THE UNIVERSITY OF CHICAGO

ESSAYS ON THE ECONOMICS OF THE FAMILY

A DISSERTATION SUBMITTED TO
THE FACULTY OF THE DIVISION OF THE SOCIAL SCIENCES
IN CANDIDACY FOR THE DEGREE OF
DOCTOR OF PHILOSOPHY

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CHICAGO, ILLINOIS
JUNE 2018

To Amanda and Verónica

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Acknowledgments

I am indebted to Magne Mogstad, Derek Neal, and Alessandra Voena for their patience in providing guidance and feedback throughout this project. I also thank Thibaut Lamadon, Petra Todd, Steve Levitt, Stephane Bonhomme, James Heckman, and seminar participants at the SOLE annual meeting, North American Summer Meeting of the Econometric Society, University of Chicago, Universidad Diego Portales, Universidad de los Andes, and Pontificia Universidad Católica for valuable comments.

Abstract

In Milwaukee, Wisconsin (1994-1997) a work-based, anti-poverty intervention, "New Hope," randomly assigned an income subsidy—similar to the EITC—and a child care subsidy to a group of economically disadvantaged families. Randomly chosen applicants had access to these benefits for three years. The experimental evaluation found positive effects of the program on labor supply, income, and child care use. Notably, the program also boosted various measures of child academic achievement. Using New Hope data, this thesis offers two novel contributions to the New Hope literature and to the broader research on income and child care subsidies. First, I find that the effects of New Hope on labor supply, earnings, and child care use vary according to whether the family had a young child (six years old or less) at home or not while they were eligible to the program. Furthermore, compared to children older than six years of age, New Hope boosted child academic performance of younger children by more than double. Second, I disentangle the mechanisms that explain the impact of New Hope on the human capital of children who were young while their families were under New Hope. To this end, I develop and estimate a dynamic-discrete choice model of the household and child academic achievement. Counterfactual analyses based on the structural framework indicate that the bulk of the impact of New Hope on the academic achievement of young children is explained because of the policy-induced increase in center-based child care use. Consistently, the larger share of the impact of New Hope on child human capital is explained by the child care subsidy component of the New Hope policy bundle.

Chapter 1

Introduction

Over the last 20 years, policymakers have used a variety of strategies to encourage the labor market participation of low-income families. Two of the most important policies have been income and child care subsidies.[1] The empirical evidence indicates that income and child care subsidies have succeeded in meeting their original goals—namely, to promote work and increase family income.[2] However, these policies could have unintended, negative consequences on child outcomes: parents could opt for center-based child care, increase the time spent in the labor market, and reduce the time caring for children.[3] These concerns about potentially negative effects of income and child care subsidies become especially relevant for families with preschoolers (Heckman and Mosso, 2014).

Some of the most compelling evidence on the effects of income and child care subsidies comes from a randomized controlled trial called New Hope. The program assigned applicants (over 18 years old) to a policy bundle that included an income subsidy—similar to the EITC—and a child care subsidy—which resembled the CCDF—tied to a full-time work

[1] In the U.S., two prominent examples of such subsidies are the Child Care Development Fund (CCDF)—created by the Personal Responsibility and Work Opportunity Act (PRWORA)—and the Earned Income Tax Credit (EITC). The CCDF is a block grant to states for the provision of child care vouchers to low-income working parents. This program was conceived as a complement to Temporary Assistance for Needy Families (TANF) that would facilitate welfare-to-work transitions. The EITC is a mean-tested cash transfer program for low-income families.

[2] See Grogger and Karoly (2009) and Moffitt (2016) for a review of the evidence. See Hoynes and Rothstein (2016) for a description of the EITC and its impacts on labor supply. See also Chan (2013) and Keane and Wolpin (2010) for evidence of changes in the welfare system within a dynamic framework. See Blundell et al. (2015) for recent evidence on the effects of tax credits on the labor supply and educational choices of single and married mothers.

[3] See Bernal (2008), Bernal and Keane (2010), Bernal and Keane (2011), and Brilli (2014) for evidence comparing income and time allocation impacts on child development. See Heckman and Mosso (2014) for a review of the evidence on the effects of income on skills accumulation over the child life-cycle.

requirement. The New Hope literature documents that the program increased annual family income by 7%, the probability of being employed in any given quarter by 8 percentage points (from a baseline of 63%), and the likelihood of using child care for young children by 15 percentage points (from a baseline of 33%). Notably, the program also boosted various measures of child academic achievement.[4]

This thesis explores the effects of income and child care subsidies on household behavior and child outcomes. It answers two related questions: How household behavioral responses to the New Hope policy change when there are young children (six years of age or less) in the household? What are the most important mechanisms by which New Hope affected early childhood human capital? The main conclusion of this thesis is that having low-cost, high-quality center-based child care available for families plays a central role in explaining the effects of New Hope on adult behavior and children. The behavioral responses of parents of young children—compared to parents of older children—can be traced back to the influence of child care on the production function of early childhood human capital. At the same time, a vast portion of the effects of New Hope on children is explained because parents were induced to use child care with a higher probability. Results from this thesis have implications on the optimal design of public policies that are originally addressed to encourage work on families with young children. If household behavior affects child human capital, and child human capital directly impacts adult utility, then household responses to income and child care subsidies will be influenced by the presence of children and child human capital will be affected by these policies. The optimal design of income and child care subsidies—and other related welfare policies—should consider the underlying production function of early childhood human capital as a critical component.

[4]See Bos et al. (1999), Huston et al. (2003), and Miller et al. (2008) for evidence on the effects of New Hope on child and family outcomes. I obtain the numbers above by using the New Hope databases. Chapter 3 presents novel evidence for the sample of families with young children.

This thesis begins with an in-depth description of available databases in Chapter 2. Here, I describe and discuss the scope New Hope data and define the main dependent and independent variables that are used in the subsequent chapters.

Chapter 3 analyzes the effects of New Hope for two types of families: those with young children at the time New Hope was running and those with older children. Using New Hope experimental data, I estimate treatment effects on child care use, employment, and income, focusing on families that had at least one child less than seven years old while New Hope was running. I show that, compared to families that did not have a young child during the New Hope period, families with at least one young child had larger effects on child care and hours worked on the intensive margin. Effects on income were similar, although effects on earnings were more pronounced for families with young children. Finally, effects on child outcomes were substantially higher for young children. I present evidence suggesting that the child care subsidy component of New Hope explains the different behaviors of families.

Finally, Chapter 4 studies the mechanisms by which New Hope affected child academic achievement. The New Hope evaluation considered only a one-arm experiment for a bundle of policies. Moreover, from the experimental data, the contribution of household behavioral changes in the effects of the policies on child outcomes cannot be disentangled. To discipline these household- and policy-level mechanisms, I develop and estimate a dynamic-discrete choice model of the household and child human capital. Using the estimated model, I decompose the treatment effects on child outcomes into what is explained by changes in income, child care use, and labor supply. Results show that more than 90% of the short-term effects of New Hope on child human capital are explained by the treatment effects on child care use. Furthermore, most of the impact of New Hope on children can be accounted by the child care subsidy.

Related literature This thesis contributes to the research that explores the intergenerational consequences of mean-tested programs. Specifically, it advances the literature on five fronts. First, this study provides new evidence on the impacts of income subsidies on child outcomes. Chetty et al. (2011), Maxfield (2013), Hoynes et al. (2015a), Manoli and Turner (2015), and Bastian and Michelmore (2017) report relatively large intergenerational effects of the EITC. Along similar lines, Dahl and Lochner (2012) use variation in the EITC schedule across states and time as an instrument to identify the causal effect of income on child test scores. Often, researchers interpret this literature as causal evidence on the effects of money on child outcomes. However, in order to interpret exposure to the EITC as a pure income effect, one needs to instrument for other household variables that affect child outcomes and that are being affected by the policy; an exogenous income shock lowers the marginal value of market time while simultaneously raising the marginal value of paying for private child care or preschool. Moreover, in a treatment effects analysis, we do not know for certain if income shifts caused by the EITC can be thought of as temporary or permanent shocks to income.[5] To understand the reduced-form effects of income subsidies, I decompose the treatment effects on child human capital in terms of household behavioral changes.

Second, this paper also contributes to the literature on child care subsidies and child skills accumulation. So far, the literature has reached varied results regarding the average effects of child care subsidies on child outcomes (Baker et al., 2008; Herbst and Tekin, 2010a,b; Havnes and Mogstad, 2011, 2015; Black et al., 2014; Cornelissen et al., 2017).[6] An emerging consensus states that children from low-income families benefit the most from child care subsidies and universal child care programs (Havnes and Mogstad, 2011, 2015; Cornelissen et al., 2017), although the channels generating this pattern of heterogeneous effects on children are still unclear.[7] Since mechanisms are not being systematically assessed in these studies, it is difficult to obtain general lessons about the effectiveness of child care subsidies out of the differing results found in the literature. To understand the effects of

[5] Dahl and Lochner (2012) justify their relatively large estimated impact by arguing that the instrument captures permanent income changes.

[6] See Elango et al. (2016) for a review of the literature.

[7] Elango et al. (2016) suggest that the returns to universal child care programs are higher for children from low-income families because the quality of alternative child care arrangements for them is lower than that of children from high-income families.

child care subsidies, I use the structure of my model to reveal the contribution of different sources of household behavior in explaining the impact of child care subsidies. Overall, I find that the effects of child care subsidies are largely consistent with evidence showing sizable impacts of early childhood education on children from economically disadvantaged families (Elango et al., 2016).

Third, I advance the research on the effects of income subsidies on single mothers' labor supply. In particular, for this group, the literature on the effects of the EITC documents positive effects on employment (3-7 percentage points depending on the study). However, there have not been studies comparing the differential responses to such policies according to the age structure of children in the family. Moreover, my work presents a novel set of results to the New Hope literature by showing heterogeneous effects by child age.[8] Heterogeneous effects along this dimension are expected given that the child human capital production function might change over time. The most obvious change occurs when children are old enough to go to school; at that age, concerns about child care availability become less important. Blundell et al. (2016) find evidence consistent with a model in which the parent cares for her child's human capital and working full-time implies a negative effect that is larger for young children. My results are somewhat against what the Blundell et. al. model would predict: I find that New Hope had larger effects on labor supply and earnings on families with young children. Results from Chapter 3 tend to favor the hypothesis that the availability of a high-quality child care and the child care subsidy might explain why families with young children had larger labor-supply responses.

Fourth, this thesis contributes to the literature on the effects of income subsidies on child outcomes for children who were not old enough to attend elementary school while they were exposed to the policy. The majority of recent papers have focused on adolescents. Bastian and Michelmore (2017) estimate the effects of changes in the EITC on children who were 13-18 years of age by the time of the policy changes. Dahl and Lochner (2012) present IV

[8] See Bos et al. (1999), Huston et al. (2001), Huston et al. (2003), Huston et al. (2005), Miller et al. (2008), and Huston et al. (2011).

estimates of the effect of income on children who were in between 8 and 14 years of age while exposed to the income shocks. Maxfield (2013) is the only paper that estimates the effects of the EITC on preschoolers. In line with my results, the author finds that the estimated, long-run effects of changes in the EITC are larger for this group than for older children.[9] From the evolution of the production function of human capital perspective (Cunha and Heckman, 2006; Heckman and Mosso, 2014), young children may be more susceptible to changes in household behavior than adolescents, which might generate heterogeneity in the effects of policies—such as the EITC—on children of different ages.

Finally, on the methodological side, I build upon the literature that combines experimental and quasi-experimental evidence with structural models (Bajari and Hortaçsu, 2005; Todd and Wolpin, 2006; Keane and Wolpin, 2007; Attanasio et al., 2011; DellaVigna et al., 2012; Attanasio et al., 2015; Voena, 2015; Autor et al., 2017). Following this line of research, my framework advances reduced-form studies by delineating the mechanisms that explain the observed policy treatment effects obtained by either experimental or quasi-experimental methods. I also contribute to the structural literature on household behavior and child welfare by exploiting experimental data to validate the model's capacity to predict the impact of different welfare policies (Bernal, 2008; Brown and Flinn, 2011; Del Boca et al., 2013; Brilli, 2014; Del Boca et al., 2014; Mullins, 2015; Bruins, 2016).[10]

[9] Another exception to the rule is Hoynes et al. (2015b), who study effects of the EITC on birth outcomes.

[10] This literature uses survey data to estimate structural models and non-experimental moments as model validation.

Chapter 2

New Hope Data on Household Choices and Child Outcomes

2.1 Summary

From August 1994 to December 1995, the MDRC—the agency in charge of the experimental evaluation—recruited the original New Hope sample. This sample consisted of 1,357 individuals; 678 of them were randomly selected to the treatment group and 679 to the control group. To evaluate the intervention's impact, the MDRC collected data on participant's labor market outcomes and families up to eight years after baseline. This chapter reviews the available data from the New Hope experiment and explains the construction of variables used in the analyses of Chapters 3 and 4.

This chapter is structured as follows. Section 2.2 describes available databases. The MDRC collected information from surveys and administrative data to measure household behavior and outcomes over time. Since surveys present a considerable amount of attrition, I analyze if baseline observed characteristics are balanced in spite of attrition. Results indicate that we cannot reject the null hypothesis that observed characteristics between treatment and control groups differ for most of the examined variables. Finally, Section 2.3 details the construction of the different household and child variables used in the subsequent chapters.

2.2 Databases

Table 2.1 presents a description of the New Hope data. It consists of four databases: the baseline survey, the parents and children's survey (the "New Hope surveys"), the teachers' survey, and administrative information from the state of Wisconsin.

At baseline, the MDRC collected information on labor market outcomes and family characteristics. Part of the questionnaire at baseline included questions regarding last month's earnings, welfare participation, and occupational status. Also, baseline data include demographic characteristics such as gender, age, and race. Finally, the survey asked for household composition: number of children and marital status.

The New Hope surveys gathered household information on work, children's welfare, and parenting practices. The survey has detailed self-reported data on hours worked and child care choices. These surveys were collected two, five, and eight years after random assignment. From these surveys, I construct measures of labor supply and child care use.

The third source of information is the teacher surveys. In these surveys, teachers gave their assessment on several child academic and behavioral indicators. Based on the reported answers, the MDRC constructed two measures: the Social Skills Rating System (SSRS) academic sub-scale and the Classroom Behavior Scale.[1] I use the SSRS for two years after baseline as the main outcome variable of child academic achievement.

Finally, I use administrative records from the state of Wisconsin. The administrative records contain information on earnings from Wisconsin's Unemployment Insurance (UI) and directly from the firms that hired participants for New Hope's Community Service Jobs (CSJ). It also includes payments of AFDC and Food Stamps (SNAP) gathered from data of the State of Wisconsin. Unlike the New Hope and teachers' surveys, the administrative data is available for the whole CFS sample and continuously up to eight years after random assignment.

[1] Section 2.3.5 provides details on these two variables.

Table 2.1: New Hope data

Data	Variables	Periods
Baseline survey	Labor market and family	At baseline (1994-1995).
New Hope Survey	Hours worked and child care use and expenditures.	Two, five, and eight years after random assignment.
Teachers' survey	Child outcomes: SSRS Academic Subscale	Two, five, and eight years after random assignment.
New Hope's and Wisconsin's administrative records	New Hope: benefits take-up (income supplement, child care payments). Wisconsin: Labor earnings (UI system), AFDC, and Food Stamps payments.	1994-2003.

Notes: This table shows the available databases (first column), the associated variables (second column), and the years in which they were collected (third column).

In my empirical analysis, I use the sample of individuals with at least one child. This sample is called the Child and Family Study (CFS). The CFS has information only for participants with at least one child between 1 and 10 years of age. The CFS sample has data on 745 adults of the original 1,357.[2] In addition, 50 adults in the CFS database do not match in the youth database, and two children in the youth database do not match in the adult CFS data. I excluded these observations from the analysis, leaving 695 adults as the main CFS sample.

Table 2.2 shows the number of individuals from which we have information from the different datasets. The first panel compares the number of adults in the whole sample and in the CFS. The last three rows of the same panel show the number of CFS adult participants who answered the New Hope surveys. The second panel presents the number of CFS children. Depending on the year, the New Hope surveys have information for 75 to 80% of the adults and 78 to 86% of the children in the CFS sample. The third panel of Table 2.2 presents the number of children with data from the teachers' survey. Not all teachers filled out the

[2] Up to two children per family were selected to be part of the CFS. According to Miller et al. (2008), if more than two children were potentially eligible to participate in the CFS survey, only two of them were randomly chosen (with preference given to opposite-sex siblings).

Table 2.2: Sample size of the Children and Family Study (CFS) across surveys

Sample	Treatment	Control	Total
Adults			
All	678	679	1,357
CFS	344	351	695
Year two	288	302	590
Year five	282	280	562
Year eight	297	300	597
Children			
All	544	561	1,105
CFS	544	561	1,105
Year two	456	489	945
Year five	432	429	861
Year eight	468	479	947
Children with teachers' report			
Year two	203	217	420
Year five	272	274	546
Year eight	272	277	549

Notes: This table shows the sample size for different surveys. The first panel (Adults) presents the number of adults from the original sample ("All"), the CFS study (adults with at least one 13-year-old or younger), and from the New Hope surveys of years two, five, and eight. The second panel (Children) shows the number of children under the same databases as the previous panel. Finally, the third panel depicts the number of children with teachers' reports in the surveys from years two, five, and eight.

questionnaires; relative to the number of children with information on the surveys from years two, five, and eight, teacher's underreport reduces the sample by 56, 37, and 42%, respectively.

To evaluate if attrition compromised balance between individual characteristics in the two experimental groups, Tables 2.3-2.7 compare participant baseline characteristics of the original CFS sample with those who answered New Hope and teachers' surveys from years two, five, and eight. Table 2.4 shows baseline characteristics of the original CFS sample. The majority of participants in the CFS are women (90%), a little more than half are African-American (58% and 53% in the treatment and control group) and 88% do not cohabitate with a spouse or partner. Moreover, only half of the sample has a high school diploma or GED certification and over 50% earned less than $10,000 in the last 12 months (1994 dollars). For all the variables in Table 2.4, there are no statistically significant differences between treatment and control groups. Even though baseline characteristics change when comparing the original CFS group to those with survey data (from Table 2.4 to Tables 2.5-2.7), for the majority of the cases (95% of them), I cannot reject the null hypothesis of different means of baseline variables at the 5% level of confidence.[3]

[3]To account for the few cases where there seem to be statistically significant differences on means of baseline variables, Chapter 3 compares regressions with and without control variables.

Table 2.3: Baseline characteristics: whole sample

Variable	(1) **Treatment**	(2) **Control**	(3) **T-C**
Age	31.35	31.06	0.30
	[9.52]	[9.05]	(0.50)
Female (%)	71.39	71.87	-0.48
	[45.23]	[45.00]	(2.45)
African-American, non-Hispanic (%)	51.77	50.96	0.81
	[50.01]	[50.03]	(2.72)
Hispanic (%)	25.81	27.10	-1.29
	[43.79]	[44.48]	(2.40)
White, non-Hispanic (%)	12.83	13.11	-0.28
	[33.47]	[33.77]	(1.83)
Others (%)	9.59	8.84	0.75
	[29.46]	[28.40]	(1.57)
Never married (%)	59.44	60.24	-0.80
	[49.14]	[48.98]	(2.66)
Married living w/ spouse (%)	12.54	11.93	0.61
	[33.14]	[32.44]	(1.78)
Married living apart (%)	9.44	9.72	-0.28
	[29.26]	[29.65]	(1.60)
Separated, divorced or widowed (%)	18.58	18.11	0.47
	[38.93]	[38.54]	(2.10)
Highschool diploma or GED (%)	45.72	45.21	0.51
	[49.85]	[49.81]	(2.71)
Highest grade completed	10.79	10.76	0.03
	[287.20]	[266.64]	(15.05)
$0 (%)	30.24	32.11	-1.87
	[45.96]	[46.72]	(2.52)
$1-999 (%)	17.40	14.14	3.27*
	[37.94]	[34.87]	(1.98)
$1,000-4,999 (%)	24.19	26.22	-2.03
	[42.85]	[44.01]	(2.36)
$5,000-9,999 (%)	16.08	17.38	-1.30
	[36.76]	[37.92]	(2.03)
$10,000-14,999 (%)	8.26	7.36	0.90
	[27.55]	[26.14]	(1.46)
$15,000 or more (%)	3.83	2.80	1.04
	[19.22]	[16.50]	(0.97)

Notes: This table compares baseline characteristics of treatment and control groups. The variables were measured at baseline. The last six rows present the proportion of individuals in different groups defined by labor earnings (past 12 months). I show sample means of each variable for treatment and control group in columns 1 and 2. The third column tests the null hypothesis that the means are equal. The sample corresponds to the original New Hope group of applicants. The first two columns present standard deviations in square brackets. In the third column, robust standard errors are in parenthesis. *,**,*** indicates significance at the 10, 5, and 1% level.

Table 2.4: Baseline characteristics: CFS sample

Variable	(1) Treatment	(2) Control	(3) T-C
Age	29.04	28.53	0.51
	[7.14]	[6.64]	(0.52)
Female (%)	90.12	91.74	-1.62
	[29.89]	[27.57]	(2.18)
African-American, non-Hispanic (%)	58.14	53.85	4.29
	[49.40]	[49.92]	(3.77)
Hispanic (%)	27.03	29.06	-2.02
	[44.48]	[45.47]	(3.41)
White, non-Hispanic (%)	10.47	14.53	-4.06
	[30.65]	[35.29]	(2.51)
Others (%)	4.36	2.56	1.80
	[20.45]	[15.83]	(1.39)
Never married (%)	62.21	62.39	-0.18
	[48.56]	[48.51]	(3.68)
Married living w/ spouse (%)	11.05	9.69	1.36
	[31.39]	[29.62]	(2.31)
Married living apart (%)	9.88	11.11	-1.23
	[29.89]	[31.47]	(2.33)
Separated, divorced or widowed (%)	16.86	16.81	0.05
	[37.49]	[37.45]	(2.84)
Highschool diploma or GED (%)	50.00	45.87	4.13
	[50.07]	[49.90]	(3.79)
Highest grade completed	11.24	11.10	0.14
	[2.15]	[2.04]	(0.16)
$0 (%)	36.63	36.75	-0.12
	[48.25]	[48.28]	(3.66)
$1-999 (%)	16.86	14.53	2.33
	[37.49]	[35.29]	(2.76)
$1,000-4,999 (%)	23.26	23.65	-0.39
	[42.31]	[42.55]	(3.22)
$5,000-9,999 (%)	13.66	14.81	-1.15
	[34.40]	[35.58]	(2.66)
$10,000-14,999 (%)	6.98	6.84	0.14
	[25.51]	[25.28]	(1.93)
$15,000 or more (%)	2.62	3.42	-0.80
	[15.99]	[18.20]	(1.30)

Notes: This table compares baseline characteristics of treatment and control groups. The variables were measured at baseline. The last six rows present the proportion of individuals in different groups defined by labor earnings (past 12 months). I show sample means of each variable for treatment and control group in columns 1 and 2. The third column tests the null hypothesis that the means are equal. The sample corresponds to participants in the CFS sample. The first two columns present standard deviations in square brackets. In the third column, robust standard errors are in parenthesis. *,**,*** indicates significance at the 10, 5, and 1% level.

Table 2.5: Baseline characteristics: CFS sample in the year-two New Hope survey

Variable	(1) Treatment	(2) Control	(3) T-C
Age	28.91	28.51	0.40
	[7.02]	[6.57]	(0.56)
Female (%)	89.58	92.72	-3.13
	[30.60]	[26.03]	(2.34)
African-American, non-Hispanic (%)	59.38	53.31	6.06
	[49.20]	[49.97]	(4.08)
Hispanic (%)	25.69	29.14	-3.44
	[43.77]	[45.52]	(3.68)
White, non-Hispanic (%)	11.11	15.23	-4.12
	[31.48]	[35.99]	(2.79)
Others (%)	3.82	2.32	1.50
	[19.20]	[15.07]	(1.42)
Never married (%)	62.50	62.25	0.25
	[48.50]	[48.56]	(4.00)
Married living w/ spouse (%)	10.42	9.93	0.48
	[30.60]	[29.96]	(2.49)
Married living apart (%)	10.42	10.26	0.15
	[30.60]	[30.40]	(2.51)
Separated, divorced or widowed (%)	16.67	17.55	-0.88
	[37.33]	[38.10]	(3.11)
Highschool diploma or GED (%)	51.74	46.03	5.71
	[50.06]	[49.92]	(4.12)
Highest grade completed	11.37	11.09	0.28*
	[2.05]	[2.08]	(0.17)
$0 (%)	37.15	37.09	0.07
	[48.41]	[48.38]	(3.99)
$1-999 (%)	15.63	14.57	1.06
	[36.37]	[35.34]	(2.95)
$1,000-4,999 (%)	23.61	25.17	-1.55
	[42.54]	[43.47]	(3.54)
$5,000-9,999 (%)	14.24	13.91	0.33
	[35.00]	[34.66]	(2.87)
$10,000-14,999 (%)	6.60	5.96	0.64
	[24.87]	[23.71]	(2.00)
$15,000 or more (%)	2.78	3.31	-0.53
	[16.46]	[17.92]	(1.42)

Notes: This table compares baseline characteristics of treatment and control groups. The variables were measured at baseline. The last six rows present the proportion of individuals in different groups defined by labor earnings (past 12 months). I show sample means of each variable for treatment and control group in columns 1 and 2. The third column tests the null hypothesis that the means are equal. The sample corresponds to participants in the CFS sample who responded the second-year survey. The first two columns present standard deviations in square brackets. In the third column, robust standard errors are in parenthesis. *,**,*** indicates significance at the 10, 5, and 1% level.

