in this direction could be the following; when running main specification only for men and women ages 45 and under who are not married (thus, probably do not pool income), the estimated effect of the policy is insignificant for both women and men (the estimates are .05100 (.04190) and .02372 (.06957), respectively), but interestingly for men the estimate is now positive. It could be the case that without the ability to substitute for childcare, women are less likely to enter the labor force, while without the ability to substitute for income, men are unlikely to exit the labor force.

Results for the second main specification (equation (2)) are detailed in Panel B of Table 7. Reported are the coefficients on $Year\_Allowance_{ipt}$ from regressions that include year and parity FE as well as controls, run for women and men aged 45 and below, separately. An increase of ₪1 in yearly allowances is estimated to decrease labor force participation -0.00014 percentage points (significant at 1%). Considering that treated women experienced reductions in yearly child allowances that averaged ₪17,295 (Table 2), the estimate entails an average increase of 2.4 percentage points in women labor force participation due to the policy change; the estimate is very close to the estimate obtained from specification (1). For men, the estimated effect is 0.00017 percentage points (significant at 1%). The estimate entails an average decrease of 2.9 percentage points in men labor force participation.

B. *Additional Regressions and Heterogeneous Treatment Effects*

Beside heterogeneous effect by age, I conjecture that in general, individuals who are less educated will be less responsive to the policy change since might not possess the skills to enter the labor force, it might take them longer to enter the labor force, they might be less demanded in the labor force or they might be less ambitious than more educated individuals. Figure 5 (right) depicts the coefficient plots for the regressions run separately for women with some high-school education, high-school graduates, those who have some college education and those who have graduated from college. As the figure shows, as education level increases the magnitude of the effect gets stronger. Results for men (not shown) do not show heterogeneous effects by education.

When examining Figure 2 (left), it seems that for some treated groups the differential increase in labor force participation occurred with a delay; as discussed in the data subsection this is logical. In order to test this hypothesis, I add two lags to the main specification (equation 1), in which the policy is "turned on" one year and two years prior to it's actual onset. The results are reported in Panel C of Table 7. Reported are the coefficients on $Post_t * Treat_p$ as well as the two lags. The outcome variable is labor force participation. For women, the contemporaneous effect remains significant at 10%, and the magnitude increased compared to the specification without the lags. However, the lag variables are not significant. For men, the contemporaneous effect lost its significance, while the one period lag is significant at 10%, and of a higher magnitude than the main result (3.9% decrease). Therefore, it seems that the policy affected man with a certain delay, while there is no evidence that women had a delayed response.

One of the main concerns of the analysis is that the treatment group might not serve as a proper counterfactual, since the groups are different on some observables, and probably unobservables as well, such that their labor supply elasticities might differ. In order to tackle this concern, I define control and treatment groups that are supposedly more similar than the ones used for the main specification. For individuals with four children or more, I define their control group as the individuals with one less child. For example, the control group for individuals of parity 5 is defined as individuals of parity 4. I then run regressions separately for each pair of control and treatment groups. Individuals with one less child might provide a better counterfactual to those with one more child as they are more similar on average. Moreover, since the share of child allowances out of the household budget increases with each additional child, it can be thought as if treatment intensifies with each additional child. Therefore, the treatment group from each pair is more intensely treated than its control. The results of these regressions are reported in Panel A in Table 7. Reported are the coefficients on $Post_t * Treat_p$ where the outcome variable is labor force participation, run for women and men aged 45 and under, separately. For women, only the estimate from the 3 vs 4 comparison is significant (at 10%), nonetheless, for all comparisons, except for the 4 vs 5 comparison, the estimates are positive. As depicted in Figure 2 (left), both groups increased their labor force participation significantly more than the control groups during the post-policy period. It could be that individuals of parity 5 responded marginally less to the policy but only compared with individuals of party 4. The results for men are only significant for men of parity 4 compared with those of parity 3.

