ESSAYS ON PRESCHOOL EDUCATION, FAMILY ECONOMIC CIRCUMSTANCES, AND CHILD OUTCOMES

by

Mariana Zerpa Reisch

A Dissertation Submitted to the Faculty of the

DEPARTMENT OF ECONOMICS

In Partial Fulfillment of the Requirements
For the Degree of

DOCTOR OF PHILOSOPHY

In the Graduate College

THE UNIVERSITY OF ARIZONA

2017

THE UNIVERSITY OF ARIZONA
GRADUATE COLLEGE

As members of the Dissertation Committee, we certify that we have read the dissertation prepared by Mariana Zerpa Reisch, titled Essays on Preschool Education, Family Economic Circumstances, and Child Outcomes and recommend that it be accepted as fulfilling the dissertation requirement for the Degree of Doctor of Philosophy.

____ Date: May 11, 2017

Jessamyn Schaller

____ Date: May 11, 2017

Price Fishback

____ Date: May 11, 2017

Ronald L. Oaxaca

____ Date: May 11, 2017

Tiemen Woutersen

Final approval and acceptance of this dissertation is contingent upon the candidate's submission of the final copies of the dissertation to the Graduate College.

I hereby certify that I have read this dissertation prepared under my direction and recommend that it be accepted as fulfilling the dissertation requirement.

____ Date: May 11, 2017

Dissertation Director: Jessamyn Schaller

STATEMENT BY AUTHOR

This dissertation has been submitted in partial fulfillment of the requirements for an advanced degree at the University of Arizona and is deposited in the University Library to be made available to borrowers under rules of the Library.

Brief quotations from this dissertation are allowable without special permission, provided that an accurate acknowledgment of the source is made. Requests for permission for extended quotation from or reproduction of this manuscript in whole or in part may be granted by the head of the major department or the Dean of the Graduate College when in his or her judgment the proposed use of the material is in the interests of scholarship. In all other instances, however, permission must be obtained from the author.

SIGNED: Mariana Zerpa Reisch

ACKNOWLEDGMENTS

I would like to thank my main advisor, Jessamyn Schaller, who has been an incredible mentor and role model. Jessamyn has always given me excellent advice, and has been extremely supportive and encouraging. I am also very grateful for the opportunities she gave me to work with her, first as her research assistant and later as her coauthor. I am very thankful to the other members of my committee, Price Fishback, Ronald L. Oaxaca and Tiemen Woutersen. Throughout the doctoral program, Price has provided me with constant support and enthusiasm. Ron has taught me how to use rigorous analytical models in labor economics, and has been very generous with his support and guidance. I am very grateful to Tiemen for teaching me econometrics, and for his continued advice and mentoring. I am also thankful to all the faculty of the Department of Economics for their support during my time in the program, and especially to Gary Solon for his invaluable guidance, and to John Drabicki for the opportunities he gave me to strengthen my skills as an economics teacher. Thank you also to the Department's staff for the invaluable support they have provided me, and most especially to Liz Jenkins, Michelle Piontek, and Laurie Princiotto.

I gratefully acknowledge the financial support of the Washington Center for Equitable Growth and the University of Arizona Graduate and Professional Student Council. Part of the research in this dissertation was conducted at the National Center for Health Statistics (NCHS) and the Agency for Healthcare Research and Quality (AHRQ) Data Centers. I am very thankful to Ray Kuntz for his support at the AHRQ Research Data Center, and to Pat Barnes of the NCHS Reserch Data Center. The findings and conclusions expressed are solely those of the authors and do not represent the views of the Washington Center for Equitable Growth, the NCHS, the AHRQ or the Department of Health and Human Services.

I have been very fortunate to make amazing friends who have made this journey a lot more enjoyable and enriching. I am very grateful for my friends and fellow students at the

University of Arizona, and for the friendship of Marianela, Ricardo, Nadia, Juan, and the many more friends who made this time in Tucson unforgettable. Thanks to my parents, Silvia and Oscar, for always encouraging me to study and pursue my goals, and to my Sister, Claudia, and my mother-in-law, Yolanda for their support. Finally, I would like to thank and dedicate this dissertation to Seba. His constant encouragement, patience, and contagious passion for Economics have pushed me forward. I am looking forward for the next part of our journey.

Contents

List of Figures

List of Tables

Abstract

The goal of this dissertation is to analyze the effects of public programs and parental labor market outcomes child health and human capital accumulation. The first chapter studies the effects of attending state pre-kindergarten programs on child development and health up to eight years after preschool age. I find that the implementation of a pre-K program in a state reduces the utilization of special education services by boys within four years of preschool age, and improves boy's developmental outcomes five to eight years after preschool age. I also find evidence that boys and girls in states with pre-K programs have increased health problems in the short-term. The second chapter analyzes the effects of parental job loss on children's health. The findings show that a father's job loss is detrimental to children's mental health, and among children in low-socioeconomic status families it is also associated with worse physical health. By contrast, the results show no evidence of maternal job loss having detrimental effects on child health. The third chapter evaluates whether two of the largest social insurance programs in the U.S.– Unemployment Insurance (UI) and public health insurance (Medicaid/CHIP)–mitigate the effects of parental job loss on children's health insurance coverage and health care access in the short run. The results show that more generous Medicaid/CHIP eligibility rules mitigate increases in out-of-pocket expenditures observed after job loss, while it only increases the likelihood of taking up public insurance slightly for children who were insured through a parent's employer before the job loss. More generous UI replacement rates, on the other hand, have a negative effect on child health insurance coverage, by decreasing the likelihood of taking up public insurance.

Introduction

This dissertation explores the effects of public programs and parental labor market outcomes on health and the formation of human capital during childhood. There is a large body of evidence on the importance of early childhood development on future outcomes. The socio-economic gaps in health and in cognitive and non-cognitive abilities that help to explain differences in adult outcomes are present even before starting school, implying a significant role for public policies that help narrow these gaps in improving equality of opportunity and reducing the intergenerational transmission of poverty.

The first chapter analyzes the effects of attending state pre-kindergarten programs on child development and health up to eight years after preschool age. Using data from the National Health Interview Survey, the Current Population Survey, state legislature and other sources, I exploit the variation in the timing of expansion of pre-K programs across states to look at the effects of a large group of state pre-K programs. The *intent-to-treat* estimates indicate that boys who live in states that had pre-K programs when they were 4 years old are less likely to receive special education services in the following four years, and their developmental outcomes are improved five to eight years after preschool age. I also find that boys and girls in states with pre-K programs have increased health problems in the short-term. The effect of this group of pre-K expansions on preschool enrollment rates is close to 8 percentage points, but I find suggestive evidence that the expansion of enrollment in state pre-K is larger and there are crowding-out effects. I use two alternative ways of approximating *treatment-on-the-treated* effects, and find that my results imply large effects on both developmental and health outcomes.

Recent research suggests that parental job loss has negative effects on children's outcomes, including their academic achievement and long-run educational and labor market outcomes. In the second chapter, co-authored with Jessamyn Schaller, we turn our attention to the effects of parental job loss on children's health. We combine health data from 16 waves of the Medical Expenditure Panel Survey, which allows us to use a fixed effects specification and still have a large sample of parental job displacements. We find that paternal job loss is detrimental to children's mental health, and among children in low-socioeconomic status (SES) families it is also associated with increases in the incidence of fair or poor physical health, injuries, and infectious conditions. By contrast, we find that maternal job loss does not have detrimental effects on child health, and in fact leads to small reductions in the incidence of infectious conditions among children in high-SES families. Increases in public health insurance coverage compensate for a large share of the loss in private coverage that follows parental displacement, and we find no significant changes in medical care utilization.

The third chapter, co-authored with Chloe East, Elira Kuka and Jessamyn Schaller, evaluates whether two of the largest social insurance programs in the U.S.–Unemployment Insurance (UI) and public health insurance (Medicaid/CHIP)–mitigate the effects of a father's job loss on children's health insurance coverage and health care access in the short run. We use simulated measures of program generosity to capture plausibly exogenous variation in state policy generosity over time, combined with data from the Medical Expenditure Panel Survey (MEPS). The MEPS allows us to follow fathers and their children before and after a job loss, and examine a wide range of outcomes including health insurance coverage, health care utilization, and health care expenditures. Our results show that, for children who were insured through a parent's employer before the job loss, more generous Medicaid/CHIP eligibility rules cause a small increase in the likelihood of taking up public insurance. We also find that out-of-pocket expenditures are less likely to increase after job loss in states with more generous Medicaid/CHIP, while we do not find robust evidence of short-term effects on health care utilization. Finally, our results show that more generous UI replacement rates have a negative effect on child health insurance coverage, by decreasing the likelihood of taking up public insurance. Our work is the first to shed light on whether the detrimental effects of parental job loss can be mitigated by transfers to the family.

Chapter 1

Preschool Attendance and Child Development and Health: Evidence from State Pre-K Programs

1.1 Introduction

Enrollment in state-funded pre-kindergarten (pre-K) programs has grown dramatically since the 1990s, making state governments as a group today's largest provider of preschool education for 4-year-olds in the United States. By the 2014-2015 school year, 42 states and the District of Columbia were offering pre-K programs for 4-year-olds, and 29% of 4-year-olds (close to 1.2 million children) attended state-funded pre-K programs, accounting for 43% of total preschool enrollment and over two-thirds of total public enrollment of 4-year-olds.[1] This is more than twice the enrollment of 4-year-olds in the federal Head Start program. The growth in pre-K enrollment was only halted during the Great Recession and has continued to grow since (NIEER, 2013, 2016). Further investments in expanding state pre-K programs are on the agenda of many states, underscoring the need for high-quality research on the scope of benefits of preschool education.[2] In order to fully understand

[1] Based on state-funded pre-K enrollment in 2014 (NIEER, 2016) and total enrollment by age and sector in 2013 (NCES, 2015).

[2] Since in 2014, the federal government under President Obama's administration awarded over $463 million in Preschool Development Grants to support the development and expansion of preschool programs in 18 states

the costs and benefits of these proposals, we need more evidence on the scope of effects of large-scale public early education interventions.

In this paper, I study the short- and medium-run effects of pre-K education on child health and development outcomes. In particular, this paper addresses two questions that can contribute to a better understanding of the impacts of attending preschool education, and state pre-K programs in particular. First, do pre-K programs have any lasting impacts on child developmental outcomes, other than those measured by test scores? This is still an open question, since studies of both state pre-K and federal Head Start programs have found positive short-run impacts on test scores that fade-out one to three years after preschool, and there has been little work studying impacts on other developmental outcomes. Second, are there any (intended or unintended) impacts of attending a pre-K program on physical health? The scarce literature that addresses health impacts of preschool attendance has found contradicting evidence, with some studies of Head Start finding evidence of positive effects in the medium run, and studies of child care subsidies finding some large negative effects on child health status and the incidence of illness in the short run. Furthermore, the few papers that have studied the impacts of state pre-K programs on test scores and child health have focused entirely on single or small groups of universal, high-quality state pre-K programs, while the majority of state pre-K programs (about two-thirds of them) are targeted towards low income children. In this paper I study the impacts of a large group of state pre-K programs that is representative of the diversity of state programs currently being implemented.

There are several potential channels for a causal effect of attending preschool on development. First, preschool programs are designed to prepare children for school, encouraging the development of cognitive and non-cognitive skills, potentially having an impact on child development. Second, preschool programs can improve the access of parents to information that can improve parental investments in child development. Third, free or reduced-price preschool access may improve labor outcomes for parents (especially mothers), which can increase family income and parental investments. However, an increase

(US Department of Education, 2015). As of today, it is unclear whether the new administration under President Trump will continue to promote the development of state pre-K programs.

in the labor market participation of mothers could also have a negative effect on child development if this reduces the quantity and/or quality of time that the mother spends with the child.

In terms of health, similar channels can be described for a causal effect of preschool on health. Preschool usually have an explicit goal of preparing children for school, including in terms of their health. This involves for example offering health checkups and requiring immunizations, teaching children and their parents healthy habits, and improving access to preventive care and to health information for parents. An earlier enrollment in school may also increase the child's direct exposure to illness through contact with other children. This could potentially have a negative effect in the short-run, though it is not clear how this would affect longer-term health outcomes. Public preschool is a form of subsidized childcare, and can also have income effects, both through the implicit income transfer that it represents and through the potential increase in parental labor supply. While the income effect can be increase parental investments in health, it can also negatively affect parental time investments in child health.

Assessing the effects of the introduction of state-funded pre-K programs for 4-year-olds on development and health outcomes throughout childhood poses two main challenges. First, simply comparing the health outcomes of children who did and did not attend a preschool program is likely to result in biased estimates of the effects of preschool education, because the families of the two groups are likely to differ in attributes that may be related to child health. The second challenge is that very few large and publicly available datasets have information about both child development and health at different ages and preschool attendance, and those that do have a very limited set of outcomes. Furthermore, to the best of my knowledge there is currently no available individual-level national data source that includes information on whether a child attends or has attended a state-funded pre-K program.

I overcome these challenges by collecting information on the timing of the introduction of state pre-K programs in 15 states between 1997 and 2005. I combine this information with individual-level data from two different national surveys, to provide evidence on the effects of the introduction of pre-K programs on a wide set of child development and health outcomes, up to eight years after preschool age. I exploit the variation in the timing of the introduction of these programs across states as an exogenous source of variation in the access of 4-year-olds to pre-K programs, while controlling for permanent differences in child outcomes across states, changes over time at the national level, individual demographic characteristics, and time-varying characteristics of each state.

I estimate the reduced-form (*intent-to-treat*) effects of the implementation of pre-K programs on child development and health, using data from the National Health Interview Survey (NHIS) for years 1998 to 2014, on the cohorts of children who were 4 years old between 1997 and 2005. I find that for boys, pre-K programs reduce the likelihood of receiving special education services during the first four years after pre-K, and have a significant negative effect on an index of developmental problems 5 to 8 years after pre-K. This suggests the existence of beneficial effects of pre-K programs on boys' developmental outcomes that persist as late as eight years after preschool age. However, I don't find similar beneficial effects on the developmental outcomes of girls.

The reduced-form effects on health outcomes suggest that pre-K programs are associated with worse health, as measured by a summary index of health problems during the first four years after pre-K, also increasing the number of days of school missed for being sick. These results are robust to changes in the specification, control variables and states included in the sample. The effects on reported health are not correlated with changes in access to health care, and there is no evidence of effects on hospitalizations or asthma-related emergency room visits.

Because of the lack of individual-level information on pre-K enrollment, I implement two alternative strategies to approximate *treatment-on-the-treated* estimates. First, I construct a proxy of the increase in pre-K enrollment after the pre-K expansions in treatment states, using the available aggregate information on state-level enrollment rates. Second, I use Current Population Survey information on 4-year-olds between 1997 and 2005, and estimate the first-stage effects on overall preschool enrollment (which includes pre-K but also other preschool programs). The estimates from the two strategies suggest that there is a sizable crowding-out effect of other preschool programs. They also suggest that *treatment-on-the-treated* effects are quite large, implying that pre-K expansions have impacts not only children who would otherwise not have attended any preschool, but also on children that are drawn from other preschool programs.

This paper sheds light on the potential of state-funded pre-K programs for improving child developmental outcomes throughout childhood. My findings of improved developmental outcomes for boys up to eight years after preschool age complement previous findings on short-term impacts of pre-K on test scores, as well as research on Head Start and other preschool programs. The results also suggest that increased access to preschool education may increase the incidence of illness in the short-run, similar to what has been found for increased access to child care subsidies. While the estimates imply large effects, they don't seem to reflect serious conditions, as there are no effects on hospitalizations or emergency-room visits related to asthma episodes. Additionally, I find no significant effects on health past four years after preschool.

The paper proceeds as follows. Section 2 discusses related literature and provides background on the state pre-K programs considered in the paper. Section 3 outlines the empirical strategy based on the introduction of pre-K programs in different states and years, and presents the data and outcome variables used. Section 4 presents the estimates of the reduced-form effects of the introduction of pre-K programs, it discusses the robustness of the main results, and presents two alternative approximations to *treatment-on-the-treated* effects. Section 5 concludes.

1.2 Background

1.2.1 Related Literature

This paper contributes to the literature on the effects of attending preschool on child outcomes. A central topic in the discussion about preschool education has been whether impacts fade out over time. This debate was ignited by studies of Head Start that show positive effects on cognitive skills immediately after preschool that fade out during the following years, at least for some groups of children (DHHS, 2010; Currie and Thomas, 1995 and 1999, Deming, 2009). This debate has resurfaced in light of recent evaluations of the effects of some specific universal state-funded pre-K programs on academic achievement.

While evaluations of short-run effects have mostly found positive effects on school readiness test scores (Gormley and Gayer, 2005; Gormley, Phillips and Gayer, 2008; Wong et al., 2008), the few papers that have evaluated the effects of these programs on test scores some years after preschool age have found mixed results on the persistence of early positive impacts (Lipsey, Farran, and Hofer, 2015; Cascio and Schanzenbach, 2013; Fitzpatrick, 2008; Hill, Gormley and Adelstein, 2015).[3]

Despite the fade-out of the effects on test scores, comparisons of siblings that attended and did not attend Head Start show long-term improvements in educational attainment, earnings, crime, and self-reported health (Garces, Thomas and Currie, 2002; Deming, 2009). Furthermore, randomized controlled trials of model early education programs such as the Carolina Abecedarian and Perry Preschool Projects have found that these small, high-quality interventions had long-run positive effects on outcomes such as educational attainment and earnings (Currie, 2001). The contrast between the medium-run effects of preschool programs (both state pre-K and Head Start) on test scores, and the long-run effects on income and other important outcomes found for Head Start and the earlier model programs, raises the question of whether preschool programs affect other short- and medium-run outcomes that might explain the long-run impacts. In particular, there is very little research evaluating the effects of preschool education on development outcomes not measured by test scores, such as special education placement, learning disabilities, and behavioral problems. A notable example is Deming's (2009) study, which shows that participation in Head Start reduces grade retention and learning disability diagnosis for children ages 7 to 14, compared to their siblings that did not attend Head Start. Carneiro and Ginja (2014) also look at the effects of Head Start on some outcomes related to cognitive and non-cognitive skills, finding that participation in Head Start reduces

[3] Lipsey, Farran, and Hofer (2015) present results for the first randomized controlled trial of a scaled up, state-funded pre-K program, the Tennessee Voluntary Prekindergarten Program. The study finds positive effects on achievement tests at the end of pre-K, but the differences with the control group fade out by the end of kindergarten. Using difference-in-difference strategies, Cascio and Schanzenbach (2013) find that the introduction universal pre-K programs in Georgia and Oklahoma had positive effects on children's test scores as late as eighth grade, for children with low-education mothers, while Fitzpatrick (2008) finds positive effects of Georgias universal pre-K program in fourth grade only for disadvantaged children living in rural areas. Hill, Gormley and Adelstein (2015) study two cohorts of Tulsa's pre-K program using propensity score matching, and find evidence of persistence of early gains in test scores through the third-grade of school only for one of the cohorts, in math but not reading, and for boys but not for girls.

behavior problems for boys at ages 12-13, and decreases the probability of special education placement for white boys only.[4]

A related literature has looked at the impacts of child care subsidies, finding some negative short-run effects on behavioral outcomes. For example, Baker, Gruber and Milligan (2008) find that a large-scale child care subsidy in Quebec, Canada had negative impacts on child behavioral outcomes. Baker, Gruber and Milligan (2015) find that these negative non-cognitive effects persist to school ages, and find negative long-run impacts on health, life satisfaction, and higher crime rates. In contrast, Havnes and Mogstad (2011) find that a large-scale expansion of subsidized child care for 3- to 6-year-olds in Norway had strong positive effects on children's educational attainment and labor market participation in the long run. However, it is hard to know whether these findings are applicable to pre-K programs, as child care subsidies differ from preschool programs in that they usually subsidize any form of childcare, affect children starting at younger ages, and may have large impacts on maternal labor supply, while not necessarily promoting access to high-quality early education programs.

This paper is the first to study the impacts of a large and representative group of state-funded pre-K programs on child development. It contributes to the discussion on the effects of preschool education on development, by providing evidence on the effects of pre-K programs on developmental outcomes throughout childhood. The simultaneous study of the short- and medium-run effects can help us build a bridge between the conflicting evidence of fading-out short-run effects of preschool education on test scores, and the positive long-lasting effects on adult outcomes of Head Start and other experimental programs.

This paper also contributes to the still scarce literature on the effects of preschool attendance on child health, by providing evidence on effects on health status and incidence of illness from 1 to 8 years after preschool age. In terms of health outcomes, studies of

[4] There are also a number of studies that look at the effect of the implementation of universal preschool or pre-kindergarten policies in other countries. For example, Berlinski, Galiani and Manacorda (2008) find that a large expansion of public pre-K in Uruguay led to small gains from preschool attendance on years of education completed and dropout rates that get magnified as children grow up. Berlinski, Galiani and Gertler (2009) show that attending pre-primary school in Argentina had a positive effect on subsequent third grade standardized test score, and on behavioral skills such as attention, effort, class participation, and discipline.

the impact of Head Start have found evidence of some positive health effects, but they have looked at a limited set of health indicators at specific ages. The Head Start Impact Study (DHHS, 2010) found positive impacts on reported health status and health insurance coverage during Kindergarten that fade out by first grade. Ludwig and Miller (2007) show that Head Start reduced childhood mortality during its implementation in the 1960s, which is likely explained by the increased access to immunizations that the program provided (Currie and Thomas, 1995). For more recent cohorts, Carneiro and Ginja (2014) find that participation in Head Start causes improvements in mental health screenings, reductions in obesity prevalence, and some indication of reductions in disability, but they find no significant effects on reported health limitations, health status, or risky behaviors, for boys at ages 12-13 and 16-17. In contrast, Baker, Gruber and Milligan (2008) find that the Quebec child care subsidy had negative impacts on child health in the short run.

One of the channels through which preschool programs can affect child health is through the direct exposure to illness. Even though there is evidence of increased infections at the onset of center-based childcare before age 2 (Côté et al., 2010; Miller, Gruber and Milligan, 2008), it is not clear that increasing preschool attendance at age 4 should increase the risk of infections, as children may already be exposed to contagious illnesses at an earlier age from other child-care arrangements and family members. If preschool attendance does increase the prevalence of infectious illnesses, this may be protective of later health. Epidemiological studies have found some support for the *hygiene hypothesis*, which states that increased exposure to certain types of infections early in life might have a protective effect against the development of asthma, allergic diseases, and viral respiratory infections (Ball et al., 2002; Illi et al., 2001; Côté et al., 2010). However, the evidence of a protective effect is mostly based on exposure in the first two to three years of life. Another potential channel for effects on health can work through changes in maternal labor supply and parental investments in child health. The literature on the effects of maternal employment has found small effects on child health overall, but larger negative effects for children in high socio-economic status families (Anderson et al., 2003; Gennetian et al., 2010; Ruhm, 2000, 2008; Morrill, 2011).

1.2.2 Background on State-Funded Pre-K Programs

The programs studied in this paper are state-funded pre-kindergarten programs for 4-year-olds that were first implemented or scaled up between 1997 and 2005, and that are qualified as state preschool programs by the National Institute for Early Education Research (NIEER). An initiative is considered to be a state preschool program by NIEER if it meets the following criteria: a) the initiative is funded, controlled, and directed by the state; b) it serves 3- and/or 4-year-old children; c) early childhood education is the primary focus of the initiative; d) it offers a group learning experience at least two days per week; e) it is distinct from the state's system for subsidized child care; f) the initiative is not primarily designed to serve children with disabilities, although it may include children with disabilities; and g) state supplements to Head Start are considered to constitute state preschool programs if they substantially expand the number of children served and the state assumed some administrative responsibility for the program.

Figure 1-1 shows a map of the U.S. with the states that implemented pre-K programs in this period (*treatment states*) in red, and states without programs (*control states*) in yellow. The states that implemented these programs, defined hereafter as **treatment states**, are: Arkansas, Florida, Kansas, Louisiana, Missouri, Nebraska, New Jersey, New Mexico, New York, North Carolina, Oklahoma, Pennsylvania, Tennessee, Vermont, and West Virginia. My control group are children from states that by 2005 had not yet implemented a state-wide pre-K policy, or which only had a very small scale pre-existing program whose impacts are unlikely to be observed at a state level. These states, denominated **control states** hereafter, are: Alabama, Alaska, Arizona, Hawaii, Idaho, Indiana, Iowa, Minnesota, Mississippi, Montana, Nevada, New Hampshire, North Dakota, Rhode Island, South Dakota, Utah, Washington, and Wyoming. Most of these states (12 out of 18) did not have a state preschool program by 2005, and those that had a state preschool program had stable enrollment rates of at most 6% of 4-year-olds by 2005.[5]

The remaining states and the District of Columbia are excluded from the main analysis because they had pre-existing programs with enrollment rates of 10% or more during

[5] Enrollment rates are available only since 2001, collected by NIEER (2006). Table 1.9 shows enrollment rates for each treatment state for years 2001-2005.

the period, and/or had a program with significant variations in enrollment during the period (**excluded states** hereafter). The reason for excluding these states from the main analysis is that they can have changes in enrollment and/or funding during the period that would make them an inadequate counterfactual for the changes in outcomes that would have happened in treatment states in the absence of the implementation of a program. These changes in enrollment may or may not be confounding with the implementation of programs in the treatment states, but they would at least introduce noise in the estimation. However, in the robustness analysis I show that my main results are not sensitive to the inclusion or exclusion of any individual state from the sample, nor are they sensitive to including all excluded states in the control group.

Table 1.1 presents a summary of the characteristics of the programs implemented in the treatment states as of 2005 (NIEER, 2006). There is a combination of full-day (5 to 7.5 hours/day) and half-day (2.5 to 3 hours/day) programs, and some states offer both types of programs depending on the decision of the school district. Full-day programs offer at least lunch and a snack, while half-day programs usually offer a snack. The heterogeneity in hours-per-day served is mirrored by a similar heterogeneity in total spending per student (including all sources of funding): the average spending in states that offer only full-day programs is \$6,118, compared to \$2,929 in states that only offer half-day programs. The average spending per student across all treatment states is \$4,848.

All but two of the states require providers to follow comprehensive early education standards (Kansas and New York adopted comprehensive early education standards after 2005), and over 70% of the states require programs to offer basic health screenings, referral and support services. There is more variation in the quality of the programs in terms of class size and staff-child ratios, and teacher and assistant teacher degree and specialization requirements, which is reflected in the variation in the program scores assigned by NIEER,[6] which range from a minimum of 3 to a maximum of 10 out of 10, with a median of 7. Within this group of programs, NIEER's score has a correlation coefficient of .59 with

[6]This score is the count of the number of benchmarks met by the program, out of a total of 10. The benchmarks are the following: comprehensive early learning standards; teachers have a BA degree; teachers are specialized in pre-K education; assistant teachers have CDA degree or equivalent; teacher in-service at least 15 hours/year; class sizes of 20 or lower; staff-child ratio of 1:10 or better; screening/referral for vision, hearing and health, and at least 1 support service; at least 1 meal a day; and monitoring site visits.

spending per student.

Five of the states (Florida, New York, Oklahoma, Vermont, and West Virginia) offer voluntary universal pre-K programs, although not all of them were at the time sufficiently funded to meet demand. Enrollment rates of 4-year-olds in these universal programs range from 29% in New York to 70% in Oklahoma. The rest of the states offer programs targeted towards children from low-income families or who have other risk factors (such as having a disability, being homeless or in foster care, and being an English language learner). Some states use income thresholds to determine eligibility of individual children, while other states determine eligibility of a school or provider by requiring a minimum percentage of children served the school or program to be below the income threshold. The most commonly used income threshold is 185% of the federal poverty line (FPL), which determines eligibility for reduced-price lunch in schools. There are two state programs that use the same income threshold as Head Start (100% of the FPL), while all others use higher cutoffs. Some programs are allowed to serve children that don't meet the eligibility criteria by charging tuition or sliding fees. However, in many cases the eligible population is underserved due to funding limitations, something that happens with Head Start as well. Enrollment rates in targeted pre-K programs range from 4% to 22% of 4-year-olds in the state. The average enrollment rate across all programs in treatment states is 23%.

1.3 Empirical Strategy and Data

1.3.1 Reduced-Form Effects of State Pre-K Expansions

I take advantage of large increases in the supply of preschool education caused by the introduction of state pre-K programs to identify its effect on short- and medium-term outcomes of children. Between 1997 and 2005, 15 states implemented or substantially expanded state-funded pre-K education programs for four-year-olds. I evaluate the reduced-form (*intent-to-treat*) effects of these preschool education supply expansions using individual-level data on child health from the National Health Interview Survey (NHIS), supplemented with state-level information on the implementation of pre-K policies and other state characteristics and policy variables. I estimate regressions that take the form of a generalized difference-in-difference specification with state and cohort fixed effects:

$$Y_{isc}^{a} = \beta_{RF}^{a}\text{Post\_Pre-K}_{sc} + \gamma^{a} X_{isc}^{a} + \delta_{c}^{a} + \delta_{s}^{a} + \varepsilon_{isc}^{a} \tag{1.1}$$

The subscript i represents a child, c is the child's pre-K cohort (the *reference year* for attending preschool, i.e. the year when the child was 4 by October), and s is the state where the child lives.[7] The superscript a indicates that the model is separately estimated for child outcomes evaluated at different number of years after preschool age. Post_Pre-K_{sc} is an indicator variable for whether state s had implemented a pre-K program by year c. X_{isc}^{a} is a vector of control variables whose components vary across specifications, but in the more general case includes individual time-invariant characteristics of the child and her family (the child's gender, race/ethnicity, and mother's educational attainment), and state policy and economic control variables that vary by state and cohort, including state characteristics when the child was 4 (year c) and current characteristics in the year when the child outcomes are observed.[8] The state fixed effects, δ_{s}^{a}, control for unobserved differences across states (e.g. permanent differences in the quality of health care or education), and cohort fixed effects, δ_{c}^{a}, control for any unobserved changes across cohorts that are common to all states (e.g. general changes in female labor supply, parents' valuation of preschool education, national changes in health outcomes).

In my main specifications I group children in two age groups: 1-4 and 5-8 years after pre-K. This allows me to discern short- and medium-run effects, while maintaining large enough samples. In this case, when the estimation sample includes children of different ages, I also include dummies for the number of years after pre-K age that the child is observed, to control for differences in the outcomes across specific ages within an age group.[9]

Identification relies on the assumption that the timing of the implementation of state

[7] I make the assumption that the state where the child currently lives is the same as the state where the child was living at age 4, because the latter is not observed.

[8] For a complete list of state-level control variables, see Section3.3.