Table 2.6: Baseline characteristics: CFS sample in the year-five New Hope survey

Variable	(1) Treatment	(2) Control	(3) T-C
Age	29.20	28.53	0.67
	[7.34]	[6.78]	(0.60)
Female (%)	90.78	92.50	-1.72
	[28.98]	[26.39]	(2.34)
African-American, non-Hispanic (%)	57.80	53.93	3.87
	[49.48]	[49.93]	(4.19)
Hispanic (%)	28.01	28.21	-0.20
	[44.99]	[45.08]	(3.80)
White, non-Hispanic (%)	10.64	15.36	-4.72*
	[30.89]	[36.12]	(2.83)
Others (%)	3.55	2.50	1.05
	[18.53]	[15.64]	(1.45)
Never married (%)	61.35	61.43	-0.08
	[48.78]	[48.76]	(4.11)
Married living w/ spouse (%)	11.70	10.36	1.34
	[32.20]	[30.52]	(2.65)
Married living apart (%)	10.28	10.71	-0.43
	[30.43]	[30.98]	(2.59)
Separated, divorced or widowed (%)	16.67	17.50	-0.83
	[37.33]	[38.06]	(3.18)
Highschool diploma or GED (%)	50.71	46.79	3.92
	[50.08]	[49.99]	(4.22)
Highest grade completed	11.29	11.10	0.18
	[2.20]	[1.96]	(0.18)
$0 (%)	34.75	38.93	-4.18
	[47.70]	[48.85]	(4.07)
$1-999 (%)	17.38	15.36	2.02
	[37.96]	[36.12]	(3.13)
$1,000-4,999 (%)	24.47	21.79	2.68
	[43.07]	[41.35]	(3.56)
$5,000-9,999 (%)	13.48	13.21	0.26
	[34.21]	[33.93]	(2.87)
$10,000-14,999 (%)	6.74	7.50	-0.76
	[25.11]	[26.39]	(2.17)
$15,000 or more (%)	3.19	3.21	-0.02
	[17.61]	[17.67]	(1.49)

Notes: This table compares baseline characteristics of treatment and control groups. The variables were measured at baseline. The last six rows present the proportion of individuals in different groups defined by labor earnings (past 12 months). I show sample means of each variable for treatment and control group in columns 1 and 2. The third column tests the null hypothesis that the means are equal. The sample corresponds to participants in the CFS sample who responded the fifth-year survey. The first two columns present standard deviations in square brackets. In the third column, robust standard errors are in parenthesis. *,**,*** indicates significance at the 10, 5, and 1% level.

Table 2.7: Baseline characteristics: CFS sample in the year-eight New Hope survey

Variable	(1) **Treatment**	(2) **Control**	(3) **T-C**
Age	28.82	28.47	0.35
	[6.92]	[6.57]	(0.55)
Female (%)	90.57	92.33	-1.76
	[29.27]	[26.65]	(2.29)
African-American, non-Hispanic (%)	61.28	55.33	5.95
	[48.79]	[49.80]	(4.04)
Hispanic (%)	25.59	27.00	-1.41
	[43.71]	[44.47]	(3.61)
White, non-Hispanic (%)	9.43	15.67	-6.24**
	[29.27]	[36.41]	(2.71)
Others (%)	3.70	2.00	1.70
	[18.92]	[14.02]	(1.36)
Never married (%)	62.63	63.67	-1.04
	[48.46]	[48.18]	(3.96)
Married living w/ spouse (%)	10.77	10.00	0.77
	[31.06]	[30.05]	(2.50)
Married living apart (%)	9.09	10.33	-1.24
	[28.80]	[30.49]	(2.43)
Separated, divorced or widowed (%)	17.51	16.00	1.51
	[38.07]	[36.72]	(3.06)
Highschool diploma or GED (%)	47.81	45.33	2.48
	[50.04]	[49.86]	(4.09)
Highest grade completed	11.28	11.18	0.10
	[2.18]	[1.97]	(0.17)
$0 (%)	36.70	38.33	-1.63
	[48.28]	[48.70]	(3.97)
$1-999 (%)	17.17	14.33	2.84
	[37.78]	[35.10]	(2.98)
$1,000-4,999 (%)	23.23	22.67	0.57
	[42.30]	[41.94]	(3.45)
$5,000-9,999 (%)	13.13	14.33	-1.20
	[33.83]	[35.10]	(2.82)
$10,000-14,999 (%)	6.73	6.67	0.07
	[25.10]	[24.99]	(2.05)
$15,000 or more (%)	3.03	3.67	-0.64
	[17.17]	[18.83]	(1.48)

Notes: This table compares baseline characteristics of treatment and control groups. The variables were measured at baseline. The last six rows present the proportion of individuals in different groups defined by labor earnings (past 12 months). I show sample means of each variable for treatment and control group in columns 1 and 2. The third column tests the null hypothesis that the means are equal. The sample corresponds to participants in the CFS sample who responded the eighth-year survey. The first two columns present standard deviations in square brackets. In the third column, robust standard errors are in parenthesis. *,**,*** indicates significance at the 10, 5, and 1% level.

Table 2.8: Baseline characteristics: CFS sample in the year-two Teachers' survey

Variable	(1) **Treatment**	(2) **Control**	(3) **T-C**
Age	30.25	29.39	0.85
	[7.05]	[6.13]	(0.83)
Female (%)	91.20	93.70	-2.50
	[28.44]	[24.39]	(3.34)
African-American, non-Hispanic (%)	60.80	55.12	5.68
	[49.02]	[49.93]	(6.23)
Hispanic (%)	23.20	26.77	-3.57
	[42.38]	[44.45]	(5.47)
White, non-Hispanic (%)	12.80	15.75	-2.95
	[33.54]	[36.57]	(4.42)
Others (%)	3.20	2.36	0.84
	[17.67]	[15.25]	(2.08)
Never married (%)	57.60	61.42	-3.82
	[49.62]	[48.87]	(6.20)
Married living w/ spouse (%)	12.80	13.39	-0.59
	[33.54]	[34.18]	(4.27)
Married living apart (%)	12.80	7.87	4.93
	[33.54]	[27.04]	(3.84)
Separated, divorced or widowed (%)	16.80	17.32	-0.52
	[37.54]	[37.99]	(4.76)
Highschool diploma or GED (%)	50.40	47.24	3.16
	[50.20]	[50.12]	(6.32)
Highest grade completed	11.31	10.84	0.47
	[2.49]	[2.05]	(0.29)
$0 (%)	37.60	30.71	6.89
	[48.63]	[46.31]	(5.98)
$1-999 (%)	14.40	14.96	-0.56
	[35.25]	[35.81]	(4.48)
$1,000-4,999 (%)	24.80	22.05	2.75
	[43.36]	[41.62]	(5.35)
$5,000-9,999 (%)	10.40	18.90	-8.50*
	[30.65]	[39.30]	(4.44)
$10,000-14,999 (%)	8.00	8.66	-0.66
	[27.24]	[28.24]	(3.50)
$15,000 or more (%)	4.80	4.72	0.08
	[21.46]	[21.30]	(2.69)

Notes: This table compares baseline characteristics of treatment and control groups. The variables were measured at baseline. The last six rows present the proportion of individuals in different groups defined by labor earnings (past 12 months). I show sample means of each variable for treatment and control group in columns 1 and 2. The third column tests the null hypothesis that the means are equal. The sample corresponds to participants in the CFS sample with teacher survey data (year two). The first two columns present standard deviations in square brackets. In the third column, robust standard errors are in parenthesis. *,**,*** indicates significance at the 10, 5, and 1% level.

Table 2.9: Baseline characteristics: CFS sample in the year-five Teachers' survey

Variable	(1) Treatment	(2) Control	(3) T-C
Age	29.52	28.52	1.00
	[7.98]	[6.46]	(0.78)
Female (%)	90.06	91.95	-1.90
	[30.01]	[27.28]	(3.09)
African-American, non-Hispanic (%)	53.80	51.72	2.08
	[50.00]	[50.11]	(5.39)
Hispanic (%)	29.82	28.74	1.09
	[45.88]	[45.38]	(4.91)
White, non-Hispanic (%)	12.28	18.39	-6.11
	[32.92]	[38.85]	(3.88)
Others (%)	4.09	1.15	2.94*
	[19.87]	[10.69]	(1.71)
Never married (%)	61.40	63.22	-1.81
	[48.83]	[48.36]	(5.23)
Married living w/ spouse (%)	13.45	10.92	2.53
	[34.22]	[31.28]	(3.53)
Married living apart (%)	11.11	9.20	1.92
	[31.52]	[28.98]	(3.26)
Separated, divorced or widowed (%)	14.04	16.67	-2.63
	[34.84]	[37.38]	(3.89)
Highschool diploma or GED (%)	53.22	44.25	8.96*
	[50.04]	[49.81]	(5.38)
Highest grade completed	11.36	11.18	0.18
	[1.98]	[1.92]	(0.21)
$0 (%)	33.33	41.95	-8.62*
	[47.28]	[49.49]	(5.21)
$1-999 (%)	16.37	13.22	3.16
	[37.11]	[33.97]	(3.83)
$1,000-4,999 (%)	24.56	22.41	2.15
	[43.17]	[41.82]	(4.58)
$5,000-9,999 (%)	16.37	12.64	3.73
	[37.11]	[33.33]	(3.80)
$10,000-14,999 (%)	5.26	6.90	-1.63
	[22.40]	[25.41]	(2.58)
$15,000 or more (%)	4.09	2.87	1.22
	[19.87]	[16.75]	(1.98)

Notes: This table compares baseline characteristics of treatment and control groups. The variables were measured at baseline. The last six rows present the proportion of individuals in different groups defined by labor earnings (past 12 months). I show sample means of each variable for treatment and control group in columns 1 and 2. The third column tests the null hypothesis that the means are equal. The sample corresponds to participants in the CFS sample with teacher survey data (year five). The first two columns present standard deviations in square brackets. In the third column, robust standard errors are in parenthesis. *,**,*** indicates significance at the 10, 5, and 1% level.

Table 2.10: Baseline characteristics: CFS sample in the year-eight Teachers' survey

Variable	(1) **Treatment**	(2) **Control**	(3) **T-C**
Age	28.37	27.96	0.41
	[6.69]	[6.36]	(0.70)
Female (%)	92.57	93.60	-1.03
	[26.30]	[24.54]	(2.73)
African-American, non-Hispanic (%)	60.57	54.07	6.50
	[49.01]	[49.98]	(5.31)
Hispanic (%)	26.29	26.74	-0.46
	[44.14]	[44.39]	(4.75)
White, non-Hispanic (%)	10.29	16.86	-6.57*
	[30.46]	[37.55]	(3.67)
Others (%)	2.86	2.33	0.53
	[16.71]	[15.12]	(1.71)
Never married (%)	64.00	66.28	-2.28
	[48.14]	[47.41]	(5.13)
Married living w/ spouse (%)	9.71	9.30	0.41
	[29.70]	[29.13]	(3.16)
Married living apart (%)	7.43	11.63	-4.20
	[26.30]	[32.15]	(3.15)
Separated, divorced or widowed (%)	18.86	12.79	6.07
	[39.23]	[33.50]	(3.92)
Highschool diploma or GED (%)	50.29	47.67	2.61
	[50.14]	[50.09]	(5.38)
Highest grade completed	11.27	11.26	0.02
	[2.46]	[1.98]	(0.24)
$0 (%)	35.43	38.37	-2.94
	[47.97]	[48.77]	(5.19)
$1-999 (%)	13.71	15.70	-1.98
	[34.50]	[36.48]	(3.81)
$1,000-4,999 (%)	24.57	20.93	3.64
	[43.17]	[40.80]	(4.51)
$5,000-9,999 (%)	14.86	15.70	-0.84
	[35.67]	[36.48]	(3.87)
$10,000-14,999 (%)	6.86	6.40	0.46
	[25.34]	[24.54]	(2.68)
$15,000 or more (%)	4.57	2.91	1.66
	[20.95]	[16.85]	(2.04)

Notes: This table compares baseline characteristics of treatment and control groups. The variables were measured at baseline. The last six rows present the proportion of individuals in different groups defined by labor earnings (past 12 months). I show sample means of each variable for treatment and control group in columns 1 and 2. The third column tests the null hypothesis that the means are equal. The sample corresponds to participants in the CFS sample with teacher survey data (year eight). The first two columns present standard deviations in square brackets. In the third column, robust standard errors are in parenthesis. *,**,*** indicates significance at the 10, 5, and 1% level.

2.3 Variables

In this section, I define the variables used in Chapters 3 and 4. I start off by describing the construction of exogenous characteristics (that is, those that are not part of the choice set in the theoretical framework underlying Chapters 3 and 4) and then I continue with choices and outcomes.[4]

2.3.1 Exogenous characteristics

The set of exogenous characteristics used in the empirical analysis are grouped into family composition and demographic variables. To approximate family composition, I consider two variables: marital status and the number of children.[5] For marital status, I use self-reported data for baseline and surveys from year two, five, and eight to construct a marriage dummy (1 for married or cohabitating and 0 otherwise) for these years. To define the number of children at each of those periods, I use baseline survey to recover the information on the number of children. For the rest of the years, New Hope surveys asked if the participant had a child in each year. I assume that if a child is born, it stays in the household throughout time and use the responses of the New Hope survey to define the number of children for each period.

Finally, from the baseline survey, I recover participant's demographic information. I observe participant's age, gender, occupational status, education (a high school dummy and the highest grade completed) and last year's earnings.

[4]Part of the set of exogenous characteristics includes marital status and fertility. In this dissertation, I do not analyze marriage and fertility decisions and instead focus on labor market outcomes and child care choices.

[5]in Chapter 4, I use marital status and number of children to determine eligibility and subsidy levels of mean-tested programs.

2.3.2 Child care

I define the child care variable using the New Hope surveys. In the second-year survey, individuals were asked about all regular child care arrangement for the past two years. Possible child care arrangements were (i) Head Start; (ii) preschool, nursery school, or a child care center other than Head Start; (iii) school-based extended day program; (iv) another child care other than in someone's home; (v) a person other than a member of the household; (vi) another member of the family of household; and (vii) no arrangements. Participants reported the number of months spent in each case (except for number (vii)). I consider a formal child care arrangement categories (i)-(iv), and an informal child care the rest of them. I define a child care dummy that takes the value of 1 if the child (as declared by the parent) spent the maximum number of months in categories (i)-(iv), and 0 otherwise. Using this information, I obtain child care choices for the period corresponding to one year after baseline.

I follow a similar procedure to recover child care choices at the fifth year. In this year, the child care options are: (i) by someone 16 years of age or younger; (ii) by an adult at home; (iii) by an adult in someone else's home; (iv) in a child care center, before or after school program, community center, or Head Start; (v) child's own supervision; (vi) by sibling; (vii) others. I define the child care dummy if the child spent the maximum number of months in option (iv).

2.3.3 Income

I compute a proxy for family income using administrative information collecting different sources of income. First, labor earnings come from the UI system of Wisconsin. UI earnings are available from 1993 until 2003. I define annual earnings as the sum of the quarterly UI records of a year.

Second, I consider income coming from the New Hope program. New Hope has two types of cash transfers: the income supplement and money from CSJs. These sources of income are available only for New Hope participants, up until three years after baseline. As UI earnings, I define annual payments.

Third, using data from annual labor earnings, I compute simulated payments from the Earned Income Tax Credit (EITC). To this end, I consider labor earnings as the sum of UI earnings and New Hope CSJs payments.[6] I compute EITC payments assuming full compliance and following the state and federal formulas for each year. This is the first paper on New Hope that includes the EITC using this procedure. Instead, the literature uses an approximate measure of the EITC. This measure was computed based on sub-group EITC total payments coming from administrative sources.[7] Because there is no information on how these subgroups were defined, I do not include this approximated EITC payments used in previous studies.[8]

Finally, I use two sources of welfare payments: AFDC and Food Stamps. The MDRC had access to the State of Wisconsin data on AFDC (which was replaced by "Wisconsin Works" after TANF) and on the money value of Food Stamp. I use these two sources of income to construct annual welfare payments.

Using all of the sources above in annual terms, I define household income for individual i at year t as follows:

$$I_{it} = E_{it} + EITC_{it} + D_i(Sup_{it} + CSJ_{it}) + W_{it}, \tag{2.1}$$

where E_{it} are labor earnings, $EITC_{it}$ is the earned income tax credit, D_i the treatment group dummy, Sup_{it} is the New Hope income supplement, CSJ_{it} are earnings from CSJs,

[6]The income of the New Hope CSJs does not show up in the UI records. The CSJs that New Hope offered were limited in time (no longer than 6 months), and so they were not eligible for unemployment insurance.

[7]This information was provided by the Wisconsin State Department of Taxation to the MDRC.

[8]See Bos et al. (1999), Huston et al. (2003), and Miller et al. (2008) for evidence on the impacts on income using the approximate measure of EITC.

and W_{it} are welfare payments (the sum of Food Stamps money value and AFDC payments). Finally, for all periods, I express income and its subcomponents in 2003 dollars.[9]

Family income as defined in equation (2.1) leaves out various others sources of income. Some of these excluded sources are the unemployment insurance, child support, and further payments from social programs. Furthermore, equation (2.1) does not consider income from other family members. The New Hope surveys collect these and others sources of income. Unfortunately, the New Hope surveys do not track income for every year. Additionally, the year-two survey only asks about "last month's income," thus income from administrative sources and surveys cannot be directly compared.

2.3.4 Labor supply

To compute measures of labor supply, I combine data from New Hope surveys and administrative records. I compute two types of measures: an employment dummy and the number of hours worked per week.

I recover employment information exploiting data from the UI records and the New Hope client database containing earnings in New Hope CSJs. For the analysis of Chapter 3, I compute a quarterly measure of employment, which takes the value of 1 if there is a positive wage in the UI or CSJs records in a given quarter, and 0 otherwise.

I define individual hours worked from the New Hope surveys. Using the second-year survey, I compute the average hours worked in a week for the baseline year and one year after. In this survey, individuals reported the "usual hours worked" in every job they had in the last two years. For every job they had, respondents reported weekly hours worked at the beginning and at the end of the job. Using the reported dates for each job spell, I compute monthly averages of hours worked per week. If more than one job was reported in a particular month, I assume that no overlapping in the spells and take the average of all jobs. If the individual did not report having a job in a particular month, I set hours

[9] I use the CPI index for the Midwest-urban area from the Bureau of Labor Statistics.

worked to zero. Then, for each calendar year, I compute the annual average of weekly hours worked—including the zeros corresponding to the months that the individual did not work. From the fifth- and eighth-year surveys, I recover hours worked for the fourth and seventh year after baseline. In these surveys, individuals reported the average hours worked at the current or most recent job in the last 12 months. I weight the reported average hours worked with a variable capturing the proportion of quarters employed in a year. I compute this variable using administrative data from the UI database and calculating the proportion that individuals stayed employed in year ($4^{-1}\sum \mathbf{1}\{wage_q > 0\}$, where $wage_q$ is quarterly labor earnings and $\mathbf{1}\{.\}$ is the indicator function).

Chapter 4 uses a measure of hourly wage to estimate the underlying wage offer process. To construct this variable, I combine administrative with survey data to compute weekly average gross earnings (in the numerator) and weakly average hours worked (in the denominator). I obtain weekly average gross earnings by averaging quarterly earnings in a particular year (from the UI data) with any salary earned in a CSJ (for those in the treatment group), adjusted to 2003 dollars. I divide weekly earnings by average hours worked in a week from survey data (see paragraph above). Because hours worked are available for periods $t = 0, 1, 4$ and 7 years after baseline, so is the hourly wage. For $t = 4$ and $t = 7$, the state CSJs from TANF enter the pool of possible wage offers. Thus, I incorporate the CSJs payments in the hourly wage calculation of $t = 4$ and $t = 7$.

2.3.5 Child outcomes

I observe data on child academic outcomes from the teachers' surveys. In this thesis, I use two measures: the academic subscale of the Social Skills Rating System (SSRS) and the Classroom Behavior Scale. I use these two measures for Chapter 3 and the first one for Chapter 4.

In the SSRS academic subscale (Gresham and Elliot, 1990), the teacher ranks the child in several subjects. These are reading skills, math, intellectual functioning, motivation, oral communication, classroom behavior, and parental encouragement. Each variable takes the following values: 1 (bottom 10%), 2 (next lowest 20%), 3 (middle 40%), 4 (next highest 20%), and 5 (highest 10%).

The Classroom Behavior Scale is a shorter version of School Adjustment Scale (Wright and Huston, 1995) and focuses on how the child behaves in class. The teacher must answer several questions on three topics: (i) behavior skills (for example, "complies with teacher requests, behaves so as not to disturb peers?"), (ii) independent skills (for example, "remains on-task with minimal supervision, manages free time constructively?"), and (iii) o transition skills (for example, "recognizes transition cues and stops ongoing behavior, moves quickly to next activity?"). All variables take discrete values from 1 (almost never) to 5 (almost always).

Chapter 3

Effects of a Welfare Experiment on Household Behavior and Child Outcomes for Families with Young Children

3.1 Introduction

How should families respond to income subsidies? As they were originally intended, income subsidies—the EITC being the prominent example for the U.S.—are usually implemented to induce families to supply more working hours. However, families with young children might be reluctant to comply: as they spend more time in the market they need to substitute parental child care for other forms (usually center-based). Not having a adequate child care substitute available may override the labor supply incentive provided by the policy.

This chapter estimates the effects of a work-based, anti-poverty program on household behavior families with young children. To this end, I use experimental data from New Hope. In the program, a randomly selected group of applicants had access to an income and child care subsidy tied to working requirements for three years. I present novel results on the effects of New Hope household behavior and child outcomes for families who had children under the age of seven while they were eligible to receive the New Hope benefits. I find that, even though the program induces parents to spend more time in the market and less time at home, effects on children were positive. Moreover, effects on labor supply were stronger for families with young children and the effects of New Hope on measures of child academic achievement for this group were twice as large as those among the older group of children. As New Hope resembles policies that were implemented later on (namely, the Child Care Development Fund and changes in the EITC schedule), my results suggest that welfare policies that encourage work can be beneficial for both adults and young children—in spite of the fewer hours adults spend at home.

New Hope was conceived as a set of policies that could help families alleviate their financial needs by encouraging work (Brock et al., 1997). Those who were assigned to the treatment group had access to two policies: an income and a child care subsidy. The income subsidy resembled the EITC: the subsidy increases with earnings, it reaches a flat region, and then it fades out. The child care subsidy took the form of a child care voucher which families used to take their children to private child care. To have access to any of these benefits, the participant must have worked at least 30 hours a week on average, each month. Data on child outcomes and household choices were collected at different points in time. The New Hope literature reports positive effects on income, labor supply, and child care use for all family types while New Hope was available (Bos et al., 1999).

I show novel effects on household behavior and child outcomes for families who had young children (six years old or less) while they were eligible for New Hope. I document heterogeneous impacts of New Hope on three household variables: child care use, income, and labor supply. First, I find that effects on child care use were substantial for children under the age of seven. For this group, New Hope increased child care use by 22 percentage points from a baseline of 40%. In contrast, child care probability for older children (which takes the form of after-school care) raised by 9 percentage points from a baseline of 24%. Second, New Hope increased income by a similar amount for both types of families (about $900, a 6% increase). However, the estimated effect on household income is a result of different impacts on income sources; New Hope had a larger effect on earnings for families with young children. Finally, QTE estimates on hours worked suggest that this larger effect on earnings for parents of young children is potentially due to bigger effects on hours worked on the intensive margin.

I show that New Hope had varied impacts for children of different ages. For children who were six years of age or less by two years after random assignment, effects on academic achievement were almost twice as large (depending on the measure) as the estimated impacts for older children. For example, on a rank measure of overall child academic performance in the classroom, New Hope boosted the probability of being in the top 30% of the class by 16 percentage points for young children (statistically significant at the 5% level). For older children, the same effect is estimated only at 4 percentage points.

Finally, I document possible mechanisms by which New Hope generates heterogeneous effects on different family types. By using a dynamic-discrete choice model (see Chapter 4), I find that a program without a child care subsidy generates effects on families with young children that are quantitatively similar to those of families with old children. This result suggests that the presence of high-quality center-based child care for young children plays a role in explaining the effects of New Hope on different family types.

The rest of this chapter is structured as follows. Section 3.2 introduces the main features of the New Hope intervention. Section 3.3 presents the main results and Section 3.4 concludes

3.2 The New Hope welfare model and context

Inspired by the welfare debate that dominated the policy agenda in the 90s, New Hope was designed to promote the transition from welfare to work. As a result, the program deepened the incentives to work that families were subjected to at the time the program was implemented.[1]

The participants—recruited in two economically disadvantaged neighborhoods in Milwaukee, Wisconsin—had to meet a few conditions to enter the program and have access to

[1] Later on, many of the policy changes in the U.S. were similar to the New Hope package. See for example Moffitt (2003).

the benefits package.[2,3] To be eligible, individuals had to be at least 18 years old and have a household income equal to or less than 150% of the federal poverty line.[4] Additionally, applicants had to be willing to work at least 30 hours per week (which was considered full-time employment for the purposes of accessing the program's benefits).[5] Beginning at baseline recruitment and lasting for 36 months, a randomly selected group of applicants had access to various benefits. To receive any of the subsidies of the program in a given month, the participants had to prove that they had worked at least 30 hours a week on average. Each 5th day of the month, New Hope agents enforced this requirement by asking for the last month's wage stubs. After reviewing those wage stubs, New Hope representatives would determine the amount of supplement that the participants were to receive. This process usually lasted 15 days. After this period, the participant would receive the payment by approximately the 20th of the same month.

Table 3.1 compares the New Hope benefits to the public system's welfare services. The table illustrates the actual New Hope "treatment:" the benefits that were given to participants compared to what the control group had access to. New Hope had three main advantages: it gave an income supplement that was larger than the EITC schedule, it increased the affordable child care supply for low-income working families, and it lowered health care costs.

[2]Participants came from the north and south side of the U.S. Highway 94 and the Menomenee River Valley. The New Hope team selected those neighborhoods (which were defined by their postal zip codes) because they had a relatively high poverty rate and ethnically diverse populations. Each area had about 40,000 residents (Bos et al., 1999).

[3]New Hope was heavily promoted during the eligibility period. The New Hope team advertised the program in posters, radio, TV, and newspapers, and sent personal letters. About 20% of potential participants in the target areas became aware of the program (Brock et al., 1997).

[4]For a household with one adult and two children, the federal poverty threshold was $12,278. For a single-person household, the threshold was $7,929 (Bos et al., 1999).

[5]Individuals applied to the program during a period of buoyant economic activity. Between 1992 and 1997, job creation at the Milwaukee Primary Metropolitan Statistical Area (which covers the Milwaukee, Washington, Ozaukee, and Waukesha counties) grew by 8.2%. For the same area, the unemployment rate diminished from 4.8% in 1992 to 3.6% in 1997 (Bos et al., 1999).