C. Endogenous Fertility Response

Endogenous fertility response could be a main confounder. Unfortunately I lack the data to fully account for trends in fertility associated with the change in policy. Nonetheless, I try to provide suggestive evidence that the bias is minimal.

The studies detailed in the literature review section that examined the effect of the reduction in child allowances on fertility analyzed three main population groups - Ultra Orthodox women, Muslim women and all other women (mostly secular Jewish women). Both studies reach a similar conclusion that the effect was strongest amongst Muslim women (one study found that the change in policy reduced their fertility by about 6%, while the results of the other are less comparable as they look at the probability of an incremental child, which they found to have decreased by 1.7%). The effect amongst Ultra Orthodox women is estimated to be -3% in one study, and statistically insignificant in the other and the effect amongst secular Jews is estimated to be statistically insignificant in one study, and -0.87% in the other. Two strategies can limit sample size to women who were not found to have a significant fertility response that would bias the results. First, I could limit the sample to women that are likely to have completed child-bearing. Unfortunately, I suspect that women of this age (as backed by the analysis) are also not likely to exhibit a labor response as well.

The second option is to limit the sample to non-Muslim women, i.e. include only Jewish women which both studies have found to to have responded only minimally in fertility changes (or not at all) to the reduction in child allowances. If I run the main specification regression (1) for non-Muslim women aged 45 and under, the estimate of the effect of the change in policy is .034 (with a standard error of .0123), which is significant at 1%, and actually higher in magnitude than the estimate for the whole sample which I present in the main regression results (.022 and significant at 5%). This provides strong evidence of a pure "labor participation effect".

I also present evidence that in my sample (Israel's Labor Force Surveys), it is not evident that the proportion of women of higher parity increased [9]. When illustrating the proportion of women, by parity in each sample year, I find no evident trends occurring after the change in policy; if anything, there is an increase in the proportion of women without any children at the expense of women of party 1 or 2. It could be that the demographic composition of each of the parity groups is changing, but there is no evidence for that. I also look at women aged 45 and under, as they are the women in my main sample; similar patterns emerge. Therefore, it could that the decline in fertility that is marginally documented in the two studies picks up on changes in fertility within the control group (for example, from 1 or 2 children to 0 children). Changes within the control or treatment groups are less detrimental than changes around the threshold of 3 children that would have migrated people from the control to the treatment group. There is no evidence that this occurs in the data.

Lastly, the "fertility effects" found in the two studies are in smaller magnitudes than the "labor force participation effects" I find, so they would probably not negate the latter alltogether.

D. Placebo Test

A placebo test is performed in order to ascertain that the results were not generated by chance. I conduct the placebo test in the following manner; first, separately for each year, and for men and women, I randomly assign each individual a parity such that the original distribution of parities remains the same. As parity treatment, I essentially assign a placebo treatment to each individual. Then, I run the main specification re-

[9]The figures are available by request

gression to obtain the effect of the placebo treatment (i.e. placebo $\hat{\beta}$) . I repeat the process 1,000 times and construct a distribution of the placebo $\hat{\beta}$. I perform this test for the labor force participation outcome, for men and women under ages 45. The results are presented in Figure 6. As expected, the distribution is centered around zero, as the placebo treatment should not exhibit an effect. The reference lines detail the effect found for the actual treatment (0.02246 for women and -0.02506 for men), as detailed in the results section. For both men and women, these are at the tail of the distribution; above the 99^{th} percentile for women, and below the 1^{st} percentile for men. This provides evidence that the results with respect to the effect of the policy were extremely unlikely to have been generated by chance.

V. Conclusion

In this paper I attempt at providing the first estimates on the causal effect on labor outcomes of a significant reduction in child allowances that was implemented in Israel in an attempt to induce populations with historically low labor force participation rates to enter the labor market. I find that the reductions increased the labor force participation of young women. The results are robust to different specifications, specifically one that takes into account treatment intensity, and one that better assigns control groups. Although i find that man decreased their labor force participation rates in response to the policy, due to weaker identification, these results should be taken with a grain of salt. I find no significant effect on men's or women's working hours. In line with a-priori expectations, I find that the the younger and more educated women are, the more they are responsive to treatment.