[9] Alternatively, I also estimate separate regressions for outcomes observed in each individual year after pre-K age. Since in this case the regressions are estimated separately for each year after pre-K, the year when an outcome is observed is a linear function of the year when the child was 4 years old. Thus, the cohort fixed effects control for changes not only across birth cohorts but also across the years when outcomes are observed.

pre-K programs is not correlated with other factors that may affect child health. In other words, I assume that the outcomes in the treatment states would have evolved in a similar way as in the control states in the absence of the introduction of a pre-K program. While, as in most non-experimental studies, the exogeneity of the policy variable cannot be directly tested, Section 1.4.2 presents evidence showing that the implementation of state-funded pre-K programs is not correlated with other state-level policies that may affect child health and development. I also discuss the sensitivity of the results to the inclusion of different sets of state-level control variables, adding state-specific linear time trends, and alternative choices of the states included in the control group. The time frame of the study begins in 1997 to avoid the potentially confounding effects of welfare reform.[10]

The estimated effects that result from the estimation of equation 1.1 can be interpreted as estimates of the *intent-to-treat* (ITT) effects of the introduction of a pre-K program. They represent estimates of the reduced-form impacts of the introduction of a pre-K program in a state on all the cohorts of children who live in the state and are age-eligible for pre-K (age 4) after the introduction of the program. This interpretation of the reduced-form estimates is similar to the ITT interpretation of the estimates of child care policy expansions of Baker, Gruber and Milligan (2008) and Havnes and Mogstad (2011).

Because pre-K policies have different characteristics in the states in which they are implemented, the ITT effect constitutes a population-weighted average across the marginal effects in the different treatment states. In particular, it averages across the different increases in the coverage of public pre-K that these expansions cause, and across phase-in and full implementation years of these expansions. In addition, it averages across the marginal effects for the different populations that are affected by these programs in different states. For example, some states implemented small pre-K expansions that targeted low-income families, many of whom are likely to be eligible for but under-served by other targeted programs such as Head Start, while in the other extreme some states implemented large programs with universal eligibility. These differences imply that the

[10]Between 1993 and 1996, 43 states received welfare waivers to requirements of the Aid to Families with Dependent Children (AFDC) program, as a first stage of the welfare reform (DHHS, 1997). In 1996 the Personal Responsibility and Work Opportunity Reconciliation Act (PRWORA) instituted the Temporary Assistance for Needy Families (TANF) program, which replaced AFDC and became effective in July, 1997.

ITT estimates are averaging across different marginal effects, for example in terms of the socio-economic status of the children affected by the policy, and their alternative child care arrangements. In this sense, an advantage of my empirical strategy is that the policy effects are estimated for a large group of pre-K policies that is representative of the diversity of programs currently being implemented by states across the U.S. This contrasts with the previous literature on state pre-K, which has focused only on universal (city or state) pre-K programs. Currently, about one third of the states that have pre-K programs offer universal access (in the sense of not having income eligibility requirements), a similar proportion to the ratio of universal programs in my sample of treatment states.

An advantage of the estimation of ITT effects is that it allows us to capture the full effects of the policy on a cohort, taking into account for example the size of the actual expansion and any take up issues, the full impact of changes in child care arrangements, and any externalities that the participation of a child in a pre-K program may have on non-participant children. However, it also has the disadvantage that it is very sensitive to the size of the expansions, making it hard to recover a *treatment-on-the-treated* (TT) parameter. In the next subsection, I discuss the strategies I use to try to approximate the TT effects.

1.3.2 Preschool Attendance and Treatment-on-the-Treated Effects

The ITT estimator presented in the previous subsection captures the average effect on all children of certain cohorts in a state of a state pre-K program being available in the state. An important drawback of this estimator is that it is hard to interpret the size of the estimated effect, because not all children in the state who we consider as "treated" actually have access to the pre-K program. Actually, for most states only a small fraction of the children in the state attend the program. This problem is not only due to non-compliance, but most importantly to the limited availability of spots in the programs, which is related to their roll out across different regions of the states, the limitations imposed by eligibility criteria, and funding limitations that restrict the number of children that can enroll. In this context, we may be interested in estimating the size of the effects of the implementation of a pre-K policy relative to the size of the expansion in pre-K coverage. In this sense, we are interested in the *treatment-on-the-treated* (TT) effect, the average effect on the children actually affected by the pre-K expansion.

Defining the TT effect poses an additional difficulty in the context of the expansion of public provision of preschool education, because the additional enrollment can be drawing children from different alternative childcare environments, which can impose different local average treatment effects. Each 4-year-old child participates in one of three possible treatments: State Pre-K, which I label k, other preschool programs, denoted by c, and no preschool (i.e. home care or other forms of informal care), labeled h. Following a notation similar to that used by Kline and Walters (2016), let $Z_i \in \{0, 1\}$ indicate whether child i lives in a state that has a pre-K program when they are four years old, and $D_i(z) \in \{k, c, h\}$ represent each child's potential treatment status as a function the pre-K expansion. The observed treatment status is $D_i = D_i(Z_i)$.

To simplify the analysis, I make the assumption that anyone who changes their behavior as a response to the pre-K expansion does so to attend pre-K.[11] In this setting we can partition the population of children into the following groups:

1. n-compliers: $D_i(1) = k$, $D_i(0) = n$
2. c-compliers: $D_i(1) = k$, $D_i(0) = c$
3. n-never takers: $D_i(1) = D_i(0) = n$
4. c-never takers: $D_i(1) = D_i(0) = c$
5. always takers: $D_i(1) = D_i(0) = k$

The n-compliers are children who switch to state pre-K if a pre-K expansion is implemented in their states, but would otherwise not be enrolled in preschool. Similarly, the c-compliers are children who switch to state pre-K if a pre-K expansion is implemented

[11] This implies that preferences across other modes of childcare are not changed because of the increased availability of pre-K. It also implies assuming that other preschool programs are not rationed. If there is excess demand for other preschool programs, it is possible that when children switch from the other programs to state pre-K, they open up slots for other children who were in other case not attending preschool. Rationing is likely to occur in Head Start programs. This would be a minor issue if the proportion of children drawn from Head Start to pre-K is relatively small, or if the treatment effects of pre-K are similar to those of Head Start.

in their states, but would otherwise be enrolled in other (private or public) preschool programs. Note that the definition of never takers is a little different in my setting, where the treatment is not actually being offered a pre-K spot, but instead just the implementation of a pre-K expansion in the state (which does not guarantee a spot in the pre-K program). Thus, the groups of n-never takers and c-never takers necessarily include children who live in a state with a pre-K policy but who do not have the possibility of enrolling in the program. Also, in this context the always takers are children who would be enrolled in state pre-K even in the absence of a pre-K expansion (for example in small pilot state programs or local pre-K programs funded by states). Unfortunately, there is no available information on individual enrollment in state pre-K programs for the different states and cohorts in the sample to provide reliable estimates of the relative sizes of these different groups.

One of the parameters of interest is the average *treatment-on-the-treated* effect, considering as treated all of the compliers, with their corresponding mix of alternative child care arrangements. With data on pre-K enrollment at the individual level, one could use an instrumental variables (IV) strategy, where in the first stage we estimate the effect of the pre-K expansion on state pre-K enrollment, and in the second stage we look at the effects of pre-K enrollment on outcomes. Such an IV strategy would yield the following treatment effect:

$$LATE_k = \frac{\mathrm{E}[Y_i|Z_i = 1] - \mathrm{E}[Y_i|Z_i = 0]}{\mathrm{E}[1\{D_i = k\}|Z_i = 1] - \mathrm{E}[1\{D_i = k\}|Z_i = 0]} \tag{1.2}$$

This local average treatment effect can be decomposed, as pointed out by Kline and Walters (2016), into a weighted average of local average treatment effects for the two groups of compliers:

$$LATE_k = S_c LATE_{ck} + (1 - S_c) LATE_{nk} \tag{1.3}$$

where $LATE_{ck} \equiv \mathrm{E}[Y_i(k) - Y_i(c)|D_i(1) = k, D_i(0) = c]$ is the average treatment effect of attending pre-K on c-compliers (those drawn from other preschool programs);

$LATE_{nk} \equiv \mathrm{E}[Y_i(k) - Y_i(n)|D_i(1) = k, D_i(0) = n]$ is the average treatment effect of attending pre-K on n-compliers (those drawn from non-preschool childcare arrangements); and $S_c \equiv \frac{\Pr(D_i(1)=k, D_i(0)=c)}{\Pr(D_i(1)=k, D_i(0)\neq k)}$ represents the fraction of compliers drawn from other preschool programs.

Because the model is exactly identified, the IV estimator for $LATE_k$ is the ratio of the reduced-form estimator ($\hat{\beta}_{RF}$) and the first-stage estimator for the effect of the pre-K expansion on pre-K enrollment. However, because none of the available national individual-level datasets have information of enrollment in state pre-K programs, I cannot estimate the first stage of this IV strategy directly. In addition, state-level information on enrollment rates in state pre-K is only available starting in 2001.

Given the limitations imposed by the availability of data, I explore two different strategies to approximate the TT effect, $LATE_k$. The first strategy is to substitute the first stage estimate of the effect of the pre-K expansions on pre-K enrollment with a proxy for the change in enrollment rates in treatment states before and after the expansions:

$$\hat{\beta}_{TT1} = \frac{\hat{\beta}_{RF}}{\Delta \text{ Pre-K Enrollment}} \tag{1.4}$$

where Δ Pre-K Enrollment $= Enrollment_T^{Post} - Enrollment_T^{Pre}$, is the difference between the weighted average enrollment rates of 4-year-olds in state pre-K programs in treatment states in the years after and before a pre-K expansion. To construct these two average enrollment rates I use the available state-level enrollment rates for the years 2001 to 2005 in treatment states. I discuss this in more detail in the data section.[12]

The second strategy consists of using individual-level information on preschool attendance, which includes attendance to pre-K but also to other preschool programs. I estimate the first-stage average impact of the introduction of this group of pre-K programs on preschool attendance of 4-year-olds. Because the NHIS does not have information on preschool attendance, I use repeated cross-sectional samples of 4-year-olds from Current

[12] The use of this proxy as a substitute for the first-stage estimate introduces additional uncertainty to the TT estimate, but I do not have a measure of this uncertainty. Thus, the reported standard errors for $\hat{\beta}_{TT1}$ are only based on the uncertainty of the reduced form estimate $\hat{\beta}_{RF}$. In particular, $\hat{se}_{\hat{\beta}_{TT1}} = \sqrt{(\hat{se}_{\hat{\beta}_{RF}}/\Delta \text{ Pre-K Enrollment})^2}$.

Population Survey (CPS) October Supplement, also augmented with state-level data. I estimate the following regression:

$$Preschool_{isc} = \beta_{FS}\text{Post\_Pre-K}_{sc} + \pi X_{isc} + \lambda_c + \lambda_s + \nu_{isc} \tag{1.5}$$

where $Preschool_{isc}$ is a variable that indicates whether the child attended a preschool program at age 4, X^a_{isc} is a vector of individual and state-level control variables, and λ_c and λ_s are cohort and state fixed effects.

I use a Two-Sample Two-Stage Least Squares strategy (TS-2SLS) in order to estimate a *treatment-on-the-treated* effects, which in this case represents the magnitude of the effect of attending pre-K at age 4 on the children who attended preschool because there was a pre-K program implemented in their state. In this strategy, I use the pre-K policy indicator (Post_Pre-K_{sc}) as an instrument for the endogenous regressor $Preschool_{isc}$, in the following model:

$$Preschool_{isc} = \beta_{FS}\text{Post\_Pre-K}_{sc} + \pi X_{isc} + \lambda_c + \lambda_s + \nu_{isc} \tag{1.6}$$

$$Y^a_{isc} = \beta^a Preschool_{isc} + \gamma^a X_{isc} + \delta^a_c + \delta^a_s + \varepsilon_{isc} \tag{1.7}$$

Because the information about preschool attendance is not available in the NHIS, the TS-2SLS strategy consists of using the estimated coefficients from the first-stage regression, along with data from the NHIS on the same variables used as explanatory variables in the first stage, to predict preschool attendance for the children in the NHIS sample, and then regress health outcomes at different ages on predicted preschool attendance at age 4.[13] Because the model is exactly identified, this TS-2SLS estimator of β^a, denoted by $\hat{\beta}_{TT2}$,

[13] In this model I only include control variables that are constant over time, as well as cohort and state fixed effects, because all control variables must be the same in both stages of the model.

is the ratio of the reduced-form estimator ($\hat{\beta}_{RF}$) and the first-stage estimator ($\hat{\beta}_{FS}$):[14]

$$\hat{\beta}_{TT2} = \frac{\hat{\beta}_{RF}}{\hat{\beta}_{FS}} \tag{1.8}$$

This instrumental variables strategy provides an estimate for the following treatment effect:

$$LATE_p = \frac{\mathrm{E}[Y_i|Z_i = 1] - \mathrm{E}[Y_i|Z_i = 0]}{\mathrm{E}[1\{D_i \neq h\}|Z_i = 1] - \mathrm{E}[1\{D_i = h\}|Z_i = 0]} \tag{1.9}$$

The denominator of the expression in equation 1.9 represents the change in preschool enrollment (including pre-K and other preschool programs) caused by the introduction of a pre-K expansion, which is different to the denominator in equation 1.2. In particular, if the expansion of pre-K enrollment crowds out some enrollment in other preschool programs, the denominator in equation 1.9 will be smaller in magnitude than the denominator in equation 1.2.

To be able to interpret the estimates of $\hat{\beta}_{TT2}$, we need to understand what kind of bias it can have as an estimate of the parameters of interest and, in particular, in what circumstances it provides a good estimate of $LATE_k$. To understand this, we can start by using the decomposition of the treatment effect $LATE_k$ that was presented in equation 1.3. Taking into account the different denominators of equations 1.2 and 1.9, we can express the effect estimated by $\hat{\beta}_{TT2}$ in the following way:

$$LATE_p = \Big[S_c LATE_{ck} + (1 - S_c) LATE_{nk}\Big] \times \left(\frac{\Pr\big(D_i(1) = k, D_i(0) \neq k\big)}{\Pr\big(D_i(1) \neq h, D_i(0) = h\big)}\right) \tag{1.10}$$

Under the assumption that the only changes in childcare arrangements induced by a pre-K expansion are changes to enroll in pre-K (no switches between home (h) and other preschools (p)), $\Pr\big(D_i(1) \neq h, D_i(0) = h\big) = \Pr\big(D_i(1) = k, D_i(0) = h\big)$. Thus, under this

[14] I compute the first-stage estimates using CPS data and the reduced-form estimates using NHIS data, separately for each age group. Following Dee and Evans (2003), I compute standard errors using the delta method, assuming there is zero covariance between the first-stage and reduced form-estimates. Under this assumption, the delta method implies that the standard errors of the TS-2SLS estimator can be approximated by: $\hat{se}_{\hat{\beta}_{TS2SLS}} = \sqrt{(\hat{\beta}^2_{RF}/\hat{\beta}^2_{FS}) * [(\hat{se}_{\hat{\beta}_{FS}}/\hat{\beta}_{FS})^2 + (\hat{se}_{\hat{\beta}_{RF}}/\hat{\beta}_{RF})^2]}$

assumption the above expression can be re-written as follows:

$$LATE_p = \left[S_c LATE_{ck} + (1 - S_c) LATE_{nk} \right] \times \left(\frac{1}{1 - S_c} \right) \tag{1.11}$$

Equation 1.11 shows that $LATE_p$, estimated by $\hat{\beta}_{TT2}$, is larger in absolute value than the parameter of interest $LATE_k$ for any given $S_c > 0$, i.e. for any degree of crowding out greater than zero.[15] The difference between $LATE_p$ and $LATE_k$ is increasing with the degree of crowding-out. If there is no crowding-out of other preschool programs, then $S_c = 0$, and $LATE_p$ is also equal to $LATE_k$. In other words, there is no difference between estimating the TT effect using enrollment in pre-K or in preschool in general, because the change induced by the policy in both enrollment rates is the same in the absence of crowding-out.

In addition, we can re-write equation 1.11 as follows:

$$LATE_p = LATE_{nk} + \left(\frac{1}{1 - S_c} \right) \times LATE_{ck} \tag{1.12}$$

Equation 1.12 is useful for interpreting how $\hat{\beta}_{TT2}$ is related to another parameter of interest, the local average treatment effects of switching from home or informal care to state pre-K ($LATE_{nk}$). First, if there is no crowding-out of other preschool programs, $\hat{\beta}_{TT2}$ provides an estimate of $LATE_{nk}$. Second, we can sign the bias for $\hat{\beta}_{TT2}$ from $LATE_{nk}$ under different assumptions about the average effect of switching from alternative preschool programs to state pre-K. If the treatment effect of attending a pre-K program is similar to the effect of attending any other preschool program, relative to home or informal care, then $\hat{\beta}_{TT2}$ provides an estimate for this treatment effect of attending preschool. If state pre-K constitutes a higher quality option than the average alternative preschool program, then $LATE_{ck} > 0$. If this is the case, $\hat{\beta}_{TT2}$ overestimates the effect of attending pre-K,

[15] If there is crowding out from other preschool programs, we would observe an increase in total preschool enrollment that is smaller than the actual increase in state-funded pre-K enrollment. I discuss some suggestive evidence of this in section 1.4.3. Also, Kline and Walters (2016) show evidence of crowding-out for Head Start, finding that about one third of Head Start participants in the Head Start Impact Study are drawn from other forms of preschool. Crowding-out can be a more important concern in states that implement universal programs than in states with targeted programs, because the latter target a population that is under-served by the previously available suppliers.

because it does not include in the denominator those children who are switching from other preschools to pre-K, when making this change has an impact on them. Similarly, if state pre-K constitutes a lower quality option than the average alternative preschool program, and then $LATE_{ck} < 0$, $\hat{\beta}_{TT2}$ underestimates the effect of attending pre-K.

1.3.3 Data Sources

National Health Interview Survey (NHIS) The reduced-form analysis of the effect of pre-K expansions on child development and health outcomes is conducted using repeated cross-sectional data from the National Health Interview Survey (NHIS) from 1997 to 2014. This part of the empirical analysis was conducted at a National Center for Health Statistics (NCHS) Research Data Center because the state of residence is restricted access information.

The NHIS sample includes children in the "Sample Child" files, supplemented with information for the same sample from the "Person Level" files, for children of ages 5 to 12. More precisely, I impute the year in which each child would have been eligible for pre-K (year when they would have been 4 years old by October) using the information on the month and year of birth, and I keep in my sample the children who are observed between 1 and 8 years after pre-K age. The number of years since they were 4 years old determines the age group a to which they belong. The full sample of children whose age is determined to correspond to 1 to 8 years after pre-K has 38,668 observations. The main estimation sample consists of children who live in treatment and control states, and I also use data from the rest of the states for robustness checks. After dropping the observations in excluded states and children with missing data in the main outcome variables and individual controls, the main sample has 17,941 observations.

I construct various outcome variables that are described in the next section using data from both the "Sample Child" and "Person Level" files. I also use a set of control variables, including the child's gender, race/ethnicity, mother's educational attainment, and mother's marital status. The control variables I use in my estimations are characteristics that do not typically change over time, so the current values can be assumed to be same as they were when the child was 4 years old.[16] Summary statistics for the NHIS data are reported in Table 3.1.

Current Population Survey (CPS) October Supplement I use individual-level CPS October data from 1997 to 2005 to evaluate the effect of the implementation of pre-K programs on preschool attendance of 4-year-olds. My main CPS sample consists of all children who are four years old in October and live in treatment and control states. I also construct an extended sample that includes four-year-olds from the 50 states and the District of Columbia. The full sample has 15,541 observations, while the main sample (after dropping observations from excluded states) has 8,880 observations.

The CPS October Supplement contains information on attendance to preschool, which comes from two questions from the October questionnaire. First, the CPS asks respondents whether children age 3 and older attend school. Second, it asks which grade they are attending: nursery (preschool or pre-kindergarten), kindergarten, or grades 1 to 12. I code a child as attending preschool if she is reported to be attending school at the nursery level. Table 1.3 shows summary statistics for the CPS sample. 60.4% of the main sample of 4-year-olds are attending preschool at the time of the survey. Those not attending preschool are either not attending school (33.2%) or are already enrolled in kindergarten (6.4%).[17] I also use CPS data on the same individual-level control variables as in the NHIS.

State-level information I supplement the NHIS and CPS data with state-level data collected from various sources. The primary source of information on the availability and characteristics of state-funded pre-K programs between 1997 and 2005 are NIEER State of Preschool reports. NIEER began collecting and reporting information on state-funded

[16] I only use the mother's marital status in robustness tests and not in the main analysis because it is potentially endogenous, and because the current marital status is not necessarily the same as when the child was age 4.

[17] Magnuson, Meyers and Waldfogel (2007) compare the 1999 CPS measure of school attendance of 3- and 4-year-olds to the more detailed data on child care arrangements from other surveys (NHES 1999, the ECLS-K 1998, and NSAF 1999), and their findings indicate that the measure of school enrollment in the October CPS is similar to measures in other studies that include center-based care, Head Start, nursery school, and pre-kindergarten. From this comparison it seems that parents do not identify informal child care and family day care as 'school', even if the latter is a licensed child care provider. Therefore, in this paper the alternative to preschool attendance includes being cared for at home or through informal child care arrangements, attending family day care, or enrolling in kindergarten.

pre-K programs in 2003, with data corresponding to the 2001-2002 school year, but each report includes background information about the programs described, including brief information about the history of program or significant recent changes. After identifying the existing programs, I establish the school year in which each initiative is effectively established or expanded as the school year in which funding is allocated, based on the corresponding state legislation collected through the Education Commission of the States (ECS) State Policy Database (ECS, 2015) and the states' legislature online databases. This information, together with the information in the CPS and NHIS about each sample child's state of residence and month and year of birth, is used for the construction of the variable that indicates whether the child was 4 years old after a pre-K policy had been implemented in the state where she lives (*Post Pre-K*).

I use various sources of information to construct control variables at the state level. I obtain the annual average state unemployment rate from the Bureau of Labor Statistics, and the state median household income from the US Census Bureau. I collect the state family-income-to-poverty ratio requirement for eligibility for Medicaid or SCHIP (whichever is lowest) for children ages 1-5 and 6-15, from National Governors Association *MCH Updates* (1997-2011), and Kaiser Commission on Medicaid and the Uninsured *50 State Updates on Eligibility Rules, Enrollment and Renewal Procedures, and Cost-Sharing Practices in Medicaid and SCHIP* (2006-2014).

I compute the federally-funded enrollment of 4-year-olds in Head Start as a percentage of the state's population of 4-year-olds in the following way. First, I compute the number of federally-funded enrollment of 4-year-olds by multiplying the total federally-funded enrollment (number of children) by the percentage of actual enrollment that corresponds to 4-year-olds, both obtained from Head Start Program Information Reports 1997-2005, from the Office of Head Start, US Department of Health and Human Services. I then divide this number by the population of 4-year-olds by state in 2000, obtained from the US Census Bureau.

I also explore the robustness of the main results of the paper to controlling for the estimated percent of 4-year-olds in the state served by the Child Care Development Fund,[18]

[18] CCDF provides child care subsidies to low-income families, and it is implemented by states through a federal

which I don't include in the main regressions because this information is not available for all years. I use data from the Office of Child Care, U.S. Department of Health and Human Services on the number of children served (monthly average by state) from 1998 to 2005, and the percentage of served children by age from 2002 to 2005. For the years 2002-2005 I compute the percentage of 4-year-olds in each state served by the CCDF, by multiplying the total number of children served by the percentage of children served who were 4-year-olds, and then dividing this number by the population of 4-year-olds in each state in the year 2000 (from the US Census Bureau data). For the years 1998-2001 I follow a similar procedure but, since the percentage of children served by age is not available, I use the average percentage by age in the years 2002-2004.

The state-level variables are merged to the CPS dataset by state and year, for the years 1997 to 2005, and they are merged to the NHIS dataset in two different ways. First, I merge the following state-level variables for the years 1997 to 2005 using the year when the child was 4 (*reference year*): annual average state unemployment rate; state median household income; state family income to poverty ratio requirement for eligibility for Medicaid or SCHIP for children between 1 and 5 years old; federally funded enrollment of 4-year-olds in Head Start; and percentage of 4-year-olds served by the CCDF (1998-2005). Second, I merge the following variables for the years 1998 to 2014, using the year when the child health outcomes are observed (i.e. at the current age): annual average state unemployment rate; state median household income; and state family income to poverty ratio requirement for eligibility for Medicaid or SCHIP for children between 6 and 15 years old.

I use information on the enrollment of 4-year-olds in pre-K programs by state reported in NIEER's State of Preschool reports for the school years 2001-2002 to 2005-2006 NIEER (2006), to construct a proxy for the change in pre-K enrollment rates in treatment states before and after a pre-K expansion. I use this proxy in one of my strategies to approximate treatment-on-the-treated effects ($\hat{\beta}_{TT1}$, presented in equation 1.4). To construct the weighted average of enrollment rates across treatment states, I use the information on the population of 4-year-olds by state corresponds to the information from the 2000 Census

block-grant. For more details see Section 1.4.2.

(US Census Bureau).

The resulting enrollment rates before and after pre-K expansions are presented in Table 1.9. *Post Pre-K Average* enrollment is computed as the simple average of the enrollment rates for the years after a state pre-K policy was implemented or expanded, if implemented in or after 2001, or the simple average of years 2001-2005, if implemented before 2001. *Pre Pre-K Average* enrollment is computed as the simple average of the enrollment rates for the years before a state pre-K policy was implemented or expanded with available information, if implemented after 2001. In my sample there are five states with pre-K programs implemented in 2001 or before. In each case, I use additional information to approximate the pre-expansion enrollment in pre-K in the state. The details for how this is done for each of the five states are provided in the footnote of Table 1.9. Finally, I compute a weighted average of the expansion of pre-K enrollment across all treatment states using the population of 4-year-olds in 2000 as weights.

1.3.4 Outcome Variables

The outcome variables analyzed in this paper can be grouped into two categories: child development and behavioral outcomes, on one hand, and general and physical health outcomes, on the other. Additionally, I complement the analysis of these outcomes by looking at health care utilization and insurance outcomes. Summary statistics of all outcome variables for treatment, control and excluded states are presented in Table 3.1.

The rich information available in the NHIS allows me to explore the impacts of pre-K on a large set of child outcomes, but this can come at the cost of a multiple inference problem: as the number of outcome variables grows, so does the probability of incorrectly rejecting a true null hypothesis of no causal effects. To reduce the scope of this problem and improve statistical power, I summarize the information of multiple outcome variables into two summary indices, following Anderson (2008).[19] I construct a development problems index and a health problems index. Each index is a weighted sum of z-scores of its component outcome variables. The z-scores are calculated by subtracting each outcome's control group mean, where the control group are children in control states, and dividing by its

[19] Recent applications of this method include Carneiro and Ginja (2014) and Deming (2009).

standard deviation. For all z-scores, a higher value indicates worse outcomes. To compute each index I average the z-scores using the inverse of their variance covariance matrix as weights. Weighting the components this way makes a more efficient use of the information than a simple average, as outcomes that are highly correlated and thus represent similar information are given less weight. Finally, I standardize each index again so that it has mean 0 and standard deviation 1 for the control group for easier interpretation.

I show results for two outcomes related to development and behavioral problems: special education placement and a development problems index. The first one is an indicator for whether the child is receiving special education or early intervention services (7.7% of the full sample). If pre-K education improves developmental outcomes and/or reduces behavioral and mental health problems, it can reduce the need for special education placement or the amount of time a child needs these services. The development problems index uses information on four outcome variables that address specific conditions that may affect learning: learning disability diagnosis, Attention Deficit/Hyperactivity Disorder (ADHD) diagnosis, limitations caused by a speech problem, and limitations caused by a behavioral problem.[20] The indicators for learning disability and ADHD are based on questions that ask the survey respondent whether they have ever been told that the child has a learning disability (7.6% of the full sample) and ADHD (7.5%), respectively. Given that they indicate whether the child has *ever* received a diagnosis, they are both weakly increasing with age. I interpret the results for these outcomes with caution, as they depend on the condition being diagnosed, and thus may be sensitive to health care utilization and may suffer from under- or over-diagnosis for specific groups.[21] The last two outcomes indicate whether the child has any limitation, when the respondent said that this limitation is

[20]Within children 3 to 21 years old served under Individuals with Disabilities Education Act (IDEA), the largest category of disability in the 2005-2006 school year was *specific learning disability* (41%), followed by *speech or language impairments* (22%), and *other health impairments*, *mental retardation*, and *emotional disturbance* (each in the order of 7-8%) (Snyder and Dillow, 2010). Even though ADHD is one of the most important mental health problems for children and it can increase the risk of academic difficulties (Currie and Stabile, 2006), it does not have its own specific category of disability. It can be classified under *other health impairments* if the child's educational performance is affected, although children with ADHD may also have be classified under the learning disability category based on another condition (Cuellar, 2015).

[21]In particular, there is large heterogeneity in the diagnosis of ADHD across ethnic and racial groups, although it is not clear whether these differences are due to differential prevalence rates. Additionally, Evans, Morrill, and Parente (2010) show that age relative to peers in class directly affects a child's probability of being diagnosed with ADHD.

caused by a speech problem (2.2% of the full sample), and a mental, emotional or behavioral problem (1.5%), respectively. All of these diagnoses and limitations, as well as special education placement, are observed around twice as often for boys as for girls.

I look at three outcome variables related to general and physical health: poor/fair reported health status, an index of health problems, and number of missed school days. The overall health status is reported by the survey respondent using a scale from 1–excellent to 5–poor (with a mean score of 1.63 for the full sample across all ages). "Missed school days" indicates the number of days of school missed due to illness or injury in the past 12 months. Children in the full sample missed an average of 3.2 days of school. Preschool education may affect health outcomes primarily through an earlier exposure to infectious illnesses, such as ear or gastrointestinal infections, which in turn could have positive or negative effects on later health, on conditions such as allergies and asthma. Most pre-K programs include health screenings and referrals, which could lead to an earlier detection of health problems, and many provide parental services that could improve access to health information for parents. Finally, if pre-K programs improve school readiness, they could reduce the incidence of conditions related to stress in children, such as frequent headaches. In light of these different potential channels, I construct the health problems index from four specific health conditions: 3+ ear infections, asthma episode, frequent headaches, and frequent diarrhea. "3+ ear infections" is a binary variable that indicates whether the child experienced 3 or more year infections in the past 12 months. This variable has a mean of 4.6% of the full sample, and it is decreasing with age. "Asthma episode" indicates whether the child had an asthma episode in the last 12 months (6.2% of the full sample). "Frequent headaches" indicates whether the child had frequent headaches/migraines in the past 12 months. This variable has a mean of 5.3% of the full sample, and it is increasing with age. "Frequent diarrhea" indicates whether the child had frequent diarrhea/colitis in the past 12 months (1.1% of the full sample).

In order to determine whether the health and development changes are associated with changes in health care, I complement the analysis of child development and health outcomes with an analysis of health care utilization and insurance. Pre-k programs may affect access to and sources of health insurance through two main channels. First, they may provide parents with information about public health insurance programs for children that they may be unaware of, potentially increasing access to health insurance and/or substituting public insurance for private insurance. Second, if pre-K programs have a positive effect on maternal labor supply, this may increase access to employer-provided health insurance. I look at three variables related to health insurance: an indicator for whether the child is covered by any health insurance at the time of interview; and two indicators for whether she has public insurance and private insurance. As pre-K programs can also increase families' disposable income, by increasing labor supply or reducing expenditures in child care, they may reduce families' financial constraints for the utilization of health care. To assess this, I create an indicator variable for whether the child had health care access problems due to financial reasons, based on two survey questions that ask whether there was a time in the past 12 months when medical care was delayed or the child did not get medical care because the family could not afford it. Finally, I look at two variables related to health care utilization: an indicator for whether the child had a hospital stay in the past 12 months, and an indicator for any ER visit related to an asthma episode in the past 12 months. Both of these variables indicate utilization of health care for potentially serious health events.