Table 3.1: New Hope versus Wisconsin's social assistance

Components	**New Hope (treatment group)**	**Wisconsin's public services (control group)**	**New Hope's value-added**
Cash assistance	Income supplement: wage subsidy + child allowance.	Earned Income Tax Credit.	Increase disposable income (earnings plus cash assistance) up to 200% depending on the level of annual earnings.
CSJs	New Hope assigned unemployed participants to temporary CSJs.	CSJs available for welfare recipients.	The New Hope CSJs were paid, and it qualified for hours worked to receive New Hope benefits.
Child care	Child care subsidy with a low copayment.	Child care subsidies to welfare receipients and for families in transition out of welfare. Head start was available as well.	Limited supply of public child care slots. In practice, NH increased supply of affordable child care.
Health insurance	Health plans with low copayment through local HMOs.	Medicaid, employer-funded plans.	New Hope complemented employer plans. Also available for families not in AFDC.

Notes: This table summarizes the main components of New Hope. It compares the New Hope benefits with equivalent services available in Wisconsin.

In this paper, I focus on two elements of the New Hope package: the income supplement and the child care subsidy. Next, I describe these two components and leave the description of the other components of New Hope for Appendix 3.5.1.

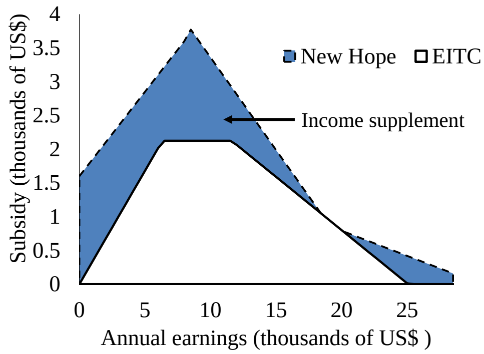
3.2.1 The income supplement

Figure 3.1, panel (a), illustrates the income supplement design for a family with one earner and one child.[6] To show how the schedule looks across the distribution of ex-ante labor earnings, the figure assumes no work requirements.[7] The New Hope income supplement

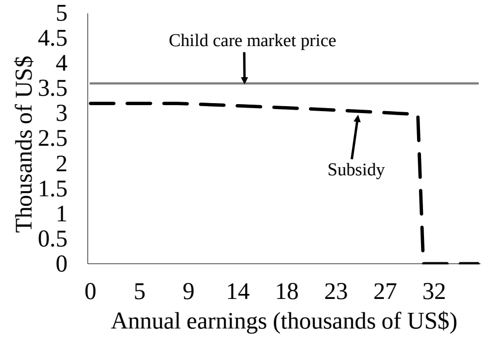
[6]The income supplement corresponds to the sum of two subsidies: an earnings subsidy and a child allowance. Appendix 3.5.1 provides the exact formula of the subsidy.

[7]Even though Figure 3.1 depicts the income supplement in terms of annual benefits, New Hope beneficiaries received their supplements on a monthly basis. The income supplement was not taxable.

Figure 3.1: New Hope income supplement and child care subsidy



(a) Income supplement



(b) Child care subsidy

Notes: Panel (a) compares the New Hope and EITC design as a function of annual earnings. The solid line shows the EITC income supplement. The difference between the dashed and the solid line represents the New Hope supplement, for each level of earnings, assuming no work requirement. Panel (b) illustrates the child care subsidy design. For this figure, the child care cost equals 3,600 dollars a year, and it is indicated in the solid line. The dashed line represents the subsidy level.

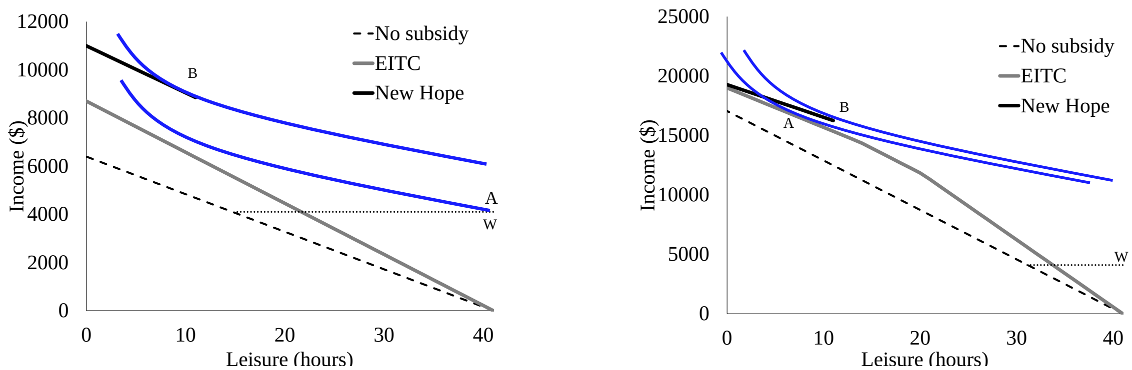
complements the EITC subsidy. In the figure, the income supplement is represented as the difference between the dashed and solid lines—that is, the New Hope subsidy is positive for a worker as long as the New Hope schedule stays above of that of the EITC (and meets the work requirement).[8,9]

To evaluate the economic incentives introduced by the program, consider two individuals, X and Y, choosing between home and labor market time (for simplicity, suppose that they do not have children). Compared to those in the control group, individuals in the treatment group had different incentives to work depending on their wage offer. The choices made by X and Y are illustrated in Figure 3.2, panels (a) and (b). In these graphs, the horizontal and vertical axis show income and time outside the labor market, respectively. Both figures present the individual's budget set under three different cases: without EITC or New Hope ("No subsidy"), the control group ("EITC"), and treatment group ("New Hope"). Since

[8]The dashed line shows a discontinuity at $19,000 because the earnings subsidy is zero at that point while the child allowance continues to phase out.

[9]Because the income supplement schedule stayed fixed whereas the EITC schedule expanded while the program was running, the treatment "intensity" varied in time. In graphic terms, the dashed line in Figure 3.1, panel (a), remained constant, whereas the solid line shifted upwards alongside the changing EITC regulations. These modifications in the EITC meant that the treatment group received lower levels of the income supplement in time.

Figure 3.2: Intensive- and extensive-margin responses to New Hope



(a) Individual X (low wage offer)

(b) Individual Y (high wage offer)

Notes: The figure illustrates extensive- and intensive-margin responses to New Hope. For individuals with two different wage rates and same structure of preferences, it presents individuals choices under different budget sets in the income-leisure plane.

New Hope requires working 30 hours or more, the New Hope budget set ends at the point of 10 hours of leisure. Additionally, the figure shows what both individuals would earn if they do not work (at point W).[10] X and Y have the same preference towards income and leisure, and so both have an equal set of indifference curves in the income-leisure plane. The only difference between the budget sets of X and Y is that the wage offer of X is lower than that of Y.

All else constant, the figure indicates that the impact of the program on labor supply depends on the wage offer. Without New Hope (if X and Y were in the control group), individuals would allocate at point A. At this point, X would not meet the 30 hours condition whereas Y would work over this threshold. In fact, the wage offer of X is low enough so that she is better off receiving welfare and not working at all. If X and Y were in the treatment group, they would choose to allocate at point B. Compared to point A, X would work more hours and receive more income. Y would earn more as well. However, Y works more or less compared to the scenario of not having the program, depending on the relative magnitudes of income and substitution effects. Figures 3.2a and 3.2b illustrate one of many situations in which the income supplement impacts labor supply and income. Overall, the New Hope

[10] I assume this value to be 4,100 dollars, which equals the sum of the average values the control group received one year after baseline from AFDC and Food Stamps (Bos et al., 1999)

income subsidy should have a non-negative effect on income and an ambiguous effect on hours worked.[11]

Because the income supplement affects behavior, it can also produce effects on child outcomes. Suppose that labor supply causes a negative effect on child human capital, while income causes a positive effect (Bernal, 2008; Dahl and Lochner, 2012).[12] The effect of the program on individual X's child is ambiguous; X has more income but works more. The impact on individual Y's child is also ambiguous. If Y works more, then she would be in the same situation as X: more income but fewer hours at home. If Y works less then we can guarantee a positive effect on children, as the individual spends more time at home and has more income. Overall, the impact of the New Hope income subsidy on children depends on the relative strength of intensive- and extensive-margin labor supply responses and the relative productivity of income and time with the child in the production function of child skills.

3.2.2 The child care subsidy

Figure 3.1 (panel b) depicts the child care subsidy schedule for the case of a single-child household paying $3,600 a year for child care. Thanks to the subsidy, families paid a relatively small copayment (shown in the figure as the difference between the solid and dashed lines). Up to $8,500 of annual family earnings, this family starts paying $400. After the $8,500 point, the copayment increases by 1% for every extra dollar of family earnings. The household stops receiving a subsidy at the point where family earnings reach 200% of the federal poverty line or $30,000, whichever comes first.[13] A child care subsidy can be used by both preschoolers and children up to 13 years old. In the case of school-age children, the child care subsidy covers "extended-day programs", that is, after-school care at the child's school or at another

[11]Given a different utility function, individual X may choose to stay at point W even in the New Hope situation. Therefore, for some individuals, the program has no impact whatsoever.

[12]In Chapter 4, I incorporate the quality of the child care center as an additional input in the production function of early childhood human capital.

[13]Among those who use the child care benefit, the total average cost of child care expenditures was $9,000 a month, or 74% of the average annual income of the control group at baseline.

center. Both for preschoolers and school-age children, child care centers must be licensed by the state of Wisconsin.

According to Brock et al. (1997), economically disadvantaged families had access to a number of child care programs offered by the Milwaukee's welfare department—with reimbursement rates and subsidy limits that were similar to the New Hope design.[14] However, families in the New Hope program had some clear advantages over families using the public system. First, participation in New Hope increased their chances of finding low-cost child care services. Parents in the public system under AFDC, for example, usually faced long waiting lists to apply for public child care subsidies. For families who were not in the welfare system, finding a low-cost child care provider was even harder (for example, obtaining a Head Start slot was almost impossible).[15] In contrast, New Hope beneficiaries had the possibility of enrolling their children in any of the county- or state-licensed child care centers available in the city. Second, qualitative evidence indicates that families who were eligible for these expanded public child care programs struggled to comprehend and navigate Wisconsin's complex system.[16] The qualitative evidence suggests that families under New Hope benefited from a simpler and more easily understood system, since New Hope gathered all subsidies into one single program.[17]

A child care subsidy can produce various behavioral changes within the household. Take the case of two individuals ("A" and "B") who would work 30 hours or more with and without the program. Without New Hope, "A" would pay for a child care service while

[14] Starting 1997, the CCDF enhanced the low-cost child care supply. Furthermore, the State of Wisconsin supplemented the federal funds from the CCDF to make the child care subsidies available to all eligible families. As a result, the public system began to offer a very similar service to that of New Hope, making the relative gain of the latter system much smaller after 1997. (Wisconsin's TANF program created a child care assistance program named "Wisconsin Shares." This program was implemented in September of 1997, and it provided child care vouchers that could be used at any of the licensed child care providers in the county.)

[15] 54% of the New Hope full sample were not under AFDC (Bos et al., 1999).

[16] Individuals had to be aware of the different child care assistance programs for which they would be eligible as their situation changed. For example, if a family had left AFDC, then they would have had to apply to a child care for working parents. If they had become unemployed and fell under AFDC again, they would have had to redo all paperwork to receive the child care assistance from the public system (Bos et al., 1999; Blau, 2003).

[17] Moreover, families could reach out to New Hope representatives whenever they had questions regarding their benefits, or if they could not find suitable child care facilities in the city.

"B" would not. For individual "A", the program only raises her disposable income while for "B" there is an incentive to take up the subsidy to use the child care option. Now consider another individual ("C") who, without New Hope, would work less than 30 hours and not use center-based child care. If she would like to use child care under New Hope, she would have to work more than 30 hours. For this individual, the economic incentive provided by the income supplement may induce her to do so.

The child care subsidy may affect child human capital for these three individuals through different mechanisms. Suppose that, relative to home care, child care has a positive impact on child human capital. Even though there is no effect on child care take-up for individual A, her child would benefit from the child care subsidy because A has more income. In contrast, individual B's child would benefit from the center-based child care if B chooses this option. One could find a negative effect of the child care subsidy in the case of individual C. Because she has to work full time in order to take up the child care subsidy, the impact on child human capital depends on the productivity of child care relative to that of labor supply in the human capital technology. Therefore, as with the income supplement, the theory does not give a clear prediction on the sign of the effect of the child care subsidy on child outcomes.

3.3 Results

The goal of New Hope was to test whether a new set of welfare policies could help families in overcoming poverty by encouraging work. Because the program was expected to change household behavior in many dimensions, effects on children skills development were expected as well. In this section, I review the documented average effects of the program on household behavior and present new evidence on heterogeneous effects on the household and child outcomes.

In what follows, I compare treatment effects for two distinct samples: participants who had at least one child under the age of seven by two years after random assignment and those who do not (all participants had at least one child at baseline). Chapter 2 presents a detailed analysis of available databases and variables. It also presents evidence suggesting that attrition does not severely compromise the balance of observed and unobserved characteristics between treatment and control groups.

3.3.1 Treatment effects on household behavior

Let $D_i \in \{0,1\}$ denote the random assignment indicator for individual i (1 for being in the treatment group and 0 otherwise). To test changes in household behavior, I estimate the following equation:

$$Z_{it} = \alpha_t^z + \gamma_t^z D_i + \nu_{i,t}^z, \qquad Z_{it} \in \{I, h_{it}, cc_t\} \tag{3.1}$$

where $I_{it,}$, h_{it}, and cc_t denote family income, parental labor supply, and center-based child care use.

Child care use. Table 3.2 shows the estimated effects of the New Hope intervention on child care probability two years after baseline (coefficient γ^{CC} in equation 3.1). In these regressions, the unit of observation is a child. The outcome variable is a dummy variable (CC_{it}) that indicates center-based child care use one year after baseline. Specifically, CC_{it} equals 1 if child i was enrolled in a center-based child care (Head Start, preschool, nursery school, or another child care other than someone's home), and 0 otherwise (home care). For school-age children, the dummy equals 1 if the child attends after-school care. I estimate three regressions: for the overall sample, for young (six years of age or less by two years after baseline) and older children. From a baseline of 33% (the control group average for the same period), the program increased child care enrollment by 15 percentage points.[18] The impact is largely explained by the effects on young children. Within this group, families increased the use of center-based child care by 22 percentage points (from a baseline of 40%). As I show in Chapter 4, the large effect on child care use plays a key role in explaining the effects of New Hope on young children.[19]

Table 3.2: The effects of New Hope on child care use

Sample	Control	Treatment	Impact
Young (0-6 years old)	39.7	61.7	22.0***
			(4.9)
Old (>6 years old)	24.7	33.7	9.0*
			(4.8)
Overall	32.7	48.0	15.3***
			(3.5)

Notes: This table shows estimates of γ (in percentage points) by the age of the child at baseline and gender of the following regression: $CC_{it} = \alpha + \gamma RA_i + \varepsilon_i$. The sample comes from the New Hope year-two survey (CFS). $CC_{it} = 1$ if the child is in child care (Head Start, preschool, nursery school, other center-based other than someone's home), 0 otherwise. *,**,*** indicates significance at the 10, 5, and 1% level.

Income. Table 3.3 presents the effects of New Hope on income. I show the results of three different regressions. Since the goal is to estimate effects on available income—not on potential income for all individuals—, the three regressions include "zeros" (5% of the sample). First, I show OLS estimates of income (on levels) on the random assignment indicator. The second regression uses the following transformation on the income variable (I_t): $ln(I_t + 1)$. The third specification corresponds to a median regression. All of the estimated effects are averages for the New Hope period. The first regression model shows that the effect on the overall CFS sample is close to $1,000, with minor differences among participants with young and old children. The OLS regression on the log of income + 1

[18]Previous evidence can be compared to the overall estimate I report in Table 3.2. Numbers are qualitatively similar compared to that those documented in the New Hope reports (Bos et al., 1999).

[19]Bos et al. (1999) show that 46.7% of the participants in the CFS sample (that is, parents with at least one child of 13 years of age or less) used the child care subsidy at least once. This information comes from the administrative database. The take-up administrative data contains detailed information about the subsidy use. Unfortunately, this data is not available in New Hope's public database.

shows a .36 effect overall, explained by an increase of .52 and .26 for families with young and old children. Effects on the 50th percentile reveal higher impacts (of $1,200 dollars for the overall sample) than those from the first regression. Moreover, estimated impacts on the 50th percentile are twice as large for the old relative to the young sample. The difference in the estimated effects of the first OLS regression with the log and median models might be accounted by heterogeneous impacts of New Hope along a highly skewed income distribution.

Table 3.3: The effects of New Hope on income

Estimate	Young	Old	Overall
OLS (income/1000)	0.929*	0.898	0.979**
	(0.474)	(0.693)	(0.388)
OLS (log of (income + 1))	0.262**	0.520***	0.359***
	(0.125)	(0.172)	(0.100)
Median regression (income/1000)	0.891	1.954**	1.262**
	(0.686)	(0.946)	(0.572)
Baseline (income/1000)	14.415	13.850	14.011

Notes: The table shows the impact of New Hope on income. The first row shows the results from a regression of income (in thousands of dollars) on the treatment dummy. The second rows uses as a dependent variable log(income + 1). The third row presents the results from a median regression. I run these regressions for participants with at least one child less than six years old ("young"), the rest of participants ("old"), and for the overall sample. Standard errors are in parenthesis. *,**,*** indicates significance at the 10, 5, and 1% level.

Table 3.4 decomposes the average effects of income into changes in its components. I divide the sources of income into labor earnings, welfare (AFDC payments and SNAP), the EITC, and the New Hope income subsidy. For participants with young children, the most important factor that explains the effects on income is the impact on earnings. For this sample, New Hope increased earnings by $650 dollars, explaining 70% of the increase in income. On the contrary, for those with older children, the most important component is the New Hope subsidy: treated participants received $455 of extra cash, explaining 51% of the rise in income. Effects on overall income would have been larger for adults with young children than for those with older children if it was not for the associated decrease in welfare; New Hope reduced welfare cash by $365 and $100 for parents of young and old children.[20]

Table 3.4: The effect of New Hope on income sources

	Young	Old	Overall
Earnings	0.650	0.322	0.640
	[69.93%]	[35.82%]	[65.42%]
Welfare	-0.365	-0.101	-0.336
	[-39.32%]	[-11.22%]	[-34.36%]
EITC	0.172	0.222	0.209
	[18.51%]	[24.69%]	[21.30%]
New Hope	0.473	0.455	0.466
	[50.88%]	[50.70%]	[47.64%]
Total	0.929	0.898	0.979
	[100%]	[100%]	[100%]

Notes: I show effects of the program on the different sources of income. In square brackets I show the share of each estimated effect in the impact on total income (last row).

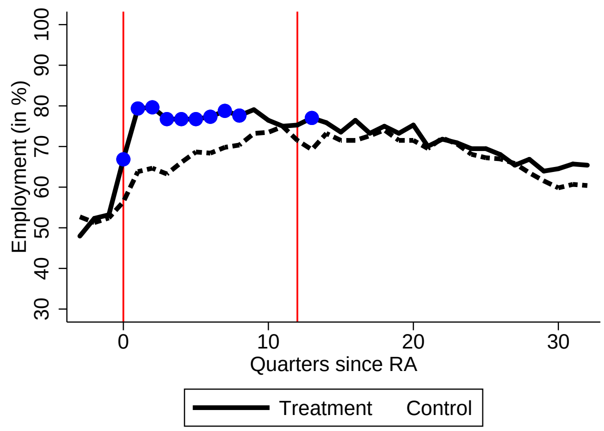
Labor supply. Figure 3.3 shows the effect of New Hope on employment probability. In the figure, an individual is employed if she had a positive UI or CSJ income in a particular quarter. Panel (a) investigates effects for the whole CFS sample of adults. The figure reveals a positive effect on employment probability while New Hope was in effect. During the eligibility period, New Hope raised employment probability by 9 percentage points on average (from a baseline average of 68%). After the program ended, the impacts are statistically insignificant.[21] Panels (b) and (c) present heterogeneous dynamics in the estimated effects by family types. While for participants with old children the estimated effects tend to remain in time (at least before New Hope ends), for adults with young children the impact on employment quickly fades out (even before New Hope ended). On average, the impact

[20]The decomposition of treatment effects sheds lights on the mechanisms that explain the program's effects on child outcomes. If impacts on child outcomes are solely due to the New Hope cash transfer, then we suspect that the effects on child outcomes are explained by this additional cash. If we also see effects on earnings—and so labor supply plays a role—then drawing such conclusions is not so straightforward, as the additional income comes at expense of parents spending less time at home.

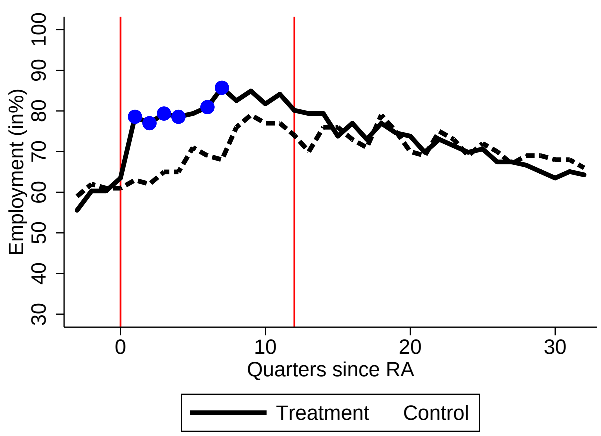
[21]This evidence is consistent with results from the New Hope reports (Bos et al., 1999; Huston et al., 2003; Miller et al., 2008).

for adults with young and old children are 8 and 10 percentage points while New Hope was available (from baseline averages of 67 and 69%, respectively). Hence, on the extensive margin, effects on labor supply are slightly stronger for families with old children.

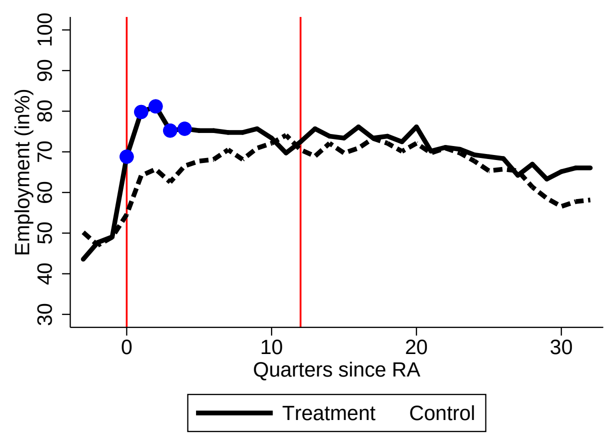
Figure 3.3: The impact of New Hope on employment probability (administrative records)



(a) Overall



(b) With old children



(c) With young children

Notes: The figure shows the effect of New Hope on employment probability by quarters since random assignment. The outcome variable is a dummy variable indicating a positive UI earnings record or CSJ wage. The solid vertical lines indicate the quarter where New Hope ends. I estimate $Employment_{it} = \alpha_t + \beta_t RA_i + \varepsilon_{it}$, where RA_i equals 1 if family i is in the treatment group and 0 otherwise. For each year t, I indicate with a solid circle (●) if the estimate of β_t is significant at the 5% level.

To provide indirect evidence on intensive- and extensive-margin labor supply effects, I follow Bitler et al. (2006) and compute Quantile Treatment Effects (QTE) on hours worked.[22]

[22]Heterogeneous effects on hours along its distribution can be expected given the varied shocks to marginal tax rates implied by the New Hope schedule (see Figure 3.1).

Figure 3.4 presents the results from this exercise. In this figure, the dependent variable is the monthly average of hours worked in a week. I use monthly data from baseline up to 15 months after random assignment. Figure 3.4, panel (a), plots the estimated QTE on the overall sample. It shows statistically significant effects from the 50th to the 80th quantiles, ranging from 5 to 15 hours. Unlike the Jobs First experiment (Bitler et al., 2006), I do not find negative effects on the upper quantiles.[23] Panels (b) and (c) compare the estimated QTE for participants with old and young children. Effects are generally bigger for parents of young children; on average, treatment effects on hours worked for families with young children surpass equivalent estimates for the rest of the families by 4 hours a week. Therefore, the larger rise in earnings of families with young children compared to those with old children (Table 3.4)—in spite of a slightly lower effect on employment (Figure 3.3)—is most likely explained by the superior effects on hours worked (conditional on working in the absence of New Hope).

3.3.2 Treatment effects on child outcomes

The induced changes in household behavior suggest that the program may have influenced child skills development as well. In this section, I estimate the impact of New Hope on measures of academic performance. I present effects by child age. All of these estimates pertain to effects on children by two years after random assignment.[24]

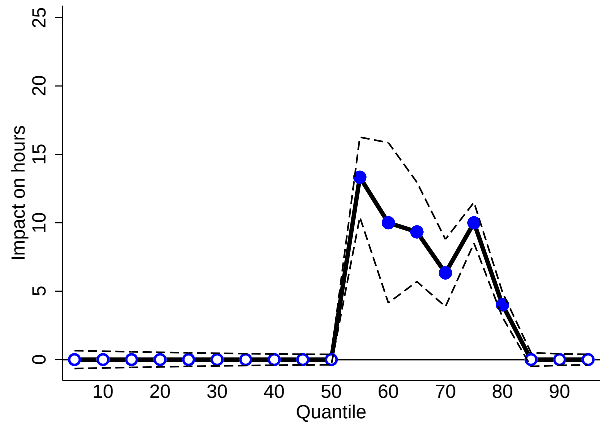
The basic regression follows:

$$Y_{it} = \alpha D_i + X_i'\beta + \varepsilon_{it}, \tag{3.2}$$

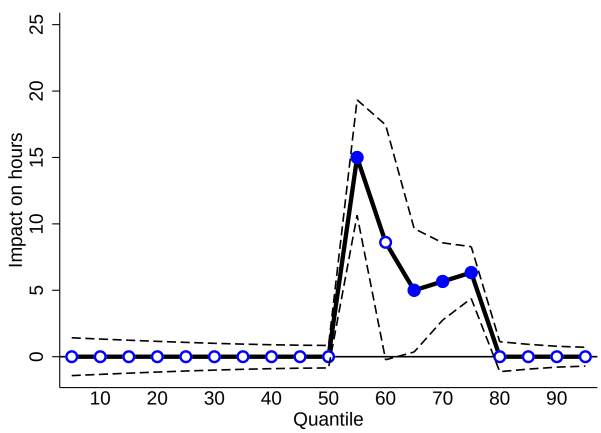
[23]These dissimilar results may be accounted by the different schedules of the New Hope and Job First experiments. In the Job First experiment, benefits end abruptly if earnings pass the federal poverty line—a 100% marginal tax rate. In the New Hope program, the marginal tax rate is far below 100% (Bos et al., 1999).