These estimates are important since similar policies are being implemented throughout the Western world at high costs to government, without their full implications being researched. In countries throughout the world, women exhibit lower labor force participation rates than men; identifying an additional mechanism which incentivizes them to enter the labor is important.

Although it seems that the policy both saved public money, and incentivized women to work, the full welfare effects of the policy over the long-term should be established, as I have only estimated the short term effect. Specifically, it is essential to analyze whether the women who entered the labor force improved their welfare, or just entered low paying part time jobs that substituted for their lost child allowances but overall hurt their welfare. Moreover, increase in the labor supply of women could affect their child's development, as they move into childcare or other non-maternal care, the care of a family member, or care for themselves; the effects, both positive and negative, have been discussed abundantly in both the economics literature and the literature on child development. [10] In addition, the effect of the policy on men should be better identified, as it is unclear whether the identifying assumptions hold firmly.

Two additional extensions could be implemented with richer data that is available. First, as I find that women increased their labor force participation but men decreased it, the net effect at the household level is ambiguous. Since most households comprise of a single unit with respect to budget and decision making, the total effect on households (and the children involved) cannot not be estimated by separately estimating the effect on men and women. Due to missing data I could not perform this test. Data that links an individual to a household could have also enabled testing the effect on women conditional on their husband's income. Second, richer data that included birth dates would have facilitated the identification of the effects by comparing the labor outcomes of mothers who had children just before the May 2003 cutoff to those who had children just after, in a regression discontinuity design. Unfortunately using the data I have, birth dates could not be accurately imputed to children because of age bins that are too broad.

VI. Appendices

A. *Imputation of Child Allowances to Individuals*

During the years 1997-2002 child allowances depended only on parity and the year; therefore, imputing child allowances to each individual was straightforward such that each individual was imputed the child allowance that prevailed that year given her parity. Beginning in 2003 however, parity no longer directly correspond to the total amount of child allowances, since mothers who gave birth before May 31st, 2003 receive different child allowance than those who gave birth after, so child's age needs to be taken into consideration. Unfortunately, the exact age of the children of each mother is not provided; in some cases, this prevents me from concluding whether the child was born before May 31, 2003 and should be imputed the "old child" allowance, or whether the child was born after that date and should be imputed the "new child" allowance. For example, in the 2003 survey, children who were detailed in the 0-1 age bin could have been born anytime from early 2001 to the end of 2003. The share of children in the survey for which exact categorization into "new child" or "old child" could not be made ranged from 10% in 2003 to %29 in 2008. Although these shares are not negligible, both the "old child" allowance and the "new child" allowances decreased due to the policy, albeit at different magnitudes, so imputing individuals these "uncertain allowances" would still capture the decrease in allowances due to the policy.

In order to impute child allowances to individuals with children whose exact category could not be determined, I first determine whether the child is more likely to be a "new child" or an "old child" according to the range of the child's possible birth dates. For example, in the 2003 survey, a child in the 0-1 age bin, is more likely have been born prior to May 31st, 2003, so I would categorize this child as a "old child" and impute the individual's child allowances accordingly. In some cases, a child is as equally likely to be a "new child" as it is to be an "old child". For example, in the 2004 survey, the possible birth dates for a child in the 0-1 age bins range from early 2002 to late 2004. In these case

[10] The relevant literature mainly focuses on the effects of mothers' employment during the early years of a child's life on various cognitive, behavioral and health outcomes, although effects on older children are pertinent in the context of this paper as well. For example, Baker, Gruber & Milligan (2008) find that an increase in women employment due to the introduction of childcare had negative effects on a variety of childhood outcomes such as anxiety and the child's health status. Ruhm (2004) finds that maternal employment during the first three years of the child's life has a small deleterious effect on estimated verbal ability of three and four year-olds and a larger negative impact on reading and mathematics achievement of five- and six-year-olds. Lower cognitive abilities due to parental employment were also found by Brooks–Gunn et al. (2002), Ruhn (2008) and Bernal (2008) amongst others. In contrast, some papers find positive benefits of maternal employment; for example Nomaguchi (2006) finds that maternal employment is related to more prosocial behavior and less anxiety; other positive benefits of maternal employment are associated with the placement of a child in childcare, which is often linked with higher cognitive abilities later on in life (see for example, Loeb et al. (2007)).