1.4 Results

1.4.1 Reduced-Form Effects of Pre-k Programs on Child Outcomes

Table 1.4 presents reduced-form estimates of the effects of a state pre-K expansion on development and health outcomes, by gender and age-group (1-4 and 5-8 years after pre-K age). For all outcome variables presented here, a positive effect can be interpreted as a detrimental effect. All regressions are estimated separately for each age-group, and include state and cohort fixed effects, individual controls for gender, race/ethnicity and maternal education, and state-level controls for federally-funded Head Start enrollment when the child was 4 years old, and SCHIP/Medicaid eligibility income-to-poverty ratio thresholds, annual unemployment rate and annual state median income, at age 4 and at the current year. I have also estimated separate regressions for each specific year after pre-K, whose point estimates and 95% confidence intervals are ploted in Figure 1-2.

The results for the pooled sample of both genders (Panel A of Table 1.4) show no statistically significant effects on the development outcomes, and positive effects on the variables indicating health problems during the first four years after pre-K. Children who live in a state with a Pre-K program at age 4 miss an average of 0.7 days of school more during the following 4 years, and have a health problems index that is 0.12 standard deviations higher. Both effects are significant at a 1% significance level.[22] Consistent with this, respondents are more likely to report that the child's health status is fair or poor, although this effect is only significant at 10% significance level. Despite these deleterious short-run effects, there are no statistically significant effects on health outcomes 5 to 8 years after pre-K, except for a weakly significant effect on reported health status.

In the separate regressions for each specific year after pre-K (top panel of Figure 1-2), the samples are too small to obtain robust estimates, but in general the results are in line with what I find for the grouped-age samples: there are no significant effects for developmental outcomes, and there are some statistically significant positive effects on the health problems index and missed days of school in the first to third years after pre-K.

Because developmental and behavioral problems are much more prevalent for boys than girls, it could be that pooled estimates mask differential effects by gender. Panels B and C of Table 1.4 show estimates of the reduced-form effects 1-4 and 5-8 years after pre-K, for the separate samples of boys and girls, respectively. Similarly, the center and bottom panels of Figure 1-2 show the point estimates and confidence intervals for the samples of boys and girls, respectively, from separate regressions for each year after pre-K. There are negative effects on special education placement for boys, with a statistically significant decrease of 3.4 percentage points (p.p.) in years 1-4 after pre-K, and a non-significant effect of smaller magnitude for years 5-8. The estimated effects on the development problems index for boys are also negative but only statistically significant for years 5-8 after pre-K, with a decrease of 0.13 standard deviations. The graph for the development index for

[22] To account for the fact that standard errors are clustered at the state level and the number of states is relatively small, all significance tests and confidence intervals are computed using a t-student distribution with $G-1$ degrees of freedom, where G is the number of states in the sample. In my main estimation sample there are 33 states.

boys in Figure 1-2 shows that the point estimates are negative every year, although never statistically significant. In contrast, the results for girls show no significant improvement in any of the development outcomes.

The finding of beneficial effects on developmental outcomes for boys are in line with some of the previous findings in the literature for Head Start. The only paper that estimates separate effects of Head Start for boys and girls is Deming (2009), who finds that Head Start has positive effects on tests scores at different ages during childhood for boys but not for girls. Carneiro and Ginja's (2014) identification strategy only allows them to estimate treatment effects for boys but not for girls; they find beneficial effects of Head Start participation for boys at ages 12-13 on outcomes such as being overweight and a behavior problems index, as well as on an index of symptoms of depression at ages 16-17, but not on cognitive test scores. By contrast, Anderson (2008) re-analyzes the impacts of the Perry Preschool Program and other model programs by gender and finds more consistent evidence of positive impacts for girls than for boys, especially on educational attainment.

When looking at the effects on the individual components of the development problems index (Appendix Table A1), the reduction of the index for boys after 4 years seems to be driven by a reduction in learning disability diagnosis, while the effects on the other components are also negative but not statistically significant. While most of the effects on the components of the index for girls are not statistically significant, there is a significantly positive effect on learning disability diagnosis 5-8 years after pre-K that would suggest a potentially deleterious effect for girls in the medium run.

To explore what specific conditions may be behind the deleterious short-run impacts on child health, Appendix Table A2 presents the effects on each of the components of the health index. The short-run effects seem to be driven by an increase in frequent diarrhea, which is the only statistically significant effect, but the point estimates of the effects have the same sign for the four outcomes. This effect goes away 5 to 8 years after pre-K. When looking at the disaggregated components of the health index by gender, there is a differential pattern for girls: there is a significant increase in the likelihood of having an asthma episode for girls 5-8 years after pre-K. These findings are consistent with an increased exposure to infections during preschool that does not produce a protective effect on the immunological system, or at least not in the short- and medium-run. However, the channel through which pre-K is causing these deleterious health effects is not necessarily the direct effect on increased exposure to illness during pre-K; it could also be, for example, through increased maternal labor force attachment or other changes in behavior.

A usual concern when looking at reported health conditions is that awareness of some health problems may be sensitive to changes in access to health care. The health outcomes that compose the health problems index are acute health problems that can easy be observed by parents, which reduces this concern. Additionally, the information on reported health conditions is supplemented by the information on missed days of school and reported health status. Although both of them are also reported by the survey respondent, it is reassuring that the estimated effects on all of them are consistent.

Changes in access to care may be more of a concern for the reporting of developmental outcomes incorporated in the development index, especially learning disability diagnosis and ADHD diagnosis. If pre-K attendance increases access to health care, either through increased maternal employment or increased access to public programs, this could improve the diagnosis and treatment of certain conditions, biasing the estimates towards finding increases in diagnosis. To explore if there is any evidence to be concerned about this, Table 1.5 shows the reduced-form effects of pre-K expansions on health care utilization and insurance outcomes. There are no statistically significant changes in the probability of having health insurance, or in the indicator for health care access problems (*Could Not Afford Care*). Not only are the estimates not significantly different from zero for these two variables (for both genders pooled and separately), but the point estimates are also very small and quite precisely estimated. There appears to be, however, a shift from private to public insurance 5-8 years after pre-K.

In estimates of the reduced-form effects on health care utilization and insurance outcomes of pre-K expansions interacted with race/ethnicity groups (presented in Appendix Table A3), I find that this shift towards public health insurance is driven by Hispanic children only, while there are no significant changes for the other race/ethnicity groups. There is an increase in public insurance after pre-K for all age-groups of Hispanic children, which also leads to a significant overall increase in the number of Hispanic children with any insurance 1-4 years after pre-K. However, there is no indication that any of the effects that I find on health outcomes are driven by this group; there are no significantly differential health effects on Hispanics (results presented in Appendix Table A4). There is, however, a significantly differential effect for Hispanics in the development problems index, indicating an increase in the diagnosis of developmental problems for Hispanics that may be related to increased access to health care. In sum, changes is access to health care may be a source of concern only for Hispanic children, but neither the deleterious health effects, nor the beneficial effects on developmental outcomes for boys are driven by this group.[23]

Table 1.5 also shows the estimated reduced-form effects on hospital stays and asthma-related emergency room visits. There are no significant short- or medium-run effects on any of these two variables, either for the pooled sample of both genders or for each gender separately. This suggest that any deleterious short-term effects are not serious enough to show up in increased hospitalizations, and if there is an increase in the incidence of asthma episodes for girls in the medium-run they do not translate into increased emergency room visits for this reason.

1.4.2 Robustness of the Reduced-Form Estimates

Correlation of pre-K programs with other state characteristics

The main threat to identification of the effects of pre-K expansions on child outcomes is the possible existence of other confounding factors that change at the state level at a similar time as the pre-K expansions are implemented. A possible concern is that states may implement pre-K policies when their economies are strong and they have enough funding. Another concern is that the implementation of pre-K programs may be correlated with the implementation of other social programs. Finally, because the reduced-form models assume that the state of birth is the same as the state where the child currently lives, an

[23] I have also explored the heterogeneity of effects by maternal education (college graduate or not) and by type of program (universal or targeted). I do not find any relevant differential effects, but the estimates are imprecise. The results are available upon request.

additional concern is that the demographic composition of the states may be changing for these cohorts of children in a way that is correlated with the timing of the implementation of pre-K policies. Table 1.6 explores these possibilities by estimating regression models like equation (2) but where the individual demographic controls and the state-level economic and policy variables are used as outcome variables instead of controls. The idea is to check whether the implementation of pre-K programs, conditional on state and cohort fixed effects, is predictive of the economic circumstances of states, their demographic composition, or the generosity of other state social programs that may have confounding effects. Although this is not a direct test of the exogeneity of the pre-K policies, it can be informative to test its conditional orthogonality with potentially confounding observed demographic, economic and state policy factors.

Table 1.6 presents the results of this exercise. The top panel refers to individual demographic characteristics, and each column shows the effects of the pre-K policy variable on different child and family characteristics, as observed for children 1-4 and 5-8 years after pre-K, controlling for state and cohort fixed effects. The outcome variables are dummy variables for female, black, Hispanic, and being born in the U.S., child age in months, dummies for the mother having a college degree and being married, and the mother's age in years. The estimates are not significantly different from zero, so there is no evidence of any changes in the demographic composition of the cohorts affected by pre-K policies.

The bottom panel of Table 1.6 presents the results for state-level variables. First, I check whether pre-K policies are predictive of the federally-funded Head Start enrollment rate in the state when the child was 4 years old. If states invest in pre-K programs as a substitute for low federal Head Start funding, or if pre-K programs draw students from Head Start programs, this would bias the estimated impacts on child outcomes because the control group would have higher participation in Head Start while the treatment group has higher participation in state pre-K. On the other hand, if states invest more in pre-K at the same time as the federal government invest in Head Start, the estimated effects would confound the effects of the two programs. The estimates in the first column of the bottom panel of the table show that the coefficients for pre-K on Head Start are not significantly different from zero. The point estimates are negative and, if significant, would imply a very small effect: Head Start enrollment is estimated to be 0.3 percentage points lower after a pre-K program is implemented.

Second, I test whether pre-K is predictive of the generosity (income-to-poverty ratio eligibility limits) of the State Child Health Insurance Program and Medicaid (whichever is lower). I look at two variables: the eligibility requirements for children of ages 1-5, measured when the child was 4 years old, and the eligibility requirements for children ages 6-15, measured when the child is observed in the NHIS. Again, none of the estimates are significantly different from zero.

Finally, the last four columns of the bottom panel of Table 1.6 present the estimates of regressions where the outcome variables are state economic conditions (unemployment rate and median income) when the child was 4 years old and when the child is observed. The estimated coefficients are small and not statistically significant. Similarly, there is no evidence of pre-K policies at age 4 being predictive of the current economic circumstances in the states where the children in the sample live, as measured by current unemployment rate and state median income.

Alternative Specifications and Samples

This section explores the sensitivity of the paper's main results to alternative model specifications, inclusion of control variables, and selection of sample states. I focus on the two main groups of effects that the results show: the beneficial effects on development outcomes of boys, and the deleterious effects on health outcomes of children of both genders in the short-run, with only medium-run effects for girls in the health problems index. Thus, I present robustness checks for the two development outcome variables for boys, the three health outcomes for both genders, and the health problems index for girls.[24]

One of the threats to the identification of treatment effects in a generalized difference-in-difference model like the one used here is if there already existed divergent trends in the outcome variables between the treatment and the control groups. After repeating the main results in Panel A, Table 1.7 presents the results when state-specific linear time trends are added as controls to the main reduced-form specification. The impacts on

[24]The results for the other outcomes and samples are available upon request.

health outcomes for both genders are virtually unchanged, with only a slightly larger point estimate on the indicator for fair/poor health status in the short run. Because the samples for each specific state and year are relatively small, the inclusion of state trends reduces the statistical power of the regression models, especially for the disaggregated samples by gender. The estimated effects on special education placement in the short-run, and development problems index in the medium-run for the sample of boys are not significantly different from zero in this specification, but this is mainly a product of larger standard errors, as the point estimates are very slightly changed with respect to the main specification. The results suggest that the estimated effect on the health problems index for girls 5-8 years after pre-K is sensitive to the inclusion of state linear time trends, with a coefficient that is half as large as in the main specification and not statistically significant. Ultimately, the only evidence of long-run health effects from the main results turns out not to be robust to the inclusion of state-specific time trends. Perhaps this is not surprising, given that I do not find significant effects for girls after four years on either missed days of school or reported health status.

As discussed in the previous section, another potential threat to identification is the variation of other public policies across states that may be affecting child outcomes at the same time as the implementation of pre-K programs. In the main specification I include controls for the generosity of state public health insurance for children and for the enrollment in Head Start program. Another policy that may be simultaneously affecting the preschool attendance and development and health outcomes of children is the Child Care Development Fund (CCDF). CCDF is a childcare subsidy program targeted towards low-income working families, implemented through a federal block grant. Although the federal government establishes some eligibility requirements, it gives states freedom to decide how to implement the subsidies. States can allocate TANF funds to the program, they can establish family income eligibility limits below the federal maximum, and they are responsible for determining eligibility controls, payment rates, and requirements for child care providers. Previous literature has found that receiving the child care subsidy increases maternal supply but has negative effects on children's cognitive and behavioral outcomes, and increases the prevalence of child obesity.[25] If the timing of changes in state regulations or funding of CCDF subsidies is correlated with the implementation of pre-K policies, the regression coefficient for preschool attendance instrumented by the pre-K policies could pick up some of the causal effects of the subsidy. Panel C of Table 1.7 shows the reduced-form results when the model includes a control variable indicating the number of 4-year-olds served by the CCDF in each state. The estimates are less precise than the main specification because there is no information on the CCDF for the first year of my sample period, and thus these regressions are estimated with a smaller sample. The main results are all in line with the main estimates, with some differences in the sizes of the effects on the development outcomes for boys (of larger magnitude) and on the health index for both genders and for the girls sample (smaller positive effects).

Because the policy variable is at the state level and the number of treatment and control states are not very large (15 and 18 states, respectively), it is possible that the results could be driven by one particular outlier state. To show that this is not the case, I re-estimate the main specification of the reduced-form regressions taking one state out of the sample at a time. The results from this exercise are shown in Figure 1-3. Each plot shows the point estimates and confidence intervals, for the outcome and sample in the column heading, of the effect of pre-K policies 1-4 and 5-8 years after pre-K (top and bottom rows, respectively). The first thing to note is that the point estimates for all the outcomes presented are fairly unchanged when each state is taken away from the sample. For the short-run effect on special education placement for boys 1-4 years after pre-K, and on the development problems index 5-8 years after pre-K, there are three states whose exclusion from the sample makes the estimates more imprecise, turning them marginally non-significantly different from zero at a 5% significance level. In both cases the three states are part of the treatment group, so it is not unexpected that taking them away from the sample would make the estimated treatment effects weaker. I also explore the robustness of the main results to the inclusion of all *excluded states* in the sample. Because

[25] Blau and Tekin (2007) show that child care subsidy receipt increases the labor supply of single mothers. Herbst and Tekin (2010) find that receiving the subsidy in the year before kindergarten is associated with lower reading and math test scores and greater behavior problems at kindergarten entry, for children from single mothers. Herbst and Tekin (2012) find that subsidized child care leads to increases in the prevalence of overweight and obesity among low-income children.

these are states by the beginning of the sample period already have state pre-K programs, and enrollment rates of 4-year-olds in those states was increasing during the period, their inclusion in the control group should cause a bias toward finding no effects of pre-K programs. The results, presented in panel D of Table 1.7, show that most of the estimated treatment effects are slightly attenuated, but none of the main conclusions are changed.

I also show that the results of the estimations for the binary outcomes (special education placement and health fair/poor) are not a product of the choice of a linear probability model, by showing the results for the average marginal effects of a Probit model with the same control variables as the main specification (panel E of Table 1.7). Finally, the last panel of Table 1.7 presents the main results for the reduced-form specification that is used in the next section to estimate the TS-2SLS models. This specification only includes state and cohort fixed effects and individual demographic controls. It omits all the child age and state-level control variables, which are not used in the first-stage regression of preschool attendance of 4-year-olds. The main estimates are practically unchanged, with very small differences in some point estimates.

In sum, the robustness checks conducted in this section indicate the robustness of the beneficial effects for boy's developmental outcomes, and the deleterious short-run effects on health outcomes for both genders. However, the medium-run impacts on girls' health problems index are not robust to some specifications, so I cannot conclude that there is robust evidence of any lasting impacts on child health five to eight years after preschool age.

1.4.3 Effects on Preschool Attendance at Age 4 and TS-2SLS Results

As discussed in section 1.3.2, I cannot directly estimate treatment-on-the-treated effects, because I do not have individual-level information on enrollment in state pre-K. I use two alternative strategies to approximate or bound the increase in enrollment in state pre-K.

The first strategy consists of approximating the increase in pre-K enrollment caused by the pre-K expansion in treatment states using the average increase in pre-K enrollment in each treatment states in the years before and after the implementation of each expansion, within the period for which state-level pre-K enrollment rates are reported by NIEER (2001 to 2005).[26] Table 1.9 shows the enrollment rates in pre-K programs by treatment state from 2001 to 2005, reported by NIEER (2006), and the average enrollment in the periods before and after each expansion. Based on these estimates, and using the population of 4-year-olds by state as weights, I estimate an average increase in enrollment in pre-K programs of 17.7 percentage points.

Table 1.10 presents the estimates of the *treatment-on-the-treated* effects of pre-K attendance on development and health outcomes using this first strategy. As explained in Section 1.3.2, these estimates are obtained by re-scaling the reduced-form effects of pre-K expansion by the estimated increase in state pre-K enrollment. The estimates for β_{TT1} after the corresponding reduced-form estimates, in the second, fifth, and eighth columns of Table 1.10, for the pooled, boys only, and girls only samples, respectively. Each panel shows the results for a different outcome variable, for the sub-samples of children observed 1-4 and 5-8 years after pre-K age.

Since the reduced-form effects on health outcomes are similar for boys and girls, I discuss these for the pooled sample. The estimated effect for the health problems index suggest that pre-K enrollment, relative to the average alternative mode of child care, increases the index during the first four years after pre-K by 59% of a standard deviation of the control group. The estimate for missed days of school suggest that the number of sick days are increased by 3.4 days in a year, which implies an increase of about 100% relative to the control mean. These estimates imply relatively large effects, especially given that they are the average effect across children observed 1 to 4 years after preschool age, and we would expect any negative health effects to fade out over time.[27]

In terms of developmental outcomes, the estimates for boys suggest that pre-K attendance reduces the likelihood of special education placement for boys, 1-4 years after pre-K, by 20 percentage points, and it reduces the development problems index 5-8 years after pre-K by 84% of a standard deviation of the control group. These estimated effects are very large but are also imprecisely estimated, so much smaller treatment effects are

[26] For states with expansions implemented before 2001, I use additional sources of information to approximate (as detailed in the footnote to Table 1.9).

[27] For the expansion of subsidized childcare in Quebec, Backer, Gruber and Milligan (2008) estimate the TT effects on the rise in the rate of nose/throat infection for children of between 237 and 451 percent. These impacts are measured at ages 0 to 2, while children are still exposed to child care.

within the confidence interval.

The second strategy I use to approximate treatment-on-the-treated effects uses the first-stage effect of pre-K expansions on overall preschool enrollment (including other preschool programs). The results of this first stage are presented in Table 1.8. My preferred specification includes state and cohort fixed effects and individual-level controls (dummies for race/ethnic group, gender, and maternal educational attainment). The average estimated effect of a pre-K policy expansion is a 7.7 percentage point increase in the probability of being enrolled in preschool, which implies a 13% increase relative to the control states' average preschool enrollment throughout the period (see summary statistics in Table 1.3).[28] The second and third columns of Table 1.8 show the estimates for the separate samples of boys and girls, respectively. The effects are statistically significant for both genders. The point estimate for the effect is larger for girls (9.8 p.p.) than boys (6 p.p.), but I cannot reject the hypothesis that the two coefficients are equal at a 5% significance level.

Given the heterogeneity of the programs studied, I explore whether there are heterogeneous impacts on enrollment across targeted and voluntary universal pre-K programs. In general, targeted programs are smaller and implemented in disadvantaged school districts, while universal programs have a more rapid and broader roll-out. While targeted programs have an average enrollment rate of 12% of their states' four-year-olds by the end of the period, enrollment in universal programs average 39%. Column 4 of Table 1.8 shows the results of interacting the Post Pre-K expansion variable with an indicator for whether the program is universal. I cannot reject the hypothesis that the coefficient for the interaction of *Post Pre-K* and the indicator for a universal program is zero, meaning that there is no statistically significant difference in the impact of the two types of programs on overall preschool enrollment rates. Given that enrollment in universal programs is higher, this suggests that there is substantial crowding out from other preschool programs in states

[28] Appendix Table A5 presents robustness tests of the first stage regression for the pooled sample of both genders. The estimates change very little when I include state-level control variables and state-specific linear trends in the regression. Including the *excluded states* in the control group reduces the estimated effect, which is expected given that we are including in the control group states that had pre-K programs whose enrollment was growing during the sample period. The estimated average marginal effects are unchanged if a Probit model is estimated instead of the linear probability model (results available upon request).

with voluntary universal pre-K.

This estimated increased in preschool enrollment is substantially smaller (less than half) than the estimated 17.7 p.p. increase in pre-K enrollment discussed above. Although the latter is not an estimate of the causal effect of the pre-K expansions on pre-K enrollment, the large difference between these two estimates suggests the presence of large crowding-out effects. Since I do not have data on the specific type of program that each child attends, I cannot directly test the hypothesis of crowding-out. The CPS asks whether the child attends public or private school (or preschool), but it is not clear whether this identifies the source of funding.[29] Nevertheless, I present the estimates of the effects of the two types of programs on reported public preschool attendance (Column 5), and private preschool attendance (Column 6) as suggestive evidence. The results of Column 5 suggest that attendance to public preschool is incremented more in states with universal voluntary pre-K programs (10.7 p.p.) than in states with targeted programs (3.8 p.p.). In the case of private preschool attendance, the estimates indicate a 4.4 p.p. increase in attendance in states with targeted programs, and a 4.4 p.p. *decrease* in states with universal programs, suggesting significant crowding-out.[30]

Table 1.10 presents the Two-Sample Two-Stage Least Squares (TS-2SLS) estimates of the effects of pre-K attendance on development and health outcomes (β_{TT2}). The results for the TS-2SLS estimates β_{TT2} are presented in the third, sixth, and ninth columns of Table 1.10, for the pooled, boys only, and girls only samples, respectively. As explained in Section 1.3.2, these estimates are obtained by re-scaling the reduced-form effects of pre-K expansion by the increase in preschool enrollment that resulted from these pre-K expansions.[31] These results provide an approximation to the magnitude of the *treatment-on-the-treated* (TT) effects of pre-K attendance on child outcomes, under the assumption

[29] Head Start programs and many state pre-K programs have public funding but are carried out by private providers, and it is not clear that parents can identify whether a particular preschool center is publicly or privately funded unless it is actually located in a public school.

[30] These results on the crowding-out effects of universal pre-K programs are similar to what Cascio and Schanzenbach (2013) find for the introduction of the universal pre-K programs of Georgia and Oklahoma in 1995 and 1998, respectively, also using October CPS data. They also find that crowding-out was larger for children whose mothers had higher educational attainment levels.

[31] Because the same control variables must be used in both stages, I use the specification of the reduced-form regressions that only includes state and cohort fixed effects and individual demographic controls (they do not include age dummies and state-level controls).

that pre-K expansions only affect children who attend preschool as a consequence of the expansion of these programs and would not attend preschool otherwise. As shown in equation 1.11, in the absence of crowding-out of other preschool programs, this estimator would give us the TT effects. However, as discuss above, there is suggestive evidence that there is significant crowding-out. If this is the case, this TS-2SLS strategy will yield estimated TT effects that are larger in magnitude than the true average TT effects.

Equation 1.12 provides some help for interpreting these estimates in the presence for crowding out. If the treatment effects of attending a pre-K program and an alternative preschool program, relative to being at home or informal child care, are similar, we would expect $LATE_{ck}$, the effect of switching from another preschool program to pre-K, to be close to zero. If this is the case, then β_{TT2} provides an estimate of $LATE_{nk}$. In other words, if pre-K is as good as any preschool program, then we would not expect any effect for the children who switch from other preschool programs to pre-K, and it would be correct to assume that the children who are "treated" are those who switch to pre-K from home or informal childcare. If pre-K programs are "better" than other preschools ($LATE_{ck} > 0$), then β_{TT2} over-estimates $LATE_{nk}$, i.e. it provides an upper bound for the effect of switching to non-center based child care arrangements to pre-K. On the contrary, if pre-K programs are "worse" than other preschools ($LATE_{ck} < 0$), then β_{TT2} under-estimates $LATE_{nk}$. Actually, a better quality of pre-K compared to alternative preschools is not the only reason why switching from other preschools to pre-K might have a positive net effect. For families that substitute public pre-K for private child care, the public provision of pre-K implies an increase in disposable income, which may in turn positively affect children. In addition, children may be simultaneously enrolled in more than one preschool program, as many programs are only part-day. Consequently, pre-K expansions may increase the amount of hours per week that children are exposed to preschool education (intensive margin), rather than just the actual attendance to preschool (extensive margin). The group of children potentially affected by pre-K expansions through the intensive margin includes children who would otherwise be attending some preschool program anyway.

For the pooled sample, where the first-stage is strongest (the F statistic of the instrument is 13.6), the estimated effects on health outcomes are large but less precisely estimated than the reduced-form effects. For example, the estimates for the health problems index suggest that pre-K attendance increases the index during the first four years after pre-K by 1.4 standard deviations of the control group, but it is only statistically different from zero at a 10% level. Similarly, the results in the last column suggest that the number of missed school days are increased by close to 8 days in a year.

The TS-2SLS estimates for the samples of boys and girls are imprecise because the first stages for the stratified gender samples are weaker due to the smaller sample sizes, with an F-statistic close to 7 for boys and 8 for girls. The TS-2SLS estimates for boys suggest that pre-K attendance reduces the likelihood of special education placement for boys 1-4 years after pre-K by 60 percentage points, and it reduces the development problems index 5-8 years after pre-K by 2.5 standard deviations of the control group. These estimated effects are extremely large but very imprecisely estimated, and are only statistically different from zero at a 10% significance level. Thus, I cannot rule out much smaller treatment effects. Even when the imprecision of the estimates does not allow me to rule out smaller effect sizes, the generally large magnitude of the point estimates suggest that the relevant "treatment" is not just preschool attendance, and that children who would be enrolled in other preschool programs otherwise may also benefit from the expansion of state pre-K education.[32]

1.5 Conclusion

In this paper, I have presented new evidence on the short- and medium-run effects of state-funded pre-K programs on child development and health outcomes. I overcome the lack of data sources with information on both attendance to pre-K programs at age 4 and health and developmental outcomes throughout childhood, by collecting information on the implementation of state pre-K programs between 1997 and 2005, and merging this information with individual-level data from the National Health Interview Survey for

[32] Deming (2009) finds that Head Start participation improves an index of cognitive outcomes (composed by learning disability diagnosis and grade retention) by 39% of a standard deviation for boys 3 to 10 years after preschool age, relative to not attending preschool. In general, he finds that Head Start has larger effects on cognitive outcomes than other other preschool programs, suggesting that Head Start is of higher quality than alternative preschool programs.

children observed 1 to 8 years after pre-K age, and with state-level control variables from various sources. I use these data to estimate the reduced-form effects of pre-K programs on a set of child development and health outcomes, including a development problems index and a health problems index that summarize information from multiple outcome variables, in regression models with state and cohort fixed effects.

My results suggest that pre-K programs have beneficial effects on development outcomes for boys both in the short and medium run, but not for girls. This is perhaps not surprising, given that the development and behavioral problems that I look at are much more prevalent for boys. The results also suggest that pre-K programs can have some deleterious health effects during the first four years after preschool age for children of both genders, which is reflected in an increase in an index of health problems and on missed school days. However, there are no significant effects on hospitalizations or asthma-related ER visits, and there is no robust evidence of any medium-run effects on health outcomes.

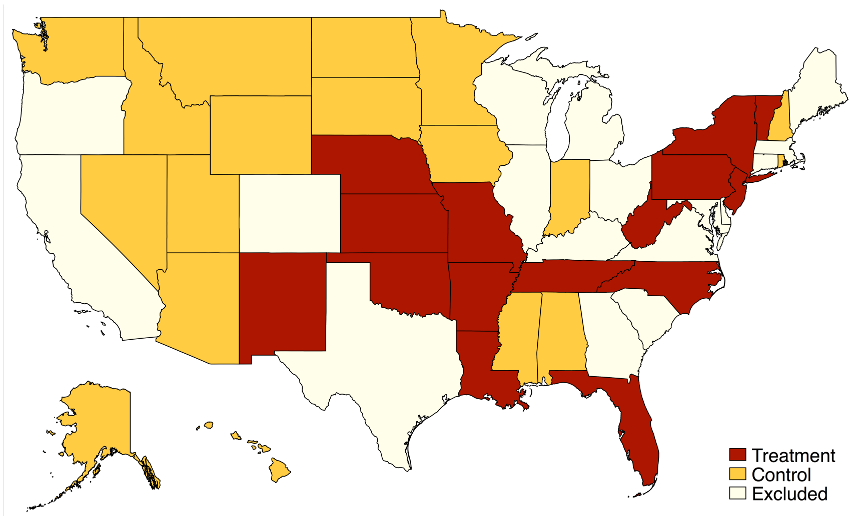
The lack of individual-level information on attendance to pre-K prevents me from having a precise estimate of the size of the pre-K expansion, which would allow me to estimate average *treatment-on-the-treated* effects. Using aggregate pre-K enrollment rates for treatment states between 2001 and 2005, I approximate the difference in enrollment rates before and after the implementation of pre-K expansions to be about 17.7 percentage points on average. I also provide estimates of how many 4-year-olds enroll in preschool as a consequence of the implementation of these programs, finding that enrollment rates are increased by 7.7 percentage points. The difference between these two estimates, although not conclusive, suggest the presence of relevant crowding-out from other preschool programs. When I use these two alternative estimates to approximate *treatment-on-the-treated* effects, the results suggest that the magnitudes of the effects are very large, although they are imprecisely estimated so I cannot reject much smaller effect sizes.

The results from this analysis have important implications for the literature on the effects of preschool education programs. This paper is the first to estimate health effects of state pre-K programs, finding deleterious short-run effects that are in line with the findings in papers that study the effects of child care subsidies, but against some of the findings of the literature on the federal Head Start program. This raises questions for future research on preschool programs, in terms of the channels that may explain these effects and the role that specific program characteristics can play to prevent these effects.

The findings of this paper also show that preschool programs can have impacts on developmental outcomes throughout childhood, as evidenced by the decrease in the development problems index for boys. This result is in line with previous findings of lasting impacts on test scores for boys, and underscores the importance of studying the heterogeneity of impacts of early education programs by gender. Finally, these results also suggest additional channels for explaining the long-term impacts on labor market, educational attainment, and crime found in previous literature on preschool programs.