[24]The literature has well established fading-out impacts after New Hope ended on the measures I present in this paper. See Huston et al. (2003) and Miller et al. (2008).

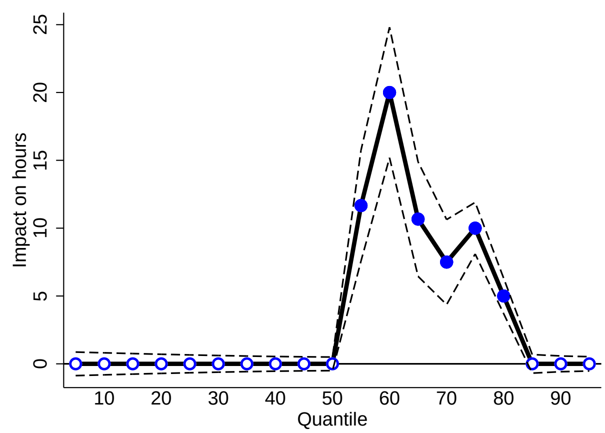
Figure 3.4: Quantile treatment effects on hours worked



(a) Overall



(b) With old children



(c) With young children

Notes: The figure shows quantile treatment effects on hours. The dependent variable is the monthly average of hours worked per week. I use monthly data from baseline up to 15 months after baseline (each observation is an individual-month pair). "•" indicates a statistically significant estimate at the 5% level.

where α represents the impact of the program on Y_{it}. Because of the program's design, D_i is independent of ε_{it}, and so standard estimation procedures are able to consistently estimate α. However, I consider the inclusion of control variables X_i as attrition in the teachers' survey is higher (see Chapter 2) and so is the probability of imbalance on unobserved attributes between treatment and control groups. All of the measures I analyze are ordinal.[25] Thus,

[25]The New Hope literature does not take into account the ordinal nature of the child development variables. See Bos et al. (1999), Huston et al. (2001), Huston et al. (2003), Huston et al. (2005), Epps and Huston (2007), Miller et al. (2008), and Huston et al. (2011). Since the scale of the raw measures is arbitrary, their results are difficult to interpret.

I define Y_{it} to be a latent variable and Y_{it}^* the observed ordinal measure, such that $Y_{it}^* = j$ if $\kappa_t^{j-1} \leq Y_{it} < \kappa_t^j$, for $j = 1, ..., J$, $\kappa_t^0 = -\infty$, and $\kappa_t^J = +\infty$. I assume $\varepsilon_{it} \sim N(0, 1)$ and estimate an ordered probit to recover the cutoffs κ_t^j and α.[26] Because α does not have a meaningful interpretation, I simulate the causal effect of the program on the predicted probabilities of scoring $Y_{it}^* = j$.

Table 3.5: The effect of New Hope on measures of child development (all children)

Measure	Baseline	Model 1 Estimate	Model 1 p-value	Model 2 Estimate	Model 2 p-value
Panel A. SSRS Academic Subscale					
Overall ($n = 411$)	0.343	0.073	0.056	**0.081**	0.034
Reading ($n = 403$)	0.349	0.077	0.058	0.076	0.066
Math ($n = 402$)	0.353	0.053	0.166	0.070	0.069
Reading grade expectations ($n = 405$)	0.351	0.043	0.263	0.041	0.298
Math grade expectations ($n = 402$)	0.348	0.034	0.382	0.045	0.244
Motivation ($n = 415$)	0.421	0.044	0.283	0.056	0.179
Parental encouragement ($n = 381$)	0.458	**0.087**	0.039	**0.090**	0.040
Intellectual functioning ($n = 410$)	0.415	**0.088**	0.032	**0.100**	0.017
Classroom behavior ($n = 414$)	0.440	0.073	0.079	0.083	0.051
Communication skills ($n = 413$)	0.477	0.077	0.056	**0.089**	0.030
Panel B. Classroom Behavior Scale					
Behavior skills ($n = 418$)	0.694	0.010	0.789	0.024	0.512
Independent skills ($n = 418$)	0.593	0.009	0.824	0.032	0.434
Transitional skills ($n = 417$)	0.581	0.018	0.657	0.032	0.414

Notes: The table shows the impact of the program on the probability that the teacher reports child i in the top 30% of the class academic performance distribution (panel A) and on the probability of reporting "often" or "almost always" (panel B). The estimates are based on an ordered probit estimation. Model 1 does not include any control variable. In model 2, I control for the age of the parent, marital status, ethnicity, a dummy variable indicating whether the parent has a high school diploma, the highest grade completed, and earnings in the past year. I show the estimates in percentage points. To construct standard errors, I draw 1,000 bootstrap samples, calculate the marginal effect of RA_i on being in the top 30% or scoring "often" or "almost always" for each dataset, and compute its standard deviation across samples. Statistically significant effects at the 5% level are indicated in bold.

Table 3.5 shows the results for all children. The Table shows the marginal impact of D_i on the probability of scoring 4 or 5—which stands for being in the top 30% in the SSRS scale and scoring "often" or "almost always" in the classroom behavior scores. For both measures, I show p-values of the null hypothesis that the effect is zero. I present the results from regressions with and without control variables (models 1 and 2, respectively). Of the 13 outcomes measured, the estimates indicate that the program had positive, statistically

[26] In the ordered probit regression, the constant term and the variance of the error term are not identified.

significant impact at the 5% level in three measures, and at the 10% level in three additional outcomes. If we control for baseline characteristics, the estimates show five cases where the impact is significant at the 5% level and two additional at the 10% level. All of the statistically significant effects are found for the SSRS Academic Subscale measure. Depending on the measure, the evidence suggests that the program increased the probability of being in the top 30% of the academic ranking of the class by 8 to 10 percentage points, from a baseline of 35 to 48%.

Table 3.6: The effect of New Hope on measures of child development (young children)

Measure	**Baseline**	**Model 1**		**Model 2**	
		Estimate	**p-value**	**Estimate**	**p-value**
Panel A. SSRS Academic Subscale					
Overall ($n = 109$)	0.333	**0.159**	0.038	**0.173**	0.039
Reading ($n = 105$)	0.333	0.122	0.130	0.134	0.132
Math ($n = 105$)	0.404	**0.159**	0.036	0.150	0.092
Reading grade expectations ($n = 107$)	0.358	0.091	0.241	0.108	0.211
Math grade expectations ($n = 107$)	0.415	0.156	0.051	0.140	0.136
Motivation ($n = 108$)	0.415	0.117	0.142	0.104	0.277
Parental encouragement ($n = 103$)	0.500	0.086	0.292	0.135	0.129
Intellectual functioning ($n = 108$)	0.407	0.131	0.106	0.124	0.165
Classroom behavior ($n = 108$)	0.370	0.160	0.057	0.157	0.092
Communication skills ($n = 109$)	0.593	0.103	0.206	0.113	0.182
Panel B. Classroom Behavior Scale					
Behavior skills ($n = 110$)	0.618	0.053	0.366	0.051	0.435
Independent skills ($n = 110$)	0.582	0.106	0.076	**0.149**	0.025
Transitional skills ($n = 110$)	0.545	0.069	.	0.081	.

Notes: The table shows the impact of the program on the probability that the teacher reports child i in the top 30% of the class academic performance distribution (panel A) and on the probability of reporting "often" or "almost always" (panel B). The estimates are based on an ordered probit estimation. Model 1 does not include any control variable. In model 2, I control for the age of the parent, marital status, ethnicity, a dummy variable indicating whether the parent has a high school diploma, the highest grade completed, and earnings in the past year. I show the estimates in percentage points. To construct standard errors, I draw 1,000 bootstrap samples, calculate the marginal effect of RA_i on being in the top 30% or scoring "often" or "almost always" for each dataset, and compute its standard deviation across samples. Statistically significant effects at the 5% level are indicated in bold.

Table 3.7: The effect of New Hope on measures of child development (old children)

Measure	Baseline	Model 1 Estimate	Model 1 p-value	Model 2 Estimate	Model 2 p-value
Panel A. SSRS Academic Subscale					
Overall ($n = 302$)	0.346	0.043	0.334	0.061	0.197
Reading ($n = 298$)	0.354	0.060	0.185	0.065	0.164
Math ($n = 297$)	0.335	0.018	0.691	0.043	0.351
Reading grade expectations ($n = 298$)	0.348	0.024	0.577	0.031	0.497
Math grade expectations ($n = 295$)	0.325	-0.006	0.897	0.014	0.735
Motivation ($n = 307$)	0.422	0.016	0.733	0.036	0.475
Parental encouragement ($n = 278$)	0.444	0.086	0.089	0.097	0.066
Intellectual functioning ($n = 302$)	0.418	0.073	0.123	0.090	0.066
Classroom behavior ($n = 306$)	0.463	0.045	0.364	0.065	0.210
Communication skills ($n = 304$)	0.438	0.065	0.180	0.087	0.075
Panel B. Classroom Behavior Scale					
Behavior skills ($n = 308$)	0.720	-0.007	0.874	0.015	0.735
Independent skills ($n = 308$)	0.596	-0.026	0.585	-0.002	0.967
Transitional skills ($n = 307$)	0.593	-0.002	0.968	0.021	0.683

Notes: The table shows the impact of the program on the probability that the teacher reports child i in the top 30% of the class academic performance distribution (panel A) and on the probability of reporting "often" or "almost always" (panel B). The estimates are based on an ordered probit estimation. Model 1 does not include any control variable. In model 2, I control for the age of the parent, marital status, ethnicity, a dummy variable indicating whether the parent has a high school diploma, the highest grade completed, and earnings in the past year. I show the estimates in percentage points. To construct standard errors, I draw 1,000 bootstrap samples, calculate the marginal effect of RA_i on being in the top 30% or scoring "often" or "almost always" for each dataset, and compute its standard deviation across samples. Statistically significant effects at the 5% level are indicated in bold.

Table 3.6 and 3.7 present estimates for young and old children. For all of the measures, the impacts on young children is at least as big as those of old children. In most cases, impacts on young children double those for old children. For the SSRS measure, the impact of New Hope ranges from 9 to 16 percentage points on the likelihood of being in the top 30%. These estimates are compared with substantially lower effects for older children, estimated in the range of 0-9 percentage points. In spite of having imprecisely estimated effects for the young sample, I find statistically significant effects on the overall academic assessment and on math (both estimated at 16 percentage points).

3.3.3 What is behind families with young children's responses to New Hope?

Previous sections present heterogeneous effects of New Hope on child care, labor supply, income, and child outcomes by the age of the youngest child. This section analyses one potential reason behind such heterogeneity. I use a structural model to show that the effects of New Hope with no child care subsidy on adults and children are similar to equivalent estimates for families of older children.

Consider a model in which a forward-looking agent makes labor supply and child care choices for many periods. The agent chooses among three possible hours worked in a discrete set—out of the workforce (0 hours), part-time (15 hours), and full-time work (40 hours)—and two child care alternatives—home care or center-based. Agent's choices affect child human capital; changes in income, labor supply, and child care choices modify the accumulation of early childhood human capital. The agent's current-value utility depends on income, labor supply, and the stock of human capital. Income depends on labor supply decisions as well as mean-tested programs—such as New Hope. In this model, participant's reactions to the New Hope "shock" are influenced by the production function of human capital. If work hours have a negative impact on child human capital, then the positive labor-supply responses to New Hope would be mitigated relative to a model in which the adult does not care for her child's human capital (or does not have children, everything else constant). Nevertheless, we could find opposite patterns if the policy considers funding for high-quality child care.

Chapter 4 formalizes and estimates such model of household choices and early childhood human capital. With the estimated model, I simulate a version of New Hope that does not include a child care subsidy. The goal is to evaluate the impact of a child care subsidy in accounting for the differences in household behavior between families with young and old children.

Table 3.8 shows the results from this exercise. The first column documents impacts on income, labor supply, child care, and child human capital of a policy that includes an income subsidy and a work requirement. The second column shows the equivalent simulated impacts of New Hope as it was conceived. All simulated treatment effects are averages from baseline up to two years after, for adults with at least one child who is no more than six years of age by two years after baseline. If the child care subsidy explains part of the heterogeneity of treatment effects across the youngest child's age, then the impacts shown in Table 3.8 should resemble effects for families with older children. Consistent with the observed difference in the treatment effects on income for families with young and old children (Table 3.3), treatment effects for families with young children are reduced when going from the full treatment (column 2) to the more limited policy (column 1) (although, levels do not necessarily coincide). Results from the simulation exercise also show a reduction in hours worked. Using numbers from Table 3.8, the full treatment raises hours worked by 9 hours a week, two more than the effect of New Hope without the child care subsidy. Even though numbers are not exactly comparable, they are in the same order of magnitude with respect to the difference between families with old and young children (see Section 3.3.1). Effects of the modified New Hope on child care use almost exactly mirrors equivalent impacts for families with older children (a 9-percentage-points effect). Finally, compared to the original New Hope, the effects of the new policy on child human capital is 60% lower. Similarly, I find that treatment effects for observed measures of child academic achievement for old children are almost half of those for young children.

These results suggest that the child care subsidy plays a role in explaining the heterogeneity in treatment effects across families. A production function of child human capital that varies with child's age is consistent with these patterns in treatment effects. Consider the following production function: $\theta_{t+1} = f_t(\theta_t, l_t, I_t, cc_t)$, where θ_t is child human capital, l_t labor supply, I_t income, and cc_t is a child care dummy that equals 1 for center-based and 0 for home care. Following findings from Chapter 4, partial derivatives are such that $f_\theta > 0$,

Table 3.8: Effects of New Hope with and without a child care subsidy

Treatment effects	(1)	(2)
Income ($)	921	1,329
Part-time (%)	-9.0	-11.7
Full-time (%)	20.5	26.1
Child care (%)	8.5	22.0
Child human capital (in σ)	0.078	0.187
Policies		
Wage subsidy	✓	✓
Child care subsidy		✓
Work requirement	✓	✓

Notes: The table shows the impact of New Hope on consumption, part-time work, full-time work, child care, and child human capital. The sample corresponds to children who are six years of age or less by two years after baseline. To estimate impacts, I take averages of annual effects from baseline up to two years after. Each policy is compared to a counterfactual where no policy is implemented.

$f_l < 0$, $f_I > 0$, and $f(\theta_t, l_t, I_t, 1) - f(\theta_t, l_t, I_t, 0) > 0$. One potential explanation for the dissimilar behaviors of families with young and old children is that partial derivatives with respect to income and labor supply vary in time. However, the fact that both income and labor supply went up thanks to New Hope (potentially compensating each other) leaves little scope to generate such heterogeneity; especially in explaining effects on children.[27] A more compelling history comes from changes in the effect of child care across ages. One can account for the observed patterns in treatment effects for different family types if the marginal effect on human capital of child care for young children is larger than that of older children. The higher child care effect for young children makes working full-time (thus complying with the work requirement) more attractive, providing an additional boost in work incentives, and explaining why labor supply effects (at least in the intensive margin) are stronger for families with young children. Moreover, the marginal benefit of a child care subsidy is higher for these families than for parents of older children, generating a larger—and substantial—increase in

[27] In order to reproduce observed effects of New Hope on the family, compared to an old child, the production function of a young child must have bigger labor-supply (in absolute value) and income marginal effects. Moreover, the marginal effect of income has to be high enough to generate substantial impacts on child human capital. In Chapter 4, I find that the productivity of time and money in the production function are relatively small, suggesting that the evolution of the marginal productivity of income and labor supply plays a minor role in explaining the differences in the treatment effects of parents of young and old children.

the use of child care. Finally, the effects on child outcomes are higher for young than for old children.

3.4 Conclusions

This chapter presents new evidence on the impact of child care and income subsidies on the family. To this end, I use experimental data from New Hope—an anti-poverty program implemented in Milwaukee (1994-1997) involving both income and child care subsidies which were tied to a full-time work requirement.

I find that the effects of New Hope on household behavior and child outcomes vary depending on whether children were exposed to the program when they were less than seven years of age or older. Effects on child care use for young children are twice as large as that of old children. Even though the average impacts on household income were similar for both types of families, they are a result of differential impacts on different sources of income: for families with young children, New Hope had a larger effect on earnings compared to families with old children, which is potentially explained by stronger effects on hours worked on the intensive margin. Effects on child outcomes were different as well. On measures of child academic achievement, New Hope had effects on young children that were, in some cases, more than double than those for old children.

Overall, effects are consistent with families responding differently to the New Hope intervention given an evolving child human capital production. Using the estimated model of Chapter 4, I find that a policy without a child care subsidy produces effects on households with young children that are quantitatively similar to those among households of older children. Arguably, a production function of child human capital that exhibits larger human capital effects of using center-based child care instead of home care, relative to the same parameter for older children, explains the effect of the child care subsidy on household behavior. In this case, work incentives and effects on early childhood human capital for families with young children are higher than those for families with older children.

3.5 Appendix

3.5.1 The benefits of New Hope

Income subsidy

The income subsidy equals the sum of an earnings subsidy and a child allowance. The earnings subsidy increases at low levels of earnings and phases out until reaching zero benefits. Let E be the annual labor earnings for a given year. The earnings subsidy (ES) is determined by the following formula:

$$ES^* = \begin{cases} 0.25 \times E & \text{if } E \leq 8,500 \\ \max\{0.25 \times 8,500 - 0.2(E - 8,500), 0\} & \text{if } E > 8,500, \end{cases}$$

and so the earnings subsidy equals zero at 19,125 dollars of earnings. These parameters do not depend on family composition or other sources of income.

Unlike the earnings supplement, the child allowance component considers family annual labor earnings. Let FE denote family earnings and n the number of children in the family. The per-child child allowance (CA) is given by

$$CA = \begin{cases} x_n^* & \text{if } FE < 8,500 \\ \max\{x^* - r(\bar{e})(FE - 8,500), 0\} & \text{if } FE \geq 8,500 \end{cases}$$

where x_n^* is the subsidy maximum level and $r(\bar{e})$ is the phase-our rate. This rate is implicitly defined by the level of earnings at which the child allowance phases out completely ($\bar{e}$).[28] This last parameter is determined as follows:

[28] $r(\bar{e})$ corresponds to the rate r at which $x_n^* - r \times (\bar{e} - 8,500) = 0$.

$$\bar{e} = \begin{cases} 30,000 & \text{if } n < 4 \\ 30,000 + e* & \text{if } n \geq 4 \end{cases}$$

where e^*varies by year of the program (starts at \$300 and reaches \$2,100 by the third year). The maximum level of child allowance depends on the number of children, as follows:

$$x_n^* = \begin{cases} x_{n-1}^* + (x_{n-1}^* - x_{n-2}^* - 100) & \text{if } n \leq 4 \\ x_{n-1}^* & \text{if } n > 4 \end{cases},$$

where $x_0^* = 0$ (child allowance when the family has no children) and $x_1^* = 1600$. Thus, the maximum level reaches $1,600$ dollars for the first child, an extra $1,500$ for the second, and so on. The maximum subsidy stays fixed at x_4^* for families with more than four children.

The New Hope income supplement $(ES + CA)$ complements the EITC. Let $EITC$ be the amount of EITC for a given level family earnings. The total income supplement (IS) follows:

$$IS = \begin{cases} (ES + CA) - EITC & \text{if } (ES + CA) > EITC \\ 0 & \text{if } (ES + CA) \leq EITC \end{cases}$$

Child care subsidy

New Hope provided child care vouchers with a relatively low copayment. To have had access to the subsidy, families must have met three basic conditions. First, only individuals with children under age 13 were eligible. Second, beneficiaries had to have worked at least 30 hours a week on average in a particular month.[29] For a two-parent family, in addition to the

[29]New Hope representatives implemented the following procedure minimize fraud. Each month, the participant and the provider sign a voucher indicating the hours and the cost of the services. By the end of the month, the child care provider submits these vouchers to New Hope representatives to receive their payments. New Hope pays the subsidy directly to the child care provider. The participant pays the copayment to the provider as well. If the participant does not submit the wage stubs, New Hope would

full-time requirement of the primary earner, the second earner had to have worked at least 15 hours a week. If the participant had been unemployed, she would have received a subsidy covering a portion of a part-time child care (up to three hours, for a maximum of three weeks). Finally, participants who were eligible to receive the child care benefit were able to enroll their children only in a state- or county-licensed provider. This definition included preschool and daycare centers for younger children and after-school programs for children in school ages.

Let p be the child care cost offered at a child care facility. The copayment ($\underline{p}$) follows:

$$\underline{p} = \begin{cases} 400 & \text{if } p > 400 \text{ and Earnings} \leq 8,500 \\ 315 + 0.01 \times \text{Earnings} & \text{if } p > 400 \text{ and Earnings} > 8,500 \\ p & \text{if } p \leq 400 \end{cases}$$

Community Service Jobs (CSJ)

New Hope staff advised participants in finding local job openings. If after a period of eight weeks the participant could not find a job, New Hope would assign her to a paid CSJ for a maximum of six months.[30] The CSJ's paid was minimum wage. Importantly, the hours worked in these CSJs qualified for the income supplement, child care subsidy, and the health insurance subsidy.

According to Brock et al. (1997), other forms of CSJs were available at that time in Milwaukee. However, unlike the New Hope program, these types of CSJ did not qualify for the state's EITC. Indeed, the state CSJ positions were meant for individuals who needed them to receive welfare grants, not as a mean to earn a salary. The New Hope CSJs were

cover only 75% of the child care cost of the month. If the participant does not submit the wage stub for the second month in a row, New Hope reps would suspend the subsidy.

[30]The Milwaukee Private Industry Council acted as the former employer, although funds came from New Hope.

given to people regardless of their employment status, while the state CSJ were not usually offered to unemployed individuals.

Health insurance

New Hope financed part of the health insurance for workers with no employer-granted health insurance or Medicaid. To have access to the health insurance, individuals must have worked at least 30 hours a week every month. If a participant became unemployed or reduce her working hours below 30, New Hope kept their health insurance up to three weeks.[31]

New Hope provided health insurance through a Health Maintenance Organizations (HMO). The program's representatives displayed a number of plans and explained in detail the ups and downs of every plan. Beneficiaries would pick from any of those plans. Nevertheless, most of the participants choose to stay with the HMO that had a contract with Milwaukee County to provide Medicaid services.

To receive health insurance through New Hope, participants had to pay a small share of its cost. The copayment was a function of household income and size. The copay began at $72 and $168 a year for a single person and households with three members or more. The maximum copay was $600 and $1,548 for single- and three-person households, respectively. If an individual had an employer health plan, New Hope would cover the difference between the insurance's premium and the New Hope copayment. Moreover, if the participant did not have a dental coverage under her employer health plan, she had the option of choosing from the New Hope available dental plans.

Many of participants opted out from the New Hope health insurance plan, as some families choose Medicaid instead. To be eligible for Medicaid, families under AFDC had to make less than 185% the federal poverty line.[32] As many New Hope families met these requirements and given that Medicaid had no premiums, the Medicaid option seemed more

[31] In practice, New Hope representatives would keep the health insurance eligibility up to three months if the participant would have demonstrated active job search efforts.

[32] After PRWORA, individuals that were eligible for Medicaid as of August 1996 maintained their eligibility status.

convenient. Nonetheless, take-up was still considerable: 47.6% of participants were covered by a New Hope health insurance at some point during the 36-months eligibility period.

Chapter 4

Understanding the Effects of Income and Child Care Subsidies on Children's Academic Achievement

4.1 Introduction

At the time New Hope was implemented, the policy was seen as a potential policy strategy to overcome poverty by encouraging labor market participation (Bos et al., 1999). However, theory suggests that there could have been negative effects on children. If participants work more hours, then they would spend less time at home and children would spend more time in center-based child care. To understand how income and child care subsidies could affect child outcomes, researchers and policy makers need to assess how these household mechanisms interplay in producing child human capital.

Given the New Hope experimental design, the program provides reliable evidence on the causal effects of New Hope on children's academic achievement—a crucial input for public policy purposes. Nonetheless, the New Hope evidence has at least two limitations. First, because all policies were bundled together, we cannot assess the role played by each individual policy in the induced changes on household choices and child outcomes. Second, the experimental data does not give us enough exogenous sources of variation to assess which household behaviors were more influential in accounting for the impact on child outcomes.[1]

[1] Grogger and Karoly (2009) conclude that experimental studies on welfare reforms and child well-being yield mixed results, where the estimated effects are likely to depend on each program's characteristics. See Heckman (2010) and Keane et al. (2011) for a discussion on the comparison of structural and reduced-form approaches.

The goal of this chapter is to disentangle the mechanisms that explain the impact of income and child care subsidies on children's academic performance. I focus on the sample of children who were six years old or less at the time New Hope was running. To address the empirical limitations of the experimental evidence, I posit and estimate a dynamic-discrete choice model of the household and child academic skills. In the model, a single-child unitary household chooses hours of work and child care types (informal home care or formal, center-based child care). Child human capital production follows a dynamic process, where household decisions and the current stock of skills are inputs in this production function. The household's budget set encompasses different mean-tested programs, including the AFDC, the EITC, and New Hope. I estimate the model using non-experimental moments while leaving experimental estimates for model validation. I exploit the estimated model to evaluate how income and child care subsidies impact household choices and thereby child skills.

I find that the impact of New Hope on child academic performance is a result of different counteracting channels, with child care playing a major role. The program-induced rise in labor supply has a negative effect on child skills acquisition. However, this negative effect is more than compensated by the positive impact on child skills caused by the increase in income and child care probability. One year after the program, a higher probability of child care use explains 97% while the rise in income explains 17% of New Hope's effects on children (the remaining -14% is explained by labor supply). After the first year, the persistence of the production function explains the evolution of the program's treatment effects on child academic skills. In the first year of the program, the impact on child human capital is relatively small. However, most of the skills acquired in that period are transferred to the next one. Since the program continued to affect income and child care use, more skills were produced on top of the existing stock, augmenting the average effect of the program over time. When the program ended (in the third year), the program's effects on income and child care dissipated and the impact on child human capital started to diminish.

I quantify the importance of each one of the New Hope policies in accounting for the impact of the program on children. My analysis shows that the child care subsidy explains most of the effects of New Hope on child academic achievement. The contribution of this policy to the overall effects on children more than doubles the contribution of the wage subsidy. Furthermore, the full-time work requirement has a negative effect on child human capital; the effects of the program would have been larger (about 0.06 standard deviations) if New Hope had not included a work requirement. The negative effect of the work requirement is explained because this policy, relative to a program without this condition, decreases parental time at home and center-based child care use.