I randomize the categorization of these children to either "old children" or "new children" and impute child allowances accordingly. When comparing the actual shares of children who received "new child" allowances each year (from the National Insurance Institute, 2009) with the imputed share, then except for the years 2006 and 2009, the shares are within 5 percentage points of each other.

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VII. Tables

Table 1—: Yearly Expenditure on Benefits for Working-Age Individuals

	Children (Actual)	Children (Survey)	Disability	Maternity Leave	Unemployment	Income Support
1997	7,910	7,591	4,621	2,405	3,193	2,291
	(1,996)	(1,896)	(112)	(124)	(86)	(89)
2001	9,345	8,908	7,429	3,255	4,376	4,376
	(2,155)	(2,111)	(142)	(127)	(105)	(142)
2005	5,196	4,796	8,816	3,264	2,348	3,253
	(2,261)	(2,253)	(171)	(141)	(59)	(140)
2009	5,802	5,353	10,571	4,728	3,172	2,683
	(2,417)	(2,399)	(200)	(158)	(73)	(112)

Notes: The figures are in fixed 2010 NIS, 1,000's. In parenthesis are the number of individuals who receive each benefit, in thousands. The table does not include reserve duty benefits and victims of hostilities benefits which only affect a small number of Israelis. The weighted total amount of child allowances imputed to women in the labor force surveys, and the weighted number of children for which allowances were received in the labor force surveys.
Source: National Insurance Institute Yearly Report, different years (columns 1, 3-6). Own calculations, Labor Force Surveys, different years (column 2).

Table 2—: Summary Statistics - Women

	1997-2001				2005-2009	
	0	1-3	4+	Diff	1-3	4+
Age	47.4	38.5	36.7	-1.9	38.7	36.8
	(12.4)	(8.6)	(6.2)	(.12)	(8.7)	(6.4)
Married	.59	.85	.95	.1	.84	.94
	(.49)	(.36)	(.23)	(.007)	(.36)	(.23)
Number of Children	0	1.8	5.0	3.2	1.9	5.1
	(0)	(.77)	(1.4)	(.08)	(.78)	(1.5)
Aged 0-9	0	1.0	2.9	1.9	1.1	3.0
	(0)	(.93)	(1.5)	(.08)	(.96)	(1.6)
Aged 10-17	0	.82	2.1	1.3	.77	2.1
	(0)	(.82)	(1.4)	(.02)	(.82)	(1.5)
High School Graduate	.68	.77	.55	-.22	.84	.67
	(.47)	(.42)	(.5)	(.04)	(.37)	(.47)
College Graduate	.35	.34	.16	-.17	.41	.23
	(.48)	(.47)	(.37)	(.01)	(.49)	(.42)
Jewish	.88	.84	.56	-.29	.81	.55
	(.32)	(.36)	(.5)	(.05)	(.39)	(.5)
Immigrant	.61	.45	.45	0	.31	.25
	(.49)	(.5)	(.5)	(.02)	(.46)	(.43)
Independent Variable						
Yearly Child Allowances	0	5,281	30,792	25,511	3,579	13,497
	(0)	(2,941)	(14,576)	(821)	(1,591)	(6,281)
Outcome Variables						
Labor Force Participation	.61	.69	.35	-.34	.73	.42
	(.49)	(.46)	(.48)	(.01)	(.44)	(.49)
Weekly Working Hours	35.5	34.6	30.1	-4.5	34.1	29.4
	(13.7)	(12.4)	(12.3)	(.46)	(12.6)	(12.5)
Observations	64,699	75,951	16,704	-	69,803	17,038