1.6 Figures and Tables

Figure 1-1: Map of Treatment, Control, and Excluded States

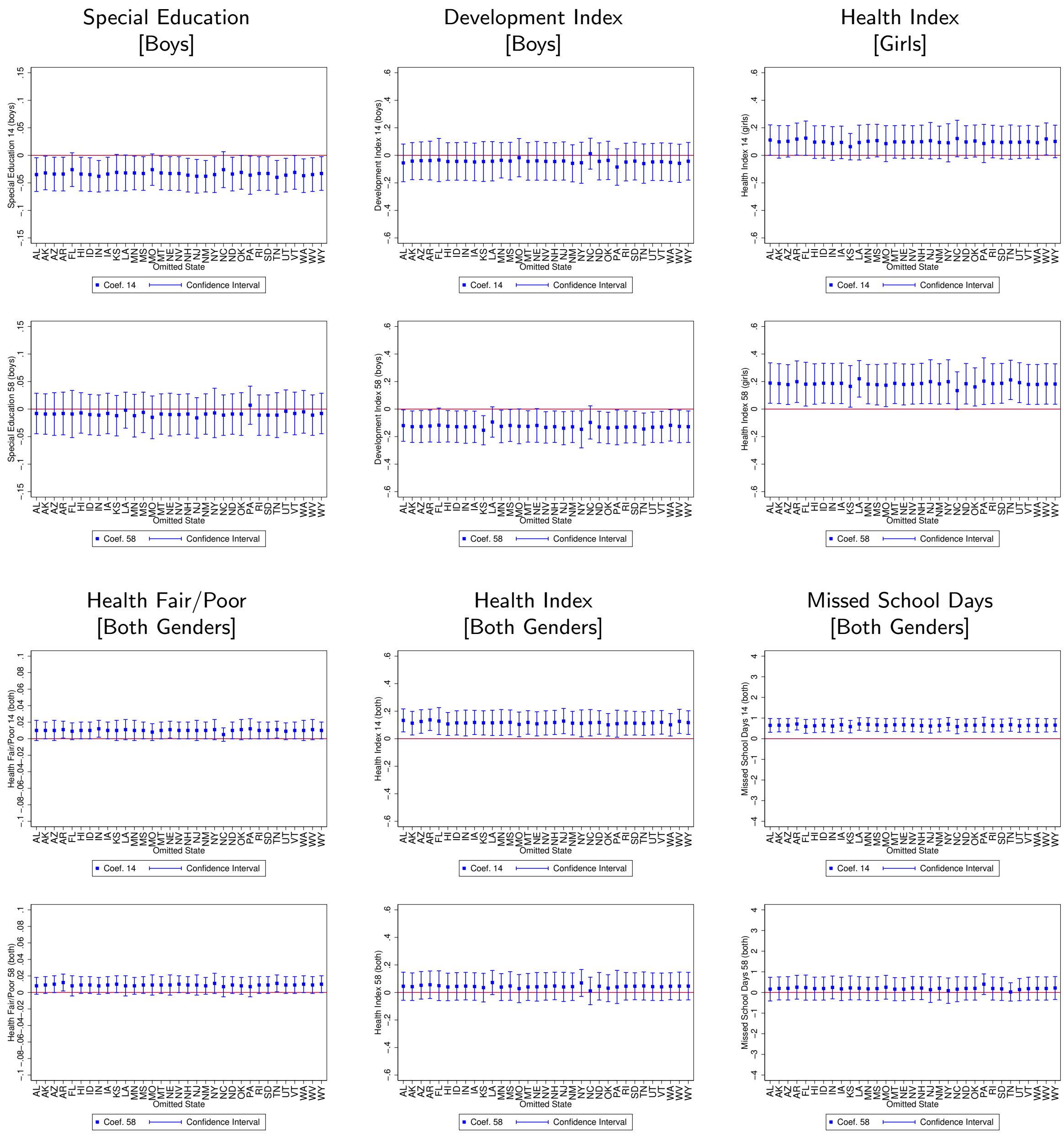


Notes: *Treatment* indicates that a state implemented or significantly expanded pre-K between 1998 and 2005. *Control* indicates that a state had not yet implemented or had only a very small state pre-K program by 2005. *Excluded* indicates that a state is excluded from the main sample because it already had a pre-K program by 1997. For more details on sample states definitions see the Background Section.

Figure 1-2: Reduced-Form Effects for Each Year after Pre-K, by Gender

Notes: Figure plots the reduced-form estimate of the effects of a pre-K expansion on developmental and health outcomes in each year after pre-K age, by gender. Shaded region is 95% confidence interval. Each point estimate is obtained in a separate regression, with the sample corresponding to both genders (top panel), boys (middle panel) or girls (bottom panel) observed in the NHIS *sample child* sample the number of years after pre-K age indicated in the x-axis. Standard errors clustered at state level.

Figure 1-3: Sensitivity of Reduced-Form Results to Each State in Sample



Notes: Figure plots the reduced-form estimate of the effects of a pre-K expansion on developmental and health outcomes when excluding one state at a time from the main sample. Each point estimate is obtained in a separate regression, with the sample corresponding to boys or both gender, as indicated in the column headings, observed 1-4 years after pre-K (top row of each panel) and 5-8 years after pre-K (bottom row of each panel).

Table 1.1: Pre-K Program Characteristics in Treatment States in 2005

State (Year Implemented)	Classroom Hrs/week	Income Targeted	Health Components	CELS	Spending/ Student	NIEER Score	4-Year-Old Enrollment
Arkansas (2004)	38	Yes	Yes	Yes	7,769	9	0.18
Florida (2005)	15	No	Yes	Yes	2,163	4	0.47
Kansas (2002)	11	Yes	Yes	No	2,554	3	0.15
Louisiana (2002)	40	Yes	Yes	Yes	5,012	8	0.22
Missouri (1999)	DL	Yes	No	Yes	2,632	6	0.04
Nebraska (2001)	DL	Yes	DL	Yes	7,418	8	0.04
New Jersey (1999)	30	Yes	Yes	Yes	9,854	9	0.18
New Mexico (2005)	14	Yes	Yes	Yes	2,269	5	0.07
New York (1998)	DL	No	Yes	No	3,512	5	0.29
North Carolina (2002)	30	Yes	Yes	Yes	3,892	10	0.12
Oklahoma (1998)	DL	No	DL	Yes	6,167	9	0.70
Pennsylvania (2004)	20	Yes	DL	Yes	4,730	4	0.06
Tennessee (2005)	28	Yes	Yes	Yes	4,061	9	0.11
Vermont (2003)	10	No	Yes	Yes	2,930	7	0.47
West Virginia (2002)	12	No	Yes	Yes	7,758	7	0.40
Mean (or % Yes)	16	0.67	0.73	0.87	4,848	6.87	0.23
Median					4,061	7	0.18

Notes: Source: NIEER (2006). Year Implemented is the year program was established or expanded. Classroom Hrs/week is the number of hours in classroom that all providers must offer per week. Income Targeted indicates there are income limits for eligibility of students, or a minimum percentage of low-income for eligibility of a school district, school or program. Health Components indicates providers required to offer physical, vision and hearing screenings and referrals (some also include dental and/or developmental screenings and referrals). CELS indicates providers are required to follow comprehensive early learning standards. Spending/Student is the total spending per enrollee in 2005-2006 (including state funding, local matching funds, and federal grants administered by the state. NIEER Score is the number of NIEER quality standards met by the program in 2005-2006 (out of 10). 4-Year-Old Enrollment is the percentage of 4-year-olds in the state that were enrolled in state funded pre-K in school year 2005-2006. DL stands for determined locally.

Table 1.2: NHIS Sample Characteristics in Treatment, Control and Excluded States

	Treatment States		Control States		Excluded States	
	Mean	S.D.	Mean	S.D.	Mean	S.D.
Individual Characteristics						
Female	0.49	0.50	0.49	0.50	0.49	0.50
Black	0.18	0.38	0.10	0.29	0.15	0.35
Hispanic	0.15	0.36	0.13	0.34	0.26	0.44
Other Race/Ethnicity	0.04	0.21	0.07	0.26	0.06	0.23
Age (years after pre-K age)	4.49	2.30	4.55	2.29	4.55	2.29
Mom High-School Graduate	0.27	0.44	0.24	0.43	0.23	0.42
Mom Some College	0.19	0.39	0.21	0.41	0.20	0.40
Mom College Graduate	0.40	0.49	0.42	0.49	0.39	0.49
State Characteristics at Pre-K Age						
Federal Head Start Enrollment (%)	12.01	3.31	12.39	6.52	11.77	2.79
SCHIP/Medicaid Income-to-Pov Ratio (Age 1-5)	2.18	0.60	2.05	0.44	2.03	0.39
Annual Unemployment Rate	4.88	0.85	4.59	1.12	5.12	1.11
Median Income ($1,000s)	52.97	7.25	56.50	7.87	58.01	5.94
State Characteristics at Current Age						
SCHIP/Medicaid Income-to-Pov (Age 6-15)	2.44	0.66	2.24	0.41	2.21	0.35
Annual Unemployment Rate	5.88	1.84	5.69	2.07	6.46	2.19
Median Income ($1,000s)	52.63	7.61	55.81	8.00	57.31	6.66
Outcome Variables						
Special Education	0.08	0.28	0.07	0.26	0.08	0.26
Development Index	0.03	1.06	0.00	1.00	0.00	0.99
Health Index	0.03	1.07	0.00	1.00	0.01	1.03
Health Status Fair/Poor	0.02	0.14	0.02	0.12	0.02	0.14
School Days Missed	3.33	5.30	3.21	4.49	3.20	5.81
Learning Disability Diagnosis	0.08	0.27	0.07	0.26	0.08	0.27
ADHD Diagnosis	0.08	0.27	0.08	0.27	0.07	0.26
Limitations Caused by Speech Problems	0.03	0.16	0.02	0.14	0.02	0.14
Limitations Caused by Behavior Problems	0.02	0.12	0.02	0.13	0.01	0.12
3+ Ear Infections	0.05	0.21	0.05	0.22	0.05	0.21
Asthma Episode	0.07	0.25	0.06	0.23	0.06	0.24
Frequent Headaches/Migraine	0.06	0.23	0.05	0.21	0.05	0.22
Frequent Diarrhea	0.01	0.11	0.01	0.10	0.01	0.11
Any Hospitalization	0.02	0.13	0.01	0.11	0.02	0.13
Asthma ER Visit	0.02	0.15	0.01	0.12	0.02	0.14
Could Not Afford Care	0.04	0.20	0.05	0.22	0.05	0.21
Any Insurance	0.91	0.28	0.90	0.30	0.90	0.30
Public Insurance	0.30	0.46	0.26	0.44	0.29	0.45
Private Insurance	0.61	0.49	0.65	0.48	0.61	0.49
Observations	12,060		6,016		20,866	

Notes: Summary statistics for the NHIS samples of children living in treatment, control, and excluded states, observed 1 to 8 years after pre-K age, for the pre-K cohorts of 1997 to 2005, observed between 1998 and 2014 (weighted using sample weights). State characteristics at pre-K age are imputed according to the state where the child currently lives and the estimated year when the child would have been 4 years old (based on month/year of birth and month/year of interview). State characteristics at current age correspond to the state of residence and the interview year. All state characteristics are merged to the NHIS samples from other data sources (see Data section for more details).

Table 1.3: CPS Sample Characteristics in Treatment, Control and Excluded States

	Treatment States		Control States		Excluded States	
	Mean	S.D.	Mean	S.D.	Mean	S.D.
Individual Characteristics						
Female	0.48	0.50	0.48	0.50	0.50	0.50
Black	0.17	0.37	0.10	0.30	0.14	0.35
Hispanic	0.14	0.34	0.12	0.33	0.24	0.43
Other Race/Ethnicity	0.05	0.22	0.07	0.26	0.06	0.24
Mom High-School Graduate	0.31	0.46	0.29	0.45	0.28	0.45
Mom Some College	0.18	0.38	0.20	0.40	0.19	0.39
Mom College Graduate	0.38	0.49	0.39	0.49	0.35	0.48
State Characteristics						
Federal Head Start Enrollment (%)	11.91	3.24	12.72	7.15	11.81	2.68
SCHIP/Medicaid Income-to-Pov Ratio (Age 1-5)	2.19	0.59	2.03	0.43	2.03	0.40
Annual Unemployment Rate	4.90	0.85	4.64	1.14	5.17	1.10
Median Income ($1,000s)	53.11	7.14	56.12	7.86	58.04	5.73
Outcome Variables						
Preschool	0.61	0.49	0.58	0.49	0.61	0.49
Public Preschoo	0.29	0.45	0.27	0.44	0.32	0.47
Private Preschool	0.32	0.47	0.31	0.46	0.28	0.45
Any School	0.68	0.46	0.64	0.48	0.68	0.47
Kindergarten	0.07	0.26	0.05	0.22	0.08	0.27
Observations	4,765		4,115		6,661	

Notes: Summary statistics for the CPS samples of children living in treatment, control, and excluded states, observed at 4 years of age, 1997 to 2005 (weighted using sample weights). All state characteristics are merged to the CPS from other data sources (see Data section for more details).

Table 1.4: Reduced-Form Effects on Development and Health Outcomes

	Special Education	Development Index	Health Fair/Poor	Health Index	Missed School Days
Panel A: Both Genders					
1-4 Years After Pre-K					
Post Pre-K	-0.016	-0.050	0.010*	0.116***	0.651***
	(0.010)	(0.056)	(0.005)	(0.042)	(0.156)
N	9069	9069	9069	9069	8825
N Treatment States	6171	6171	6171	6171	5993
5-8 Years After Pre-K					
Post Pre-K	0.014	-0.050	0.009*	0.044	-0.006
	(0.010)	(0.043)	(0.005)	(0.049)	(0.023)
N	9007	9007	9007	9007	8921
N Treatment States	5889	5889	5889	5889	5821
Panel B: Boys					
1-4 Years After Pre-K					
Post Pre-K	-0.034**	-0.044	0.006	0.138**	0.587**
	(0.015)	(0.067)	(0.009)	(0.060)	(0.270)
N	4643	4643	4643	4642	4510
N Treatment States	3154	3154	3154	3153	3061
5-8 Years After Pre-K					
Post Pre-K	-0.009	-0.127**	0.006	-0.067	0.226
	(0.018)	(0.056)	(0.008)	(0.066)	(0.432)
N	4576	4576	4576	4576	4537
N Treatment States	3015	3015	3015	3015	2983
Panel C: Girls					
1 to 4 Years After Pre-K					
Post Pre-K	0.002	-0.053	0.017*	0.099*	0.750**
	(0.010)	(0.057)	(0.010)	(0.058)	(0.337)
N	4426	4426	4426	4425	4315
N Treatment States	3017	3017	3017	3017	2932
Girls, 5 to 8 Years After Pre-K					
Post Pre-K	0.041*	0.069	0.014	0.185**	0.207
	(0.021)	(0.085)	(0.008)	(0.071)	(0.287)
N	4431	4431	4431	4431	4384
N Treatment States	2874	2874	2874	2874	2838

Notes: Each cell shows results for separate regressions, for the outcome variable indicated in the column heading, and the sample (gender and age–number of years after pre-K age) indicated in each panel heading. All regressions include state and cohort fixed effects, individual-level control variables for maternal education and race/ethnicity (and gender in the Panel A), age dummies, and state-level control variables. Robust standard errors (clustered by state) in parentheses. * p<0.1, ** p<0.05, *** p<0.01.

Table 1.5: Reduced-Form Effects on Health Care Utilization and Insurance

	Hospital Stay	Asthma ER Visit	Could Not Afford Care	Any Insurance	Public Insurance	Private Insurance
			Panel A: Both Genders			
1-4 Years After Pre-K						
Post Pre-K	0.007	0.005	-0.007	-0.008	-0.003	-0.008
	(0.005)	(0.008)	(0.006)	(0.008)	(0.022)	(0.020)
N	9065	8376	9069	8858	8841	9044
N Treatment States	6168	5673	6171	6025	6017	6153
5-8 Years After Pre-K						
Post Pre-K	0.005	-0.004	-0.011	0.013	0.053***	-0.040**
	(0.006)	(0.007)	(0.012)	(0.011)	(0.019)	(0.017)
N	9003	8497	9007	8793	8763	8988
N Treatment States	5887	5519	5889	5764	5755	5875
			Panel B: Boys			
1-4 Years After Pre-K						
Post Pre-K	0.008	0.006	-0.017	-0.019	0.016	-0.051**
	(0.007)	(0.016)	(0.010)	(0.014)	(0.027)	(0.025)
N	4640	4222	4643	4536	4528	4631
N Treatment States	3152	2850	3154	3071	3068	3145
5-8 Years After Pre-K						
Post Pre-K	0.009	-0.009	-0.017	0.014	0.066**	-0.061**
	(0.007)	(0.009)	(0.012)	(0.019)	(0.030)	(0.024)
N	4574	4265	4576	4476	4467	4564
N Treatment States	3014	2784	3015	2951	2948	3005
			Panel C: Girls			
1-4 Years After Pre-K						
Post Pre-K	0.008	0.006	0.004	0.002	-0.023	0.035
	(0.008)	(0.007)	(0.012)	(0.017)	(0.041)	(0.032)
N	4425	4154	4426	4322	4313	4413
N Treatment States	3016	2823	3017	2954	2949	3008
5-8 Years After Pre-K						
Post Pre-K	0.002	0.002	-0.004	0.009	0.040*	-0.023
	(0.008)	(0.006)	(0.018)	(0.015)	(0.022)	(0.025)
N	4429	4232	4431	4317	4296	4424
N Treatment States	2873	2735	2874	2813	2807	2870

Notes: Each cell shows results for separate regressions, for the outcome variable indicated in the column heading, and the sample (gender and age–number of years after pre-K age) indicated in each panel heading. All regressions include state and cohort fixed effects, individual-level control variables for maternal education and race/ethnicity (and gender in the first panel), age dummies, and state-level control variables. Robust standard errors (clustered by state) in parentheses. * p<0.1, ** p<0.05, *** p<0.01.

Table 1.6: Prediction of Demographic Characteristics and State-Level Controls

	Female	Black	Hispanic	Born in U.S.	Age (Months)	Mom College	Mom Married	Mom's Age (Years)
1-4 Years After Pre-K								
Post Pre-K	0.031	0.016	0.013	-0.004	-0.019	0.015	-0.000	-0.196
	(0.019)	(0.028)	(0.014)	(0.008)	(0.177)	(0.031)	(0.018)	(0.286)
N	9069	9069	9069	9062	8381	9069	9069	9023
5-8 Years After Pre-K								
Post Pre-K	0.002	-0.011	-0.014	0.008	-0.331	0.006	0.002	0.199
	(0.016)	(0.023)	(0.014)	(0.010)	(0.205)	(0.033)	(0.024)	(0.377)
N	9007	9007	9007	9002	8290	9007	9007	6361

	Fed. Head Start Enroll. at 4	SCHIP/ Medicaid (1-5) at 4	SCHIP/ Medicaid (6-15)	Unemploy-ment Rate at 4	Unemploy-ment Rate	Median Income at 4	Median Income
1-4 Years After Pre-K							
Post Pre-K	-0.286	0.222	0.026	-0.137	0.030	-0.097	0.108
	(0.217)	(0.135)	(0.077)	(0.173)	(0.185)	(0.673)	(0.600)
N	9069	9069	9069	9069	9069	9069	9069
5-8 Years After Pre-K							
Post Pre-K	-0.291	0.209	-0.048	-0.132	-0.144	-0.080	0.182
	(0.219)	(0.132)	(0.071)	(0.158)	(0.244)	(0.636)	(0.333)
N	9007	9007	9007	9007	9007	9007	9007

Notes: Each cell shows results for separate regressions, for the outcome variable indicated in the column heading, and the sample of both genders for the age (number of years after pre-K age) indicated in each panel heading. All regressions include state and cohort fixed effects, and age dummies. Robust standard errors (clustered by state) in parentheses. * p<0.1, ** p<0.05, *** p<0.01.

Table 1.7: Alternative Specifications and Samples of Main Reduced-Form Effects

	(1) Main	(2) Trends	(3) CCDF	(4) All States	(5) Probit	(6) FE+Ind.
Special Education [Boys]						
Post Pre-K *(1-4 Years After Pre-K)*	-0.034**	-0.028	-0.044***	-0.027**	-0.036*	-0.036**
	(0.015)	(0.020)	(0.016)	(0.013)	(0.015)	(0.015)
Post Pre-K *(5-8 Years After Pre-K)*	-0.009	-0.004	-0.012	-0.009	-0.008	-0.013
	(0.018)	(0.021)	(0.024)	(0.020)	(0.018)	(0.018)
Development Index [Boys]						
Post Pre-K *(1-4 Years After Pre-K)*	-0.044	-0.004	-0.014	-0.020		-0.057
	(0.067)	(0.067)	(0.078)	(0.068)		(0.066)
Post Pre-K *(5-8 Years After Pre-K)*	-0.127**	-0.110	-0.194***	-0.105*		-0.149**
	(0.056)	(0.093)	(0.067)	(0.061)		(0.063)
Health Fair/Poor [Both]						
Post Pre-K *(1-4 Years After Pre-K)*	0.010*	0.017**	0.013*	0.010**	0.013*	0.010**
	(0.005)	(0.007)	(0.008)	(0.004)	(0.006)	(0.005)
Post Pre-K *(5-8 Years After Pre-K)*	0.009*	0.012	0.013**	0.009*	0.009	0.008
	(0.005)	(0.007)	(0.006)	(0.005)	(0.005)	(0.005)
Health Index [Both]						
Post Pre-K *(1-4 Years After Pre-K)*	0.116***	0.123**	0.081*	0.082**		0.104**
	(0.042)	(0.053)	(0.045)	(0.036)		(0.044)
Post Pre-K *(5-8 Years After Pre-K)*	0.044	0.003	0.014	0.019		0.040
	(0.049)	(0.051)	(0.055)	(0.049)		(0.055)
Health Index [Girls]						
Post Pre-K *(1-4 Years After Pre-K)*	0.099*	0.108*	0.097	0.112*		0.085
	(0.058)	(0.062)	(0.082)	(0.062)		(0.061)
Post Pre-K *(5-8 Years After Pre-K)*	0.185**	0.079	0.139*	0.098		0.158**
	(0.071)	(0.067)	(0.075)	(0.075)		(0.077)
Missed School Days [Both]						
Post Pre-K *(1-4 Years After Pre-K)*	0.651***	0.612***	0.620***	0.561***		0.595***
	(0.156)	(0.157)	(0.187)	(0.121)		(0.159)
Post Pre-K *(5-8 Years After Pre-K)*	-0.006	0.014	0.069	0.217		0.105
	(0.023)	(0.366)	(0.301)	(0.235)		(0.245)

Notes: Each cell shows results for separate regressions, for the outcome variable and gender indicated in the panel heading, and for the sample of children observed the number of years after pre-K age indicated in the row headings. Column (1) repeats the main results presented in Table 1.4 (specification includes controls for state and cohort fixed effects, age dummies, individual demographic controls and state-level controls). The specification in Column (2) includes all the controls of the main specification and state-specific linear time trends. The specification in Column (3) includes all the controls of the main specification and the number of 4-year-olds served by the CCDF in each state. Column (4) shows results of the main specification when *excluded* states are included in the sample. Column (5) shows results of marginal effects from Probit models for the binary outcomes, with the same controls as the main specification. Column (6) presents results for regressions that only include state and cohort fixed effects and individual demographic controls. Robust standard errors (clustered by state) in parentheses. * $p<0.1$, ** $p<0.05$, *** $p<0.01$.

Table 1.8: First Stage Effects of Pre-K Expansions on Preschool Enrollment

	Preschool [Both]	Preschool [Boys]	Preschool [Girls]	Preschool [Both]	Public [Both]	Private [Both]
Post Pre-K	0.077***	0.060**	0.098***	0.082***	0.038	0.044
	(0.021)	(0.023)	(0.034)	(0.025)	(0.023)	(0.029)
Post Pre-K * Universal				-0.018	0.069**	-0.088***
				(0.025)	(0.031)	(0.024)
Observations	8880	4610	4270	8880	8880	8880
F(Post Pre-K)	13.6	6.8	8.4			

Notes: Each column shows regression estimates of the effect of Pre-K expansions on preschool attendance, except for the last two columns, where the outcome variable is public preschool and private preschool attendance, respectively. Each regression is estimated on the sample of children age 4 in the October CPS 1997-2005 in Treatment and Control States; the genders included in the sample are indicated in the column header between brackets. All regressions include state and cohort fixed effects, and individual controls (indicator variables for race, maternal education, and gender when both genders are included in the sample). Robust standard errors (clustered by state) in parentheses. * $p<0.1$, ** $p<0.05$, *** $p<0.01$.

Table 1.9: State Pre-K Enrollment in Treatment States 2001-2005

State	State Pre-K Enrollment					Year	Post Pre-K	Pre Pre-K	Enrollment	4-Year-Old
	2001	2002	2003	2004	2005	Implemented	Average	Average	Increase	Population
Arkansas	6	6	6	12	18	2004	12	6	6	36,909
Florida	0	0	0	0	47	2005	47	0	47	194,475
Kansas	6	15	15	15	15	2002	15	6	9	38,444
Louisiana	12	21	22	20	22	2002	22	12	10	64,196
Missouri	5	4	4	4	4	1999	4	0^a	4	75,416
Nebraska	2	3	4	3	4	2001	3	1^b	2	23,881
New Jersey	20	24	26	26	25	1999	23	11^c	14	114,766
New Mexico	1	1	1	1	7	2005	7	1	6	26,461
New York	25	30	30	29	29	1998	28	4^d	24	256,184
North Carolina	1	6	9	10	12	2002	8	1	7	107,107
Oklahoma	56	59	64	68	70	1998	60	32^e	28	47,075
Pennsylvania	2	2	2	5	6	2004	5	2	3	152,001
Tennessee	2	3	3	3	11	2005	11	3	9	74,575
Vermont	9	10	36	45	47	2003	41	9	32	7,421
West Virginia	24	29	33	35	40	2002	31	24	7	21,141
All Treatment States										1,240,052

Notes: Enrollment rates correspond to years 2001-2005 for each treatment state, from NIEER (2006). The population of 4-year-olds by state corresponds to the information from the 2000 Census (US Census Bureau). *Post Pre-K Average* enrollment is computed as the simple average of the enrollment rates for the years after a state pre-K policy was implemented or expanded, if implemented in or after 2001, or the simple average of years 2001-2005, if implemented before 2001. *Pre Pre-K Average* enrollment is computed as the simple average of the enrollment rates for the years before a state pre-K policy was implemented or expanded with available information, if implemented after 2001. For states with pre-K programs implemented in 2001 or before, the pre-pre-K enrollment rates were computed using information from other sources, as indicated in the note corresponding to each figure.

[a] No pre-K program existed before 1999.

[b] A pilot program was being implemented before 2001. Because no information is available, I make the (conservative) assumption that the expansion duplicated the size of the pilot program. This yields an pre-expansion average enrollment similar to the average enrollment rates of control states in 2001.

[c] Pre-K expansion corresponds to program implemented for Abbot Districts, so pre-expansion enrollment was computed by adding Non-Abbot districts' enrollment in 2002 (NIEER, 2003) and the estimated Abbot districts enrollment of 4-year-olds in 1998, from Frede et al. (2009).

[d] EPK enrollment in 2001.

[e] Mainly operates in public schools, so assumption that increase is captured by enrollment in public preschools in CPS is reasonable. From CPS, public preschool enrollment in Oklahoma: 0.23 in 1993-1997 and 0.51 in 1998-2002, so increase of 28 p.p.

Table 1.10: TS-2SLS Effects on Development and Health Outcomes

	Both Genders			Boys			Girls		
	$\hat{\beta}_{RF}$	$\hat{\beta}_{TT1}$ (ΔPre-K $= 0.177$)	$\hat{\beta}_{TT2}$ ($\hat{\beta}_{FS}$ $= 0.077$)	$\hat{\beta}_{RF}$	$\hat{\beta}_{TT1}$ (ΔPre-K $= 0.177$)	$\hat{\beta}_{TT2}$ ($\hat{\beta}_{FS}$ $= 0.060$)	$\hat{\beta}_{RF}$	$\hat{\beta}_{TT1}$ (ΔPre-K $= 0.177$)	$\hat{\beta}_{TT2}$ ($\hat{\beta}_{FS}$ $= 0.098$)
Special Education									
1-4 Years After Pre-K	-0.018 *	-0.102 *	-0.235	-0.036 **	-0.203 **	-0.604 *	0.000	0.000	0.000
	(0.010)	(0.056)	(0.145)	(0.015)	(0.084)	(0.343)	(0.009)	(0.050)	(0.091)
5-8 Years After Pre-K	0.011	0.062	0.144	-0.013	-0.073	-0.218	0.038 *	0.214 *	0.386
	(0.010)	(0.056)	(0.136)	(0.018)	(0.100)	(0.314)	(0.019)	(0.106)	(0.235)
Devel Probl Index									
1-4 Years After Pre-K	-0.055	-0.310	-0.718	-0.057	-0.321	-0.957	-0.051	-0.288	-0.518
	(0.052)	(0.290)	(0.706)	(0.066)	(0.368)	(1.167)	(0.057)	(0.318)	(0.606)
5-8 Years After Pre-K	-0.065	-0.367	-0.848	-0.149 **	-0.840 **	-2.501 *	0.058	0.327	0.589
	(0.042)	(0.234)	(0.594)	(0.063)	(0.352)	(1.429)	(0.081)	(0.452)	(0.848)
Health Fair/Poor									
1-4 Years After Pre-K	0.010 *	0.056 *	0.131 *	0.007	0.039	0.117	0.016	0.090	0.163
	(0.005)	(0.028)	(0.074)	(0.009)	(0.050)	(0.158)	(0.010)	(0.056)	(0.116)
5-8 Years After Pre-K	0.008	0.045	0.104	0.006	0.034	0.101	0.011	0.062	0.112
	(0.005)	(0.028)	(0.071)	(0.008)	(0.045)	(0.140)	(0.009)	(0.050)	(0.099)
Health Probl Index									
1-4 Years After Pre-K	0.104 **	0.587 **	1.357 *	0.129 **	0.728 **	2.165	0.085	0.479	0.864
	(0.044)	(0.246)	(0.682)	(0.059)	(0.329)	(1.294)	(0.061)	(0.340)	(0.688)
5-8 Years After Pre-K	0.040	0.226	0.522	-0.046	-0.259	-0.772	0.158 **	0.891 **	1.605
	(0.055)	(0.307)	(0.732)	(0.066)	(0.368)	(1.147)	(0.077)	(0.430)	(0.960)
Missed Days School									
1-4 Years After Pre-K	0.595 ***	3.356 ***	7.766 **	0.551 **	3.108 **	9.247	0.660 *	3.722 *	6.706
	(0.159)	(0.888)	(2.956)	(0.260)	(1.451)	(5.629)	(0.331)	(1.848)	(4.086)
5-8 Years After Pre-K	0.105	0.592	1.371	0.214	1.207	3.591	0.065	0.367	0.660
	(0.245)	(1.368)	(3.219)	(0.392)	(2.188)	(6.722)	(0.272)	(1.518)	(2.773)

Notes: For each sample, the first column indicates the estimate for the reduced-form effect of a pre-K expansion on the outcomes indicated in the left column; the second column indicates the estimated TT effect β_{TT1} given the proxy for the expansion in pre-K enrollment indicated in the heading; and the third column indicates the TT effect β_{TT2} estimated using a TS-2SLS strategy where the first stage outcome is enrollment in any preschool program. For more details on the two strategies to approximate TT effects, see section 1.3.2. All reduced-form and first-stage regressions include state and cohort fixed effects, and individual-level control variables for maternal education and race/ethnicity (and gender in Panel A). Robust standard errors (clustered by state) in parentheses. * $p<0.1$, ** $p<0.05$, *** $p<0.01$.