I study the consequences of the EITC and a permanent child care subsidy (similar to Wisconsin's version of the CCDF subsidy) targeted to young children (six years of age or less) on child academic skills. I find that both policies have an economically significant potential to impact child skills development through its effects on household behavior—especially in the long run. The EITC increases child academic skills by 0.08 standard deviations for children exposed for at least seven years to the policy. A child care subsidy has larger effects: 0.35 standard deviations by the end of the seventh year. The impact of the EITC on children is mostly explained by the greater use of center-based child care, with year-by-year small additions to the human capital stock that are being accumulated over time. Labor supply plays only a negligible role and its negative effect on child skills does not offset the positive effect of having more income and a higher child care likelihood.[2] Similarly, I find that most of the effect of the child care subsidy on child skills comes from a higher probability of attending a center-based child care.[3] In both policies, self-productivity sustains an increasing average treatment effect.

[2]Dahl and Lochner (2012) exploits EITC variation to instrument for income and uncover causal effects on child test scores. The authors argue that the IV estimates on the effects of income on child test scores hold once accounting for labor supply effects (Dahl and Lochner, 2016).

[3]Black et al. (2014) find that child care subsidies in Norway impacted child outcomes for the most part due to a larger average disposable income.

The remaining of this chapter is structured as follows. Section 4.2 presents the dynamic-discrete choice model. Section 4.3 discusses its estimation. Section 4.4 documents the model's estimates and explains its implications for the dynamics of skills acquisition. Finally, Section 4.5 assesses the consequences of income and child care subsidies on household decisions and child outcomes.

4.2 A dynamic-discrete choice model of labor supply, child care, and child's skills

This section presents a dynamic model of the household and child human capital. Since the model incorporates all of the economic constraints defined by New Hope, the EITC, and welfare programs as part of the individual's budget set, it is capable to reproduce the effects of income and child care subsidies on child human capital. Thus, the model is able to shed lights on the household mechanisms by which these policies impact child human capital.

The basic timing and features of the model are as follows. At the beginning of time, a forward-looking agent receives the New Hope "shock" and draws an initial value of child human capital and child age. Each period, the agent observes her household composition, a wage offer, and the current level of child skills and makes labor supply (not working, part-time, or full-time work) and child care choices (center-based child care or home care) up until the child turns 18 years old. These choices are shaped by various shocks to the agent's budget set—New Hope and the welfare system— and by a dynamic production function of child skills. Next, I present the model formally and explain its components in detail.

Utility function. The individual's current-period utility function corresponds to

$$U(c_t, h_t, \theta_t) = \ln c_t + \alpha^p \mathbf{1}\{h_t = 15\} + \alpha^f \mathbf{1}\{h_t = 40\} + \eta \ln \theta_t, \tag{4.1}$$

where h_t are weekly working hours. It can only take three values: 0, 15 (part-time work), and 40 (full-time work). c_t is per capita consumption; it represents the average consumption family members enjoy after paying for child care services. α^p, α^f capture the psychic costs or benefits of part-time work, full-time work. η is the preference for the current stock of child human capital. θ_t denotes child human capital. Here, parents observe a "true" value of child human capital, that is, an underlying factor that drives academic achievement. The presence of θ_t in equation (4.1) implies that the individual makes her choices based on a weighted average of the stock of human capital across time—not just on its long-run value.[4]

Single and married individuals have the same utility function. For single agents, equation (4.1) represents the utility function of the parent that cares for her child's human capital.[5] For married individuals, for all t, the spouse receives no income.[6] All choices are made by the caregiver of the child. Having a spouse affects choices by adjusting consumption per-capita (more mouths to feed) and the budget set (welfare rules differ by marriage status).

Human capital production function. The technology of child human capital follows

$$\theta_{t+1} = \exp\left(\gamma_0 + \gamma_1 cc_t \mathbf{1}\{a_t \leq 6\}\right) \theta_t^{\gamma_2} c_t^{\gamma_3} \tau_t^{\gamma_4}, \tag{4.2}$$

where cc_t is a child care dummy—equal to 1 for center-based child care and 0 for home care or any informal care at someone's home—and τ_t are weekly hours the individual spends with the child. The indicator function next to the child care dummy implies that only an individual with a young child (age $a_t \leq 6$) can use the child care option. The coefficients γ_k, for $k = 1, \ldots, 4$, represent the effect of current-period inputs on next-period child human capital. The constant in the production function (γ_0) is normalized so that $E[\ln \theta_t] = 0$ for $t > 0$. γ_1 is a total-factor-productivity parameter. It captures the human capital gain from center-based child care relative to home care.

[4] Hence, the fading-out effects of New Hope can emerge as a rational outcome of the model.

[5] Nearly 90% of participants are single or living alone with the child.

[6] Data on spouse's earnings is available only in New Hope surveys from year two. Among those married or cohabiting (10% of the sample), 44 and 36% of the total income of married respondents in the second-year survey comes from the own participant's and her spouse's earnings, respectively.

Equations (4.1) and (4.2) imply that per-capita consumption enters individual's utility both directly and indirectly through the production function. We can interpret the indirect effect in two ways. First, part of what the agent purchases can also affect child human capital (e.g. books, food, etc). Second, having more money at home can relieve stress in the household, which can potentially enhance the parent-child relationship.

An additional child in the family does not directly impact utility (equation 4.1) or the process of human capital formation (equation 4.2). Having more children influences choices only through per-capita consumption and the budget set; an additional child in the household, all else equal, lowers c_t and changes the eligibility for welfare programs. For a family with more than one child, the adult takes into account the evolution of a representative child's human capital.[7]

Wages. Each period, the individual receives an hourly wage offer (w_t). Following Bernal (2008), Chan (2013), and Del Boca et al. (2013), the offer depends on a vector of observable individual characteristics X_t^w. Furthermore, the wage offer also depends on an individual productivity that follows an AR(1) process. Formally, the wage offer process is given by

$$\begin{aligned} \ln w_t &= X_t^{w\prime}\beta^w + \nu_t^w, \\ \nu_t^w &= \rho\nu_{t-1}^w + \epsilon_t^w, \\ \epsilon_t^w &\sim N(0, \sigma_w^2), \end{aligned} \tag{4.3}$$

where X_t^w includes age, age squared, a dummy variable for high school diploma, a constant, and the log of t. This last term captures a non-linear trend of the wage offer process. The agent knows the coefficient associated with $\log(t)$ beforehand, and so she anticipates an average trend on her wage offer.

[7] Household choices would differ from a multiple-children model—as Todd and Wolpin (2006) and Tartari (2015b)—only in the case where there are young and old children at the same period in a given household (which occurs in 28% of the cases). Compared to such framework, average choices should not deviate as much (even though, at the individual level, choices would be different). Another option would be to disregard families with more than two children (Bernal, 2008; Del Boca et al., 2013), which would mean losing more than 50% of the sample.

Parental education and child human capital are related via individual choices. The level of parental education is an input in the wage process, which in turn affects labor supply and child care choices. Because human capital is affected by income, time, and child care, parental education has an indirect effect on child skills.

Budget set. The budget set incorporates various features of the welfare system. Income is a function of labor supply, earned income, and various mean-tested programs. Conditional on eligibility, the agent receives payments from New Hope, the EITC, the AFDC or Food Stamps.[8] Eligibility for these programs and payment amounts depend on working hours, earned income, and family composition.

Income can be represented as follows. Let k_t and m_t be the number of children and a marriage indicator (1 if the household has two adults married to each other or living together and 0 otherwise). Income (I_t) is given by

$$
\begin{aligned}
I_t = {} & w_t h_t \times 52 + EITC_t(w_t h_t \times 52, k_t, m_t) + NH_t(w_t h_t \times 52, k_t, m_t) \\
& + B_t + S_t. \qquad (4.4)
\end{aligned}
$$

In the equation above, $EITC_t(.)$ corresponds to EITC payments. If the individual is eligible to receive these payments, she always complies.[9] The same happens with the New Hope payments, $NH_t(.)$. B_t and $SNAP_t$ are cash transfers from AFDC (or TANF) and Food Stamps (now known as SNAP). As with New Hope and the EITC, the individual always chooses to take up the benefits of AFDC and Food Stamps payments (Blundell et al., 2016).[10]

[8]New Hope administrative data does not include other forms of welfare payments.

[9]The EITC national take-up rate is estimated at over 80% (Scholz, 1994; Plueger, 2009; Hoynes and Rothstein, 2016). Furthermore, New Hope representatives took care to advise participants about how to take advantage of the EITC (Bos et al., 1999). As for New Hope, assuming eligibility on an annual basis and using the definitions of hours worked and gross income that is consistent with the data for estimating the structural model, I estimate a take-up rate of 92%.

[10]An alternative structural framework would allow individuals to choose whether they want to take up the benefits and include taste parameters ("welfare stigma" coefficients) associated with each program. However, I do not have enough sources of exogenous variation to identify stigma coefficients (Keane and Moffitt, 1998; Chan, 2013). Figures 4.5 and 4.6 from the Appendix show that take-up rates for the AFDC and Food Stamps do not follow an obvious pattern across income quantiles.

However, she faces a random i.i.d. take-up shock, which captures the possibility of being misinformed about the welfare system. Specifically, at the beginning of each period, the individual draws two values, $\rho_t^B, \rho_t^S \in \{0,1\}$, from a pair of known, time-invariant binomial distributions, indicating whether the individual takes up the corresponding payment or not. These information shocks affect only AFDC and Food Stamps. Hence, the available money from AFDC and Food Stamps follows:

$$B_t \equiv \rho_t^B B_t^*(w_t h_t \times 52, k_t, m_t),$$
$$S_t \equiv \rho_t^S S_t^*(w_t h_t \times 52, k_t, m_t),$$

where $B_t^*(.)$ and $S_t^*(.)$ are the potential AFDC and Food Stamp payments.

Each of the payment functions $EITC_t(.)$, $NH_t(.)$, $B_t^*(.)$, and $S_t^*(.)$ are given by precise formulas determining eligibility and payment levels. They are a function of the level of earnings, labor supply, and family composition (k_t and m_t). These rules may change from year to year.[11] Nonetheless, at $t = 0$, families have perfect information regarding the evolution of rules of the welfare system (the uncertainty faced in this context is in future misinformation shocks about AFDC and Food Stamps).[12] Moreover, eligibility rules are always enforced, including the New Hope work requirement.[13]

During the time frame of the model, AFDC is replaced by TANF. In the case of Wisconsin, the Wisconsin Works program (W-2) was implemented. This program eliminated AFDC's unconditional cash transfers and established time limits for welfare utilization. Specifically, W-2 offered paid community service jobs at a flat rate. So from 1997 onward ($t = 2$), the

[11] See Appendix 4.7.1 for details.

[12] In the case of New Hope, the program's representatives explained the details of the benefits package to all participants. Furthermore, representatives were available throughout the eligibility period to answer any questions participants might have had (Brock et al., 1997).

[13] New Hope agents implemented various procedures to ensure that requirements were met. See Chapter 3 for details.

wage of the state-provided CSJ is part of the pool of potential log-wage offers (equation 4.3). Participants do not face time limits for W-2.[14]

Per-capita consumption. Consumption is defined as money net from child care expenditures. Agents have access to child care services at a fixed, known price p for all t. Nonetheless, if the agent is in the New Hope treatment group and works full-time, she gets a lower copayment, $\underline{p} \leq p_t$ which depends on the level of earnings (see Section 3.2). Formally, the cost function equals:

$$\delta(p, D, h_t) \equiv \begin{cases} \underline{p}\mathbf{1}\{h_t = 40\} + p\mathbf{1}\{h_t < 40\} & \text{if } D = 1, \\ p & \text{otherwise.} \end{cases} \tag{4.5}$$

The individual cannot save or borrow.[15] Per-capita consumption is thus given by:

$$c_t = \frac{I_t - cc_t \times \delta(p, D, h_t)}{1 + m_t + k_t}. \tag{4.6}$$

Parental time. Time with the child (τ_t) depends on labor supply choices (h_t) and child care ($cc_t \in \{0, 1\}$). Let $\overline{T}$ be the total available time the adult has in a week. Mathematically, τ_t is defined as

$$\tau_t \equiv cc_t(\overline{T} - 40) + (1 - cc_t)\left(\overline{T} - h_t\right) \tag{4.7}$$

The logic behind equation (4.7) is as follows. If the child spends all week in home care ($cc_t = 0$), then the labor supply choice determines how much time the adult spends with the child. In this case, there are three possible scenarios. If the individual does not work, then she spends all the available time with the child ($\tau_t = \overline{T}$). If she works part-time, then she

[14]The model's version of W-2 does not include time limits because it would require having data on labor supply beyond 2003.

[15]There is little evidence suggesting that individuals are able to save for future consumption. In the control group, from the year-five interview, 58% manifested some concern about not having enough money to buy food. Additionally, a large share does not access to banking services, 42% of individuals do not have a checking account, and 52% do not have a savings account.

must spend 15 hours a week away from home, so $\tau_t = \overline{T} - 15$. Analogously, $\tau_t = \overline{T} - 40$ if she works full-time. If the child spends her time in child care ($cc_t = 1$), then she spends 40 hours a week outside the house being cared in a child care center. Hence, if $cc_t = 1$, then $\tau_t = \bar{T} - 40$ no matter how many hours the adult spends working.

Equations (4.1), (4.2), and (4.7) make evident the benefits and costs the agent faces when choosing labor supply and child care. Intuitively, child care allows the individual working more and thus having more income without reducing child human capital (if $\gamma_4 > 0$ in equation 4.2). Thus, she can consume more (equation 4.1) and have more income to produce further child human capital. At the same time, child care has a direct, positive effect on child human capital (if $\gamma_1 > 0$ in equation 4.2). Hence, the benefits of child care are twofold: it produces child human capital and it lowers the cost of labor supply.

Leisure and time spent with a child have the same effect on utility. Following equations (4.2) and (4.7), any time allocated outside the labor market (if the child is at home) has a constant effect on human capital.[16] Moreover, given the utility function (equation 4.1), the adult enjoys her leisure hours (dislikes her work hours) to the same degree regardless of whether or not the child is at a child care center or remains at home.

Family composition. Marriage formation and childbearing are exogenous processes. Each period, the individual draws a marital status (1 if married and 0 if single) and childbearing values (1 if there is a new child in the family and 0 otherwise) from known binomial distributions with probability parameters m_t^* and k_t^*. These probabilities depend on observed participant characteristics and past family composition, as follows:

$$m_{t+1}^* = f_m(X_t^m, m_t), \tag{4.8}$$

$$k_{t+1}^* = f_k(X_t^k, k_t, m_t), \tag{4.9}$$

[16] Because New Hope data does not have time diaries, I cannot distinguish between passive or active time with the child (Del Boca et al., 2013; Brilli, 2014). Nonetheless, the literature consistently shows that non-working mothers do spend more time with their children than working mothers (Guryan et al., 2008).

where m_t equals 1 if the participant is married or living with her partner and 0 otherwise, and k_t indicates the number of children in the household. X_t^m includes a constant and age of the adult. X_t^k includes a constant, age, and age squared of the adult.

The dynamic problem. In each period, given a set of state variables, the individual solves a dynamic problem of labor supply and child care choices. The state variables of the problem are collected in the vector $\boldsymbol{s}_t = (D, m_t, k_t, a_t, \theta_t, \boldsymbol{X}, \nu_t^w, \boldsymbol{\rho}_t, p)$, where $\boldsymbol{X}$ contains the wage offer, marriage, and childbearing processes observed control variables, $\boldsymbol{X} \equiv \left(X^w, X^m, X^k\right)$, and $\boldsymbol{\rho}_t$ the misinformation shocks to welfare take-up, $\boldsymbol{\rho}_t \equiv \left(\rho_t^B, \rho_t^S\right)$. For a given $\boldsymbol{s}_t$, each period the agent maximizes the present discounted value of the utility stream by choosing labor supply and child care type. Let $\mathcal{C} = \{0, 1\}$ and $\mathcal{H} = \{0, 15, 40\}$ be the choice sets of child care and labor supply. We can represent the entire choice set, for any period, as $\mathcal{J}(a_t) = \mathcal{C} \times \mathcal{H}$ if the child is young ($a_t \leq 6$) and $\mathcal{J}(a_t) = \mathcal{H}$ otherwise ($a_t > 6$).

Because agents draw different initial values for the child's age, each individual solves a problem of a different time horizon. Let $T(a_0) \equiv 18 - a_0$ be the terminal period for an individual with a a_0-year-old child. Thus, for a one-year-old child arriving in period $t = 0$, the parent solves the dynamic problem for 17 years after baseline, stopping when the child turns 18.

Let $u(\boldsymbol{s}_t, j)$ be the current-period utility for a given state $\boldsymbol{s}_t$ and choice $j \in \mathcal{J}(a_t)$. For any t, the problem of the individual is represented in the usual recursive formula

$$V_t(\boldsymbol{s}_t) = \max_{j \in \mathcal{J}(a_t)} \{V_t^j(\boldsymbol{s}_t)\} \quad \text{subject to (4.1)-(4.9)},$$

$$V_t^j(\boldsymbol{s}_t) = u(\boldsymbol{s}_t, j) + \beta E[V_{t+1}(\boldsymbol{s}_{t+1}) \mid \boldsymbol{s}_t, j] \qquad t < T(a_0).$$

The model is closed with initial and terminal conditions. At baseline ($t = 0$), individuals draw a pair of values defining family composition (m_0, k_0) and pair of values of initial age and child human capital (a_0, θ_0). The initial θ_0 is related to the parent's unobserved characteristics. In particular, the initial shocks to unobserved productivity and child human capital, ε_0^θ and ε_0^w, follow a joint normal distribution with correlation coefficient ρ_θ.

In the final period, the individual can no longer invest in child human capital. The associated terminal value function is such that

$$V_{T(a_0)}^j(\boldsymbol{s}_{T(a_0)}) = \max_{j \in \mathcal{J}_{T(a_0)}} \left\{ \tilde{u}(\boldsymbol{s}_{T(a_0)}, j) \right\} + \eta \ln \theta_{T(a_0)}.$$

4.3 Identification and estimation

4.3.1 Identification

To better understand the sources of the data that identify the model, I divide the discussion into two related concepts: global (or point) identification and local identification.

Global identification. Identification of the structural parameters builds on distributional and functional form assumptions.[17] Nevertheless, to reduce the dependence on such structural assumptions, the policy environment within the model offers various sources of exogenous variation: the TANF implementation, EITC expansions, and the New Hope random assignment. Moreover, the money received from New Hope and welfare programs depends on family structure (number of children and marriage status) and there are several discontinuity points in the rules of the different programs. All of these policy changes within the model affect labor supply and child care decisions without directly changing preferences or the wage offer equation. Therefore, global identification hinges on the comparison of family choices and outcomes across periods (before and after policies are implemented), treatment groups, family composition, and at different points of the wage offer distribution.[18]

[17] See Rust (1994) and Magnac and Thesmar (2002).

[18] In Appendix 4.7.3, I estimate the wage offer equation using a control function approach. To this end, I use the model-predicted propensity score to account for non-random selection into work in the log wage equation—hence, I implicitly use all of the sources of variation discussed above as exclusion restrictions. The results confirm that both sets of estimates give similar coefficients on the wage offer equation.

To identify the production function (equation 4.2), I use observed data on inputs and the SSRS measure of overall academic achievement. The econometrician observes a noisy measure (M_t) of child human capital. It is an ordinal measure that is given by

$$M_t = \begin{cases} 1 & \text{if } \ln\theta_t + \epsilon^z_{t,m} \leq \kappa_{1,t} \\ 2 & \text{if } \kappa_{1,t} < \ln\theta_t + \epsilon^z_t \leq \kappa_{2,t} \\ \vdots & \qquad \vdots \\ 5 & \text{if } \ln\theta_t + \epsilon^z_t > \kappa_{4,t}, \end{cases} \tag{4.10}$$

where θ_t is observed by the families but not by the econometrician and ϵ^z_t is measurement error that follows a known distribution.

A key identifying assumption is that the production function is constant in time and it does not vary by age (except in the child care component). Observing human capital measures for different samples at different points in time helps to identify the production function and the structure of the measurement system. Appendix 4.7.2 provides the formal identification argument. Intuitively, identification follows in two steps. First, one can show identification of the technology of young children at home care with data on inputs (consumption and hours at home) and one measure of human capital observed for two periods. This result follows because the production function does not vary by period. Once the technology for a young child at home is identified, we can use the fact that cutoffs ($\kappa_{j,t}$, for $j = 1, 2, 3, 4$) are constant across child age and child care types to identify the TFP parameter.[19]

[19] Factor loadings—the coefficient associated with $\ln\theta_t$—equal 1 for every period. This assumption is necessary for identification in the absence of baseline measures of child human capital. By assuming factor loadings are known, the requirement of having multiple measures at baseline is no longer needed. A byproduct of this assumption is that I only need one measure per period to identify the production function. Furthermore, I do not need to impose the requirement that the coefficients of the production function sum up to one. See Agostinelli and Wiswall (2016a) for the necessary conditions to identify a production function with an unknown scale.

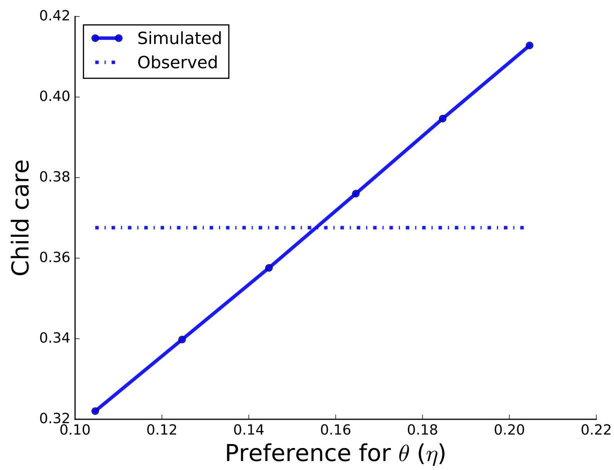
All cutoffs $\kappa_{j,t}$, can vary in time. The assumption of time-varying cutoffs restricts the family of productions functions that can be estimated. In particular, this assumption implies that the constant term in the production function (equation 4.2) cannot vary freely.[20]

Local identification. Some non-experimental moments contain identifying information at the local level; around a vicinity of the true value of a structural parameter, and holding the rest of parameters constant, there is only one value of the parameter that generates the sample moment. To illustrate the sources in the data that contribute to the identification of the structural parameters, Figure 4.1 plots the relationship between a simulated moment and a structural parameter, holding the rest of the parameters fixed.[21] Here, the simulated moment has a monotonic relationship with the structural parameter, crossing the observed moment only once. In this way, the chosen moment contains sufficient identification power at the local level to pin down the structural parameter. In this case, I plot the preference for human capital (η in equation 4.1) and the proportion in the sample who use child care. A higher preference for θ means that the agent is willing to sacrifice more consumption for higher levels of child human capital. Thus, for a given child care cost and holding other parameters fixed at their estimated values, a bigger η implies a larger probability of choosing center-based child care. The simulated probability of child care monotonically increases, crossing the observed value in the data only once. At this crossing point, η is locally identified. As I explain next, to exploit these sources of identification, this and other moments that meet the single-crossing property are used directly in the estimation procedure.

[20] Only with constant cutoffs across time (or that follow a predictable pattern), one would be able to identify γ_0 as a free parameter and thus let the production function have a time-varying TFP (Agostinelli and Wiswall, 2016a).

[21] Voena (2015) and Autor et al. (2017) follow the same approach to show local identification.

Figure 4.1: Local identification of the preference for human capital



Notes: The figure plots the proportion of children in child care from the model against the structural parameter of preference for human capital (solid line). This parameter is locally identified at the crossing point with the corresponding observed moment (depicted in the dotted line).

4.3.2 Estimation

For estimation purposes, I proceed in two steps.[22] In the first step, I estimate the parameters of some of the exogenous processes straight from the data. In the second step, I estimate the rest of the structural models using the simulated method of moments.

External estimation and calibration. Table 4.1 summarizes the sources for external estimation an calibration. To obtain the parameters governing the probability of being married (m^*_{t+1}) and of childbearing (k^*_{t+1}), consider the following linear probability models:

$$m_{t+1} = X^m_t \beta^m + m_t \gamma^m + \epsilon^m_{t+1}, \tag{4.11}$$

$$k_{t+1} - k_t = X^k_t \beta^k + k_t \gamma^k + m_t \gamma^{k,m} + \epsilon^k_{t+t}. \tag{4.12}$$

Since I do not have marriage data for two years in a row, I cannot directly estimate the parameters of equations (4.11) and (4.12). To circumvent this problem, I estimate a linear

[22]This method is a standard practice in the literature. For instance, see Gourinchas and Parker (2002), De Nardi et al. (2010), Voena (2015), and Blundell et al. (2016). The goal of the two-step procedure is to keep estimation computationally feasible.

probability model of m_{t+1} on m_{t-1} and X^m_{t-1}, and use the resulting reduced-form parameters to identify β^m and γ^m.[23] I implement a similar method to identify β^k, γ^k, and $\gamma^{k,m}$.[24] Given the estimated parameters of equations (4.11) and (4.12), the parameters of the binomial distribution determining the probabilities of marriage and childbearing (equations 4.8 and 4.9) are given by $m^*_{t+1} = X^m_t \widehat{\beta}^m + m_t \widehat{\gamma}^m$ and $k^*_{t+1} = X^k_t \widehat{\beta}^k + k_t \widehat{\gamma}^k + m_t \widehat{\gamma}^{k,m}$.

I determine the rest of the parameters of the exogenous processes to match different observed statistics. I set the probability of obtaining a free child care to 0.57, corresponding to the share of individuals in the control group, conditional on $cc_t = 1$, who declare not paying for child care. To calibrate the child care price, I take the value reported in Bos et al. (1999) corresponding to the average sum of individual copayments ($750 a month) and weight it by the proportion of control-group children who are not in Head Start (0.43×750). I define the probability of receiving AFDC and Food Stamps—conditional on being eligible—as the average take-up observed in the data (60% and 70%, respectively). Finally, I follow Chan (2013) and set the discount factor to $\beta = 0.86$, which is a middle point between the equivalent parameters of Swann (2005) and Keane and Wolpin (2010).