Notes: The table provides summary statistics for the main sample of working ages women (25-64), by parity. The table provides means, with standard deviations in parentheses. All variables are weighted by the sample weights. 1997-2001 is the period before the policy change and 2005-2009 is the period after the policy change. Individuals of parity 1-3 are the control group and individuals of parity 4 and above are the treatment group. Column (5) details the difference in each characteristic between the treatment and control group. The differences were calculated by regressing each covariate as the dependent variable on the $Treat$ variable. The standard errors of these differences were clustered at the locality level and are reported in parentheses. Except for $Immigrant$, all other differences are significant at 1%.
Source: Own calculations, Labor Force Surveys, different years.

Table 3—: Pre-Policy Time Trends

Treat X 1997	-.009
	(.017)
Treat X 1998	.009
	(.016)
Treat X 1999	.013
	(.012)
Treat X 2000	-.011
	(.014)
Treat X 2001	.004
	(.011)
Observations	172,761

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Notes: The coefficients on $Yeart*Treat_p$ for pre-policy years are reported from an event-study model using regression specification (1) with labor force participation as outcome. Omitted year is 2001. Robust standard errors clustered at the locality level in parentheses. Sample weights are used in all regressions. The sample includes women aged 45 and under.
Source: Own calculations, Labor Force Surveys, different years.

Table 4—: Balance Tests for Observables

	Difference in Trend
Age	-.28**
	(.110)
Married	.008
	(.007)
Years of Schooling	.163
	(.139)
Jewish	.018
	(.018)
Immigrant	4.42e-16
	(7.18e-16)
Observations	172,761

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Notes: The coefficient on $Post_t * Treat_p$ from regression specification (1) are reported where each respective variable is the dependent variable in separate regressions. Robust standard errors clustered at the locality level in parentheses. Sample weights are used in all regressions. The sample includes women aged 45 and under.
Source: Own calculations, Labor Force Surveys, different years.

Table 5—: Main Regression Results - Labor Outcomes

	(1)	(2)	(3)	(4) Age<55	(5) Age<45	(6) Age<35
Panel A: Women						
Labor Force Participation	.04341***	.01639	.00007	.01935*	.02246**	.03778***
	(.01312)	(.01005)	(.02121)	(.00994)	(.00979)	(.01315)
Observations		233,858		225,588	172,761	80,916
Weekly Working Hours	-.04195	-.10152	-.78572	-.08073	.07166	-.39662
	(.44629)	(.46446)	(.99879)	(.46105)	(.47031)	(.66885)
Observations		123,050		120,696	91,267	39,108
Panel B: Men						
Labor Force Participation	-.01579	-.02023**	.00091	-.01911**	-.02506**	-.03845**
	(.00973)	(.00948)	(.02593)	(.00943)	(.01000)	(.01829)
Observations		217,088		201,984	140,284	59,544
Weekly Working Hours	.29459	.11719	1.2276	.09689	.39036	-.51711
	(.34926)	(.34776)	(.83214)	(.35907)	(.39819)	(.79822)
Observations		159,362		151,297	105,165	42,325
Year and Group FE	Yes	Yes	Yes	Yes	Yes	Yes
Group Specific Liner Trends	No	No	Yes	No	No	No
Controls	No	Yes	Yes	Yes	Yes	Yes

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Notes: Robust standard errors clustered at the locality level in parentheses. Sample weights are used in all regressions. Columns (1)-(3) provide the coefficient on the $Post_t * Treat_p$ variable. Separate regressions are run for the two dependent labor outcome variables - labor force participation and weekly working hours, for men and for women, using different specifications as indicated. Controls include marriage status, age, age squared, education, religion, immigrant status and the number of small children. Sample for weekly working hours conditions on employment. Columns (4)-(6) provide the results using the main specification (year and group FE and controls), for different age groups as indicated.
Source: Own calculations, Labor Force Surveys, different years.