Chapter 2

Short-run effects of parental job loss on child health[1]

2.1 Introduction

During the Great Recession, millions of American workers lost jobs as firms restructured, relocated, downsized, and closed in response to changing demand conditions. From January 2007 through December 2009a period encompassing the official beginning and end of the recessionnearly one in six US workers experienced job displacement (Farber, 2011). Not only was the rate of job loss significantly higher during this period than during previous postwar recessions, the rate of reemployment was lower and the average duration of unemployment was longer. The severity of the recent economic downturn has generated renewed interest among researchers in the consequences of job displacement for workers and their families.

Though a substantial literature documents the effects of displacement on outcomes such as earnings, employment, health, and fertility for displaced workers, less is known about the consequences of displacement for another group of potential victims—the children of displaced workers. Given that job displacement causes changes in family income, parental time use, and the physical and mental wellbeing of parents, it is likely to alter

[1] Based on joint work with Jessamyn Schaller.

family dynamics and affect parental investments in children. Recent studies of the effects of job displacement on children's academic outcomes suggest that this is the case, finding that parental job loss is associated with increased likelihood of grade repetition and worse performance on standardized tests (Ananat et al., 2011; Stevens and Schaller, 2009). Parental job loss has also been found to have long-run effects on children from low-income families, reducing their educational attainment and earnings in adulthood (Oreopolous et al., 2008; Page et al., 2009). However, the mechanisms by which parental job loss translates into worse outcomes for children in the short and long run are not well understood.

In this paper, we study the effects of parental job loss on children's physical and mental health. While previous work has shown that job loss is associated with increased mortality and worse physical and mental health among adults,[2] only a few papers have examined the effects of parental job loss on children's health and none have looked at the effects of parental job loss on a broad set of health outcomes.[3] Child health is an important outcome because it is both an indicator of current welfare and a predictor of future outcomes including adult health, educational attainment, and earnings, and thus a potential mechanism for the intergenerational transmission of economic shocks (Currie, 2009).

We exploit detailed data from the Medical Expenditure Panel Survey (MEPS) that allow us to examine the effects of parental job displacement on many different measures of child health and to investigate potential mechanisms behind these effects. The MEPS is a large-scale representative survey that collects information on health outcomes, health insurance coverage, and health care utilization, as well as demographic characteristics and employment, for each member of responding households over a two-year period. We obtain a large sample of children with displaced parents by combining data from 16 waves of the survey, covering the period from 1996 through 2012. To address concerns about potential

[2]See, for example, Browning and Heinesen, 2012; Schaller and Stevens, 2015; Sullivan and von Wachter, 2009

[3]To our knowledge there are only three existing papers on the topic: Liu and Zhao (2014) study the effects of mass layoffs on child height and weight in China, Mork et al. (2013) look at the correlation between parental unemployment and children's hospital stays in Sweden, and Lindo (2011) studies the effects of parental job loss on health at birth using US data.

endogeneity of parental displacement, we limit our sample to workers with at least one year of job tenure and focus on job losses that occur for plausibly exogenous reasons. We include child fixed effects in our main specification so that our estimates are identified by changes in health status after displacement for a given child rather than comparisons between the children of displaced workers and children of continually employed workers, and show that there are no measurable changes in child health in the period prior to parental displacement. Because of the large number of potential outcome variables, we address concerns about multiple inference by creating a set of summary health indices,[4] and by implementing a stepdown method for multiple hypothesis testing (Romano and Wolf, 2005).

Our first finding is that paternal job loss is harmful to children's mental health. Specifically, we find that a father's job loss results in an increase in a summary mental health index of 0.08 standard deviations—approximately 1.8 times the gap in mental health between the highest and lowest quintiles of family earnings in our sample. The negative effect of paternal job loss on children's mental health is robust to p-value adjustments that account for multiple inference concerns, and is apparent for children in low-socioeconomic status (SES) and high-SES families alike. Among children in low-SES families, we additionally find that paternal job loss is associated with increases in the likelihood of fair or poor physical health, infectious illness and trauma (injuries). By contrast, children in high-SES families experience reductions in the incidence of trauma following paternal job loss. We separately show that maternal job loss has no significant effects on children's physical or mental health in the short run, except to slightly reduce the incidence of infectious illness among children in high-SES families—an effect that may be driven by the substitution of maternal care for market-based childcare following displacement.

Turning to the effects of parental job loss on child health insurance coverage and health care utilization, we find that increases in public insurance coverage largely counteract the loss of private insurance coverage after job loss so that the estimated overall effect on children's insurance coverage after displacement is much smaller than corresponding estimates for adults (see Schaller and Stevens, 2015). We find little evidence of changes

[4]This approach follows Kling et al. (2007), Deming (2009), and Hoynes, Schanzenbach and Almond (2016)

in routine, diagnostic, or emergency medical care following parental job loss.

Taken as a whole, our results show that the mental and physical health of children in low-SES families is negatively affected by recent paternal job loss—a finding that potentially helps to explain the long-run effects of paternal job loss on the education and labor market outcomes of children in low-SES families seen in other research. While children in high-SES families also experience negative mental health effects of parental job loss, our results show that both paternal and maternal job loss are associated with improvements in some physical health measures in high-SES families, which suggests that changes in time use after job loss, and in particular the substitution of parental care for market-based childcare, may be an important mechanism driving short-run changes in health among children in high-SES families.

The rest of the paper is organized as follows. Section 2 provides theoretical background and discusses the mechanisms through which parental job loss might be expected to affect child health. Section 3 summarizes several strands of related literature. Sections 4 and 5 describe the data and empirical strategy, respectively, and include discussions of endogeneity and multiple inference concerns. Section 6 presents our main results, including a robustness check, and Section 7 explores heterogeneity in the effects of parental job loss on child health by socioeconomic status, child age, and gender. Section 8 discusses the robustness of our estimates to controlling for local economic conditions, as well as the heterogeneity of the effects of parental job loss according to the state of the economy, and Section 9 concludes.

2.2 Theoretical Background and Potential Mechanisms

In the standard model of child health production in economics (Grossman, 2000; Currie, 2009), parents are assumed to maximize an inter-temporal utility function whose arguments in each period are the stock of child health, the consumption of other commodities, and leisure. The health stock in any given period is a function of the health stock of the previous period, its depreciation rate, and the health investments made in the previous period. The health production function depends on both exogenous productivity shifters and permanent individual productivity shifters. Finally, the investment inputs in this production function include material inputs (including health care) and parental time inputs.

Within this framework, there are a few ways in which parental job loss can affect a child's health stock. First, the reduction in income associated with job loss can affect consumption and health investments, such as nutritious food, preventive health care, and the practice of physical exercise. Second, the loss of a job can cause the loss of employer-provided health insurance for the worker and his/her dependents. This will affect the price and the quality of available health care and may lead to reduced use of health care, especially related to preventive care, treatment of chronic conditions, and purchase of prescription drugs. For children, however, the effects of job loss on health insurance coverage may be mediated by the availability of the other parent's employer-provided health insurance, as well as the availability and take-up of public health insurance programs such as Medicaid and the State Children's Health Insurance Program (SCHIP).

Job loss may also change the availability of parental time and its allocation towards child health production, especially in the short run. A parent who recently lost a job may spend more time caring for the child, increasing non-market time inputs to health production, which may also increase the amount of health care received by the child (doctor visits, for example). Additionally, increased availability of parental time, combined with a reduced income, may cause children to spend less time in daycare, preschool, or after-school activities, which may reduce their exposure to illness or change their likelihood of incurring injuries. A final avenue by which parental job loss might lead to changes in child health is increased parental stress caused by job loss and the associated income shock. Parental stress might affect child health directly by causing children to experience more stress themselves or it might affect the quality of care that children receive.

The many potential mechanisms discussed above make it unclear whether we should expect job loss to lead to improvement or deterioration in child health on average. We can, however, make predictions about how these effects might vary depending on family, parent, and child characteristics. For one thing, the effects of job loss are likely to depend on the gender of the displaced parent. Theoretical and empirical research in psychology, sociology, and economics suggests that the stress effects of job loss are greater for men than for women (Leana and Feldman, 1988; Waters and Moore, 2002; Eliason and Storrie, 2009; Kuhn et al., 2009) and that the impact of parental job loss on families and children is greater when fathers are displaced (Kalil and Ziol-Guest, 2008; Rege et al., 2011). This has been attributed both to the fact that male job loss often results in a larger income shock and to a cultural emphasis on the role of the men as breadwinners. Meanwhile, it is possible that maternal job displacement may be associated with improved outcomes for children, as maternal employment has been found to have negative effects on child health (Anderson et al., 2003; Gennetian, 2010; Morrill, 2011) and women are more likely to take on home-production and caregiving roles during periods of joblessness (Aguiar et al., 2013; Lindo, Schaller, and Hansen, 2013).

Other important sources of potential heterogeneity in the effects of fathers' and mothers' job losses include family earnings, parental educational attainment, and the number of earners in the family. Families with lower income or less education and single-earner families may experience more stress upon job loss and are likely to have fewer resources with which to moderate shocks to earnings and insurance coverage. Meanwhile, shifts in time use may be greater in high-SES families as displaced parents (particularly secondary earners) may choose to remain unemployed longer. It is also possible that the effects of parental displacement may be heterogeneous by child age and gender. Though this type of heterogeneity is difficult to characterize a priori, one point is worth noting: with outcomes such as infectious illness, for which changes in time use are an important mechanism, it is possible that the effects will be more pronounced among children who are not yet school-aged. At the same time, any differences across age groups will be muted if young children are exposed to infectious illness through parents or older siblings (or vice versa), or if older children are also changing their time use (for example, reducing participation in after-school care or extra-curricular activities) in response to job loss.

2.3 Related Literature

The literature on job displacement has only recently started to look at the consequences on children. Previous papers discuss the effects of parental job displacement on children's future earnings, finding different results for different countries and samples.[5] Some papers have looked at how parental job displacement affects educational outcomes of children, finding that it increases the likelihood of grade repetition (Stevens and Schaller, 2011; Kalil and Ziol-Guest, 2008), worsens school performance (Ananat et al., 2011; Rege et al., 2011), and reduces the likelihood of enrolling in post-secondary education (Coelli, 2011). Notably, papers that separately examine male and female displacements typically find negative effects following fathers' job losses only (Kalil and Ziol-Guest, 2008; Rege et al., 2011). Meanwhile, those that stratify by income find that the negative effects of parental job displacement are stronger among low-income families (Oreopoulos et al., 2008; Page et al., 2009).

So far, the only paper that has looked at the effects of parental job loss on child health in the US is Lindo (2011). Using data from the Panel Survey of Income Dynamics (PSID), Lindo compares the birth-weight of siblings born before and after a job loss. The results indicate that job displacement of the husband reduces the birth-weight of subsequent children by 4.5%, with larger treatment effects below the median of the birthweight distribution. Other papers have looked at child health effects of job displacements in other countries. Liu and Zhao (2014) look at job displacement in the context of mass layoffs from publicly owned firms in China following the reforms initiated in the 1990s. They find that the father's job loss has a large negative impact on height and weight of children, whereas they don't find evidence of an effect of mother's job loss. Mork et al. (2014) look at the effect of parental unemployment on child health outcomes using administrative data from Sweden. They find that children with unemployed parents are 1 percent more likely to be hospitalized in the same year as the job loss, and 5 percent more likely in the long

[5] Oreopoulos et al. (2008) show that fathers' job displacement has a large negative effect on children's young adult earnings, using data for Canada. Page et al. (2009) only find significant effects for children that initially come from low income households in the U.S., but their sample is small. Bratberg et al. (2008) use administrative data from Norway, a country with a much lower intergenerational correlation of earnings, and find that job displacement reduces future earnings of the worker but not of their children.

run. However, due to data limitations they are not able to separately identify the effects of plausibly exogenous job displacement from all causes of job loss.

The evidence on the effects of job displacement on adult health is more abundant. Our paper is closest in methods to Schaller and Stevens (2015). Using data from the MEPS, they look at the effect of involuntary job loss on a worker's health outcomes in the short-run. They find that job loss has substantial negative effects on mental health and that it increases the likelihood of activity limitations and fair or poor self-reported physical health. However, they find no effects on the likelihood of reporting a number of specific chronic health conditions, including arthritis, diabetes, high cholesterol, and hypertension, and they find reductions in the incidence of infectious illness among adults after job loss. Other papers that look at job displacement and adult health have found significant effects on adult mortality, suicide risk, cardiovascular health, risky behaviors such as alcohol abuse and smoking, traffic accidents and mental illness (Sullivan and Von Wachter, 2009; Deb et al., 2011; Classen and Dunn, 2012; Browning and Heinesen, 2012; Black, Devereux and Salvanes, 2015).

A second strand of literature related to this paper is that on the stability of health insurance coverage and the effects of unemployment on access to health care. The loss of insurance coverage following displacement could potentially lead directly to changes in health status if it causes individuals to reduce their utilization of medical care. Among adults, Gruber and Madrian (1997) find that job separations (including both layoffs and quits) have a large impact on the probability of having any insurance. Schaller and Stevens (2015) also find significant effects of involuntary job loss on insurance coverage in their study of adults in the MEPS: a 10 percentage point reduction in insurance coverage following job loss among the full adult sample, and a 26 percentage point reduction in coverage among workers that were insured through their employer prior to displacement. They also find negative effects on health care utilization among workers who were insured through their employer prior to displacement.

For children, the effects of job loss on health insurance coverage are likely to be smaller than those for adults. While a majority of both adults and children are insured through an employer-provided policy, there have been large expansions in the eligibility of children for public health insurance. Publicly provided child health insurance has the potential to insulate children from the consequences of job instability. Cawley and Simon (2005) and Cawley et al. (2013) study the effects of state unemployment rates on health insurance coverage for both adults and children, and find that an increase in the unemployment rate significantly decreases the probability of being insured for men, but not for women and children, who they argue are relatively insulated from these fluctuations due to public insurance policies. To our knowledge, the only paper that looks at the effects of parental job loss on child health insurance coverage is that of Fairbrother et al. (2010), which finds large increases in children's likelihood of becoming uninsured in the three months after parental displacement. However, the authors categorize any job separation as a job loss, and they do not control for unobserved characteristics that may be correlated with both a job separation and loss of insurance.

Finally, as job displacement constitutes an arguably exogenous shock to both employment and income, studying its effects on child health can provide insight into the nature of the causal effects of parental employment and family income on child health. With regard to employment status, existing research has documented negative effects of maternal employment on child health outcomes (see, for example, Anderson et al., 2008; Gennetian et al., 2010; Ruhm, 2000; Morrill, 2011), though none have used job displacement as a source of identifying variation. With regard to income, though there is well documented evidence of a positive cross-sectional correlation between family income and child health (Currie, 2009 provides a review of these studies), it has proven difficult to identify causal effects. It could be that unobserved characteristics of the parents or the environment in which the child is raised are correlated with both family income and child health. So far, the few papers that do try to establish the causal effect of income on child health only look at health at birth.[6]

In our paper we are able to build significantly on the existing literature by using

[6]Conley and Bennett (2000, 2001) use mother fixed effects and find that income at time of birth does not have a significant effect on birth-weight in general, but they do find effects for children whose mothers had low birth-weight themselves. A caveat of these papers is that the data they use from the PSID has a relatively small sample. Hoynes, Miller and Simon (2015) exploit variations caused by tax reforms in the generosity of the federal Earned Income Tax Credit (EITC) as a source of exogenous variations in family income. They find that an increase in the EITC income increases the mean birth-weight and reduces the incidence of low birth-weight. They also find that it increases the use of prenatal care and reduces smoking by pregnant women.

a dataset that allows us to (i) identify plausibly exogenous sources of job separation, (ii) link parents to their children and follow them over several survey waves, (iii) obtain information on health insurance coverage, health care utilization, and a variety of health outcomes from the same source, and (iv) explore heterogeneity in the treatment effects of parental job displacement on child health along several dimensions, including family income, parental education, and family structure.

2.4 Data

We use data from the Medical Expenditure Panel Survey (MEPS), maintained by the Agency for Healthcare Research and Quality. Since 1996, each year the MEPS selects a new nationally representative subsample of households participating in the previous year's National Health Interview Survey conducted by the National Center for Health Statistics. In each new panel the respondents are interviewed in five rounds spanning two full calendar years. Round length varies across rounds and across households - in our sample, reference periods are between three and five months, with an average duration of 4.2 months. The survey collects data on reported health status and specific medical conditions, as well as health insurance coverage, health care use, demographic and socioeconomic characteristics, and employment. The information provided by the household respondents is complemented with information collected from a sample of medical providers, which is primarily used by the MEPS as an imputation source to supplement or replace household reported information on visits, diagnosis, and expenditures. Our sample includes 16 waves of the MEPS, covering the period 1996-2012. We limit our sample to children who were 1 to 16 years old and had at least one parent employed at the time of the first interview (round) of the survey.[7]

The MEPS is ideally suited for this analysis for several reasons. First, it provides rich information on child health that includes parental ratings of general health and mental health status, as well as specific health conditions and mental disorders, which are doc-

[7]We do not count self-employed parents as employed when defining our sample. We trimmed 6.4% of the children in the sample because they did not have data for all five rounds of the survey. Another 4.4% of children were dropped from the sample because they had missing data on parental education, mother's marital status, or health outcomes, and 9% of children did not have either parent employed in the first round of the survey.

umented in conjunction with health care use and expenditures. This provides a broad picture of child health and allows us to identify groups of health conditions that are common and/or costly among children and are likely to be related to parental job loss in the short run. Second, it allows us to examine potential mechanisms, such as changes in insurance coverage and health care utilization, using the same dataset. Finally, because it is comprised of many short panels, the MEPS provides a relatively large sample of children with displaced parents, which is unusual in studies of displacement that rely on survey data. This enhances our statistical power and allows us to explore heterogeneity in the effects of parental displacement on child health.

Our indicators for involuntary job displacement are constructed from a section of the MEPS survey in which respondents are asked to choose the main reason why they changed jobs since the last interview from a list of possible responses. In our analysis, we define involuntary displacement as displacement for one of three reasons: "job ended," "business dissolved or sold," or "laid off."[8] Based on this definition, we create a post-displacement indicator variable that turns to one at the interview immediately following displacement and remains "turned on" in all future rounds.

We restrict our samples so that for the analysis of fathers' displacements, the at-risk sample includes all children whose fathers have at least one year of job tenure in the first round of the survey, and for the analysis of mothers' displacements, the at-risk sample includes all children whose mothers have at least one year of job tenure in the first round of the survey. Defining the samples this way ensures that the displaced parents in the sample are somewhat attached to the labor market prior to job loss and that the samples used to help identify the control variables (such as age and seasonal effects) are as similar

[8]Other possible responses include: retired, illness or injury, quit to have a baby, quit to go to school, quit to take care of home or family, quit because wanted time off, quit to take another job, unpaid leave, or other. Although the three causes for job loss we consider are likely to be involuntary, it is possible that layoffs and jobs that end are correlated with unobservable shocks that are also related to child health. Though other possible responses such as "quit to take care of home or family," "illness or injury," and "quit to take some time off," are likely to capture many job changes that are potentially endogenous, we are not able to identify workers who were fired for cause. Appendix Table A1 presents our main results using a definition of displacement that includes only firm closures—an approach that is common in the literature on job displacement. However, limiting the definition of involuntary displacements results in a substantial decrease in the number of displacements that we observe. We also note that the baseline characteristics of individuals displaced in business closure events are quite different from those of the full sample of displaced workers.

as possible to the treatment groups. We note, however, that there are differences in baseline characteristics between children in the father-employed sample and children in the mother-employed sample that make direct comparison of the effects of paternal and maternal job displacement more difficult.[9]

A detailed description of the sources and construction of the health-related variables used in this analysis is provided in Appendix B. The outcome variables that we examine can be divided into the following categories:

(i) *Health outcomes*: We make use of two sources of health information available in the MEPS. First, respondents (usually parents) are asked to rate the health and mental health status of each child in the family according to the following categories: excellent, very good, good, fair, and poor. We generate indicators for whether a child's health and mental health were reported to be fair or poor. We also use data from the MEPS Medical Conditions files, which include information on specific health conditions associated with doctor visits, hospital stays, disability days, or prescription drug purchases, to construct a set of summary health indices reflecting acute (infectious), chronic, and trauma-related (injuries, burns and poisoning) physical health conditions, and mental health conditions. We focus on a carefully selected set of health conditions that might plausibly respond to parental job loss in the short-run and are likely to be apparent externally or display immediate symptoms (and therefore to be diagnosed immediately).

(ii) *Health insurance status*: We look at whether the child is covered by any insurance, private insurance, or public insurance (including Medicaid, SCHIP, Tricare, and other public hospital/physician coverage) at the time of interview.

(iii) *Health care and prescription drug utilization and expenditures*: These include indicators for checkups and well-child visits, diagnostic visits, emergency visits, mental

[9]Many studies of displaced workers restrict their samples to workers with three or more years of job tenure prior to displacement. We estimated models in which we restricted our analysis to the children of workers with three years of tenure in round 1. The estimates were very similar, though less precise. We have also estimated our results on the sample of children whose mother and father were both employed with at least one year of job tenure in round 1, including indicators for both mothers' and fathers' displacements in the same regression. The results, shown in Appendix Table A2, are similar to our main results. Additional analyses for children with different family types (single earner, single parent) are discussed in section 7.1.

health visits, and prescription drug use during each round.

Because of the large array of health outcomes identifiable in the MEPS data and because we are interested in exploring heterogeneous effects of parental job loss for a variety of subgroups, we face a multiple inference problem. One way in which we address this issue is by aggregating health outcomes into a set of summary standardized health indices. As discussed in Anderson (2008), summary indices increase statistical power, which is particularly helpful in analysis of effect heterogeneity across subgroups. Moreover, because each index represents a single test, adding additional outcomes to an index does not increase the probability of a false hypothesis rejection. Following recent empirical studies (Kling et al., 2007; Deming, 2009; Hoynes, Schanzenbach and Almond, 2016), we generate our summary indices by normalizing each variable (subtracting the Round 1 sample mean for the treated group and dividing by the standard deviation) and averaging across the variables within each index. We construct indices representing four health categories: (1) acute (infectious) illness, (2) chronic conditions, (3) trauma-related (physical injury) conditions, and (4) mental health. Details about the components of each of these indices are provided in Appendix B.

Before proceeding, we emphasize that, as in most of the existing literature, all measures of child health in the MEPS are reported by survey respondents (usually the mother). As such, it is possible that changes in these measures may result from changes in the respondent's own mental state, rather than changes in the child's actual health. We have explored this possibility with regressions of both parents' mental health outcomes on both maternal and paternal job displacement, and find no statistically significant associations. Thus, it appears that respondents are not measurably more pessimistic in all of their responses following job loss. Another issue is that, because a medical condition is identified in the data when a health event related to the condition occurs, changes in our health indices may be related to changes in the consumption of health care. We explore the potential for changes in the frequency of medical care to influence reporting directly by looking for changes in the use of routine care after displacement, and again find no statistically significant association. Nonetheless, we interpret our findings with these caveats in mind.

Table 1 presents Round 1 summary statistics by parental displacement status for the father-employed and mother-employed samples. A number of statistically significant differences between the columns highlight the importance of our empirical approach, which includes individual fixed effects and linear time trends that are allowed to vary depending on baseline health status. Specifically, the children of displaced workers are less likely to be white and their parents are less likely to have a college education. The children of displaced workers also are more likely to come from single-earner and single-mother families and families with income below 200 percent of the poverty line, and have lower levels of private health insurance coverage and higher levels of public insurance coverage prior to job loss. Looking at health outcomes, Table 1 shows significant differences between the health of children whose fathers were displaced after Round 1 and children whose fathers were not displaced. In particular, children in the control group are more likely to report acute and chronic health conditions and more likely to have mental health problems. Finally, the means in Table 1 reveal important differences between the father-employed and mother-employed samples that are important to keep in mind when comparing the effects of paternal and maternal job loss. Specifically, black children and children in families with income below 200 percent of the poverty line make up a larger share of the mother-employed sample.

2.5 Empirical Approach

We estimate a series of fixed-effects models, each with a different health-related dependent variable. Our main regression equation is as follows:

$$Y_{it} = \alpha_i + \beta D_{it} + \gamma X_{it} + \delta_t + \varepsilon_{it} \tag{2.1}$$

where Y_{it} is the outcome variable for child i in round t, α_i is a child-specific fixed effect, D_{it} is an indicator for post-parental displacement periods, X_{it} is a vector of time-varying control variables, and δ_t is a set of round dummies. Child fixed effects are included to account for permanent characteristics of children and families that may be related both with child health and the likelihood of parental displacement. The time-varying controls include dummies for child age and the calendar year in which the interview took place, month of interview dummies to control for seasonality in both health outcomes and the likelihood of parental displacement, and separate linear time trends for each of the five baseline health categories. We also control for the length of the round in days, which varies across individuals even within the same panel and round due to variation in interview dates across households. Observations are weighted by MEPS individual sample weights.[10] To adjust for correlations across children within families and correlation within families over time, the standard errors are clustered at the household level.

As discussed in the previous section, the large number of outcome variables in our analysis and our interest in exploring heterogeneity across subgroups open us up to a potential concern about multiple inference. While our summary health indices help to address this issue by reducing the number of hypotheses being tested, we still have a large number of outcome variables in our analysis. For this reason, we also control the familywise error rate (FWER)—the probability of rejecting at least one true null hypothesis—using the step-down algorithms described in Romano and Wolf (2005).[11] The details of this method are described in Appendix C. The resulting adjusted p-values, displayed in our results tables, can be interpreted as the probability that a result as extreme as the observed individual test-statistic will appear when there is no causal basis for any effect.

Within this empirical framework, causal identification of the effects of parental job loss relies on the assumption that the job loss is exogenous with respect to family and child outcomes. In other words, there must be no unobservable *time-varying* factors that are correlated both with the probability of worker displacement and with child health outcomes. It must also be the case that changes in child health do not directly cause changes in the likelihood of parental displacement. While we cannot entirely rule out either of these possibilities, we address concerns about endogeneity in several ways. First, we choose our definition of job displacement carefully, focusing on reasons for job changes

[10] Following Solon, Haider and Wooldridge (2015), we have also conducted our analysis without using sample weights. Though there are some differences between the results from the unweighted analysis and our main results, the discrepancies between the two sets of results are consistent with the known oversampling of minority groups in the MEPS and the heterogeneity in treatment effects that we observe between groups. Unweighted results are available from the authors upon request.

[11] Other recent applications of stepdown methods for FWER correction include Anderson (2008), Barrow et al. (2014), Finkelstein et al. (2012), and Kling et al. (2007).

that are likely to be involuntary and exogenous to child health. Limiting our sample to workers with at least one year of job tenure also helps to address potential endogeneity. Finally, we check for a potential red flag by estimating models in which we include an indicator for the survey round *prior* to displacement to look for any changes in child health that might occur prior to the event.

2.6 Main Results

2.6.1 Parental Job Loss and Child Health

We begin, in Table 2, by estimating the effects of fathers' and mothers' job losses on parent ratings of child health and mental health and on our four summary health indices. The results in the top panel show that paternal job loss has robust negative effects on child mental health. In particular, a father's job loss results in an increase in the mental health summary index of 0.076 standard deviations. This effect is significant at the 1 percent level even after the multiple hypothesis testing (MHT) adjustment. To put the magnitude in context, this coefficient is more than 1.8 times as large as the difference between the mean mental health index for children in the top and bottom quintiles of family earnings in our full sample, and approximately 90 percent of the difference in means between children in two-parent dual-earner families and children in single mother families. Table 2 also shows a weakly significant effect of paternal job loss on the likelihood of reporting fair or poor mental health: an increase of 0.006, or 40 percent of the baseline mean for the treated group. However, this result does not remain significant when p-values are adjusted for multiple-hypothesis testing. In Appendix Table A2, we explore the nature of the increase in the mental health summary index after father's job loss by breaking down the index into its separate components. These results show that the overall effect is driven by relatively large increases in each of the three components of the index: in addition to the increase in the likelihood of reporting mental health to be fair or poor, the incidence of depression and anxiety and the incidence of headache, malaise and fatigue increase by 140 percent and 88 percent respectively, relative to their baseline means in the treated group, following paternal job displacement.

By contrast, Table 2 shows that maternal job loss does not have significant negative effects on child mental health. Instead, maternal job loss is associated with small reductions (0.029 standard deviations) in the acute (infectious) illness summary index that are significant at the 5 percent level according to the naive p-value but do not stand up to MHT adjustment. Coefficients for each separate component of the acute index (in Appendix Table A2) suggest that these small negative effects are driven by decreases in the incidence of otitis (ear infection), intestinal infections, and other infections—of 19.7 percent, 16.9 percent, and 25.3 percent of baseline means, respectively—and not by changes in acute respiratory conditions (by far the most common diagnosis in the acute category) or influenza.

The differences between the patterns in the effects of fathers' and mothers' job losses in Table 2 are interesting in light of the existing literature and potential mechanisms at work. As discussed in Section 2, previous theoretical and empirical research suggests that the stress effects of paternal displacement are likely to be larger than those from maternal job loss. Our finding that children suffer worse mental health following paternal displacement is consistent with this story. Considering the long-run implications of this finding, short-run changes in mental health following paternal job loss might have broader impacts on children's health and academic achievement that could translate into the long-run effects on educational attainment and labor market outcomes that have been found in other studies. Research also tells us that mothers are more likely to spend time as caregivers during periods of unemployment. Thus, the finding that the incidence of infectious illness is possibly decreased among children after maternal displacement can either be explained by reductions in mothers' own exposure to infectious illness in the workplace or by changes in children's exposure from reductions in the use of out-of-home childcare. We note, however, that these effects are small and it is difficult to know whether short-run reductions in infectious illness are a net positive or negative for children's health over the long run.

2.6.2 Parental Job Loss, Health Insurance Coverage, and Health Care Utilization

To investigate the mechanisms behind the health effects observed in Table 2, we next explore the effects of parental job loss on health insurance coverage and healthcare utilization in Table 3. If parents forego treatment for the conditions in question as a result of a lack of insurance coverage or a change in the source of coverage, these results have potentially important implications for the interpretation of our main results. While we believe that the acute nature of many of the health conditions that we consider makes it is unlikely that parents would not seek treatment for these conditions even in the absence of health insurance, we acknowledge the possibility that the observed reduction in the index reflecting acute infectious conditions following maternal displacement may reflect reductions in the likelihood of diagnosis and treatment. Reduced diagnosis may also be masking increased incidence of other health conditions as well. If we find significant decreases in health insurance coverage and routine healthcare use following displacement, then we have reason to be concerned about this issue.