Table 4.1: Calibrated and externally estimated parameters

Parameter/equation	**Source for estimation/calibration**	**Values**
Probability of being married	OLS: m_{t+1} on m_{t-1} and X^m	$m^*_{t+1} = 0.21 - 0.002age + 0.8m_t$.
Probability of childbearing	OLS: $(k_{t+1} - k_t)$ on m_{t-1}, k_{t-1} and X^k	$k^*_{t+1} = -0.11 + 0.05age -$ $+0.0003age^2 - 0.006k_t - 0.1m_t$
Child care price	Bos et al. (1999)	$323 monthly
Take-up probabilities of AFDC and SNAP	Average AFDC and SNAP take-up conditional on eligibility	0.6 and 0.7

Notes: The table describes the sources for estimation or calibration of the structural parameters determined outside the estimation procedure.

Internal estimation. In a second step, I use the simulated method of moments to estimate the rest of the parameters. The procedure compares the estimated moments of

[23]To estimate this regression, I use data for the second-year survey and baseline information.

[24]Precisely because I do not have data for two periods in a row, I was unable to implement a logistic form to estimate the marriage and childbearing processes.

an auxiliary model using observed data on choices and exogenous variables with equivalent estimates from the model-simulated data (Gourieroux et al., 1993).

I use backward induction to solve the model and obtain paths of simulated choices. Because $\boldsymbol{s}_t$ contains continuous variables, obtaining an exact solution for $V_t(\boldsymbol{s}_t)$ at every point of the state space is computationally unfeasible. Thus, following Keane and Wolpin (1994) and Keane et al. (2011), I compute $V_t(\boldsymbol{s}_t)$ for a grid of the state space and then use linear interpolation (in which I include polynomial terms of the state variables) to approximate $V_t(\boldsymbol{s}_t)$ for values outside the grid. The size of the grid equals 4,536. Finally, I use Monte Carlo integration (with 50 draws) to estimate the multivariate integral.[25]

The estimation problem can be stated as follows. Let $\hat{g}$ be the vector of moments extracted from the data. I solve the model $M = 1,000$ times for a sample size of $n = 691$ children and compute the required moments from simulated data. Let $\{\boldsymbol{\epsilon}_t^m\}_{m=1}^M$ denote the structural random shocks (fixed across the estimation procedure). Let ψ be the vector of structural parameters of the model. Define $\{y_{it}^m(\psi)\}_{m=1}^M$ as the simulated choices associated with the M draws of structural random shocks. Let $\hat{g}^m(\psi)$ be the equivalent moment associated with the m draw. I estimate the structural parameters ψ by solving

$$\hat{\psi} = \arg\min_{\psi \in \Psi} [\hat{g} - \hat{g}(\psi)]' W [\hat{g} - \hat{g}(\psi)],$$

where $\hat{g}(\psi) = \frac{1}{M}\sum_{m=1}^M \hat{g}^m(\psi)$ and $\widehat{g}$ is the vector of auxiliary estimates from the data.

Following Del Boca et al. (2013) and Blundell et al. (2016), I define W as the inverse of the diagonal of the estimated variance-covariance matrix of $\hat{g}$. I do not use the efficient weighting matrix because of its poor small-sample properties (Altonji and Segal, 1996). I use the bootstrap method (1,000 samples) to estimate W.

Targeted moments. Table 4.2 lists the set of auxiliary estimates used in estimation. Estimation exploits a set of unconditional and conditional moments. The matched moments

[25] I set the grid size and the number of draws for the Monte Carlo integration in order to balance precision and computational time.

are non-experimental, while experimental moments are left for validation. Based on the argument given in Section 4.3.1 (see Figure 4.1), estimation targets moments that provide identification at the local level.[26] To estimate preferences for hours of work and child human capital, I use the labor supply and child care choices of children's parents. To estimate the production function process, I use the correlation of consumption and parental time with the child (as defined in equation 4.7) with the raw SSRS rankings. To estimate the measurement system, I include various moments capturing the distribution of children in the SSRS rankings from two and five years after random assignment.[27] Finally, to estimate the wage offer process, the auxiliary model includes the OLS coefficients of a regression of log wages onto the variables discussed in the context of equation (4.3). Table 4.2 shows that the model successfully replicates the target moments.

4.4 Model estimates

4.4.1 Estimates

Table 4.3 presents the estimated structural parameters. Panel A shows the parameters of the utility function (equation 4.1). Since this function is expressed in log-consumption units, taste parameters represent what the agent is willing to pay (in terms of a percentage change in current-period consumption) to compensate a marginal increase in one input while keeping the others fixed. Child human capital is positively valued by the agent, with $\eta = 0.16$. This value implies that the individual is willing to sacrifice 18% (= 0.16/0.88, where 0.88 is the estimated standard deviation of $\ln \theta_t$) of consumption for a one-standard-deviation increase in child human capital.[28] As it is common in the literature, I find that the implied

[26]Appendix 4.7.4 shows that each of the chosen moments locally identifies a structural parameter.

[27]I use the "overall" SSRS measure. This variable shows the reported rankings based on the overall academic performance of the child in a classroom. In the PCA analysis from Section 3.3, this measure has the highest correlation with the first PCA component in years two and five.

[28]Here, the literature provides a wide range of estimates: Bernal (2008) estimates an almost 0 coefficient, while Del Boca et al. (2013) documents that a one-percent increase in child human capital is more valued than the same increase in consumption.

extensive-margin marshallian elasticity is larger than the intensive-margin elasticity (0.49 and 0.43); although, the extensive-margin elasticity is probably at a lower bound of the range of estimates from the literature (Meghir and Phillips, 2010; Blundell et al., 2016).

Panel B shows the estimated parameters of the wage offer process (equation 4.3). For a given period, the estimates imply a negative and almost linear effect of age on the wage offer.

Table 4.2: Target moments

Moments	**Simulated**	**Data**	**S.E. data**
A. Labor supply and child care decisions			
$Pr(\text{child care}_t \mid RA = 0)$, $t = 1$ age ≤ 6	0.367	0.368	0.033
$Pr(\text{part-time}_t \mid RA = 0)$, $t = 0$	0.288	0.296	0.014
$Pr(\text{full-time}_t \mid RA = 0)$, $t = 0$	0.442	0.446	0.016
B. $log(wage_t) = X_t'\beta + \epsilon_t$			
Coefficient on age	-0.024	-0.020	0.006
Coefficient on age squared	0.000	0.000	0.000
Coefficient on high school dummy	0.246	0.247	0.043
Coefficient on $log(t)$	0.373	0.382	0.038
Constant	1.534	1.549	0.063
σ^2	0.474	0.466	0.053
AR(1) shock (ρ)	0.377	0.368	0.056
C. SSRS and household choices			
$Corr[SSRS_2, SSRS_5]$	0.375	0.377	0.058
$Corr[\text{consumption}_1, SSRS_2]$	0.024	0.027	0.037
$Corr[\text{leisure}_1, SSRS_2]$	-0.048	-0.043	0.043
$E(SSRS_2 \mid cc = 1) - E(SSRS_2 \mid cc = 0)$	0.237	0.253	0.137
$Corr(SSRS_2, \ln w_0)$	-0.006	-0.012	0.061
D. SSRS ($t = 2$)			
$Pr(SSRS = 2)$	0.152	0.152	0.021
$Pr(SSRS = 3)$	0.359	0.360	0.025
$Pr(SSRS = 4)$	0.225	0.226	0.023
$Pr(SSRS = 5)$	0.149	0.149	0.020
E. $t = 5$			
$Pr(SSRS = 2)$	0.216	0.216	0.020
$Pr(SSRS = 3)$	0.292	0.292	0.023
$Pr(SSRS = 4)$	0.174	0.174	0.019
$Pr(SSRS = 5)$	0.188	0.188	0.019

Notes: This table compares the simulated and observed estimated moments that are targeted in estimation. $SSRS_t$ corresponds to overall SSRS measure of academic achievement in period t. In this measure, teachers rank children in a five-point scale based on the overall academic performance in the classroom. $time_t$ corresponds to time with the child (τ_t). cc_t is child care in period t for children who are less than six years old. The rest of the variables are constructed following Appendix **??**.

Table 4.3: Estimated structural parameters

Parameter	**Estimate**	**S.E.**
A. Utility function		
Preference for part-time work (α^p)	-0.008	0.173
Preference for full-time work (α^f)	-0.271	0.080
Preference for human capital (η)	0.182	0.064
B. Wage offer		
Age	-0.006	0.002
High school	0.212	0.008
$\log(t)$	0.347	0.005
Constant	1.314	0.078
AR(1) error term	0.651	0.011
Variance of error term	0.307	0.055
C. Production function		
Child care TFP (γ_1)	0.222	0.110
Lagged human capital (γ_2)	0.957	0.026
Consumption (γ_3)	0.029	0.006
Time at home (γ_4)	0.239	0.687
$Corr(\varepsilon_0^\theta, \varepsilon_0^w)$	-0.040	0.013
D. SSRS ($t = 2$)		
κ_1	-1.502	0.010
κ_2	-0.723	0.035
κ_3	0.569	0.047
κ_4	1.539	0.067
E. SSRS ($t = 5$)		
κ_1	-1.425	0.182
κ_2	-0.374	0.143
κ_3	0.645	0.129
κ_4	1.360	0.159

Notes: This table shows the estimated parameters of the model presented in Section 4.2. The utility function follows $U(c_t, h_t^p, h_t^f, \theta_t) = \log c_t + \alpha^p h_t^p + \alpha^p h_t^f + \eta\theta_t$. The wage offer obeys $\ln w_t = X_t^{w\prime}\beta^w + \epsilon_t^w$, where X_t^w includes a constant, age, age squared, a dummy for high school diploma and $\epsilon_t^w \sim N(0, \sigma_w^2)$. The production function is given by $\theta_{t+1} = \exp\left(\gamma_0 + \gamma_1 cc_t \mathbf{1}\{a_t \leq 6\}\right) \theta_t^{\gamma_2} c_t^{\gamma_3} \tau_t^{\gamma_4}$.

Nonetheless, the wage offer increases for everyone, capturing a growing labor demand.[29] A high school diploma increases the wage offer by 23%. This estimate is higher than the return to high school graduation of 10% for men estimated by Heckman et al. (2016a,b). The variance of the wage process (0.31) is higher and the autocorrelation coefficient of the unobserved component of the wage offer (0.65) is lower compared to what Blundell et al.

[29]Employment probability for both treatment and control groups grows throughout the covered period 1994-2003. See Miller et al. (2008) and Section 3.2.

(2016) find for women without a high school diploma. Hence, relative to the Blundell et al. sample, New Hope participants face a larger degree of uncertainty regarding future wage shocks.

I show the estimated parameters of the production function and measurement system in Panels C-E. Consumption has a positive effect on human capital. To evaluate the size of this effect, the standard practice is to compute the effect of a 1,000-dollar increase on human capital. In my model, this marginal effect depends on baseline consumption, labor supply, the initial human capital stock, and family composition. Assuming no behavioral responses and holding constant labor supply, a 1,000-dollar boost rises child skills by 0.4% of a standard deviation, which is smaller than what is reported in Dahl and Lochner (2012) and Dahl and Lochner (2016).[30] Nonetheless, as Dahl and Lochner (2012) suggest, their estimate may capture a long-term impact. As I show below, I can reproduce a bigger impact on child human capital when policies giving cash to families are in place for a long time.

Time at home has a positive effect on children's human capital. Conducting a similar experiment as the paragraph above, I compute the effect of going from $h_t = 40$ to $h_t = 0$ (*ceteris paribus*), assuming no one uses child care. This labor supply change rises child human capital by only 7% of a standard deviation. This small effect is at odds with the literature that places a relatively large value of time inputs in the production function of child skills (Cunha et al., 2010; Del Boca et al., 2013; Attanasio et al., 2015). Nevertheless, newer evidence suggests that the productivity of time inputs may be even negative when within-home interactions are of low quality (Elango et al., 2016).

Child care has a sizable effect on child human capital. My estimates imply that choosing child care instead of home care in period t increases child skills by 0.34 standard deviations in period $t+1$. This large effect coincides with findings showing sizable effects of child care relative to home care. Using an index of cognitive skills measures, Kline and Walters (2016) find that the impact of attending Head Start on those who are drawn from home care equal

[30]Bernal (2008) and Del Boca et al. (2013) also find that money plays a modest role in explaining child cognitive outcomes.

0.37 standard deviations. For a different cognitive skills measure, Feller et al. (2016) estimate an effect of 0.23 standard deviations of Head Start versus home care.

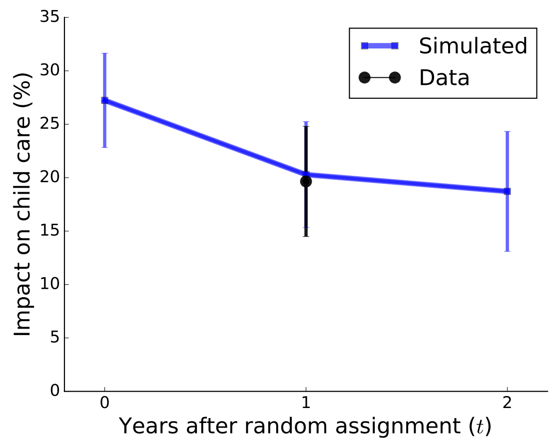
Estimates indicate that the human capital production function contains substantial persistence. The estimated AR(1) coefficient in the production function —the so-called "self-productivity" coefficient in the context of the human capital technology (Cunha and Heckman, 2006)—equals 0.94. The relatively high persistence in the production function is a consistent finding in the literature (Cunha and Heckman, 2006; Cunha et al., 2010; Attanasio et al., 2015); for a similar linear production function, Cunha and Heckman (2008) find an autoregressive coefficient equal to 0.97. Coupled with dynamic complementarities, a strong self-productivity component implies that any shock to the human capital process at early ages has almost permanent consequences for skills production in the future. This feature of the human capital technology has important implications for predicting the effects of policies such as the EITC on human capital acquisition in the long run.

4.4.2 Validation

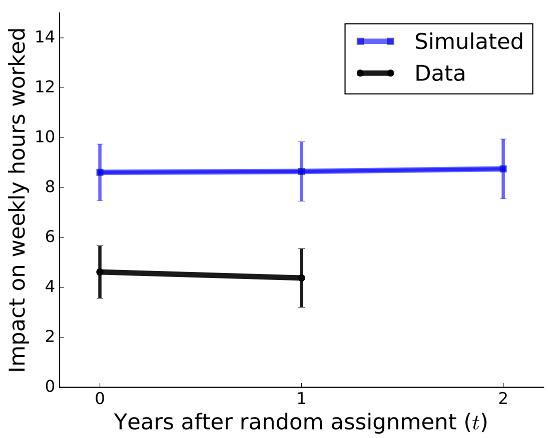
Before presenting the counterfactual experiments, I analyze the model's capacity to predict non-targeted moments. The moments that were not used in estimation exploit the experimental variation induced by the New Hope random assignment. This form of validation is rarely used in the structural literature on child outcomes and household behavior (Bernal, 2008; Del Boca et al., 2013). Figure 4.2 compares the model-generated impact of New Hope on child care, hours worked, and consumption with the estimated effects from the experimental data. The model predicts a higher impact on hours worked (eight hours a week in the model versus 4 to 5 hours in the data), an almost equivalent impact on child care (20 percentage points in the model and in the data), and a higher impact on log per capita consumption (0.7 log-consumption units versus 0.3 in the data).

The lack of predictive power in some of the experimental moments should not affect overall conclusions. The impact of New Hope on child human capital (as I show next)

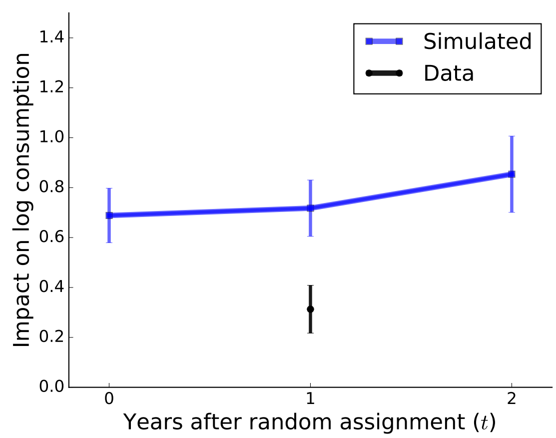
Figure 4.2: Simulates versus observed treatment effects on household variables



(a) Child care



(b) Hours



(c) Log consumption

Notes: The figure compares the estimated effects of New Hope using simulated and actual data. Panel (a) shows treatment effects on child care use. Panel (b) presents treatment effects on hours worked. Finally, panel (c) depicts treatment effects on log consumption.

is mostly explained by the child care component, with income and time playing a minor role. This result is explained because the productivity parameters of income and time are relatively small (see the discussion in the previous section). Hence, the upward bias in predicting the effects of New Hope on income and labor supply is heavily discounted in the production function. Moreover, taking into account the overshooting in the treatment effects on income and hours worked would only reinforce the general qualitative conclusions about the importance of child care in explaining the effects of New Hope on child outcomes.

4.5 Explaining the impact of income and child care subsidies

4.5.1 Understanding the effects of New Hope

Mediation analysis

In this section, I assess the role of labor supply, income, and child care in New Hope's impact on child skills.[31] To this end, I implement a mediation analysis based on the structural dynamic model.[32] Because the objective is to analyze the mediating roles of labor supply, money, and child care, all of the subsequent analyses are based on the sample who were six years of age by period $t = 2$—and so they were exposed to the child care subsidy policy throughout the New Hope period.

Consider the following representation of the early human capital production function:

$$\theta_{t+1}^d = f(\theta_t^d, \tau_t^d, cc_t^d, c_t^d)$$

where $d \in \{0, 1\}$ indicates assignment to the treatment group. The individual-level treatment effect of the program corresponds to $\ln \theta_{t+1}^1 - \ln \theta_{t+1}^0 = f(\theta_t^1, \tau_t^1, cc_t^1, c_t^1) - f(\theta_t^0, \tau_t^0, cc_t^0, c_t^0)$.

[31] With the exception of Epps and Huston (2007), previous literature does not have a formal analysis of the mediating factors that lead to the observed impacts on child outcomes. See Huston et al. (2001, 2005, 2011).

[32] This is similar to the approach taken in Heckman et al. (2013) and Attanasio et al. (2015). To understand the factors mediating the effects on child skills from early childhood interventions, the authors posit a framework that relies less on structural assumptions to estimate the production function. However, since these papers do not explicitly model parents behavior, they cannot generate the types of policy counterfactuals that I present in this paper.

This term can be decomposed as

$$\begin{aligned}
\ln\theta_{t+1}^{1}-\ln\theta_{t+1}^{0} &= \underbrace{[f(\theta_t^1,\tau_t^1,cc_t^1,c_t^1)-f(\theta_t^1,\tau_t^1,cc_t^1,c_t^0)]}_{\text{explained by consumption}} \\
&+ \underbrace{[f(\theta_t^1,\tau_t^1,cc_t^1,c_t^0)-f(\theta_t^1,\tau_t^1,cc_t^0,c_t^0)]}_{\text{explained by child care}} \\
&+ \underbrace{[f(\theta_t^1,\tau_t^1,cc_t^0,c_t^0)-f(\theta_t^1,\tau_t^0,cc_t^0,c_t^0)]}_{\text{explained by time}} \\
&+ \underbrace{[f(\theta_t^1,\tau_t^0,cc_t^0,c_t^0)-f(\theta_t^0,\tau_t^0,cc_t^0,c_t^0)]}_{\text{explained by self-productivity}} \qquad (4.13)
\end{aligned}$$

where each term on the right-hand side identifies the contribution of the corresponding input in explaining the effect of the program.[33]

Figure 4.3 presents the results of decomposing the impact of New Hope as in equation (4.13) for children who were six years old or less by $t = 2$. In this figure, each area depicts the contribution of a change in one input in period t to the program's impact on child human capital in period $t + 1$. The total effect of the program on human capital equals the sum of all areas. For period $t = 1$, child care explains most of the effect of the program (97%), while income and labor supply explain a smaller proportion (17% and −14%). Because of the low estimated productivity of time at home, the negative effect produced by the fall in time spent at home is more than compensated by the positive effects from child care and income. This interaction explains the positive impact of New Hope on child human capital in period $t = 1$.

In the first year after baseline, the total impact on human capital is relatively small: the sum of the contributions adds up to 0.08 standard deviations. However, the effect of New Hope on child human capital becomes larger once we consider the dynamic of human capital accumulation in the long run. We can see this process by looking at the impact at two years after baseline. In this period, the contribution of income and child care to the effect of New Hope on human capital equals 2% and 7% of a standard deviation of human capital,

[33] Given the linearity of $f(.)$ (see equation 4.2), the order of the terms in equation (4.13) does not affect the estimated contributions.

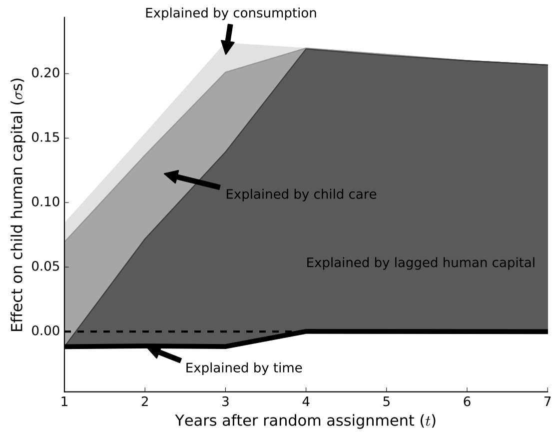
respectively. Furthermore, given the autoregressive coefficient in the production function, a large share of the human capital acquired in period $t = 1$ remains for period $t = 2$; human capital accumulation in $t = 2$ is mostly explained by the human capital gain from the previous period (54%). The additional "investment" in the form more money and child care use plus the influence of self-productivity make the effect of the program on child human capital larger in $t = 2$ than in $t = 1$. Therefore, as the program induces changes in behavior leading to further increases in child human capital, the program's impact on child human capital grows with time. As I show in Section 4.5.2, this process has important implications for interpreting the effect of permanent policies, such as the EITC and the CCDF.

After the program ends, the model predicts a decline in the treatment effects of the program on child skills. The fade-out is explained by the convergence in the levels of income, working hours, and child care use after individuals exit the program. In Figure 4.3, we can see the fade-out starting $t = 4$, where the contributions of income, time, and child care shrink to zero. After this period, following the AR(1) coefficient of the technology, human capital slowly depreciates. Even though the model predicts a decreasing treatment effect after the program ends, it does so at a slower rate than the one that can be estimated using the New Hope data.[34]

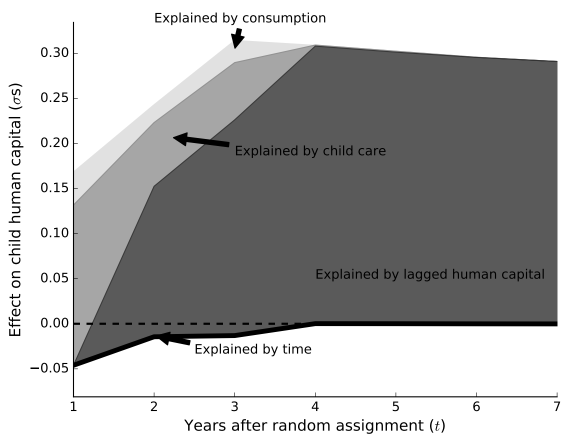
Across all periods, the induced impact on working hours is not large enough to cause an overall negative effect on child human capital. However, this average effect might mask heterogeneity of responses across the wage offer distribution (see Chapter 3). To investigate heterogeneous impacts, panels (b) and (c) of Figure 4.3 plot the structural decomposition of equation (4.13) for two groups: those who would work full-time with the program but not without it (the extensive-margin group) and those who would not work full-time with the program or would work full-time in both scenarios (the intensive-margin group). Relative to the overall average, children of participants who are induced to work full-time have a larger

[34]One potential explanation for a more rapid fade-out is that teachers may have diverted educational resources toward less-advantaged students. The teacher's responses are not modeled here, and so a more pronounced fade-out cannot be exactly reproduced.

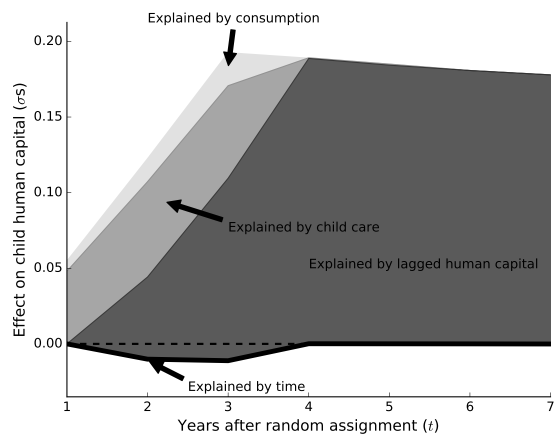
Figure 4.3: Decomposition of the impact of New Hope on child human capital (θ_t)



(a) Overall



(b) Extensive margin



(c) Intensive margin

Notes: The figure plots the share of each input in explaining the effect of the program on the child human capital (θ_t). Panel (a) plots the decomposition for the whole sample. Panel (b) shows the decomposition for the group which were induced to work full-time with the program (that is, they would have work for less than 40 hours a week without it). Panel (c) depicts the decomposition for the group that is not induced to work full-time (would work 40 hours a week without the program or would not work 40 hours with the program).

negative effect originated from the additional parental labor supply. Nevertheless, for this group, the positive contributions of income and child care more than offset that negative labor supply effect. Summing up the individual contribution, by $t = 3$, the program's impact on the extensive-margin group is larger than the average, reaching 0.31 standard deviations. In contrast, for the intensive-margin group, in period $t = 3$, the program increased child human capital by 0.19 standard deviations. The reason for the worse results for the intensive-margin group lies in the lower effects of the program on income and child care use in period $t = 1$ relative to those from the extensive-margin group.

One can use the result of this exercise to quantify the effect of having more income on child skills, holding constant labor supply and child care effects. The mediation analysis provides the necessary ingredients to compute this marginal effect. From equation (4.3), I compute $f(\theta_t^1, \tau_t^1, cc_t^1, c_t^1) - f(\theta_t^1, \tau_t^1, cc_t^1, c_t^0)$ and compare it to the actual rise in consumption $c_t^1 - c_t^0$. The model predicts that New Hope raises consumption by \$1,000 and the contribution of this change in the total effect on skills $(f(\theta_t^1, \tau_t^1, cc_t^1, c_t^1) - f(\theta_t^1, \tau_t^1, cc_t^1, c_t^0))$ equals 0.014 standard deviations. To put this number in perspective, Dahl and Lochner (2012) find that a 1,000-dollar increase in income leads to a raise in 0.06 boost in math and reading test scores. If we assume a three-person household, the Dahl and Lochner estimate implies that a \$1000-dollar increase in per capita income rises test scores by 0.18 standard deviations.