Table 6—: Regression Results for the Effect on Labor Force Participation for Ages<45

	(1)	(2)	(3)	(4)
	Women		*Men*	
Post*Treat	.04765***	.02246**	-.02674**	-.02506**
	(.01318)	(.00978)	(.01127)	(.01000)
During*Treat	.00163	-.00491	-.00213	-.00444
	(.01153)	(.00920)	(.01086)	(.00981)
Controls				
Number of Children (1 omitted)				
2		-.00138		-.03510***
		(.00486)		(.00555)
5		-.19499***		-.19976***
		(.01922)		(.03899)
Marriage Status (married omitted)				
Divorced		.01726***		-.10421***
		(.00693)		(.01668)
Single		.02370**		-.04587**
		(.01085)		(.01918)
Age		.05091***		.09194***
		(.00451)		(.01616)
Age Square		-.00069***		-.00116****
		(.00006)		(.00020)
Education (less than HS omitted)				
High School Graduate		.12741***		.05217***
		(.00732)		(.00508)
College Graduate		.28970***		-.00145
		(.01341)		(.01431)
Religion (Jewish omitted)				
Muslim		-.39268***		.05758***
		(.01290)		(.02067)
Immigrant		-.02458***		.02115***
		(.00742)		(.00549)
Raises small children		-.0527***		-.01130***
		(.00323)		(.00407)
Observations	172,761		140,284	
Year and Group FE	Yes	Yes	Yes	Yes
Group Specific Liner Trends	No	No	No	No
Controls	No	Yes	No	Yes

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Notes: Robust standard errors clustered at the locality level in parentheses. Sample weights are used in all regressions. Year and group FE are included in all regressions. The table provides the coefficients on all covariates where the dependent variable is labor force participation without controls (columns (1) and (3)) and with controls (columns (2) and (4)), run separately for men and women below the age of 45.
Source: Own calculations, Labor Force Surveys, different years.

Table 7—: Additional Results

	3 vs. 4	4 vs. 5	5 vs. 6	6 vs. 7+
Panel A: Each Group Compared to a Group with One Less Child				
Women				
Labor Force Participation	.02251*	-.01537	.01986	.02974
	(.01365)	(.02206)	(.01953)	(.02368)
Observations	56,193	27,032	13,837	12,072
Men				
Labor Force Participation	-.02434**	-.00481	.00418	.02490
	(.01217)	(.02223)	(.03138)	(.03550)
Observations	61,828	29,654	14,880	12,642
Panel B: Continuous Treatment (Yearly Child Allowances)				
Women				
Labor Force Participation	-.0000014***			
	(.00000049)			
Observations	229,302			
Men				
Labor Force Participation	.0000017***			
	(.00000065)			
Observations	215,361			
	Post*Treat	Post*Treat(-1)	Post*Treat(-2)	
Panel C: Delayed Effect (Including Lags)				
Women				
Labor Force Participation	.02874*	.01724	-.01504	
	(.01598)	(.01718)	(.01380)	
Observations		172,761		
Men				
Labor Force Participation	-.00327	-.03918*	.01523	
	(.02365)	(.02274)	(.02274)	
Observations		140,284		

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Notes: Robust standard errors clustered at the locality level in parentheses. Sample weights are used in all regressions. All regressions include individuals aged 45 or below. In all regressions the main specification is used (year and group FE and controls). The table provides the coefficient on the $Post_t * Treat_p$ variable where the dependent variable is labor force participation in three models, each run for men and women separately. In Panel A, each two successive groups are used in separate regressions where the one with one less child serves as the control group. In Panel B, a continuous measure of treatment (yearly child allowances) is used. In Panel C, two lags are added to the main specification; the coefficient on the main effect as well as the two lags is reported.
Source: Own calculations, Labor Force Surveys, different years.