The effects of parental job loss on children's health insurance status are shown in the first three columns of Table 3. The results show that both paternal and maternal job losses lead to reductions in private insurance coverage and increases in public insurance coverage. We see that while the effects of parental job loss on private insurance coverage are fairly substantial (with decreases of 15.4 percent in both the father-displaced and mother-displaced samples), these effects are largely counteracted by increases in the likelihood of public coverage (26 percent in the father-displaced sample and 19 percent in the mother-displaced sample). As a result, the likelihood of having insurance coverage from any source is reduced by only 5-6 percent following displacement. These effects are substantially smaller than the effects found by Schaller and Stevens (2015), who use the MEPS to study the effects of job displacement on adult health outcomes, insurance, and utilization.[12] Thus, our results suggest that families are making use of the public safety net following

[12] Schaller and Stevens (2015) find that job displacement results in a 14.4 percent reduction in the likelihood of having any insurance for adults in the MEPS sample. Part of this difference can be explained by differences in the availability of public insurance coverage to adults; only 8 percent of displaced adults in their sample had public coverage in round 1.

involuntary displacement.

In the remaining columns of Table 3, we explore whether parental job loss results in changes in children's medical care utilization. We acknowledge that changes in utilization may be driven simultaneously by changes in family income, changes in insurance status and source of coverage, and changes in health status, and interpret our findings with caution. Perhaps not surprisingly, given the relatively small changes in insurance coverage that we observe, we find no significant effects of parental displacement on the likelihood of receiving a checkup or well-child visit during the survey round. Thus, it appears that family income shocks and changes in insurance coverage do not substantially affect the use of routine medical care in the short run. This finding is reassuring, as it suggests that our health effects are unlikely to be driven by changes in the likelihood of diagnosis.[13] We also find no significant effects of parental displacement on diagnostic or emergency visits. We do see an increase in the probability of a mental health visit following paternal displacement, which is consistent with the mental health results from the previous table. This effect, though small in absolute terms, is large in relative terms, representing an increase of more than 100 percent from the baseline mean in the displaced sample. Its significance drops just outside the 10 percent range after MHT adjustment.

2.6.3 Timing of the Effects

Next, we estimate models in which we include three separate displacement indicators: one for the period prior to displacement, one for the period in which displacement occurs, and one for the periods after displacement. There are two reasons to do this. The first reason is

[13] To further alleviate the concern that sick children might be less likely to visit the doctor and thus less likely to be diagnosed with a particular medical condition following parental displacement, we additionally investigated the raw and regression-adjusted correlations between parent-reported general health ratings, which are not mechanically related to specific medical events, and the likelihood of checkup or diagnostic visits. We wanted to see if these correlations are different for children whose parents were recently displaced than for other children. The idea behind this exercise is that parents' ratings of their child's overall health status should reflect not only conditions for which the child visited a doctor, but also conditions that the family chose to treat at home or opted not to treat. If the relationship between reported general health and doctor visits is weaker following displacement, we might worry that some conditions are not being officially "diagnosed" in our data. We find that the correlations seen immediately following parental displacement are very similar to those for the rest of our sample. Though they do not necessarily reflect causal relationships, the fact that these correlations don't change following parental displacement suggests that the likelihood of getting treated for a particular health condition also does not change dramatically.

that previous research has shown that the earnings losses associated with job displacement may begin as early as two years before the displacement occurs (Jacobson et al. 1993). Though the reasons for the pre-displacement decline in earnings are unknown, this pattern could mean that child health may be affected by changes in income, parental time use, and stress *before* displacement. This could affect the magnitude of our estimated coefficients if our pre-period is contaminated with treatment effects. For example, if the pre-period treatment effects on a health outcome are negative, then we will be underestimating the total treatment effect in our main specification. The second reason for estimating these models is to use the pre-displacement indicators as a placebo test to reduce concerns about the endogeneity of parental displacement. However, this relies on the assumption that there are *no* treatment effects in the pre-period. If we were to find significant deterioration in child health in the period prior to parental displacement, it would be difficult to sort out the reasons for this—we may be able to attribute it to early treatment effects, as described above, but we would also be concerned that the health shock is related to the reason for the subsequent job displacement.

Estimates showing the timing of the health effects of fathers' and mothers' job losses are presented in Table 4. We do not see any significant health effects in the period prior to displacement. Moreover, the patterns of the coefficients for both the mental health effects of fathers' job loss and the reduction in the acute index following maternal job loss are consistent with effects that show up in the round of displacement and persist in the rounds after displacement. This suggests that any decreases in income, increases in stress levels and changes in time use associated with job loss do not measurably affect children's health before job loss occurs and mitigates concerns about reverse causality and omitted variable bias.

2.7 Effect Heterogeneity

As discussed in Section 2, it is possible that the effects of parental job displacement seen in the full sample are masking important heterogeneity in the treatment effects along a number of dimensions. In this section, we explore heterogeneity in the treatment effects of parental displacement by family earnings, parental education, family structure, child age, and child gender.

2.7.1 Socioeconomic Status (SES) and Family Structure

One of the notable findings from studies of the long-run effects of parental job displacement is that the effects tend to be concentrated among relatively disadvantaged households. Oreopoulos et al. (2008) and Page et al. (2009) find that the strongest effects of parental job loss on children's labor market and educational outcomes in adulthood are found at the bottom of the income distribution. Differences in the short-run health impacts of parental displacement by family income could potentially contribute to this result. Thus, in Tables 5 and 6, we explore whether the effects of fathers' and mothers' job losses on child health differ depending on the family's socioeconomic status prior to displacement, using both family earnings and parents' educational attainment as proxies for socioeconomic status.

We also investigate differences by family structure and the number of earners, comparing the effects of paternal displacement in dual-earner and single-earner families and comparing the effects of maternal displacement in two-parent versus single-mother families. A priori, it is difficult to predict how the effects will differ by family type. Single earner families may have fewer resources with which to respond to an earnings shock, and displacement is more likely to cause a child to lose private health insurance coverage when only one parent is employed. On the other hand, Kalil and Ziol-Guest (2008) provide evidence that the negative effects of paternal displacement on children's academic outcomes are more pronounced in two-earner households and suggest that this is because fathers are distressed at losing their "breadwinner" status. Meanwhile, mothers in two-parent families may be more likely to remain out of the labor force longer following displacement than single mothers, so it may be more likely to observe reductions in infectious illness and other effects related to changes in childcare arrangements in these families.

The coefficients in Table 5 indicate that the negative effects of paternal job loss on child mental health are present in both the low- and high-SES groups, and p-values confirm that any differences between groups are not statistically significant. However, as in previous studies, we find striking patterns in the effects of paternal job displacement when stratify by socioeconomic status (SES) in the physical health regressions. Though not all of the relevant coefficients remain individually significant after MHT adjustments, the patterns in Table 5 consistently suggest that paternal job loss has negative effects on the physical health of children in low-SES families in addition to its negative mental health effects. In particular, we see increases in the likelihood of reporting fair or poor physical health, increases in the acute health index, and increases in trauma-related conditions after paternal job loss for children in low-SES groups that are not apparent in the full sample. Looking at the estimated effects of paternal displacement in high-SES families, the patterns are quite different. In these samples, paternal job loss has no significant effect on parent-reported physical health, the coefficients in the acute index regressions are negative, and paternal displacement is associated with statistically significant *reductions* in traumatic injury.

Considering the effects of maternal displacement, presented in Table 6, we see that the reduction in the acute health index seen in Table 2 is substantially larger among children in high-earnings families, children with college-educated parents, and children in two-parent families than it is for children in low-SES and single-parent families. Unlike those estimated using the full sample, the coefficients on maternal displacement in high-earnings and two-parent families remain significant after MHT adjustment, and the differences across groups are all statistically significant at the 5 percent level. This pattern supports the theory, discussed in Section 2, that mothers in high-SES families can be choosing to substitute home care for market-based childcare during unemployment.

2.7.2 Child Age and Gender

Next, in Table 7, we estimate the effects of parental job loss on child health by the age and gender of the child. Recall from Section 2 that we might expect to see the largest reductions in infectious illness among young children if parents are substituting home care for market-based childcare after job loss, but otherwise it is difficult to predict how the effects should vary by age or gender. To check for heterogeneity by age, we separate our data into three age groups: age 1-5 (pre-school aged), age 6-12 (primary and middle school), and age 13-18 (teens). Though the estimated health effects of parental displacement do vary across age groups for most outcomes, the differences are never statistically significant. We do see a pattern that suggests that negative effects of maternal job loss on acute illness are the largest for the youngest age group, which is consistent with the story that these changes may be related to changes in the source and quality of childcare after job loss, but we cannot reject the hypothesis that the effects are the same for all age groups. We do not find statistically significant differences in the coefficients by child gender for any outcome, though the reduction in trauma after paternal displacement appears to be larger for boys than for girls.

2.7.3 Understanding Heterogeneity in the Effects of Paternal Displacement

Given that the mental health effects of paternal job loss appear to be similar in low- and high-SES families, the striking patterns in the physical health effects of paternal job loss discussed in Section 7.1 are puzzling. With regard to the increase in acute illness seen in low-SES families, one potential explanation is that children in disadvantaged families are exposed more frequently to illness after job loss, either because of changes in parental employment or because of changes in the source and quality of childcare. It is also possible that paternal job loss results in changes in vaccination habits or that children in low-SES families have fewer resources available to help them mitigate the stress that results from job loss and as a result, develop poor sleep, nutrition, or other habits that cause them to have weaker immune systems. Perhaps more surprising is the dramatic contrast between the effects of paternal job loss on trauma in low- and high-SES families. One potential explanation for this is differences in the effects of paternal job loss on the likelihood of physical child maltreatment by socioeconomic status. Using state-level data, Lindo et al. (2013) find that increases in male layoffs per capita are associated with increases in rates of physical child abuse, which is consistent with the increase in injuries seen among children in less-educated families. It is possible that rates of abuse in high-SES families either do not respond to paternal employment changes in the same way, or that parental employment is positively correlated with the propensity for abuse in highly-educated families (Lindo et al. do not stratify by family income or educational attainment). Alternatively, as with infectious illness, the changes in trauma could be related to changes in childcare arrangements after job loss. If low-SES families were more likely to put their children in external childcare or after-school activities after job loss (perhaps because the mother has increased her work hours) while high-SES families were more likely to keep one parent at home with the kids or remove their children from sports and extracurricular activities after job loss, it could explain the patterns in injuries that we see in Table 5. However, in the absence of data on the cause of injury, childcare arrangements, and activity participation, which are not available to us in the MEPS, we can only speculate and leave these questions as subject matter for future research.

2.8 Economic Conditions

A final factor not yet considered is the state of the local economy at the time of displacement. A large literature has shown that macroeconomic conditions are associated with health, mental health, time use, and other outcomes for adults. As displacements are more likely to occur when macroeconomic conditions are bad, it is possible that our displacement indicator is picking up the effects of experiencing an economic downturn, rather than the direct effects of involuntary job loss. Another way in which macroeconomic conditions might play a role in our analysis is as a source of heterogeneity in our estimated coefficients. In particular, it is possible that the effects of job displacement on child health might vary depending on the state of the local economy at the time that the displacement occurs. However, the direction of the changes in unclear. It is possible that job displacement might carry less stigma during an economic downturn, as displacement is widespread when the economy is suffering, but displacement may also result in more financial strain and a longer period of unemployment during an economic downturn. It is also important to keep in mind that selection into job displacement is also likely to be different during an economic downturn, so any differences in the estimated coefficients may be the result of a change in the composition of the treated group.

To explore the link between parental job loss and local economic conditions, we use restricted information on the geographic location of the MEPS respondents, obtained with special permission from the AHRQ. First, we estimate our health regressions with an additional control for the state monthly unemployment rate at the time of each interview. The results, presented in the first section of each panel of Table 8, show that while increases in local unemployment rates are associated with increases in the incidence of acute and chronic conditions, the effects of parental displacement are unchanged when local economic conditions are included in the regressions. Next, we examine whether the treatment effects of parental job displacement are different during an economic downturn by interacting the displacement variables with indicators for whether the state unemployment rate is high (above 5) or low at the time of the displacement. The results again show no role for local economic conditions in mediating the effects of parental job displacement on child health; the estimated effects are usually of the same sign when unemployment is high or low, and the differences are not statistically significant, with the only exception of the effect of father's job loss chronic conditions (which is only positive and statistically significant when the unemployment rate is low). Our main results—the effects of paternal job loss on mental health conditions and the effects of maternal job loss on acute conditions—do not appear to be mediated by the state of the local economy.

As an alternative way of exploring whether macroeconomic conditions matter for the effects of parental displacement on child health, we split our sample into two parts, separating panels that end prior to 2008 (the start of the Great Recession) from panels that end in 2008 or later. This approach is somewhat crude, given that there may be changes in health behaviors, sample composition, survey methodology, or other unobservable factors over the time period that contribute to differences in the estimated effects. However, it gives us some idea of whether the health effects of parental displacement are substantially different in the later years of our data, when the national economy was in the midst of a severe downturn and slow recovery. These results, shown in the last section of each panel of Table 8, show that our main results are not different in the two time periods. Although the estimated effects of paternal job loss on mental health conditions and of maternal job loss on acute conditions appear to have a larger magnitude in the period before 2008, the differences with the estimated effects for the period after 2008 are not statistically significant.

2.9 Discussion and Conclusion

This study examines the short-run effects of involuntary parental job loss on children's health. Our results show that the health effects of parental job loss depend on the gender of the displaced parent. In particular, for children of all ages and socioeconomic backgrounds, we find that paternal job loss has robust detrimental effects on child mental health, while maternal job loss does not. These findings are in line with theoretical and empirical work in psychology and sociology on differences between men and women in the psychological impacts of job separation (e.g. Leana and Feldman, 1988; Waters and Moore, 2002; Kalil and Ziol-Guest, 2008) and with empirical economic research on the psychological effects of plant closures (Eliason and Storrie, 2009; Kuhn, Lalive, and Zweimuller, 2009). They are also consistent with studies showing that paternal job loss has negative impacts on children's academic achievement, while maternal job loss does not (Kalil and Ziol-Guest, 2008; Rege et al., 2011).

Our results also show that the effects of both fathers' and mothers' job losses on child health depend substantially on the socioeconomic status of the family in which the displacement occurs. In families with low earnings or parental educational attainment, the negative effects of paternal job loss are not limited to mental health status—we additionally find that a father's job loss increases the likelihood that parents report their child's health to be fair or poor, the incidence of infectious illness (acute conditions index), and the incidence of physical injuries (trauma conditions index). By contrast, among children in high-SES families, paternal job displacement does not have negative effects on children's physical health, and is actually found to reduce the incidence of physical injuries. Turning to maternal job loss, among children in low-SES families, we continue to find no significant effects of a mother's displacement. However, in high-SES and two-parent families, we find that maternal displacement is associated with small statistically significant reductions in infectious illness.

Another important finding from this study is that public health insurance programs such as Medicaid and the SCHIP are providing an effective safety net for children. It does not appear that the changes in health status that we observe are due to reduced diagnosis resulting from changes in insurance coverage, as we find only limited effects of job loss on children's health insurance coverage and no effects on the utilization of routine and diagnostic medical care. When we look at health insurance coverage by source, we find a substantial increase in the probability of having public insurance coverage following displacement, which largely counteracts the decrease in private coverage. As a result, our estimated effects of job displacement on the likelihood of children having coverage from any source are substantially smaller than the corresponding estimates for adults using the MEPS data (Schaller and Stevens, 2015). As the share of the population eligible for Medicaid has recently expanded in some states through the Affordable Care Act, this safety net may become larger still.

One limitation of our study is that we cannot extend our observation period beyond the scope of the MEPS panel, which is only two years in length. As a substantial fraction of displaced workers are likely to regain employment soon after displacement, it is likely that the reductions in contagious illness that we observe will disappear over time. It is also possible that the effects of job displacement related to income loss and stress will become larger over time. Job displacement is associated with permanent decreases in earnings and increased likelihood of future displacement (Jacobson et al., 1993; Stevens, 1997), so an initial displacement may be only the beginning of a tumultuous period for a family. Increased stress in the period immediately following displacement may also take time to translate into worse physical health. We also acknowledge that it is difficult to foresee whether temporary reductions in contagious illness in childhood translate into any changes in longer-term health, human capital, or labor market outcomes. According to the "cohort morbidity phenotype" theory of Finch and Crimmins (2004), the inflammatory processes that result from early life exposure to infectious illness persist from early age into adulthood and may ultimately be related to old-age mortality. On the other hand, a substantial literature in medicine and public health is dedicated to exploring the hypothesis that daycare attendance and early exposure to infectious disease in fact protect against the development of asthma, allergy, and other diseases later in life (see, e.g. Ball et al., 2000 and Nafstad et al., 2005).

Though we acknowledge that we cannot draw any firm conclusions about the long-term welfare effects of parental job displacement from our findings due to these limitations, we emphasize that the results from this study highlight the importance of considering not only changes in income, but also of changes in mental health, parental time use, and childcare arrangements, when studying the effects of job displacement on individuals and families.

2.10 Tables

Table 2.1: Round 1 Summary Statistics

	Father Employed Sample			Mother Employed Sample		
	Father Not Displaced	Father Displaced	P-Value (Difference)	Mother Not Displaced	Mother Displaced	P-Value (Difference)
Parent-Reported Health						
Health Fair/Poor	0.022	0.023	0.887	0.024	0.032	0.127
Mental Health Fair/Poor	0.015	0.013	0.653	0.018	0.022	0.416
Summary Health Indices						
Acute Index	0.037	0.000	0.006	0.006	0.000	0.705
Chronic Index	0.046	0.000	0.034	-0.009	0.000	0.729
Trauma Index	-0.008	0.000	0.603	0.029	0.000	0.039
Mental Index	0.068	0.000	0.000	-0.021	0.000	0.363
Health Insurance Coverage						
Any Insurance	0.930	0.900	0.000	0.917	0.871	0.000
Private Insurance	0.814	0.705	0.000	0.794	0.649	0.000
Public Insurance	0.137	0.218	0.000	0.141	0.244	0.000
Health Care Utilization						
Checkup	0.147	0.153	0.561	0.136	0.149	0.290
Diagnostic Visit	0.291	0.273	0.191	0.283	0.269	0.348
Emergency Visit	0.027	0.032	0.355	0.031	0.026	0.249
Mental Health Visit	0.010	0.007	0.072	0.013	0.020	0.190
Prescription Drug	0.340	0.313	0.056	0.335	0.329	0.692
Demographic and Socioeconomic						
Male	0.515	0.506	0.510	0.507	0.497	0.538
Age	8.441	8.332	0.444	9.113	8.937	0.259
Black	0.077	0.105	0.001	0.149	0.186	0.001
Hispanic	0.171	0.249	0.000	0.141	0.209	0.000
Parents HS or Less	0.314	0.393	0.000	0.322	0.435	0.000
Below 200% Poverty	0.231	0.322	0.000	0.340	0.472	0.000
Single Earner	0.353	0.400	0.001	-	-	-
Single Mother	-	-	-	0.228	0.311	0.000
Observations	22665	1969		19726	1618	

Note: Data are from the 1996-2012 waves of the Medical Expenditure Panel Survey (MEPS). The sample includes children who were 1-16 years old and whose father (columns 1-3) or mother (columns 4-6) was employed with at least one year of job tenure in the first round. Estimates are weighted using MEPS sampling weights.

Table 2.2: Effects of Parental Job Loss on Child Health

Father's Job Loss	Physical Health				Mental Health	
	Fair/Poor	Acute Index	Chronic Index	Trauma Index	Fair/Poor	Mental Index
Father Displaced	0.005	0.003	0.019	-0.023	0.006*	0.076***
	(0.004)	(0.014)	(0.018)	(0.014)	(0.004)	(0.024)
Naive p-value	0.269	0.815	0.275	0.104	0.064	0.001
Adj. p-value	0.597	0.808	0.597	0.377	0.277	0.009
Mother's Job Loss	Physical Health				Mental Health	
	Fair/Poor	Acute Index	Chronic Index	Trauma Index	Fair/Poor	Mental Index
Mother Displaced	-0.001	-0.029**	0.000	-0.008	0.002	-0.016
	(0.003)	(0.013)	(0.015)	(0.017)	(0.004)	(0.023)
Naive p-value	0.677	0.030	0.998	0.630	0.694	0.485
Adj. p-value	0.979	0.157	1.000	0.979	0.979	0.957

Notes: Data are from the 1996-2012 waves of the Medical Expenditure Panel Survey (MEPS). The sample includes children who were 1-16 years old and whose father (top panel) or mother (bottom panel) was employed with at least one year of job tenure in the first round. Construction of health indices is described in Appendix B. All regressions include individual fixed effects, dummies for age, calendar year of interview, month, and survey round, a control for the length of the round in days, and linear time trends specific to the health status reported in the first round. Standard errors (in parentheses) are clustered at the household level (* $p < .10$, ** $p < .05$, and *** $p < .01$). Adjusted p-values reflect familywise error control for the group of hypotheses of each panel as discussed in Section 5 and Appendix C. Estimates are weighted using MEPS sampling weights.

Table 2.3: Effects of Parental Job Loss on Insurance Coverage and Health Care Utilization

Father's Job Loss	Health Insurance			Health Care Utilization				
	Any	Private	Public	Any Rx	Checkup	Diagnostic	Emergency	Psych
Father Displaced	-0.055***	-0.109***	0.057***	-0.010	-0.010	-0.001	-0.010	0.008**
	(0.013)	(0.014)	(0.011)	(0.013)	(0.012)	(0.013)	(0.006)	(0.003)
Naive p-value	0.000	0.000	0.000	0.474	0.426	0.952	0.104	0.025
Adj. p-value	0.000	0.000	0.000	0.795	0.795	0.948	0.346	0.117

Mother's Job Loss	Health Insurance			Health Care Utilization				
	Any	Private	Public	Any Rx	Checkup	Diagnostic	Emergency	Psych
Mother Displaced	-0.046***	-0.100***	0.047***	-0.015	0.005	-0.007	-0.001	-0.007
	(0.014)	(0.016)	(0.011)	(0.014)	(0.013)	(0.014)	(0.006)	(0.006)
Naive p-value	0.001	0.000	0.000	0.270	0.680	0.632	0.881	0.189
Adj. p-value	0.007	0.000	0.002	0.696	0.898	0.950	0.874	0.622

Notes: Data are from the 1996-2012 waves of the Medical Expenditure Panel Survey (MEPS). The sample includes children who were 1-16 years old and whose father (top panel) or mother (bottom panel) was employed with at least one year of job tenure in the first round. All regressions include individual fixed effects, dummies for age, calendar year of interview, month, and survey round, a control for the length of the round in days, and linear time trends specific to the health status reported in the first round. Standard errors (in parentheses) are clustered at the household level (* $p < .10$, ** $p < .05$, and *** $p < .01$). Adjusted p-values reflect familywise error control for the group of hypotheses of each panel as discussed in Section 5 and Appendix C. Estimates are weighted using MEPS sampling weights.

Table 2.4: Timing of the Effects of Parental Job Loss on Child Health

Father's Job Loss	Physical Health				Mental Health	
	Fair/Poor	Acute Index	Chronic Index	Trauma Index	Fair/Poor	Mental Index
Before Disp.	0.005	-0.004	0.009	-0.000	0.005	0.023
	(0.007)	(0.020)	(0.019)	(0.023)	(0.004)	(0.029)
	[0.981]	[0.981]	[0.981]	[0.994]	[0.956]	[0.981]
Round of Disp.	0.007	-0.017	0.028	-0.013	0.007	0.094***
	(0.007)	(0.021)	(0.022)	(0.020)	(0.005)	(0.036)
	[0.956]	[0.981]	[0.904]	[0.981]	[0.904]	[0.107]
After Disp.	0.009	0.016	0.021	-0.032	0.011**	0.083**
	(0.008)	(0.022)	(0.025)	(0.020)	(0.005)	(0.033)
	[0.952]	[0.981]	[0.976]	[0.746]	[0.337]	[0.132]
Mother's Job Loss	**Physical Health**				**Mental Health**	
	Fair/Poor	Acute Index	Chronic Index	Trauma Index	Fair/Poor	Mental Index
Before Disp.	-0.000	0.004	-0.004	-0.010	-0.006	-0.013
	(0.005)	(0.018)	(0.020)	(0.024)	(0.004)	(0.024)
	[1.000]	[1.000]	[1.000]	[1.000]	[0.886]	[1.000]
Round of Disp.	0.001	-0.032*	-0.002	-0.001	-0.001	-0.007
	(0.005)	(0.018)	(0.020)	(0.025)	(0.006)	(0.034)
	[1.000]	[0.690]	[1.000]	[1.000]	[1.000]	[1.000]
After Disp.	-0.004	-0.022	-0.003	-0.025	-0.003	-0.037
	(0.004)	(0.018)	(0.024)	(0.024)	(0.005)	(0.029)
	[0.994]	[0.960]	[1.000]	[0.978]	[1.000]	[0.936]

Notes: Data are from the 1996-2012 waves of the Medical Expenditure Panel Survey (MEPS). The sample includes children who were 1-16 years old and whose father (top panel) or mother (bottom panel) was employed with at least one year of job tenure in the first round. Construction of health indices is described in Appendix B. All regressions include individual fixed effects, dummies for age, calendar year of interview, month, and survey round, a control for the length of the round in days, and linear time trends specific to the health status reported in the first round. Standard errors (in parentheses) are clustered at the household level (* $p < .10$, ** $p < .05$, and *** $p < .01$). Adjusted p-values for each coefficient, presented in square brackets, reflect familywise error control for the group of hypotheses of each panel as discussed in Section 5 and Appendix C. Estimates are weighted using MEPS sampling weights.

Table 2.5: Effects of Father's Job Loss on Child Health, by Family Socioeconomic Status

By Family Earnings	Physical Health				Mental Health	
	Fair/Poor	Acute Index	Chronic Index	Trauma Index	Fair/Poor	Mental Index
Disp*Low	0.020**	0.076***	0.010	0.029	0.009	0.090*
	(0.010)	(0.023)	(0.023)	(0.019)	(0.008)	(0.049)
	[0.242]	[0.019]	[0.868]	[0.542]	[0.616]	[0.381]
Disp*High	-0.002	-0.029*	0.023	-0.047**	0.005	0.069***
	(0.005)	(0.017)	(0.023)	(0.018)	(0.004)	(0.025)
	[0.868]	[0.457]	[0.674]	[0.094]	[0.542]	[0.063]
P(Low=High)	0.031	0.000	0.671	0.004	0.635	0.696
By Parental Education	**Physical Health**				**Mental Health**	
	Fair/Poor	Acute Index	Chronic Index	Trauma Index	Fair/Poor	Mental Index
Disp*Less Than HS	0.007	0.075***	0.011	0.037**	0.017**	0.100**
	(0.007)	(0.023)	(0.030)	(0.018)	(0.007)	(0.043)
	[0.834]	[0.028]	[0.916]	[0.342]	[0.201]	[0.252]
Disp* HS Grad	0.013*	0.030	-0.031	0.068***	0.006	0.112*
	(0.007)	(0.021)	(0.027)	(0.024)	(0.009)	(0.061)
	[0.456]	[0.745]	[0.834]	[0.064]	[0.868]	[0.475]
Disp*College	0.001	-0.023	0.042*	-0.073***	0.004	0.055**
	(0.006)	(0.021)	(0.025)	(0.020)	(0.004)	(0.026)
	[0.456]	[0.745]	[0.834]	[0.064]	[0.868]	[0.475]
P-value Education	0.448	0.006	0.132	0.000	0.295	0.529
By Family Type	**Physical Health**				**Mental Health**	
	Fair/Poor	Acute Index	Chronic Index	Trauma Index	Fair/Poor	Mental Index
Disp*Single Earner	0.013*	0.028	0.006	-0.030	0.004	0.045
	(0.007)	(0.026)	(0.026)	(0.023)	(0.005)	(0.029)
	[0.523]	[0.830]	[0.973]	[0.830]	[0.886]	[0.670]
Disp*Dual Earner	-0.000	-0.013	0.028	-0.019	0.008*	0.096***
	(0.005)	(0.016)	(0.023)	(0.018)	(0.005)	(0.033)
	[0.973]	[0.886]	[0.830]	[0.830]	[0.523]	[0.058]
P(Dual=Single)	0.127	0.175	0.538	0.705	0.511	0.244

Notes: Subgroup estimates are obtained in each panel by interacting the parental displacement indicator with each subgroup. Otherwise, the father employed sample and specification are the same as those in Table 2. Family earnings categories are defined by earnings (in 2010 dollars) above/below 200 percent of the 2010 federal poverty line. Parental education categories are defined by the educational attainment of the parent with the most education. Single earner families include those in which the father but not the mother was employed in the first round. Standard errors (in parentheses) are clustered at the household level (* $p < .10$, ** $p < .05$, and *** $p < .01$). Adjusted p-values for each coefficient, presented in square brackets, reflect familywise error control for the group of hypotheses of each panel as discussed in Section 5 and Appendix C. Estimates are weighted using MEPS sampling weights.

Table 2.6: Effects of Mother's Job Loss on Child Health, by Family Socioeconomic Status

By Family Earnings	Physical Health				Mental Health	
	Fair/Poor	Acute Index	Chronic Index	Trauma Index	Fair/Poor	Mental Index
Disp*Low	-0.002	0.010	-0.004	-0.011	-0.004	-0.001
	(0.005)	(0.017)	(0.021)	(0.022)	(0.007)	(0.028)
	[0.999]	[0.998]	[1.000]	[0.999]	[0.999]	[1.000]
Disp*High	-0.000	-0.063***	0.004	-0.006	0.006	-0.029
	(0.004)	(0.019)	(0.022)	(0.025)	(0.004)	(0.034)
	[1.000]	[0.014]	[1.000]	[1.000]	[0.686]	[0.988]
P(Low=High)	0.753	0.004	0.785	0.880	0.234	0.533

By Parental Education	Physical Health				Mental Health	
	Fair/Poor	Acute Index	Chronic Index	Trauma Index	Fair/Poor	Mental Index
Disp*Less Than HS	0.001	0.022	-0.031	-0.001	-0.011	-0.045
	(0.010)	(0.024)	(0.050)	(0.028)	(0.007)	(0.032)
	[1.000]	[0.994]	[1.000]	[1.000]	[0.886]	[0.906]
Disp*HS Grad	-0.007	-0.007	0.030	0.004	0.003	0.004
	(0.006)	(0.021)	(0.024)	(0.032)	(0.007)	(0.034)
	[0.968]	[1.000]	[0.968]	[1.000]	[1.000]	[1.000]
Disp*College	0.001	-0.052***	-0.009	-0.016	0.004	-0.019
	(0.004)	(0.019)	(0.020)	(0.023)	(0.006)	(0.034)
	[1.000]	[0.112]	[1.000]	[0.999]	[0.999]	[1.000]
P-value Education	0.493	0.042	0.354	0.848	0.243	0.561

By Family Type	Physical Health				Mental Health	
	Fair/Poor	Acute Index	Chronic Index	Trauma Index	Fair/Poor	Mental Index
Disp* Single Mom	-0.006	0.013	0.005	-0.027	-0.004	-0.032
	(0.006)	(0.021)	(0.026)	(0.028)	(0.009)	(0.038)
	[0.980]	[0.995]	[1.000]	[0.980]	[0.996]	[0.980]
Disp*Two Parent	0.001	-0.048***	-0.002	0.000	0.004	-0.008
	(0.003)	(0.016)	(0.018)	(0.021)	(0.004)	(0.028)
	[1.000]	[0.040]	[1.000]	[1.000]	[0.980]	[1.000]
P(Two Parent=Single)	0.360	0.020	0.823	0.435	0.371	0.613

Notes: Subgroup estimates are obtained in each panel by interacting the parental displacement indicator with each subgroup. Otherwise, the regression sample and specification are the same as those in Table 2. Family earnings categories are defined by earnings (in 2010 dollars) above/below 200 percent of the 2010 federal poverty line. Parental education categories are defined by the educational attainment of the parent with the most education. Single mother families include those in which there is no father present in the first round. Standard errors (in parentheses) are clustered at the household level (* $p < .10$, ** $p < .05$, and *** $p < .01$). Adjusted p-values for each coefficient, presented in square brackets, reflect familywise error control for the group of hypotheses of each panel as discussed in Section 5 and Appendix C. Estimates are weighted using MEPS sampling weights.