Why does the structural estimation imply a much lower effect of money on child human capital relative to the Dahl and Lochner estimates? My framework suggests two potential reasons. First, the extent to which we can interpret reduced-form studies as pure income effects depends on controlling for other factors. Second, as the authors themselves recognize, the Dahl and Lochner instruments may capture the effects of permanent policies (or temporary policies that affect permanent income). To provide a more direct comparison with the literature, in Section 4.5.2, I predict the effects of the EITC on child human capital. Here, I show that the long-run effects of the EITC do resemble the Dahl and Lochner results.

The effects of New Hope policy bundle

Grogger and Karoly (2009) review the impacts of a series of welfare experiments on children human capital. They suggest that the varied results coming from these experiments may be explained by the different types of policies that each program included. In this section, I study which components in New Hope were more influential in changing behavior within the family and thus children outcomes. I simulate different versions of New Hope and analyze its effects on labor supply, child care use, income, and child academic skills.

Table 4.4 presents the average treatment effect of various combinations of the New Hope policies. In each row, the table depicts the effect of a particular policy on average labor supply, child care use, and consumption per capita from $t = 0$ to $t = 2$, and on child human capital for all periods. Column (6) shows the effects of New Hope as it was originally conceived. To study the full potential effect of these policies, I analyze the sample of children under six years old in period $t = 2$.

Table 4.4: The effects of the New Hope policy bundle

ATE	(1)	(2)	(3)	(4)	(5)	(6)
Consumption (US$)	820	1047	921	172	311	1329
Part-time	0.124	0.145	-0.090	0.013	-0.038	-0.117
Full-time	0.026	-0.014	0.205	-0.034	0.085	0.261
Child care	0.089	0.291	0.085	0.274	0.155	0.220
θ_t (σs)	0.095	0.245	0.078	0.204	0.122	0.187
Wage subsidy	✓	✓	✓			✓
Child care subsidy		✓		✓	✓	✓
Work requirement			✓		✓	✓

Notes: The table shows the impact of New Hope on consumption, part-time work, full-time work, child care, and child human capital. The sample corresponds to children who are six years of age or less by $t = 2$. To estimate impacts, I take averages of annual effects across $t = 0$ to $t = 2$. Each policy (indicated with "✓") is compared to a counterfactual scenario where no policy is implemented.

The exercises from Table 4.4 reveal that New Hope's effect on child outcomes is mostly explained by the child care subsidy component. The child care subsidy alone increases human capital by 0.20 standard deviations (column 4). The wage subsidy, as a unique policy, increases human capital by 0.095 standard deviations (column 1).

The New Hope work requirement causes a negative effect on children's human capital. To see this result, consider columns (4) and (5). In these experiments, I compute the impacts of the child care subsidy with and without the work requirement. Combined with the work requirement, the child care subsidy increases child skills by 0.12 standard deviations (column 5), 0.08 less than the effect of a child care subsidy as a unique policy (column 4). The smaller impact of the combined policy package is explained by a larger effect on employment and a lower child care take-up. Compared to the program with only a child care subsidy, adding the work requirement implies a twelve-percentage-points increase in the average effect on full-time employment. In addition, the effect of the policy bundle on the probability of child care use is eleven percentage points lower than that of the child care subsidy regime alone. The resulting higher labor supply and lower child care take-up of the bundled policy suggest that, for a group of participants, the child care option is valuable only if the policy does not consider the full-time work requirement.

Even though the work requirement may be detrimental to child human capital, the policy could be used nevertheless as a tool to promote work. Yet the counterfactual experiments points to the contrary. The work requirement makes the wage subsidy less attractive for a sample of individuals who would prefer staying out of the labor market but would work part-time under a program without a work requirement. The wage subsidy policy alone (column 1) boosts employment by 15 percentage points, whereas the wage subsidy tied to the work requirement (column 3) increases employment by 12 percentage points. Moreover, the effects on income of both policies are similar.

Table 4.4 shed lights on the potential complementarities arising from putting wage and child care subsidies together. Consider a wage subsidy tied to a work requirement (column 3). This policy raises full-time employment by 21 percentage points. If we add to this policy bundle the child care subsidy (column 6), we see that the effect on full-time work increases an additional four percentage points. Hence, a child care subsidy causes a positive effect on full-time employment, only when it is bundled together with other work incentives. Notably, the child care policy in isolation does not increase full-time employment by these 4 percentage points—in fact, there is a slightly negative effect. This additional impact on full-time employment is explained because some individuals would work only if they have access to a low-cost child care—otherwise, working full-time would mean sacrificing too much child human capital. Since parents are spending less time at home, this mechanism causes a (probably small) negative impact on child skills. Furthermore, depending on the context, one could find that this channel may dominate the positive effects of spending more time at a formal child care center and end up with an overall negative effect of child care.[35] These results suggest that evaluating policies in isolation may ignore other channels—that could potentially reverse the sign of the total impact—derived from the complementarities of policies that are implemented one on top of the other.[36]

4.5.2 The EITC and child care subsidy

For the 2013 fiscal year, 68 billion dollars were spent in the EITC, making it one of the largest mean-tested cash transfers programs in the U.S. (Hoynes and Rothstein, 2016; Moffitt, 2016). Additionally, since the establishment of the CCDF in 1996, child care subsidies have been an important component in the U.S. policy directed at low-income families. In this section, I estimate the impacts of the EITC and a child care subsidy as permanent policies. I evaluate effects on child human capital and examine its causes in terms of income, time, and child care.

In this experiment, the "treatment" consists in having the EITC or a child care subsidy (or both).[37] In period $t = 0$, the individual receives an unexpected policy change. The agent knows exactly how the policy parameters evolve for the rest of the periods. New Hope and work requirements are shut down in this simulation. In contrast to New Hope, the EITC

[35] This situation may occur if the productivity of child care in the production function is sufficiently low (see discussion from Section 3.2).

[36] The bulk of the literature evaluating welfare reforms does not consider that combined policies may not be equal to the reported effects of different policies from different studies. See for example Moffitt (2003) and Moffitt (2016).

[37] Instead of calibrating the actual parameters of the child care subsidy implemented in Wisconsin ("Wisconsin Shares"), I expand the policy implemented in New Hope for all years (without a work requirement). Wisconsin's child care subsidy followed a similar structure to that of New Hope (Bos et al., 1999).

Table 4.5: The effect of the EITC and a child care subsidy

ATE	(1)	(2)	(3)
Consumption (US$ 1,000)	0.12	0.06	0.20
Part-time	0.00	0.02	0.02
Full-time	0.02	0.01	0.03
Child care	0.05	0.16	0.18
EITC (1995-2003)	✓		✓
Child care subsidy		✓	✓

Notes: The table shows the average effect of the EITC and a child care subsidy on consumption, part-time work, full-time work, child care, and child human capital. The sample corresponds to children who are six years of age or less by $t = 2$. To estimate impacts, I take averages of annual effects across all periods for consumption, part-time work, and full-time work, and over $t = 0$ to $t = 2$ for child care.

and the child care subsidy are permanent policies.[38] As with the previous simulations, this experiment uses the sample of children under six years of age or less by $t = 2$.

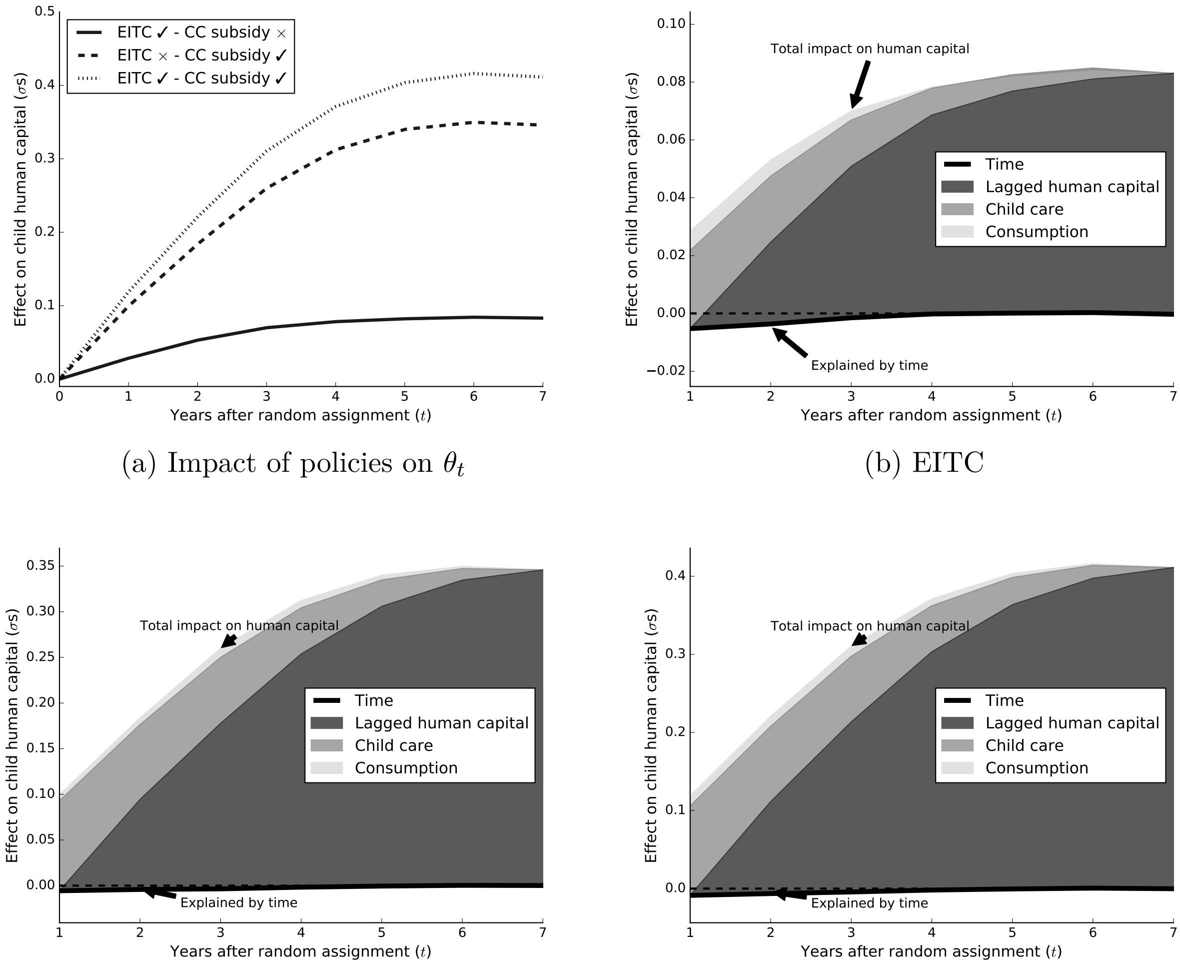
Table 4.5 and Figure 4.4 present the results of these experiments. Table 4.5 presents the average effects for all periods considered (from $t = 0$ to $t = 8$) on household variables. The EITC increases annual per-capita consumption by 120 dollars, the probability of being employed by two percentage points, and the probability of child care by five percentage points.[39] The child care subsidy has similar effects on employment but raises child care probability by 16 percentage points. Both policies combined have larger effects on household variables (column 3).

Figure 4.4, panel (a), depicts the impact on human capital (in standard deviation units) over time. All policies have economically significant impacts once children are being exposed to them for several years. The EITC treatment effect on human capital depicts a concave pattern, reaching 0.08 standard deviations by seven years after random assignment. The effects of the child care subsidy also show a concave profile—as children become older and the

[38]In this model, I am silent about the labor supply effects for married individuals coming from intra-household responses to spouse's income. In this regard, Eissa and Hoynes (2004) find that the EITC has a negative effect on married women's labor force participation, but the impact is relatively small. Moreover, approximately 90% of the women in my sample are single at baseline.

[39]Similarly, Meyer and Rosenbaum (2001) find that the 1993 EITC expansion increased annual employment of single mothers by 3 percentage points.

Figure 4.4: The impact of the EITC and child care subsidy on child human capital (θ_t)



(a) Impact of policies on θ_t

(b) EITC

(c) Child care subsidy

(d) Both policies

Notes: The figure presents the impact of the EITC and child care subsidy on child human capital. Panel (a) collects the impacts of the policies in isolation and of both combined on θ_t, for $t = 1, ..., 8$. Panels (b)-(d) plot the share of each input in explaining the effect of a policy on child human capital.

child care option is not on the individual's choice set—attaining a maximum of 0.35 standard deviations by six years after baseline. The two policies together produce an increasing, positive effect on child human capital. By seven years after baseline, the impact of the EITC and child care subsidy bundle equals 0.41 standard deviations.

Panels (b)-(d) illustrate the results of the mediation analysis (equation 4.13) applied to the EITC and the child care subsidy experiments. As with Figure 4.3, each area represents the contribution of the change in one household input in explaining the total effect of the policy on child human capital. Panel (b) explores the mechanisms behind the impact of the

EITC. In the first year after baseline, the small impact of the policy on human capital is almost entirely explained by the child care component, followed by a smaller income effect. Even though the EITC produces a positive effect on labor supply, the negative contribution to the overall impact on human capital is not big enough to offset the positive effects of income and child care. A large part of the added human capital in period $t = 1$ passes over to the next period, where additional boosts on income and child care probability increase the child human capital stock even further.

Panel (c) shows the decomposition analysis on the child care subsidy policy. The policy's impact is nearly entirely explained by the child care contribution. The additional disposable income (from individuals who were already using child care) and higher labor supply (given a reduced marginal cost of working full-time) are not economically relevant to explain the effects on human capital.

Finally, Panel (d) depicts the decomposition analysis on the combination of the EITC and child care subsidy. Similar to New Hope, the bulk of the human capital effect is accounted by child care. Once again, self-productivity sustains human capital growth from one period to the next, and human capital investment—in the form of higher income and child care use—increases the stock as time passes by. This process implies large effects of both policies taken together in the long run.[40]

4.5.3 Explaining the reduced-form effects of income and child care subsidies through the lens of the structural model

A ubiquitous threat when it comes to interpreting the findings of the literature on the intergenerational effects of welfare policies is that other household behavioral responses to the policy changes are usually not controlled for. Because some of these household responses directly affect child outcomes, it is impossible to disentangle the effect of one particular

[40] Johnson and Jackson (2017), find similar evidence on the complementarity of policies (Head Start and public school spending) in explaining the effects on child outcomes.

household input on child outcomes. As a result, reduced-form studies do not always reveal which mechanisms account for the observed effects.

The structural framework I follow helps to understand the sources of the reduced-form effects of income and child care subsidies. Suppose a policy maker wishes to evaluate the efficacy of giving (unconditionally) cash to families in terms of its effects on children and parental employment. Reduced-form impacts are a result of the increased incentives to spend less time in the labor market and a higher likelihood of choosing child care. Using my model, I find that most of the effects of the EITC actually come from families using more child care.[41] Likewise, most of the child care subsidy effect comes from a higher use of child care.

The reduced-form evidence on the EITC effects on children documents a positive, large effect (Maxfield, 2013; Hoynes et al., 2015a; Bastian and Michelmore, 2017). A general consensus is that a $1,000-boost in income from the EITC increases child test scores by about 0.06 standard deviations. However, the literature has been silent about the timing of this $1,000-boost. If we take this value as a short-term gain, we end up with a substantial effect of the policy (Nichols and Rothstein, 2016): in five years, the impact would be 0.3 standard deviations. To explain such large effects, Dahl and Lochner (2012) suggest that changes in EITC are more correlated to permanent rather than transitory income. My results are consistent with this hypothesis. Given a relatively small productivity of income in the production function of skills, the impact of having the EITC for one year is relatively small. However, self-productivity accumulate these impacts over time. Figure 4.4, panel (b), which depicts the impacts of the EITC alone on child human capital in time, implies a medium-run effect (five years after the average child was exposed to the policy) that is close to what the reduced-form literature finds.

[41] Dahl and Lochner (2016) update the results of Dahl and Lochner (2012) using further instruments to account for the endogeneity of the labor supply variable. They find that results regarding the impact of income on test scores hold even after adding labor supply instrument. They do not account for child care use as a possible input in the test score equation.

One common, emerging result in the literature of child care subsidies is that their impact on children from low-income families is larger than average (Havnes and Mogstad, 2015; Cornelissen et al., 2017). My model predicts that similar, large effects are explained by the fact that the productivity gap of child care relative to home care is sizable (see Figure 4.4, panel c). The large TFP estimate mirrors almost equivalent results from Kline and Walters (2016) and to a lesser extent from Feller et al. (2016). Overall, my results show strong effects from child care policies that are consistent with the literature that documents large short- and long-run effects of early childhood education on children from economically disadvantaged families (Elango et al., 2016).

Finally, there is no evidence about how wage and child care subsidies complement each other in explaining child outcomes. Most of the reduced-form evidence builds on exogenous policy shocks to estimate the effect of either an income or a child care subsidy in isolation. In the context of New Hope, as policies were tied together, one cannot reach definitive conclusions about the effectiveness of income or child care subsidies as unique policy instruments. In Section 4.5.1, I exploit the structure of my model to show that most of the effect of New Hope on children is explained by the child care component. Moreover, as I show in Section 4.5.1, there may be underlying complementarities between policies, adding further complications when evaluating the effects of income and child care policies.

4.6 Conclusions

In this chapter, I present new evidence on the impact of child care and income subsidies on child outcomes. To this end, I use experimental data from New Hope—an anti-poverty program implemented in Milwaukee (1994-1997) which involved both income and child care subsidies that were tied to a full-time work requirement. With these data, I estimate a dynamic-discrete choice model of the household and child academic achievement. I use the model to explain the channels by which New Hope impacted academic achievement and predict the effect of permanent policies such as the EITC and the CCDF on child outcomes.

The structural framework followed in this paper allows for a better understanding of the separate impacts of income and child care subsidies on child human capital. The results suggest that most of the observed New Hope impact on child outcomes is explained by a positive mediating effect of income and child care which more than compensates the negative effect coming from the increase in labor supply. Consistently, I find that policies such as the EITC and the CCDF have positive effects on children's academic achievement. In any case, most of the effects of income and child care subsidies are explained because these policies induce parents to use center-based child care.

A common result arising from the counterfactual experiments is that, after just a few years, these policies only weakly affect child outcomes. This phenomenon occurs because income has a relatively low productivity in the skills production function. However, self-productivity maintains the bulk of the skills stock acquired in a certain period onto the next one. This process implies that small human capital additions caused by income and child care subsidies accumulate in the long run. If policies are shut down, skills fade out in time, as it is reported in the New Hope case.

Two limitations narrow the external validity of my results. First, my findings are only relevant for those who were willing to participate in the New Hope program. Compared to those who were not interested in participating in the program, New Hope's applicants may be better equipped with observed and unobserved characteristics. Second, because of the scale of the New Hope experiment, I cannot analyze general equilibrium effects.[42] Notwithstanding the limitations due to the characteristics of the New Hope experiment, the findings from this paper suggest that income and child care subsidies have an economically significant potential to impact children's academic achievement through the mediating effects

[42]These two issues are also likely to be found in papers using structural models to explain findings from randomized controlled trials. See for example Todd and Wolpin (2006), Attanasio et al. (2011), and Attanasio et al. (2015).

on household behavior. Future research should quantify the importance of income, labor supply, and child care—and other channels—in explaining the impacts of these policies in more general settings.

4.7 Appendix

4.7.1 Welfare parameters

In this appendix, I show the welfare functions that determine disposable income (equation 4.4). I consider three mean-tested programs: the EITC, AFDC, and Food Stamps payments.

The EITC

The EITC parameters vary by state, year, and the number of children (k_t).[43] Denote annual gross earnings as $E_t = w_t h_t \times 52$. Following Chan (2013), there are four key parameters for the federal EITC: the phase-in and phase-out rates ($r^k_{1,t}$ and $r^k_{2,t}$), and the bracket thresholds ($b^k_{1,t}$ and $b^k_{2,t}$), where the index k denote the number of children. k goes from 1 to 3, since the parameters of the EITC schedule do not vary for families with more than three children. In year t and for a family with $k_t = k$ number of children, the federal EITC payment ($EITC^f_t$) follows:

$$EITC_t = \begin{cases} r^k_{1,t} E_t & \text{if } E_t < b^k_{1,t} \\ r^k_{1,t} b^k_{1,t} & \text{if } b^k_{1,t} \leq E_t < b^k_{2,t} \\ \max\left\{ r^k_{1,t} b^k_{1,t} - r^k_{2,t}(E_t - b^k_{2,t}), 0 \right\} & \text{if } E_t \geq b^k_{2,t} \end{cases}$$

In the case of Wisconsin, the state EITC payment ($EITC^s_t$) is determined as a fraction of the federal payment: $r^k_{s,t} EITC^f_t$, where $0 < r^k_{s,t} < 1$ varies by number of children and year. The total EITC payment equals $EITC_t = EITC^f_t + EITC^s_t$.

[43] The federal parameters can be found at `http://www.taxpolicycenter.org/sites/default/files/legacy/taxfacts/content/PDF/historical_eitc_parameters.pdf`. The parameters for the state of Wisconsin are obtained from `http://users.nber.org/~taxsim/state-eitc.html`.

The AFDC and TANF

The AFDC parameters vary by family composition and by year. Starting 1997, the state of Wisconsin implemented "Wisconsin Works" (W-2), under the TANF umbrella. Instead of giving cash transfers like most states did, W-2 offered paid CSJs for up to 5 years. In terms of the model then, a W-2 salary becomes part of the potential wage offer (equation 4.3).

Until 1996 (that is, periods $t = 0$ and $t = 1$), the standard AFDC program was in place. Let B_t^* be the cash transfer an individual under welfare could get, given by:

$$B_t^* = \max\left\{\min\left\{\overline{B}, \overline{B} - (E_t - 30) \times .67\right\}, 0\right\},$$

where $\overline{B}$ is the so-called "benefit standard," the maximum amount of welfare an individual is entitled to. Individuals enter the program if $E_t \leq c$. The parameters c and $\overline{B}$ vary by family size and state.[44] This formula captures the \$30-and-a-third policy implemented in 1967: the recipient keeps the first 30 dollars she makes. Above that value, for each dollar she earns, she must "pay a tax" of 0.67 (the marginal tax rate is 67%). In practice, the formula is designed for monthly figures, so I adapted parameters to accommodate for annual income.

SNAP

The Supplemental Nutrition Assistance Program (SNAP)—formerly known as Food Stamps—is the largest nutrition program in the U.S. The program provides money vouchers to eligible individuals to spend food in grocery stores.

Unlike the AFDC, SNAP eligibility and voucher parameters have not changed much in time. It does not vary by state either. Let E_n be net income, E gross earned income, B welfare payments (including AFDC and New Hope cash transfers), SD a standard deduction,

[44] The exact values can be found in the Welfare Rules Database for Wisconsin, Area 1.

and e the poverty guideline. To receive SNAP, a household must meet the gross and net income tests:[45]

$$E < 1.3e,$$

$$E_n < e,$$

where net income follows[46]

$$E_n = 0.8E + B - SD$$

The SNAP benefits are determined by the following formula:

$$S^* = \max\left\{MaxB - 0.3E_n, 0\right\},$$

where $MaxB$ is the Maximum allotment. All income thresholds and other parameters are adjusted following Social Security's Cost-of-Living Adjustments.[47]

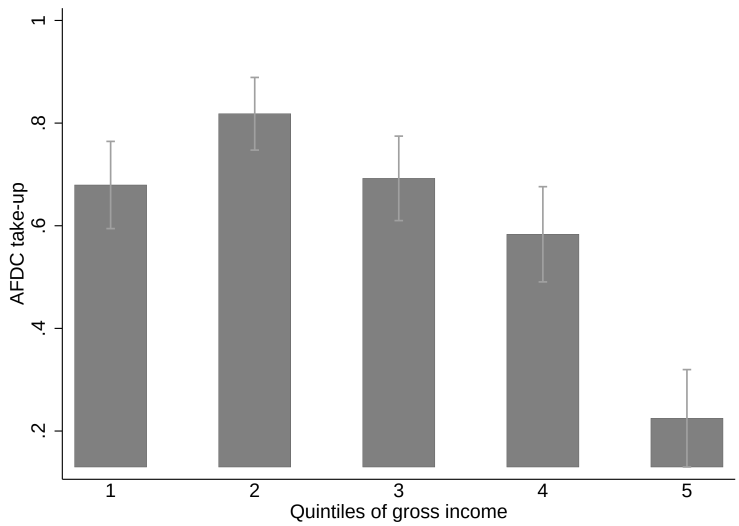
[45] Also, if a family is living only with AFDC payments, then it is automatically eligible. For the purpose of this paper, I assume that if a participant is not working, then she is eligible for SNAP payments.

[46] The actual formula includes a standard shelter deduction, which I assume to be zero for all families.

[47] Maximum allotments ($MaxB$) are taken from http://www.fns.usda.gov/snap/fy-2004-allotments-and-deduction-information. See https://www.ssa.gov/oact/cola/colaseries.htmlforcost-of-livingadjustments. For poverty guidelines, see https://www.ssa.gov/policy/docs/statcomps/supplement/2014/3e.html#table3.e8.

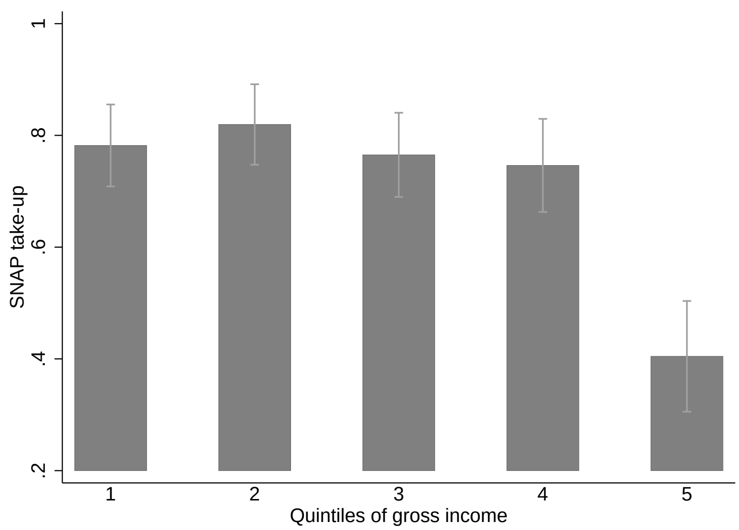
Take-up rates of AFDC and SNAP

Figure 4.5: Take-up rate of AFDC



Notes: The figure shows the estimated take-up rate across gross income quintiles during the year of random assignment (1994-1995). The take-up rate is defined as the proportion of eligible families who received a AFDC payment during the year. Gross income corresponds to the sum of quarterly earnings in the year of random assignment. The bars show the point estimate of the take-up rate with a 95% boostrap confidence interval (1,000 draws).