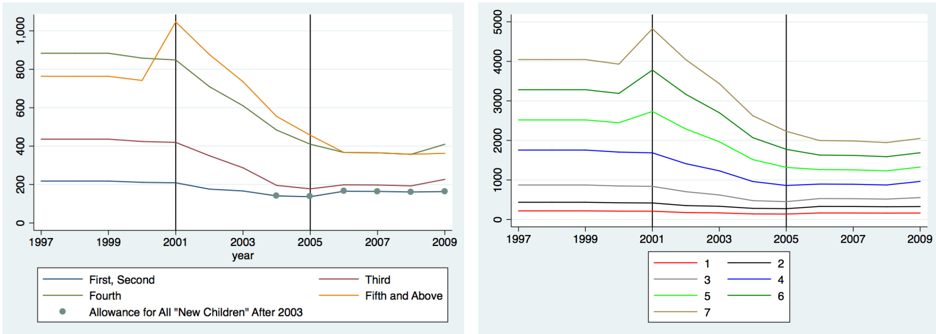

Figure 1. : Monthly Allowance per Child (left). Total Monthly Allowance per Family, by Parity (right). (NIS - 2010 Prices)

Notes: The left figure details the monthly allowance *per child*. After May 31, 2003, allowances diverged according to the birth date of the child. The line graph illustrates the allowances for ”old children” (those born before the date) while the connected dots illustrate the allowances for ”new children” (those born after the date); these are the same per child, no matter the parity. The right figure details the total monthly allowance per family which is the sum of the allowances for each of its children. The total was calculated for families consisting of ”old children” only. The reference lines encompass the policy change years.
Source: Lau, 2016, Asiskovitz, 2007 and National Insurance Institute Yearly Report, different years.

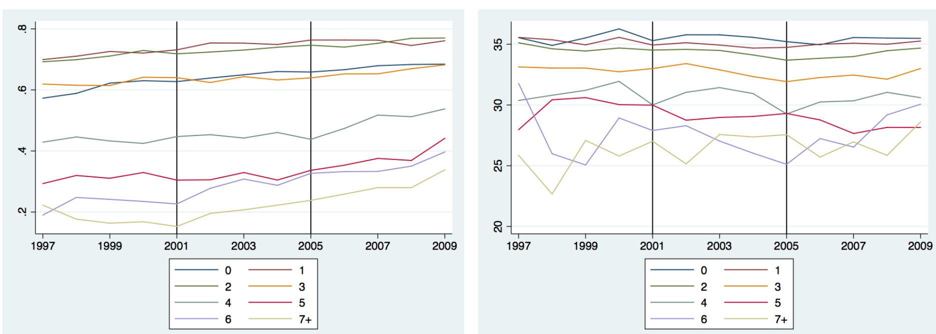

Figure 2. : Labor Force Participation Rates (left) and Weekly Working Hours (right) - Women, Ages 25-64, by Parity

Notes: Labor force participation is defined as the share of the group who are either employed or looking for a job. Working hours are defined as the hours that each employed (either full-time or part-time) individual worked in the reference week, not including absentees from work. The measures were calculated using the sample weights. The reference lines encompass the policy change years.
Source: Own calculations, Labor Force Surveys, different years.

Figure 3. : Labor Force Participation Rates - Women, Ages 25-64, by Parity.

Notes: Labor force participation is defined as the share of the group who are either employed or looking for a job. The measures were calculated using the sample weights.The reference lines encompass the policy change years.
Source: Own calculations, Labor Force Surveys, different years.