Table 2.7: Effects of Parental Job Loss on Child Health, by Child Age and Gender

Father's Job Loss	Physical Health				Mental Health	
	Fair/Poor	Acute Index	Chronic Index	Trauma Index	Fair/Poor	Mental Index
Age 1-5	0.013	-0.027	-0.003	-0.035	0.010**	0.058*
	(0.012)	(0.032)	(0.033)	(0.028)	(0.005)	(0.033)
	[0.945]	[0.980]	[0.996]	[0.932]	[0.459]	[0.741]
Age 6-12	0.003	0.032*	0.013	-0.028	0.007	0.098***
	(0.004)	(0.019)	(0.021)	(0.021)	(0.005)	(0.037)
	[0.983]	[0.746]	[0.989]	[0.914]	[0.914]	[0.153]
Age 13-18	-0.003	-0.008	0.060	0.002	0.001	0.060
	(0.006)	(0.020)	(0.042)	(0.024)	(0.008)	(0.053)
	[0.994]	[0.994]	[0.896]	[0.996]	[0.996]	[0.945]
P(All Equal)	0.424	0.182	0.481	0.531	0.650	0.701
Male	0.009	0.003	0.010	-0.041*	0.009**	0.071**
	(0.006)	(0.019)	(0.018)	(0.023)	(0.004)	(0.031)
	[0.649]	[0.999]	[0.985]	[0.510]	[0.362]	[0.196]
Female	0.001	0.003	0.029	-0.004	0.004	0.081**
	(0.006)	(0.019)	(0.029)	(0.016)	(0.005)	(0.034)
	[0.999]	[0.999]	[0.935]	[0.999]	[0.966]	[0.181]
P(Male=Female)	0.288	1.000	0.575	0.199	0.415	0.832
Mother's Job Loss	**Physical Health**				**Mental Health**	
	Fair/Poor	Acute Index	Chronic Index	Trauma Index	Fair/Poor	Mental Index
Age 1-5	-0.009	-0.054*	-0.014	-0.027	0.001	-0.009
	(0.005)	(0.028)	(0.033)	(0.027)	(0.003)	(0.013)
	[0.818]	[0.655]	[1.000]	[0.994]	[1.000]	[1.000]
Age 6-12	-0.001	-0.029	-0.007	-0.019	0.005	-0.000
	(0.004)	(0.018)	(0.022)	(0.025)	(0.006)	(0.025)
	[1.000]	[0.851]	[1.000]	[1.000]	[0.997]	[1.000]
Age 13-18	0.005	-0.005	0.024	0.027	-0.003	-0.046
	(0.008)	(0.019)	(0.023)	(0.037)	(0.010)	(0.067)
	[1.000]	[1.000]	[0.990]	[1.000]	[1.000]	[1.000]
P(All Equal)	0.272	0.327	0.513	0.481	0.690	0.799
Male	0.002	-0.031*	0.009	-0.011	0.000	0.000
	(0.005)	(0.018)	(0.022)	(0.019)	(0.005)	(0.023)
	[0.999]	[0.549]	[0.999]	[0.998]	[0.999]	[0.999]
Female	-0.005	-0.026	-0.009	-0.006	0.003	-0.031
	(0.004)	(0.018)	(0.019)	(0.028)	(0.006)	(0.037)
	[0.892]	[0.804]	[0.999]	[0.999]	[0.999]	[0.984]
P(Male=Female)	0.239	0.821	0.518	0.889	0.738	0.455

Notes: Subgroup estimates are obtained in each panel by interacting the parental displacement indicator with each subgroup. Otherwise, the regression sample and specification are the same as those in Table 2. Standard errors (in parentheses) are clustered at the household level (* $p < .10$, ** $p < .05$, and *** $p < .01$). Adjusted p-values for each coefficient, presented in square brackets, reflect familywise error control for the group of hypotheses of each panel as discussed in Section 5 and Appendix C. Estimates are weighted using MEPS sampling weights.

Table 2.8: Parental Job Loss and Local Economic Conditions

Father's Job Loss	Physical Health				Mental Health	
	Fair/Poor	Acute Index	Chronic Index	Trauma Index	Fair/Poor	Mental Index
Father Displaced	0.005	0.003	0.019	-0.023	0.007*	0.075***
	(0.004)	(0.014)	(0.018)	(0.014)	(0.004)	(0.023)
Unemployment Rate	0.000	0.008**	0.012***	0.003	0.000	0.006
	(0.001)	(0.004)	(0.005)	(0.004)	(0.001)	(0.006)
Disp*Low Unemployment	0.010	0.007	0.072***	-0.019	0.006	0.069*
	(0.008)	(0.025)	(0.022)	(0.021)	(0.006)	(0.040)
Disp*High Unemployment	0.002	0.001	-0.010	-0.025	0.007	0.079***
	(0.005)	(0.018)	(0.024)	(0.018)	(0.004)	(0.029)
P(Low=High)	0.457	0.832	0.009	0.834	0.893	0.836
Disp*Pre 2008	0.009*	0.003	0.013	-0.039**	0.008**	0.084***
	(0.005)	(0.018)	(0.020)	(0.018)	(0.004)	(0.028)
Disp*Post 2008	-0.003	0.004	0.031	0.008	0.003	0.059
	(0.009)	(0.022)	(0.035)	(0.022)	(0.006)	(0.042)
P(Pre=Post)	0.237	0.967	0.652	0.095	0.427	0.631
Mother's Job Loss	Physical Health				Mental Health	
	Fair/Poor	Acute Index	Chronic Index	Trauma Index	Fair/Poor	Mental Index
Mother Displaced	-0.001	-0.029**	-0.000	-0.008	0.002	-0.016
	(0.003)	(0.013)	(0.015)	(0.017)	(0.004)	(0.023)
Unemployment Rate	0.000	0.010***	0.011**	0.007	0.002*	0.006
	(0.001)	(0.004)	(0.005)	(0.005)	(0.001)	(0.004)
Disp*Low Unemployment	-0.007	-0.024	-0.023	-0.009	0.003	-0.016
	(0.005)	(0.022)	(0.027)	(0.035)	(0.005)	(0.029)
Disp*High Unemployment	0.002	-0.032*	0.012	-0.008	0.001	-0.016
	(0.004)	(0.016)	(0.018)	(0.018)	(0.006)	(0.031)
P(Low=High)	0.142	0.769	0.278	0.994	0.816	0.989
Disp*Pre 2008	-0.003	-0.042***	-0.007	-0.017	0.004	-0.018
	(0.003)	(0.016)	(0.019)	(0.023)	(0.004)	(0.029)
Disp*Post 2008	0.001	-0.004	0.012	0.009	-0.003	-0.012
	(0.006)	(0.023)	(0.026)	(0.024)	(0.008)	(0.035)
P(Pre=Post)	0.559	0.171	0.566	0.430	0.468	0.887

Notes: Heterogeneous effects by low and high unemployment rate are obtained by interacting the parental displacement indicator with an indicator for the state unemployment rate being below or above 5%, respectively. Heterogeneous effects before and after 2008 are obtained by interacting the parental displacement indicator with an indicator for the first interview of the panel occurring before or after 2008, respectively. All regressions include the state unemployment rate at the time of interview as a control variable. Otherwise, the regression sample and specification are the same as those in Table 2. Standard errors (in parentheses) are clustered at the household level (* $p < .10$, ** $p < .05$, and *** $p < .01$). Estimates are weighted using MEPS sampling weights.

Chapter 3

Do State Social Insurance Programs Mediate the Effects of Parental Job Loss? Evidence from the Medical Expenditure Panel Survey[1]

3.1 Introduction

Job loss has the potential to affect access to health care for the children of the displaced worker, both through the potential loss of employer-provider health insurance, and through a negative income shock. While job displacements have been found to be associated with large reductions in insurance coverage and health care utilization for adults (Gruber and Madrian, 1997; Schaller and Stevens, 2015), the effects of parental job losses on children's health insurance, access to health care, and health have been found to be smaller but still significant for children (Schaller and Zerpa, 2016).

In this paper, we evaluate whether two of the largest social insurance programs in the U.S.–Unemployment Insurance (UI) and public health insurance (Medicaid and the Children's Health Insurance Program)–mitigate the effects of a father's job loss on children's

[1]Based on joint work with Chloe East, Elira Kuka and Jessamyn Schaller.

health insurance coverage and health care access in the short run. In particular, we study whether more generous Medicaid/CHIP eligibility rules and more generous UI benefits improve the health insurance coverage and access to health care of children after father's job loss. We also explore how the generosity of these programs affect the changes in child health expenditures after a job loss. Our work is the first to shed light on whether the detrimental effects of parental job loss can be mitigated by transfers to the family.

The very large growth in UI payouts and Medicaid spending during the Great Recession underscore the importance of studying the effectiveness of these programs in protecting families against negative labor market shocks. We explore their role as a source of insurance for children against the negative effects of parental job losses on health care access, which is an important aspect of consumption smoothing that might not be apparent when looking at income and monetary family consumption. Documenting this additional type of consumption smoothing is relevant for quantifying not only the private benefits of social insurance programs, but also their social benefits, as the effects on continued health care access can potentially have externalities for the society in terms of reduced long term health care provision costs.

Medicaid and the Children's Health Insurance Program (CHIP) have the potential to protect children against loss of insurance coverage by providing continued coverage for children from low-income families, as well as working as a safety net for the loss of private health insurance in the event of a job loss. By providing access to low cost-sharing health insurance, it can also protect children against the income effects of job loss on health care utilization. Unemployment Insurance, however, can have ambiguous effects on health care access for children. On one hand, to the extent that it represents an income transfer, more generous UI benefits can increase health care consumption. Indeed, Kuka (2015) shows that higher UI generosity increases health insurance coverage and health care utilization and improves self-reported health for unemployed adults. On the other hand, to the extent that UI benefits are taken into account in public health insurance income eligibility rules, more generous UI benefits may potentially harm health care access for children.

Using data from the Medical Expenditure Panel Survey, we exploit differences in generosity of UI and Medicaid/CHIP programs across states to identify the effects of these programs on child health insurance, health care utilization and expenditures. The MEPS allows us to follow fathers and their children before and after a job loss, and examine a wide range of outcomes including health insurance coverage, health care utilization, and health care expenditures. We interact an indicator for father's job loss with indicators of program generosity in child fixed effects regression models to assess the protective effect of program generosity after parental job loss. Instead of using measures of each individual child's eligibility for Medicaid or the amount of UI benefits for which the child's parent is eligible, we use simulated measures of each of these program's generosity that capture plausibly exogenous variation in state policy generosity over time. Computing simulated measures of program generosity, instead of actual Medicaid/CHIP eligibility and UI replacement rate, allows us to avoid potential endogeneity problems, because individual eligibility and benefits depend (non-linearly) on family income and work history, which can potentially be correlated with health outcomes. In addition, using the simulated measures of generosity allows us to estimate reduced-form coefficients that are of direct policy interest, because they provide estimates of the marginal effects of changes in the legislated characteristics of the programs.

To construct a simulated measure of the generosity Medicaid/CHIP's eligibility rules, we use a fixed nationally representative sample of children with unemployed fathers from the Current Population Survey. We simulate the eligibility of each child in this national sample in each state and year, according to their age, and collapse the information to obtain a simulated measure of the percentage of children in this national sample that would be eligible for Medicaid or CHIP in each state-year-age cell. Finally, for each child in our MEPS sample, we impute the simulated Medicaid/CHIP generosity using the percentage of eligible children that was calculated for their state of residence, year of interview, and age group. To construct our simulated measure of Unemployment Insurance generosity, we use a fixed nationally representative sample of unemployed fathers from the Survey of Income and Program Participation. Based on each individual's past earnings and the number of children they have, we compute the percentage of their past weekly earnings that would be replaced by UI benefits (after-tax replacement rate) if they lived in each state and year, and collapse the information to obtain the average replacement rate that unemployed fathers in this national sample would be eligible for in each state-year-family size cell. Finally, for each child in our MEPS sample, we impute the simulated UI generosity using the average replacement rate that was calculated for their state of residence, year of interview, and family size.

Our results show that more generous Medicaid/CHIP eligibility rules increase the likelihood of taking up public insurance for children who were insured through a parent's employer before the job loss. A 10 percentage point increase in simulated Medicaid/CHIP eligibility is associated with a 0.5 percentage point increase in the likelihood of having public health insurance coverage after job loss for this group of children. While we do not find robust evidence of short-term effects on health care utilization, we find that out-of-pocket health expenditures are less likely to increase after job loss in states with more generous Medicaid/CHIP. Regarding UI, our results show that more generous UI replacement rates have a negative effect on child health insurance coverage by decreasing the likelihood of taking up public insurance. A 10 percentage point increase in the simulated UI replacement rate is associated with a 3.8 percentage points lower likelihood of having Medicaid/CHIP coverage, and close to an equivalent increase in the likelihood of going at least one month without any source of coverage.

These findings underscore the importance of taking into account the dynamic role of public health insurance as social insurance against (frequent) negative labor market shocks. They also shed light on the relevance of considering how the eligibility rules of different social insurance programs interact when families experience negative shocks that are ubiquitous in current labor markets.

3.2 Background

3.2.1 Medicaid and CHIP

Medicaid/CHIP is the largest means-tested transfer program in the United States, with a total spending of $545 billion in 2015 (CMS*a*, 2017). While Medicaid provides different types of services for different groups of beneficiaries, one of its main functions is to serve as the primary source of health insurance for children from low-income families. Almost

46 million children were enrolled in Medicaid/CHIP some time during fiscal year 2016 (CMS*b*, 2017). The largest changes in public health insurance eligibility for children since the establishment of the Medicaid program were produced by the Balanced Budget Act (BBA) of 1997, which introduced the Children's Health Insurance Program (CHIP). This program provided states with additional funds that they could use to extend eligibility to cover additional ages and income groups, and/or to design new programs, introducing more flexibility to the Medicaid (Buchmuller, Ham and Shore-Sheppard, 2016). The number of states providing coverage to children with family income up to 200% and 300% of the Federal Poverty Line (FPL) expanded substantially during our period of study, especially during the first 5 years after the implementation of the BBA. For example, in 1997 there were only 6 states that had income limits at or above 200% of the FPL for children of all ages; by 2002, there were 46 (NGA 1997, 2003). Another change that occurred during the period is the elimination in many states of asset test requirements for eligibility, which started with the reforms of the 1980s. By 1997, 36 states had already dropped asset tests as a requirement for Medicaid eligibility of children, and by 2002, 45 states had done so (Kaiser Commission, 2012).

The literature on the effects of Medicaid expansions on child health insurance coverage and health care utilization generally finds that they increase health care access and improve the utilization of health care services (e.g., Currie and Gruber, 1996; Ham, Li and Shore-Shepard, 2009; LoSasso and Buchmueller, 2004; Gruber and Simon, 2008; Miller, 2012). The role of Medicaid as a source of insurance against negative health shocks has also been studied, and Medicaid has been found to lead to a lower likelihood of bankruptcy and lower medical debt among low-income households (Gross and Notowidigdo, 2011; Finkelstein et al., 2012). However, to the best of our knowledge, this paper is the first to explore the role of Medicaid/CHIP as a source of insurance against the negative shock of parental job loss on children's health insurance coverage and access to health care.

A more generous Medicaid/CHIP program can work as insurance against the loss of private insurance, for children who become eligible for the program or take it up after parental job loss.[2] It can also provide continued coverage to children enrolled in Medi-

[2] The existence of anti-crowd-out measures that require a waiting period without insurance to become eligible

caid/CHIP before job loss. In this sense, more generous Medicaid/CHIP eligibility rules can mitigate the health insurance coverage and health care access effects of job loss. Not only does it provide coverage, but also the coverage can be more generous (in terms of lower cost sharing) than private insurance, which can potentially reduce out-of-pocket expenditures and increase the utilization of preventive care. Thus, we expect that more generous eligibility rules would mitigate the effects of parental job loss on health insurance coverage, potentially reducing out-of-pocket expenditures, and improving access to health care, especially preventive care and other types of health care that may be delayed in the event of a negative income shock.

3.2.2 Unemployment Insurance

The Unemployment Insurance (UI) program provides temporary income transfers to individuals who experience involuntary job loss. While other programs also provide assistance to low-income families (TANF, EITC, Food Stamps), UI is the program that is most responsive to negative labor market shocks, both in terms of caseloads and spending (Bitler and Hoynes , 2016). The response of UI spending was particularly large during the last recession; in 2010, UI benefit payments totaled more than $144 billion, including regular state benefits, and extended and emergency benefits.[3]

The UI program is federally mandated but it is administered at the state level. UI benefits are calculated as a function of past earnings, and statutory replacement rates (share of pre-unemployment earnings) are approximately 50 percent (1/26 of base quarterly earnings) in most states. However, there is variation across states in how they determine the base earnings to calculate benefits, as well as nominal minimum and maxi-

for public insurance could impose a limitation for the capacity of Medicaid/CHIP to work as insurance against loss of private insurance. States can only impose a waiting period for Medicaid coverage for children if they receive a federal waiver, and there are only two states that have received such waivers. CHIP programs that are not a Medicaid expansion are required to adopt provisions against crowding-out of private insurance, and during this period most states required waiting periods before a child becomes eligible for CHIP. However, typically states provided exemptions for CHIP waiting periods in the event of a job loss.

[3]Regular benefits have a fixed duration, typically of 26 weeks. UI benefits can be extended for 13-20 additional weeks in states experiencing high unemployment rates. In large economic recessions, longer emergency extensions have been passed by Congress. In this paper we focus on UI generosity in terms of the replacement rate, rather than the duration of the benefits, because the short-term nature of our panel data is best suited for this.

mum benefit amounts, and the minimum earnings required for eligibility. These state laws change frequently to update these minimum and maximum amounts of benefits. States can also determine additional benefits for workers with dependent children, which vary as a function of the number of children. As a consequence, the effective replacement rate is a non-linear function of earnings that has substantial variation across states, years, and number of children in the family (Kuka, 2015). As detailed in Section 3.3, we construct a simulated measure of UI generosity (average replacement rate) that varies by state, year, and number of children in the family, using a representative sample of unemployed fathers. Our constructed simulated replacement rates range from 25% to 68%, with an average in our sample of 41% (see Table 3.1).

Previous work has shown that higher UI benefits mitigate the fall in consumption associated with job loss (e.g. Gruber, 1997; East and Kuka, 2014), decrease precautionary savings (Engen and Gruber, 2001), increase spousal labor supply (Cullen and Gruber, 2000), and improve consumer credit markets (Hsu, Matsa and Melzer, 2014). Our paper is closest to Kuka (2015), who studies whether UI generosity mitigates the negative health effects of job loss on displaced workers. We use the same simulated measure of UI generosity, a strategy that is similar to Gruber (1997). Kuka (2015) finds that higher UI generosity increases health insurance coverage and health care utilization of displaced workers, and also improves self-reported health, although she finds no strong effects on reported health conditions. Our paper contributes to this literature by studying how UI generosity interacts with parental job loss in affecting children's health insurance coverage and health care utilization. In addition to looking at child outcomes, our paper also differs from Kuka (2015) in that our empirical strategy makes use of panel data, which allows us to estimate models with child fixed effects, and identify the effects of UI generosity and parental job loss from changes in outcomes before and after job loss for children exposed to different generosity of UI programs.

It is important to note that the effects of UI can be different for children than for adults. Both for adults and children, UI represents an income transfer that can have positive effects on health care consumption. However, higher UI generosity can also potentially harm eligibility for public insurance, especially for children, for whom public insurance programs in the United States are much more generous than for working-age adults. Thus, in theory more generous UI has an ambiguous effect on health insurance coverage and health care utilization. Conditional on having health insurance, however, we would expect more generous UI to improve access to health care. As a consequence, the overall effects of UI generosity on health expenditures after job loss are difficult to predict.

3.3 Data

We use various sources of data to analyze how the Medicaid/CHIP and UI programs mediate the effects of parental job loss on child health insurance coverage, health care utilization, and health care expenditures. The main source of data is the Medical Expenditure Panel Survey (MEPS), maintained by the Agency for Healthcare Research and Quality. In each year since 1996, the MEPS selects a new nationally representative subsample of households participating in the previous year's National Health Interview Survey conducted by the National Center for Health Statistics. In each new panel the respondents are interviewed in five rounds spanning two full calendar years. Round length varies across rounds and across households. In our sample, reference periods are between three and five months, with an average duration of 4.2 months. The survey collects data on reported health status and specific medical conditions, as well as health insurance coverage, health care use, demographic and socioeconomic characteristics, and employment.

Our sample includes 16 waves of the MEPS, covering the period 1996-2012. We limit our sample to children who were 1 to 16 years old and whose father was employed (excluding self-employed) at the time of the first interview (round) of the survey. Like many studies of displaced workers, we restrict our main analysis to male (fathers') job losses. The effects of parental job loss on child outcomes are quite different depending on which parent is displaced, especially when the mother is the second earner in the family. In addition, there are differences in baseline characteristics between children with employed fathers and with employed mothers that make direct comparison of the effects of paternal and maternal job displacement more difficult. However, we conduct additional analyses with mothers' job losses as robustness checks. In particular, we have estimated our results on the sample of children for whom the families' *"primary earner"* (employed father or employed single mother) was employed with at least one year of job tenure in round 1, where we construct indicators for *primary earner*'s displacements.

We use the MEPS to obtain child-level information on health insurance coverage, health care utilization, health expenditures and health outcomes. We match children with information for their parents, and obtain information on each parent's employment, job tenure, earnings, and other demographic and socio-economic characteristics. Our indicators for involuntary job displacement are constructed from a section of the MEPS survey in which respondents are asked to choose the main reason why they changed jobs since the last interview from a list of possible responses. In our analysis, we define involuntary displacement as displacement for one of three reasons: "job ended," "business dissolved or sold," or "laid off."[4] Based on this definition, we create a post-displacement indicator variable that turns to one at the interview immediately following displacement and remains "turned on" in all future rounds. Because it is comprised of many short panels, the MEPS provides a relatively large sample of children with displaced parents, which is unusual in studies of displacement that rely on survey data. This enhances our statistical power and allows us to explore heterogeneity in the effects of parental displacement on child health.

As is standard in the job displacement literature, we restrict our samples so that for the analysis of fathers' displacements, the at-risk sample includes all children whose fathers have at least one year of job tenure in the first round of the survey.[5] Defining the sample this way ensures that the displaced fathers in the sample are somewhat attached to the labor market prior to job loss (i.e. that job loss is, in fact, a "shock" to employment) and that the samples used to help identify the control variables (such as age and seasonal effects) are as similar as possible to the treatment groups.

The outcome variables that we examine can be divided into the following categories: health insurance coverage, health care utilization, and health expenditures:

[4] Other possible responses include: "retired," "illness or injury," "quit to have a baby," "quit to go to school," "quit to take care of home or family," "quit because wanted time off," "quit to take another job," "unpaid leave," or "other."

[5] For the analysis of mothers' displacements, the at-risk sample includes all children whose mothers have at least one year of job tenure in the first round of the survey.

- *Health insurance status:* We look at whether the child is covered by any insurance, private insurance, or public insurance (including Medicaid/CHIP, Tricare, and other public hospital/physician coverage), and Medicaid/CHIP in particular, at the time of interview. We also construct an indicator for whether the child spent at least one whole month without health insurance coverage during the round period (i.e. between interviews).[6]

- *Health care utilization:* We use information on the reported reason for each medical visit observed in the MEPS data to create indicators for checkups and well-child visits, diagnostic visits, emergency room visits, mental health visits, and dental visits. In particular, we look at checkups and well-child visits to identify any changes in health care utilization unrelated to the health status of the child.

- *Health care expenditures:* We use information on expenditures on all medical events (all doctor visits, hospital stays, and prescription drug purchases), their reported reason, and the distribution of these expenditures across different sources of coverage (out-of-pocket, insurance by source of coverage). We create variables for total expenditures, total out-of-pocket expenditures, total expenditures covered by Medicaid/CHIP, total expenditures covered by private insurance, and dental expenditures. We adjust all expenditures by the consumer price index to express them terms of 2010 dollars.

We supplement our individual-level MEPS dataset with state-level information from various sources, which we merge to the MEPS sample by each individual's state and interview date. To control for the state's economic conditions, we include the monthly state unemployment rate, merging it by month and year of interview and state of residence for each individual and round. The rest of the state-level control variables are merged to

[6] The shortest period without insurance that we can observe in the data is a full calendar month. Survey respondents are asked about each family member's insurance coverage and the source of coverage at the time of the interview as well as during each calendar month. We define a gap in insurance as being reported as uninsured for at least one calendar month. A person is reported in the MEPS as uninsured during a month if they are not covered by one of the following insurance sources: Tricare, Medicare, Medicaid or other public hospital/physician or private hospital/physician insurance (including Medigap plans). Persons covered only by state-specific programs that provide non-comprehensive coverage, and those without hospital/physician benefits are not considered to be insured.

the main dataset by state and year. These include: state expenditures on food stamps, Supplemental Security Income, welfare programs, and Retirement and Disability Benefits; state EITC as fraction of the federal EITC; maximum AFDC/TANF benefits; and whether a state had a welfare reform waiver or passed TANF by the given year.[7]

Finally, we use simulated measures of state Medicaid/CHIP and Unemployment Insurance generosity to identify exogenous variation in the generosity of these programs that does not depend on individual characteristics. Our measure of Medicaid and CHIP generosity is constructed by building a calculator of Medicaid or CHIP eligibility for a national sample of children. We use information from Hoynes and Luttmer (2011), the National Governor's Association Maternal and Child Health Updates, and the Kaiser Family Foundation. This calculator takes information about state of residence, year of observation, age of child, and family income as a fraction of the poverty line (which is based on year and family size) to determine if each child is eligible.[8] We then use pooled samples from Current Population Survey (CPS) for the years 1996-2013 to calculate eligibility using this calculator. With the CPS we define several demographics groups to calculate their specific eligibility, which we match to the relevant samples in the MEPS data.[9] We keep the observed family income as a fraction of the poverty line, the demographic group (e.g. if the father is unemployed) and the observed age and drop the observed state and year. For every observation we assign each year and each state and then calculate each individual's

[7]The monthly state unemployment rate was obtained from the Bureau of Labor Statistics. Spending information was collected from the Bureau of Economic Analysis Regional Economic Accounts (BEAREA) and from Bitler and Hoynes (2016). We constructed per capita spending using state population information from the National Cancer Institute SEER data. Maximum AFDC/TANF benefits obtained from Robert Moffitts website: http://www.econ2.jhu.edu/people/moffitt/datasets.html. State EITC as fraction of Federal EITC was obtained from the Tax Policy Center. The information on whether a state had a welfare reform waiver and when they implemented welfare reform comes from Bitler, Gelbach and Hoynes (2005).

[8]We do not take into account asset tests, which were still under implementation in some states during our sample period.

[9]The demographic groups depend on the employed/unemployed state of the mother and the father, and on the demographics of the parents (education and one or two parent families). In our main analysis, we use the measure constructed for all the children with unemployed fathers, by child age. In the CPS we do not have a measure of job loss so we instead use whether the father is unemployed at the time of the survey, and whether the unemployment is involuntary. In our analysis for the employed *primary earner* parent sample, we also use the simulated eligibility for children with unemployed fathers, while in our analysis of mother job loss with the employed mother sample, we use a simulated measure of eligibility constructed for children with unemployed mothers. Following the same definitions we use in the MEPS data, we drop self-employed from the samples and we define one and two-parent families if the mother, or both the mother and the father is living with the child at the time of the survey.

eligibility as if they lived in each year and each state. We then collapse the data down to the state, age and year level (using the survey weights) to calculate the fraction of children eligible in each state, year, age, and demographic group (e.g. dad unemployed).

Our simulated measure of UI generosity is constructed using a fixed national sample from the Survey of Income and Program Participation (SIPP). The sample is composed of households where the father is unemployed, observed in the first month of unemployment, between the years 1993-2013. We use this sample and a UI calculator containing data on state UI laws, from Kuka (2015), to calculate UI weekly benefits. The formula used to calculate benefits varies by state, year, and the number of children of the unemployed individual.[10] We calculate the benefits for which each individual in this fixed national sample would be eligible if they lived in each state in each year, based on their pre-unemployment earnings, their number of children, and each state and year's UI laws regarding the percent of earnings to be replaced by UI, the minimum and maximum amount of weekly earnings, and the minimum amount of earnings required for eligibility to the program. We then divide these benefits by the individual's weekly earnings to obtain a simulated replacement rate. We then collapse the data down to the state, number of children and year level (using the survey weights) to calculate an average replacement rate for each state, year, and family size.

Table 3.1 presents Round 1 summary statistics by parental displacement status for the father-employed sample. A number of statistically significant differences between the columns highlight the importance of our empirical approach, which includes individual fixed effects and linear time trends that are allowed to vary depending on baseline health status. Specifically, the children of displaced workers are less likely to be white and their parents are less likely to have a college education. The children of displaced workers also are more likely to come from single-earner families and families, and have lower levels of private health insurance coverage and higher levels of public insurance coverage prior to

[10]Like we did for the measure of Medicaid/CHIP eligibility, in our main analysis we use the measure constructed for all the children with unemployed fathers, in this case by family size. We also constructed measures of other demographic groups depending on the employed/unemployed state of the mother and the father, and the parent's education levels. In our analysis for the employed *primary earner* parent sample, we also use the simulated generosity for all children with unemployed fathers, while in our analysis for the employed mother sample, we use the simulated measure of generosity constructed for all children with unemployed mothers.

job loss. Despite these differences in health insurance coverage, we don't observe any significant differences in our measures of health care utilization and expenditures. In terms of the characteristics of the states where they live, Table 3.1 shows only a few significant differences, with children of displaced fathers being more likely to live in states with less generous state EITC benefits, and with lower spending in retirement and disability benefits. The state unemployment rate is higher for the displaced sample, but the difference is very small (0.17 percentage points). There are no significant differences in our measures of simulated UI replacement rate or Medicaid/CHIP generosity.