Figure 4.6: Take-up rate of SNAP



Notes: The figure shows the estimated take-up rate across gross income quintiles during the year of random assignment (1994-1995). The take-up rate is defined as the proportion of eligible families who received at least one SNAP check over the course of the year. Gross income corresponds to the sum of quarterly earnings in the year of random assignment. The bars show the point estimate of the take-up rate with a 95% boostrap confidence interval (1,000 draws).

4.7.2 Identification of the production function

In this appendix, I show how we can identify the human capital production function (equation 4.2) using ordinal measures of academic achievement. The proof borrows insights from Cunha et al. (2010) and Agostinelli and Wiswall (2016a) in showing the identification of a production function under a particular measurement error structure.

I focus on the following production function:

$$\ln \theta_{t+1} = f(\theta_t, c_t, \tau_t) + \mu cc_t \mathbb{I}\{a_t \leq 6\} \tag{4.14}$$

where $f(.)$ is such that, for some point $(\theta_t^{'}, c_t^{'}, \tau_t^{'})$, $f(\theta_t^{'}, c_t^{'}, \tau_t^{'})$ does not depend on an unknown coefficient.[48] The above function describes a technology that varies by child care choice (and thus by age), but it is otherwise constant in time—a key feature of the identification result.

The measures of academic achievement are ordinal variables. These measures rank the child in the classroom academic achievement distribution. For a measure M_t in period t, we have

$$M_t = \begin{cases} 1 & \text{if } \ln \theta_t + \epsilon_t^z \leq \kappa_{1,t} \\ 2 & \text{if } \kappa_1 < \ln \theta_t + \epsilon_{t,m}^z \leq \kappa_{2,t} \\ \vdots & \\ 5 & \text{if } \ln \theta_t + \epsilon_t^z > \kappa_{4,t}. \end{cases} \tag{4.15}$$

where

$$\epsilon_t^z \sim N(0,1) \qquad \forall t. \tag{4.16}$$

[48] Agostinelli and Wiswall (2016a) introduces the concept of "known location and scale" (KLS). A production function is KLS if, for two non-zero vectors $(\theta_t^{'}, c_t^{'}, \tau_t^{'})$ and $(\theta_t^{''}, c_t^{''}, \tau_t^{''})$ such that $\theta_t^{'} \neq \theta_t^{''}$, $c_t^{'} \neq c_t^{''}$, and $\tau_t^{'} \neq \tau_t^{''}$, $f(\theta_t^{'}, c_t^{'}, \tau_t^{'})$ and $f(\theta_t^{''}, c_t^{''}, l\tau_t^{''})$ do not depend on unknown parameters. For the production function from equation (4.2), the KLS property holds only if the sum of the coefficients sum up to 1, that is, a constant-return-to-scale production function. Because I am assuming a classical measurement error structure, I do not need the production function to be KLS.

Underlying equation (4.16) there are two essential assumptions. First, the coefficient associated to $\ln\theta_t$ equals 1 for all t—a classical measurement-error model. Second, the cutoffs are constant across child age and child care types.

The problem is to identify (4.14) and the parameters of the measurement system (4.15), given (4.16). I follow Agostinelli and Wiswall (2016a) to prove the following Lemma.

Lemma 1. *Suppose the production function and the measurement system follow (4.14), (4.15), and (4.16). Then* $\Phi^{-1}\left[Pr\left(M_{t+1}=5 \mid \ln\theta_t=\bar{\theta}, \ln c_t=\bar{c}, \ln\tau_t=\bar{\tau}\right)\right]$ *(where* $\Phi(.)$ *denotes a standard normal cdf) is identified with two measures* M_t *and* M_{t+1} *and it is equal to* $f\left(e^{\bar{\theta}}, e^{\bar{c}}, e^{\bar{\tau}}\right) - \kappa_{4,t+1}$.

Proof. To simplify notation, let us abstract for a moment of any age-induced difference in the technology of human capital, and so $\ln\theta_{t+1} = f(\theta_t, c_t, \tau_t)$. First, note that the cutoffs from period t ($\kappa_{j,t}$) are identified by the normality assumption on the measurement error.[49] Second, given equations (4.14) and (4.15),

$$\Phi^{-1}\left[Pr(M_{t+1}=5 \mid \theta_t=\bar{\theta}, c_t=\bar{c}, \tau_t=\bar{\tau})\right] = f\left(e^{\bar{\theta}}, e^{\bar{c}}, e^{\bar{\tau}}\right) - \kappa_{4,t+1}.$$

Similarly for period t, we can express $\ln\theta_t=\bar{\theta}$ as $\bar{\theta} = \Phi^{-1}\left[Pr\left(M_t=5 \mid \theta_t=\bar{\theta}\right)\right] + \kappa_{4,t}$, where $\kappa_{4,t}$ is known and $\Phi^{-1}\left[Pr\left(M_t=5 \mid \theta_t=\bar{\theta}\right)\right]$ can be constructed using observed data.

Hence,

$$\begin{aligned}\Phi^{-1}\left[Pr(M_{t+1}=5 \mid \theta_t=\bar{\theta}, c_t=\bar{c}, \tau_t=\bar{\tau})\right] = \Phi^{-1}\{Pr(M_{t+1}=5 \mid \bar{\theta}=\Phi^{-1}\left[Pr\left(M_t=5 \mid \theta_t=\bar{\theta}\right)\right] \\ +\kappa_{4,t}, c_t=\bar{c}, \tau_t=\tau)\}\end{aligned}$$

Because the expression $\Phi^{-1}\left[Pr\left(M_t=5 \mid \theta_t=\bar{\theta}\right)\right]+\kappa_{4,5}$ is known for any $\bar{\theta}$, the equation above shows that we can identify $\Phi^{-1}\left[Pr(M_{t+1}=5 \mid \theta_t=\bar{\theta}, c_t=\bar{c}, \tau_t=\tau)\right]$ with data on M_t, c_t, and l_t. □

[49]The proof follows the standard discrete-choice analysis. In general, a constant term and the variance are not identified in an discrete-ordered framework.

Now we can use $\Phi^{-1}\left[Pr(M_{t+1}=5 \mid \theta_t=\bar{\theta}, c_t=\bar{c}, \tau_t=\bar{\tau})\right]$ to identify the production function. This result is summarized in the following proposition.

Proposition 1. *Suppose that (i) the conditions from Lemma 1 hold and (ii) for some point* $(\theta_t^{'}, c_t^{'}, \tau_t^{'})$*,* $f(.)$ *is known (do not depend on unknown coefficients). Then the technology of academic skills formation (equation 4.14) is identified.*

Proof. Let us start with the production function of a young child ($a_t \leq 6$) in home care ($cc_t = 0$). Using Lemma (1), we can identify $\Phi^{-1}\left[Pr(M_{t+1}=5 \mid \theta_t=\bar{\theta}, c_t=\bar{c}, \tau_t=\bar{\tau})\right]$ for any $\bar{\theta}, \bar{c}$, and $\bar{\tau} \in \mathbb{R}$. Furthermore, since we know the value of $f(.)$ for some point, we can eliminate the unknown parameter $\kappa_{5,t+1}$. Choose a point $(\hat{\theta}, \hat{c}, \hat{\tau})$ such that $f\left(e^{\hat{\theta}}, e^{\hat{c}}, e^{\hat{\tau}}\right) = \alpha$ is known, and note that

$$\Phi^{-1}\left[Pr(M_{t+1}=5 \mid \theta_t=\bar{\theta}, c_t=\bar{c}, \tau_t=\tau)\right] - \Phi^{-1}\left[Pr(M_{t+1}=5 \mid \theta_t=\hat{\theta}, c_t=\hat{c}, \tau_t=\hat{\tau})\right] = f_y\left(e^{\bar{\theta}}, e^{\bar{c}}, e^{\bar{\tau}}\right) - \alpha.$$

Therefore, given that the left-hand side is identified because of Lemma 1, we can identify the production function $f(.)$ in home care by varying $\bar{\theta}, \bar{c}$, and $\bar{\tau}$ over their support. Since, by assumption, the production of old and young children in home care is the same, we have also identified the production function of old children.

To identify the production function of a child in child care, we need to identify the TFP parameter (μ). To this end, we can exploit the fact that the cutoffs do not vary by child care choices.[50] Given that $f(.)$ is identified for any $\bar{\theta}, \bar{c}$, and τ and by Lemma 1, we can obtain the TFP term as follows:

$$\Phi^{-1}\left[Pr(M_{t+1}=5 \mid \theta_t=\bar{\theta}, c_t=\bar{c}, \tau_t=\bar{\tau})\right] - f\left(e^{\bar{\theta}}, e^{\bar{c}}, e^{\bar{\tau}}\right) - \kappa_{4,t+1} = \mu.$$

[50] The KLS property precludes from estimating production functions with TFP dynamics (for example, $f(.) + \mu$). Given that cutoffs are constant within the group of young children, we can recover the TFP parameter in the young child production function. Agostinelli and Wiswall (2016a) states a similar argument to identify a general class of production functions.

Finally, $\kappa_{1,t+1}$, $\kappa_{2,t+1}$ and $\kappa_{3,t+1}$ can be identified using any production function. For example, using a similar reasoning to that of Lemma 1, we can identify $\Phi^{-1}[1 - Pr(M_{t+1} \geq 4 \mid \theta_t = \bar{\theta}, c_t = \bar{c}, \tau_t = \bar{\tau})]$. Given that $f\left(e^{\bar{\theta}}, e^{\bar{c}}, e^{\bar{\tau}}\right)$ is identified, then

$$\Phi^{-1}[1 - Pr(M_{t+1} \geq 4 \mid \theta_t = \bar{\theta}, c_t = \bar{c}, \tau_t = \bar{\tau})] - f\left(e^{\bar{\theta}}, e^{\bar{c}}, e^{\bar{\tau}}\right) = -\kappa_{3,t+1}.$$

Following an analogous argument, we can identify $\kappa_{1,t+1}$, $\kappa_{2,t+1}$ and $\kappa_{3,t+1}$. $\square$

4.7.3 Control function estimation for the wage offer

This section describes the procedure to obtain consistent estimates of the wage offer equation using a control function approach. The comparison of these estimates with that of the structural framework provides a simple way of evaluating the validity of structural assumptions.

Consider the log wage equation. Here, we only observe data for those who choose to work. Let $d_i = 1$ denote that individual i works and 0 otherwise. We have that

$$\log w_i = X_i'\beta + \varepsilon_i, \tag{4.17}$$

where $E[\varepsilon_i \mid X_i, d_i = 1] \neq 0$. Suppose that the decision to work depends on X_i and on a vector of variables Z_i not included in equation (4.17). We can define $E[\varepsilon_i \mid X_i, d_i = 1] \equiv \xi(X_i, Z_i)$. If we are able to account for $\xi(X_i, Z_i)$, then we have a well-behaved equation, as follow:

$$\log w_i = X_i'\beta + \xi(X_i, Z_i) + \underbrace{\varepsilon_i - \xi(X_i, Z_i)}_{\nu_i}, \tag{4.18}$$

where $E[\nu_i \mid X_i, Z_i] = 0$.

A flexible way of estimating $\xi(X_i, Z_i)$ is to form polynomials of the propensity score for the probability of working, $p(X_i, Z_i)$. I use the structural model to obtain individual-level estimated values for $p(X_i, Z_i)$. In doing it, I am implicitly using the exogenous shocks to the budget set (differential exposure to changes in welfare, the EITC, and New Hope) as the instruments Z_i. I estimate equation (4.18) by adding third-degree polynomials of $\hat{p}(X_i, Z_i)$ obtained through the structural simulation. I estimate the constant by taking the sample mean of $\log w_i - X_i'\hat{\beta}$, where $\hat{\beta}$ are the OLS coefficients of equation (4.18). Table 4.6 show the results.

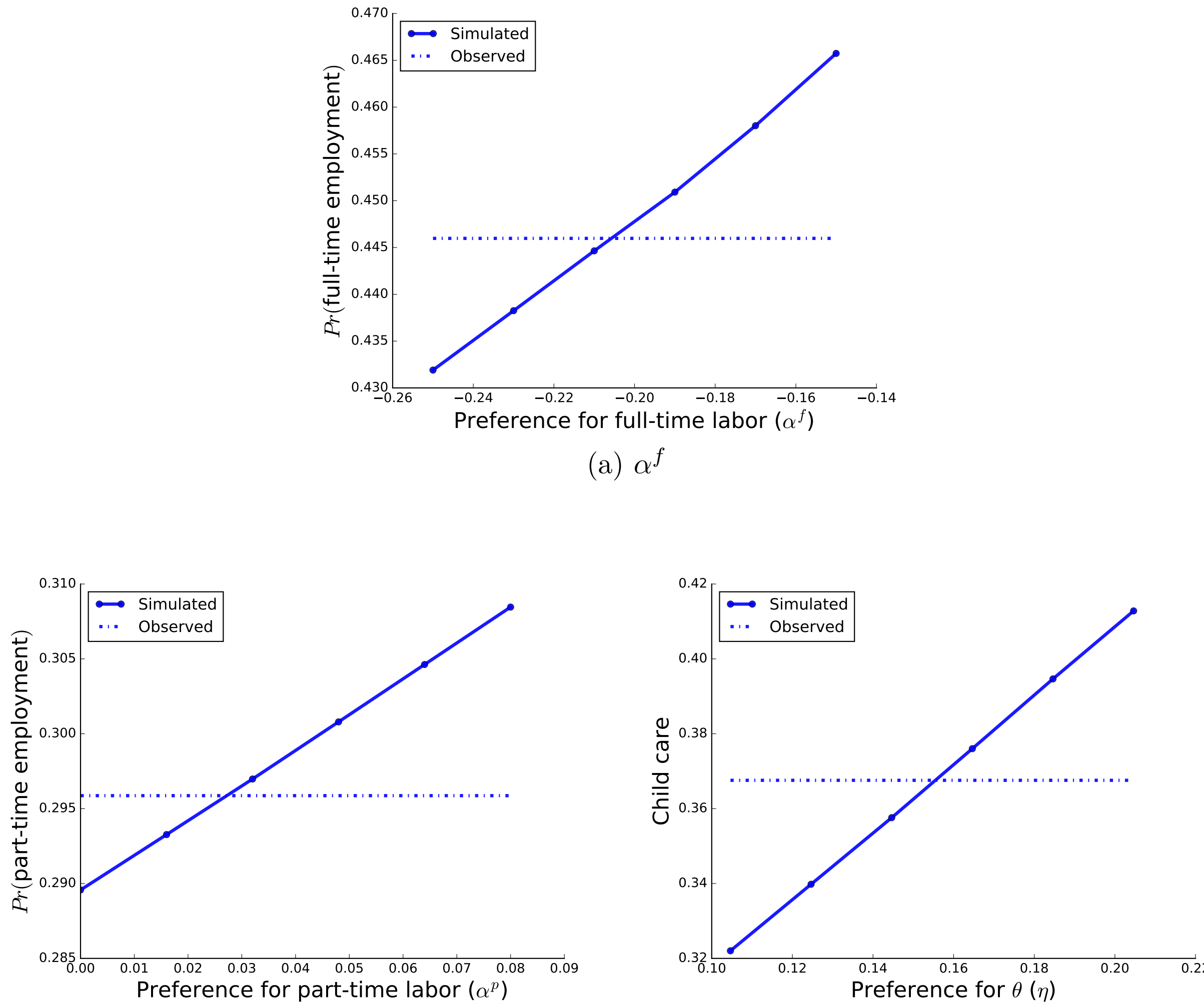
Table 4.6: Auxiliary estimates used in the GII estimation

Variables	Structural	Control function
Age	-0.022	-0.022
Age^2	0.000	0.000
High school	0.227	0.283
$\log(t)$	0.384	0.403
Constant	1.449	1.452

Notes: The table compares the estimated coefficients of the wage offer process obtained from the structural framework (first column) and from the control function approach (second column).

4.7.4 Local identification from targeted moments

Figure 4.7: Target moments locally identify structural parameters: utility function



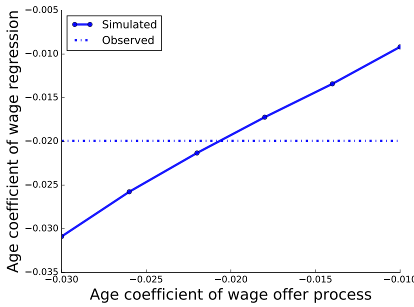
(a) α^f

(b) α^p

(c) η

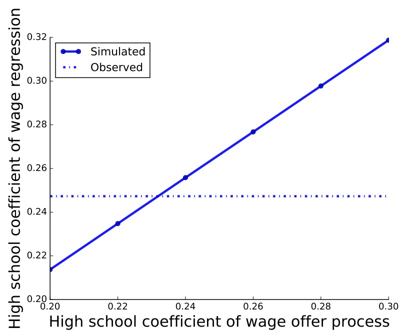
Notes: The figure depicts how a target moment is able to locally identify a structural parameter. The solid line illustrates the simulated moment as a function of a structural parameter. The dashed line shows the observed moment.

Figure 4.8: Target moments locally identify structural parameters: wage offer

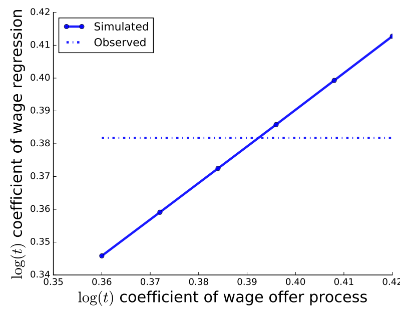


(a) Age

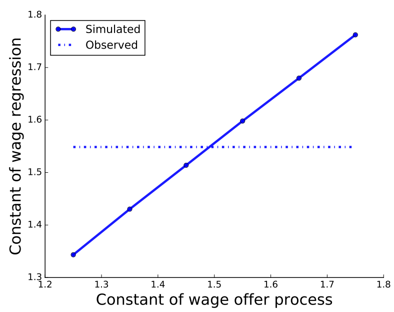
(b) Age^2



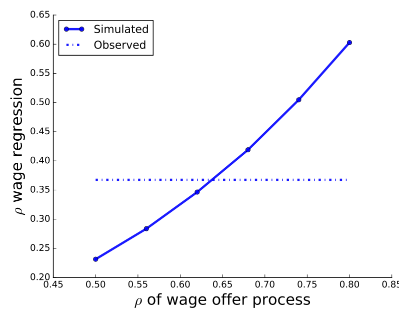
(c) High school



(d) $\ln t$



(e) Constant

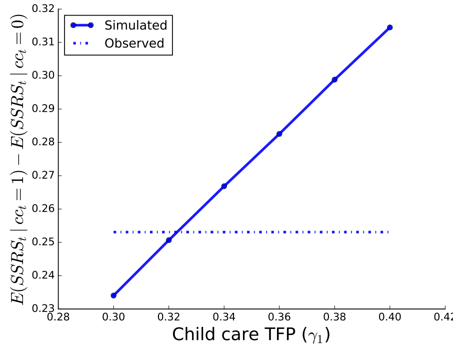


(f) ρ

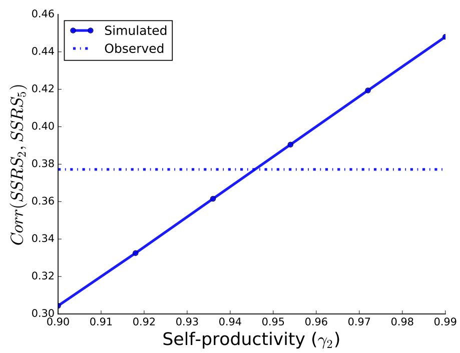
(g) Variance

Notes: The figure depicts how a target moment is able to locally identify a structural parameter. The solid line illustrates the simulated moment as a function of a structural parameter. The dashed line shows the observed moment.

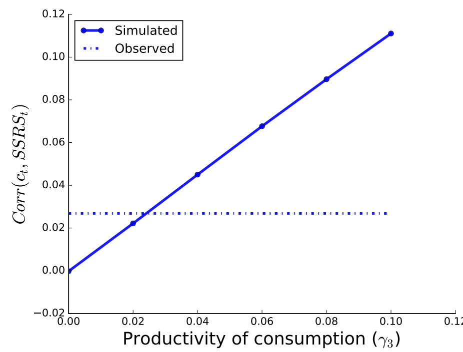
Figure 4.9: Target moments locally identify structural parameters: production function



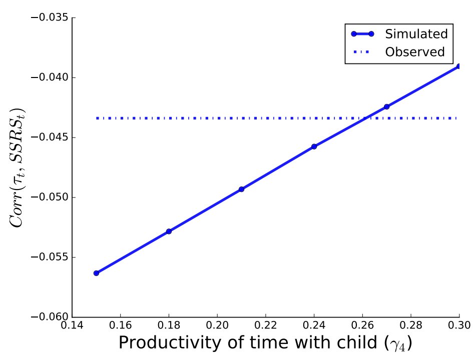
(a) $\gamma 1$



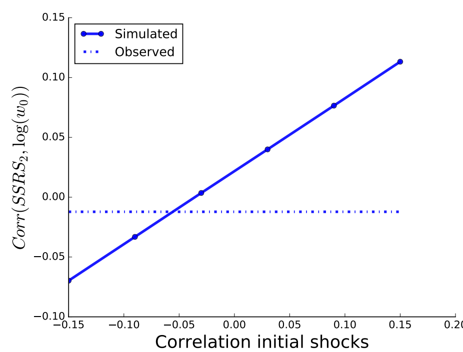
(b) $\gamma 2$



(c) γ_3



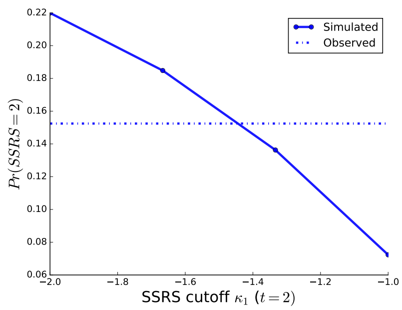
(d) γ_4



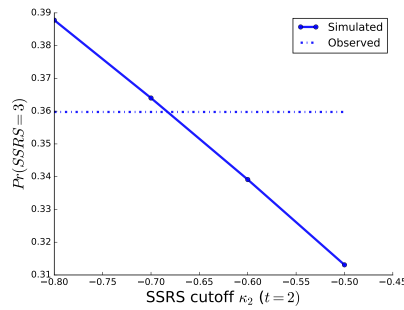
(e) $Corr(\varepsilon_0^\theta, \varepsilon_0^w)$

Notes: The figure depicts how a target moment is able to locally identify a structural parameter. The solid line illustrates the simulated moment as a function of a structural parameter. The dashed line shows the observed moment.

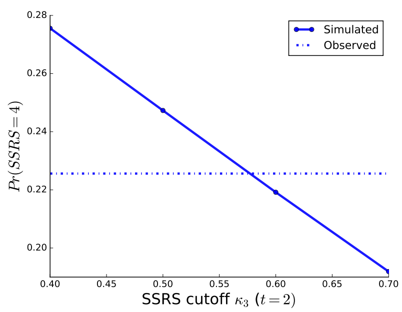
Figure 4.10: Target moments locally identify structural parameters: measurement system



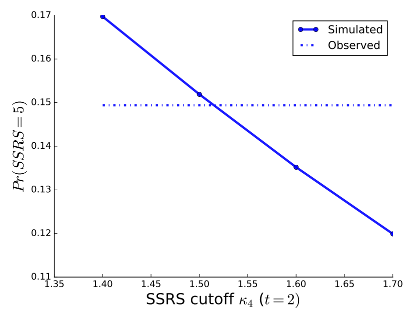
(a) κ_1 $(t = 2)$



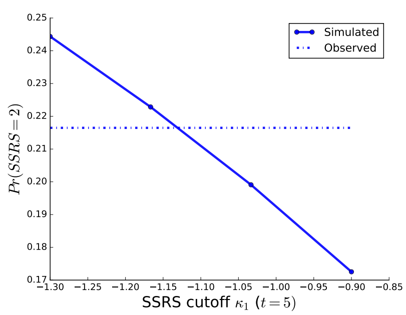
(b) κ_2 $(t = 2)$



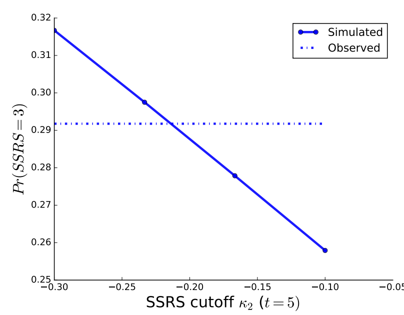
(c) κ_3 $(t = 2)$



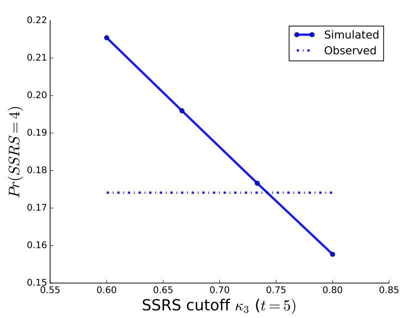
(d) κ_4 $(t = 2)$



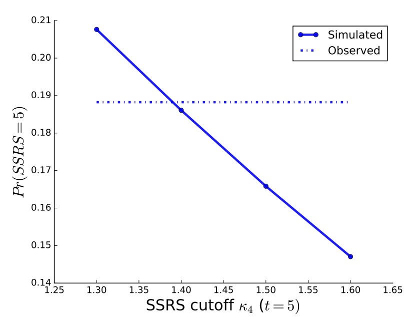
(e) κ_1 $(t = 5)$



(f) κ_2 $(t = 5)$



(g) κ_3 $(t = 5)$



(h) κ_4 $(t = 5)$

Notes: The figure depicts how a target moment is able to locally identify a structural parameter. The solid line illustrates the simulated moment as a function of a structural parameter. The dashed line shows the observed moment.

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