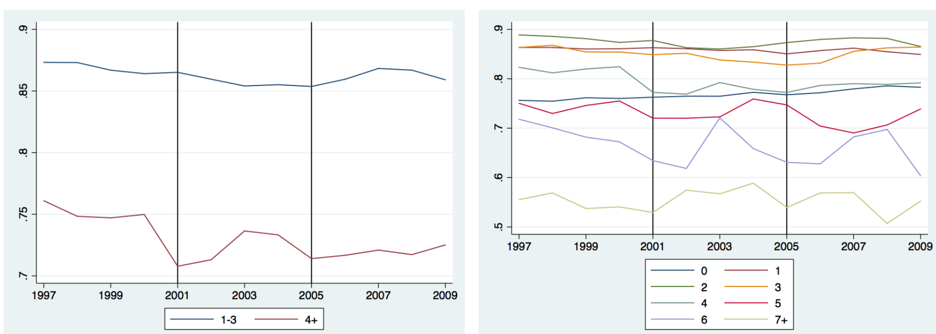

Figure 4. : Labor Force Participation Rates - Men, Ages 25-64, by Parity. Control and Treatment Groups (left), All Parities (right).

Notes: Labor force participation is defined as the share of the group who are either employed or looking for a job. The measures were calculated using the sample weights.The reference lines encompass the policy change years.
Source: Own calculations, Labor Force Surveys, different years.

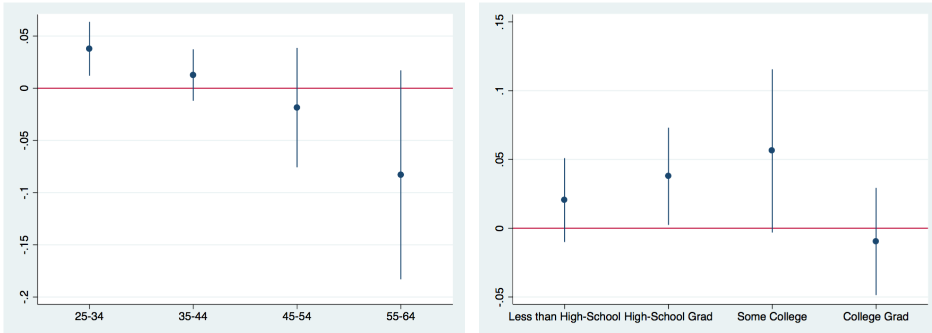

Figure 5. : Coefficient Plots for Different Subgroups - Women. Age (Left), Education (Right).

Notes: Regression for each subgroup is run separately, using the main specification (year and parity FE and controls). The coefficient and confidence intervals on $Post_t * Treat_p$ are reported where the dependent variable is women's labor force participation. Regressions on education subgroups include women aged 45 and under.
Source: Own calculations, Labor Force Surveys, different years.

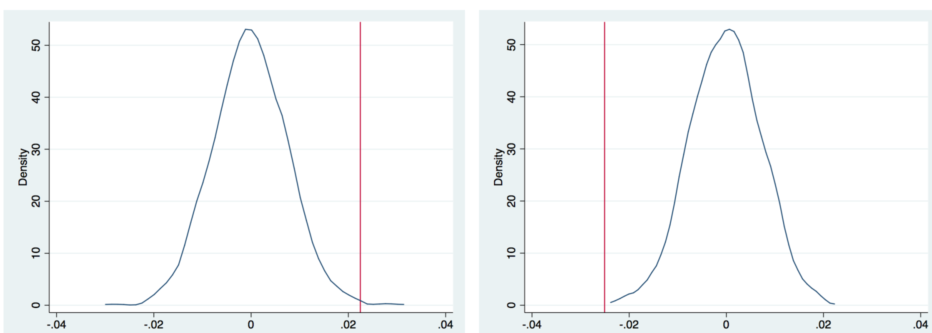

Figure 6. : Distribution of the Placebo Estimates. Women (Left), Men (Right).

Notes: The estimates are of the coefficient on $Post_t * Treat_p$ and are result of randomly assigning treatment and running the main specification (including year and group FE and controls) regression for ages 45 and below where the dependent variable is labor force participation. 1,000 randomizations are conducted. The estimate observed in the actual data is indicated by the reference lines.
Source: Own calculations, Labor Force Surveys, different years.