3.4 Empirical Approach

We identify how the effects of parental job loss interact with the generosity of each social insurance program (Medicaid/CHIP and UI) by running individual fixed-effects regression models, where an indicator for post-father's job loss periods is interacted with a measure of generosity the program. Our identification relies on both the job loss and the variation in the measure of generosity of each program being exogenous, given our controls.

Our identification strategy for the causal effect of parental job loss follows closely the empirical strategy used by Schaller and Zerpa (2016). It relies on estimating child fixed-effects models, limiting the sample to children whose father has been at the same job for at least one year, and choosing a definition of job displacement based on reasons for job changes that are likely to be involuntary and exogenous to child health. Within this setting, identification relies on the assumption there are be no unobservable *time-varying* factors that are correlated both with the probability of worker displacement and with child health outcomes.

To identify the causal effects of Medicaid/CHIP and UI generosity, and their interaction with job loss, we construct simulated measures of program generosity that rely on the variation over time within states in their generosity driven by changes in legislation. The advantage of using simulated measures of program generosity is that, unlike individual eligibility and benefits, they do not rely on individual characteristics that may be correlated with job loss and with child outcomes. Similar measures of program generosity have been widely used in the literature (Currie and Gruber, 1996; Gruber, 1997; Cohodes et al., 2016; East and Kuka, 2015). We estimate separate models to identify the roles of Medicaid/CHIP, on one hand, and UI, on the other. Details about how we construct each of the simulated generosity measures are provided in Section 3.3

To identify the effects of Medicaid/CHIP, we estimate a series of fixed-effects models with the following main specification:

$$Y_{iast} = \alpha_i + \beta_1 D_{iast} + \beta_2 D_{iast} \times Med\_Sim_{ast} + \beta_3 Med\_Sim_{ast} + \mathbf{X}'_{iast}\gamma + \delta_t + \nu_a + \varepsilon_{iast} \quad (3.1)$$

where Y_{iast} is the outcome variable for child i, of age a, in state s, at time t. The models are estimated via OLS estimation; when the outcome is binary, we estimate a linear probability model. The main regressors of interest are D_{iast}, an indicator for post-father's job loss rounds, Med_Sim_{ast}, a measure of simulated generosity of Medicaid/CHIP eligibility rules that varies at the state-age-year level, and the interaction between the two ($D_{iast} \times Med\_Sim_{ast}$). Our main coefficient of interest is β_2, which represents the differential effect of job displacement according to the generosity of the state's Medicaid/CHIP program.

The model includes individual fixed effects (α_i), as well as dummies for age in years (ν_a). To control for the effects of time, we include dummies by year of interview and by interview round. Because the interview for a given round can take place in different months of the year for different households, we also include dummies for calendar month to further control for seasonal effects. In addition, we control for Medicaid/CHIP generosity (Med_Sim_{ast}). In addition, we include other time-varying controls in the vector X_{iast}, which includes the length of the round (number of days between interviews), simulated Unemployment Insurance generosity, dummies for the number of children in the family, and state-year level controls for the generosity of other policies and the state unemployment rate during the month of the interview. We also include separate linear time trends for each of five baseline health status categories. We cluster standard errors by state.

For Unemployment Insurance, we estimate analogous models as follows:

$$Y_{icst} = \alpha_i + \beta_1 D_{icst} + \beta_2 D_{icst} \times UI\_Sim_{cst} + \beta_3 UI\_Sim_{cst} + \mathbf{X}'_{icst}\gamma + \delta_t + \nu_c + \varepsilon_{icst} \quad (3.2)$$

where Y_{icst} is the outcome variable for child i, of family size (number of children) c, in state s, at time t. Here, β_2 represents the differential effect of displacement according to the generosity of the state's UI policies ($D_{icst} \times UI\_Sim_{cst}$). The simulated measure of UI generosity (replacement rates) varies across state, year, and family size. In this case, X_{icst} includes the length of the round, the simulated measure of Medicaid/CHIP generosity, dummies for child age, and state-year level controls for the generosity of other policies and the state unemployment rate during the month of the interview. Like in equation 3.1, we also include separate linear time trends for each baseline health status category, and we cluster standard errors by state.

Our main sample includes children residing with a father who was employed and had at least a year of tenure in the first round. As a robustness check, we also run regression on an alternative sample where we examine the effects of *primary earner* parents' displacement. This sample includes, in addition to the children in our main sample, children with single mothers[11] who were employed and with one year of tenure in the first round. In regressions estimated for the *primary earner* sample, the job displacement variable refers to fathers' or single mothers' displacements.[12]

We expect the effects of job loss on health insurance and health care utilization and expenditures to be stronger for children who were insured by a parent's employer. For this reason, we explore the heterogeneity of the effects by source of insurance in the first round. We construct a variable that indicates whether the child had health insurance provided by a parent's employer during the first round, and interact this dummy with job loss, with program generosity, and with the interaction between job loss and program generosity. We also explore the heterogeneity of effects by parental education, using a

[11] We define the sample of single mothers as those children whose mother was living in the household and the father was not.

[12] We have also run regressions for the sample of children whose mother (single or not) was employed in the first round, to explore the effects of maternal job displacements. Results for this sample are available upon request.

similar strategy with a variable that indicates whether a child's parents' highest education level is complete high school or less.

3.5 Results

3.5.1 Baseline Results: Effects of Fathers' Job Losses

To establish a baseline, we first estimate the effects of job loss on each of our outcomes of interest with child fixed-effects models that control for our simulated measures of Medicaid/CHIP and UI generosity. The models estimated are similar to those estimated in Schaller and Zerpa (2016, SZ hereafter), with the only difference that we add more control variables, including the simulated generosity measures and other state characteristics.

Our results, presented in Table C1, reproduce very closely the results found by SZ for the overlapping outcomes. Father's job loss leads to a decrease in the likelihood of having private health insurance coverage of 11 percentage points, which is partially offset by a close to 6 percentage point increase in public insurance coverage, coming entirely from the Medicaid/CHIP program. As a result, children are less likely to have health insurance coverage after the job loss. In terms of health care utilization, children are more likely to use mental health care after a father's job loss. SZ find that this increase in utilization of mental health care is related to an increase in children's mental health problems, which is the only effect on health outcomes that they find for the full sample.[13] We also find that job loss leads to a decrease in dental health visits. In terms of health care expenditures, the only significant effect we find is an increase in the expenditures covered by Medicaid/CHIP.

3.5.2 Paternal Job Loss and Medicaid/CHIP generosity

The results for our estimates of the interaction of Medicaid/CHIP generosity and job loss are in Table 3.2. These results correspond to the estimates of the models presented in equation 3.1. The top panel of the table shows the estimates for health insurance coverage

[13] When they look at the heterogeneity of effects across demographic groups, SZ also find some negative effects of father job loss on the physical health of children from families of lower socioeconomic status.

outcomes. The estimates for the interaction of father displacement with Medicaid/CHIP generosity show no evidence of statistically significantly different effects of job loss in states with more generous policies on any of the health insurance coverage outcomes.

The estimates for health care utilization outcomes (center panel of Table 3.2) also show no differential effects of job loss in states with more generous Medicaid/CHIP eligibility for most outcomes, with the exception of dental visits. While job loss decreases the likelihood of visiting a dentist, this negative effect is mitigated in states with more generous Medicaid/CHIP eligibility rules. Our estimates imply that the net effect of job loss for the average generosity (percent of children with unemployed fathers eligible) in our sample (52%) is a decrease of 2.8 percentage points (p.p) in the likelihood of a dental visit, and that a 10 percentage point increase in Medicaid generosity increases the likelihood of a dental visit by 1.5 p.p.

The results presented in the bottom panel of Table 3.2 show that there is a statistically significant effect of Medicaid/CHIP generosity on the change in out-of-pocket expenditures after job loss. There is a statistically significant decrease in out-of-pocket expenditures for children that live in states with more generous Medicaid/CHIP programs of $18.5 for a 10 p.p. increase in simulated generosity. For the average simulated Medicaid/CHIP generosity, the net effect of job loss on out of pocket expenditures is negative but not statistically significant.

We would expect Medicaid/CHIP to have more relevance as an insurance against the effects of job loss for children who previously had employer provided insurance, as this is the group most likely to lose coverage after a father's job loss. In Table 3.3, we present the heterogeneity of these effects for children who were covered by a parent's employer insurance in the first round. As expected, the results presented in the top panel of Table 3.3 show that the negative effect of job loss on private insurance is larger (and only negative and statistically significant) for children who had employer provided coverage. These children are as a consequence more likely to experience lack of coverage after the job loss. For this group, job loss decreases the probability of having private health insurance coverage by 19.2 p.p. In contrast, the probability of having private health insurance coverage increases by 7.7 p.p. after a father's job loss.[14]

When we interact the simulated measure of Medicaid/CHIP generosity with job loss for the group of children who previously had insurance provided by a parent's employer, we find that these children are more likely to take up public insurance if they live in states with more generous Medicaid/CHIP eligibility rules. Our estimates imply that a 10 percentage point increase in Medicaid/CHIP generosity is associated with a 0.53 p.p. increase in the likelihood of having public health insurance coverage after job loss. The estimated effects on Medicaid/CHIP insurance in particular, and on overall health insurance coverage, are also positive, but they are not statistically significant. These results suggest that Medicaid/CHIP works more effectively as a social insurance policy for children of unemployed fathers in states with more generous eligibility rules.

The top panels of Tables 3.3 and C2 also shows another interesting set of results. The group of children who did not have employer provided insurance are more likely to gain coverage after job loss. For children in this group with the average level of Medicaid/CHIP generosity, the positive effect of job loss on health insurance coverage is of 7.7 p.p. and it is mainly explained by an increase in private insurance coverage (Table C2). Since we are looking here at a group who did not have employer-provided health insurance before, job loss can lead to re-employment with an employer that provides health insurance. When interacting job loss with Medicaid/CHIP generosity for this group, we find that children in this group are also more likely to gain public insurance coverage after job loss in states with lower Medicaid/CHIP generosity (Table 3.3). This result could be explained by an increased likelihood of becoming (or staying) eligible for public insurance after job loss due to the fall in family income. A 10 percentage point increase in simulated Medicaid/chip generosity is associated with a 1.47 percentage point decrease in the likelihood of taking up public insurance after father's job loss. This suggests that, in states with more generous eligibility rules, the drop in income caused by job loss has less of an effect on eligibility into public insurance, possibly because they are more likely to be eligible for public insurance even in the absence of job loss.

[14] The overall impacts of job loss can be calculated from Table 3.3 using the average simulated Medicaid/CHIP generosity for our sample, but they can also be more easily found by looking at the results shown in Appendix Table C2.

The results for health care utilization (center panel of Table 3.3) indicate that, for the group of children with employer-provided insurance in the first round, more generous Medicaid/CHIP leads to slightly larger increases in mental health care utilization after job loss. Finally, the results for health care expenditures (bottom panel of Table 3.3), show that the larger decrease in out-of-pocket expenditures associated with a more generous Medicaid/CHIP eligibility is only significant for the group of children who used to have employer provided health insurance. For this group of children, the estimated marginal effect of a 10 p.p. increase in simulated Medicaid/CHIP generosity interacted with job loss is a decrease of $24.3 in out-of-pocket expenditures.

3.5.3 Paternal Job Loss and UI generosity

We present the results for our main estimates of the interaction of UI generosity and job loss in Table 3.4. These results correspond to the estimates of the models presented in equation 3.2. The results in the top panel of the table indicate that a more generous UI replacement rate reduces the likelihood of gaining eligibility for Medicaid/CHIP after job loss, leading to a higher likelihood of having a gap in coverage of at least one month. Recall that a more generous UI replacement rate can affect insurance coverage through two opposite channels; UI could improve access to private insurance through an income effect; or the additional income could harm eligibility for Medicaid/CHIP. The second channel is more relevant for children than for working-age adults, because children are more likely to be eligible for public insurance. In fact, Kuka (2015) finds that a more generous UI program increases the likelihood of having health insurance coverage for adults, with a probability of being insured 2.7 p.p. higher for a 10 p.p increase in UI replacement rates. Our results indicate that the opposite happens for children; a 10 p.p increase in replacement rates is associated with a 3.8 p.p. lower likelihood of having Medicaid/CHIP insurance and a 4.3 p.p. higher likelihood of having a gap in coverage after father's job loss. It should also be noted that our results don't support a relevance of the income effect channel for child health insurance, since we find no evidence of a positive effect of UI generosity on private insurance coverage after job loss.

The results for health care utilization and expenditures are presented in the center and bottom panels of Table 3.4, respectively. We do not find any statistically significant differential effects of UI generosity on health care utilization. However, we do observe a significant positive effect on total expenditures, which is completely explained by a positive effect on expenditures covered by private insurance. This result may be surprising, as we did not see evidence of higher UI generosity increasing the likelihood of private health insurance coverage. This might be explained, however, by a positive effect of UI generosity on expenditures conditional on keeping private insurance coverage after job loss.

We also estimated the heterogeneity of effects of UI by source of coverage in the first round, presented in Table 3.5. In the top panel, the negative and significant estimate for the interaction of father displacement with simulated UI generosity, and the opposite sign of the effect for the group of children with employer provided insurance in the first round, suggest that the negative effects of UI generosity on Medicaid/CHIP take up after job loss are concentrated in the group of children who did not have employer-provided insurance. On the other hand, the results presented in the bottom panel suggest that the increase in expenditures associated with more generous UI replacement rates is concentrated in the group of children who previously had private health insurance.

3.5.4 Including Single Mothers' Job Losses

The decision to focus on father job losses comes with the disadvantage that it entails focusing on two-parent families, while the effects of job loss and the capacity of social insurance programs to respond to negative shocks may be different for single mother families. In particular, one-earner families are more likely to be eligible for Medicaid/CHIP, especially in the event of a job loss. In addition, the effects of parental job loss on child health have been found to be different depending on whether it is the mother or the father who loses their job. Schaller and Zerpa (2016) find no evidence of negative effects of maternal job loss on child health; on the contrary, they find a decrease in the incidence of acute illness (infections) after mother's job losses. In this section, we explore the robustness of our main results to the inclusion of employed single mother families in the sample, and the consideration of both paternal and maternal job losses within this sample. We are still excluding maternal job losses in two-parent families, where mothers are more likely to be second earners.

We begin by estimating the effects of *primary earner* parent's job loss on all outcomes (Table C3). The effects are similar to those found for the effects of father's job loss, with the exception of the effect on mental health care utilization, which is smaller than that found for father's job loss and only statistically significant at a 10% level. As discussed before, SZ find that father's job loss leads to an increase in mental health problems, but not mother's job loss. The estimates for the increase in public insurance and Medicaid/CHIP are slightly higher than for father's job loss (6.3 and 6.2 p.p., respectively).

The results Table C4 replicate those in Table 3.3 but including single mothers' job losses. All the discussed effects are similar. The effects of Medicaid/CHIP generosity on the probability of taking up public insurance after job loss for the group of children who used to have employer-provided insurance are more clear in this sample, because we not only find a statistically significant effect on public insurance coverage, but also a similar and statistically significant effect on Medicaid/CHIP coverage that we did not find with fathers' job losses only. A 10 percentage point in simulated Medicaid/CHIP generosity has a marginal effect of 0.95 percentage points in the probability of taking up public insurance (and in particular Medicaid/CHIP) after job loss.

The effects of the interaction of job loss and UI generosity are more precisely estimated when we also include single mother's job losses, estimated for the employed-*primary earner* parent sample. The results, shown in Table C5, indicate statistically significant negative effects of similar size on Medicaid/CHIP and public insurance in general, as well as a statistically significant decrease in the likelihood of having any insurance coverage. However, the estimated effects of UI generosity on total expenditures and expenditures covered by private insurance are smaller and not statistically significant at a 5% level for the *primary earner* sample.

3.6 Conclusion

Medicaid/CHIP and Unemployment Insurance are large social insurance programs whose design equips them to respond to negative labor market shocks. Despite the large increases in caseloads and spending in Medicaid/CHIP and Unemployment Insurance during economics downturns, and particularly during the Great Recession, little is known about the effectiveness of these programs in protecting children against the negative effects of parental job loss. In this paper we evaluate how the generosity of Medicaid/CHIP and UI affect how the health insurance coverage and health care access of children respond to fathers' job losses in the short run.

To estimate these effects, we use data on health insurance coverage, health care utilization, and health expenditures for children, from repeated two-year panels of the Medical Expenditure Panel Survey (MEPS) that span the period between 1996 and 2012. We combine these data with state-level data from different sources to construct measures of Medicaid/CHIP and UI generosity, as well as various state-level controls. We study the heterogeneity of the effects of parental job loss for different levels of Medicaid/CHIP and UI generosity, by estimating child fixed effects models that regress each outcome on an indicator of father job loss that is interacted with simulated measures of program generosity. The use of simulated measures of Medicaid/UI generosity and UI replacement rates, instead of individual eligibility and benefits, allows us to exploit exogenous variation in the generosity of these programs across states.

Our results show that, for children who were insured through a parent's employer before the job loss, more generous Medicaid/CHIP eligibility rules increase their likelihood of taking up public insurance, partially mitigating the impacts of job loss on health insurance coverage. We also find that out-of-pocket expenditures are less likely to increase after job loss in states with more generous Medicaid/CHIP, while we do not find robust evidence of short-term effects on health care utilization. These findings suggest an important role for public health insurance in protecting children who are at risk of losing insurance coverage when parents experience negative labor market shocks. The short-panel nature of our data does not allow us to estimate the implications of these short-term effects on health insurance coverage for children's long-term health and health care utilization. The estimation of these long-term impacts is an important avenue for future research, as it can allow us to quantify the private and social benefits of this labor-market insurance role of public health insurance, taking into account the potential externalities that it may have on long-term health care costs.

Finally, our results for the role of UI show that more generous UI replacement rates have a negative effect on child health insurance coverage, by decreasing the likelihood of taking up public insurance. This suggests that the insurance power of Medicaid against the effects of parental job loss may be hindered by the interaction between UI benefits and Medicaid/CHIP eligibility rules. This has important public policy implications, underscoring the importance of analyzing how different programs interact when families receive negative shocks.

3.7 Tables

Table 3.1: Round 1 Summary Statistics

	Not Displaced	Displaced	Difference	p-value
Insurance				
Any insurance	0.930	0.900	-0.029***	(0.000)
Private insurance	0.814	0.705	-0.109***	(0.000)
Public insurance	0.137	0.218	0.082***	(0.000)
Medicaid/CHIP	0.112	0.198	0.087***	(0.000)
Gap in coverage of 1 month or more	0.076	0.124	0.048***	(0.000)
Health Care Utilization				
Checkup visit	0.154	0.162	0.008	(0.441)
Diagnosis visit	0.308	0.285	-0.022	(0.108)
ER visit	0.030	0.032	0.003	(0.658)
Mental health visit	0.012	0.009	-0.003	(0.236)
Dental visit	0.272	0.266	-0.006	(0.647)
Expenditures				
Total expenditures	457.034	434.140	-22.894	(0.759)
Out-of-pocket expenditures	121.693	96.024	-25.669*	(0.090)
Expend. Cov. Medicaid/CHIP	37.765	39.073	1.308	(0.903)
Expend. Cov. Private insurance	270.874	284.309	13.435	(0.839)
Dental expenditures	136.904	129.109	-7.795	(0.695)
Demographic Characteristics				
Male	0.515	0.506	-0.010	(0.510)
Age	8.441	8.332	-0.109	(0.444)
Black	0.077	0.105	0.028***	(0.001)
Hispanic	0.171	0.249	0.078***	(0.000)
Parents HS or less	0.314	0.393	0.079***	(0.000)
Single Earner	0.353	0.400	0.047***	(0.001)
Number of children in family	2.233	2.240	0.007	(0.826)
State Characteristics				
Unemployment rate	5.714	5.880	0.166***	(0.009)
Max AFDC/TANF Benefits	536.0	536.9	0.859	(0.892)
Welfare Reform or Waiver	0.938	0.949	0.012	(0.101)
State EITC as Fraction of Federal	0.046	0.035	-0.012***	(0.000)
Spend on Food Stamps Per Capita	0.120	0.120	0.000	(0.880)
Spend on SSI Per Capita	0.157	0.160	0.003*	(0.082)
State Spend on Welfare Per Capita	0.206	0.209	0.004	(0.193)
Spend on Retirement & Disability Per Capita	2.175	2.140	-0.035***	(0.001)
Simulated Medicaid/CHIP generosity	0.518	0.518	0.001	(0.893)
Simulated UI replacement rate	0.409	0.405	-0.003*	(0.062)
Sample size	22665	1969		

Note: Data are from the 1996-2012 waves of the Medical Expenditure Panel Survey (MEPS). The sample includes children who were 1-16 years old and whose father was employed with at least one year of job tenure in the first round. Estimates are weighted using MEPS sampling weights.

Table 3.2: Effect of Medicaid/CHIP Generosity Interacted with Father's Job Loss

	Health Insurance				
	Any	Private	Public	Med/CHIP	Gap
Father displaced	-0.035	-0.094**	0.060***	0.051**	0.034
	(0.030)	(0.037)	(0.021)	(0.023)	(0.028)
Father disp*Sim Med/CHIP Gen	-0.039	-0.029	-0.007	0.010	0.066
	(0.053)	(0.067)	(0.037)	(0.041)	(0.050)
Sim Med/CHIP Gen	-0.006	-0.020	0.006	0.004	0.002
	(0.015)	(0.020)	(0.009)	(0.010)	(0.017)
	Health Care Utilization				
	Checkup	Diagnosis	ER	Mental	Dental
Father displaced	-0.005	0.050	-0.011	0.000	-0.107***
	(0.028)	(0.037)	(0.014)	(0.009)	(0.039)
Father disp*Sim Med/CHIP Gen	-0.010	-0.090	0.009	0.013	0.151**
	(0.052)	(0.070)	(0.025)	(0.017)	(0.075)
Sim Med/CHIP Gen	0.002	0.022	-0.017	0.013*	-0.046
	(0.023)	(0.036)	(0.012)	(0.007)	(0.040)
	Health Care Expenditures				
	Total	OOP	Med/CHIP	Private Ins.	Dental
Father displaced	108.110	79.380*	35.311	-24.981	23.591
	(167.937)	(40.182)	(29.222)	(183.340)	(64.346)
Father disp*Sim Med/CHIP Gen	-182.397	-185.264**	43.013	-11.587	-63.013
	(282.803)	(72.489)	(57.248)	(313.652)	(108.090)
Sim Med/CHIP Gen	-5.164	-108.500	6.250	94.902	-127.450**
	(823.117)	(260.394)	(237.375)	(775.389)	(318.382)
Individuals	24634	24634	24634	24634	24634
Displacements	1969	1969	1969	1969	1969

Note: Data are from the 1996-2012 waves of the Medical Expenditure Panel Survey (MEPS), and includes five rounds of observations in each panel. The sample includes children who were 1-16 years old and whose father was employed with at least one year of job tenure in the first round. In addition to the variables displayed in the left column, all regressions include individual fixed effects, controls for age, number of siblings, interview year and calendar month, round fixed effects, round length, and state-level controls (simulated UI replacement rates, unemployment rate, max AFDC/TANF Benefits, welfare reform or waiver, state EITC as fraction of federal, and per capital spending on Food Stamps, SSI, Welfare, and Retirement & Disability). Estimates are weighted using MEPS sampling weights.

Table 3.3: Heterogeneity of Effects of Medicaid/CHIP Generosity by Source of Insurance

	Health Insurance				
	Any	Private	Public	Med/CHIP	Gap
Father displaced	0.174**	0.073	0.092**	0.061	-0.163**
	(0.075)	(0.070)	(0.046)	(0.047)	(0.062)
Father disp*Employer Ins	-0.269***	-0.213***	-0.042	-0.014	0.253***
	(0.071)	(0.078)	(0.059)	(0.057)	(0.064)
Father disp*Sim Med/CHIP Gen	-0.180	-0.005	-0.147**	-0.075	0.169*
	(0.107)	(0.116)	(0.072)	(0.070)	(0.094)
Father disp*Empl Ins*Sim Med/CHIP Gen	0.138	-0.096	0.200**	0.125	-0.086
	(0.099)	(0.126)	(0.094)	(0.085)	(0.099)
	Health Care Utilization				
	Checkup	Diagnosis	ER	Mental	Dental
Father displaced	-0.072*	0.077	-0.006	0.012	-0.127*
	(0.037)	(0.055)	(0.024)	(0.008)	(0.065)
Father disp*Employer Ins	0.086*	-0.033	-0.006	-0.015	0.027
	(0.049)	(0.074)	(0.030)	(0.012)	(0.086)
Father disp*Sim Med/CHIP Gen	0.054	-0.060	0.030	-0.020*	0.188
	(0.073)	(0.097)	(0.040)	(0.011)	(0.121)
Father disp*Empl Ins*Sim Med/CHIP Gen	-0.073	-0.054	-0.035	0.046**	-0.049
	(0.085)	(0.126)	(0.052)	(0.023)	(0.155)
	Health Care Expenditures				
	Total	OOP	Med/CHIP	Private Ins.	Dental
Father displaced	125.296*	6.276	124.384	-4.645	-20.747
	(73.115)	(24.563)	(83.043)	(36.696)	(37.190)
Father disp*Employer Ins	-21.709	94.906	-114.872	-26.533	57.721
	(232.288)	(61.853)	(89.327)	(223.923)	(77.255)
Father disp*Sim Med/CHIP Gen	-156.521	-19.205	-76.850	-72.141	55.035
	(136.195)	(40.648)	(163.235)	(49.334)	(70.575)
Father disp*Empl Ins*Sim Med/CHIP Gen	-44.990	-223.964**	148.839	83.902	-161.906
	(407.774)	(110.874)	(194.858)	(391.139)	(139.806)
Individuals	24634	24634	24634	24634	24634
Displacements	1969	1969	1969	1969	1969

Note: Data are from the 1996-2012 waves of the Medical Expenditure Panel Survey (MEPS), and includes five rounds of observations in each panel. The sample includes children who were 1-16 years old and whose father was employed with at least one year of job tenure in the first round. In addition to the variables displayed in the left column, all regressions include individual fixed effects, controls for age, number of siblings, year of interview, calendar month of interview, interview round, round length, and state-level controls (simulated Medicaid/CHIP generosity, simulated UI replacement rate, unemployment rate, max AFDC/TANF Benefits, welfare reform or waiver, state EITC as fraction of federal, and per capital spending on Food Stamps, SSI, Welfare, and Retirement & Disability). Estimates are weighted using MEPS sampling weights.

Table 3.4: Effect of Unemployment Insurance Generosity Interacted with Father's Job Loss

	Health Insurance				
	Any	Private	Public	Med/CHIP	Gap
Father displaced	0.101	-0.046	0.157**	0.209***	-0.105
	(0.082)	(0.123)	(0.060)	(0.072)	(0.078)
Father disp*Sim UI Repl. Rate	-0.383*	-0.156	-0.247	-0.376**	0.426**
	(0.206)	(0.311)	(0.151)	(0.177)	(0.194)
Sim UI Repl. Rate	0.401*	0.236	0.349	0.377*	-0.355
	(0.207)	(0.193)	(0.215)	(0.205)	(0.247)
	Health Care Utilization				
	Checkup	Diagnosis	ER	Mental	Dental
Father displaced	-0.117	0.125*	-0.056	-0.014	-0.075
	(0.090)	(0.072)	(0.050)	(0.022)	(0.098)
Father disp*Sim UI Repl. Rate	0.263	-0.299*	0.119	0.053	0.114
	(0.229)	(0.178)	(0.123)	(0.057)	(0.230)
Sim UI Repl. Rate	-0.132	-0.103	0.175	0.090	-0.085
	(0.250)	(0.237)	(0.137)	(0.060)	(0.305)
	Health Care Expenditures				
	Total	OOP	Med/CHIP	Private Ins.	Dental
Father displaced	-688.391*	42.197	73.974	-751.647**	46.734
	(347.138)	(66.399)	(105.590)	(356.376)	(122.549)
Father disp*Sim UI Repl. Rate	1726.199**	-145.616	-40.075	1772.984**	-137.582
	(801.033)	(162.012)	(259.757)	(824.058)	(270.515)
Sim UI Repl. Rate	451.485	-215.007	339.088	508.333	-86.883
	(826.722)	(264.638)	(238.027)	(779.108)	(319.904)
Individuals	24634	24634	24634	24634	24634
Displacements	1969	1969	1969	1969	1969

Note: Data are from the 1996-2012 waves of the Medical Expenditure Panel Survey (MEPS), and includes five rounds of observations in each panel. The sample includes children who were 1-16 years old and whose father was employed with at least one year of job tenure in the first round. In addition to the variables displayed in the left column, all regressions include individual fixed effects, controls for age, number of siblings, interview year and calendar month, round fixed effects, round length, and state-level controls (simulated Medicaid/CHIP generosity, unemployment rate, max AFDC/TANF Benefits, welfare reform or waiver, state EITC as fraction of federal, and per capital spending on Food Stamps, SSI, Welfare, and Retirement & Disability). Estimates are weighted using MEPS sampling weights.

Table 3.5: Heterogeneity of Effects of UI Generosity by Source of Insurance

	Health Insurance				
	Any	Private	Public	Med/CHIP	Gap
Father Displaced	0.318**	0.092	0.208	0.382**	-0.318**
	(0.151)	(0.158)	(0.159)	(0.150)	(0.155)
Father disp*Employer Ins	-0.332	-0.231	-0.063	-0.236	0.328
	(0.199)	(0.198)	(0.203)	(0.175)	(0.218)
Father disp*Sim UI Repl. Rate	-0.596*	-0.054	-0.485	-0.895**	0.609*
	(0.337)	(0.377)	(0.371)	(0.359)	(0.354)
Father dips*Emp Ins* Sim UI Repl. Rate	0.348	-0.076	0.317	0.716*	-0.309
	(0.453)	(0.457)	(0.457)	(0.400)	(0.514)
	Health Care Utilization				
	Checkup	Diagnosis	ER	Mental	Dental
Father Displaced	0.083	0.188	-0.048	-0.051	-0.067
	(0.165)	(0.134)	(0.050)	(0.035)	(0.165)
Father disp*Employer Ins	-0.275	-0.096	-0.015	0.053	-0.012
	(0.202)	(0.191)	(0.083)	(0.044)	(0.214)
Father disp*Sim UI Repl. Rate	-0.310	-0.357	0.143	0.129	0.103
	(0.385)	(0.329)	(0.120)	(0.089)	(0.396)
Father dips*Emp Ins* Sim UI Repl. Rate	0.793*	0.095	-0.027	-0.109	0.018
	(0.463)	(0.441)	(0.199)	(0.110)	(0.503)
	Health Care Expenditures				
	Total	OOP	Med/CHIP	Private Ins.	Dental
Father Displaced	332.097	-36.683	317.826	-15.005	99.400
	(259.352)	(59.949)	(219.368)	(106.878)	(117.193)
Father disp*Employer Ins	-1.4e+03**	108.557	-348.528	-1.0e+03**	-77.822
	(557.129)	(119.706)	(276.855)	(463.349)	(186.773)
Father disp*Sim UI Repl. Rate	-720.419	80.884	-581.457	-70.710	-223.197
	(602.245)	(140.252)	(498.730)	(243.078)	(276.268)
Father dips*Emp Ins* Sim UI Repl. Rate	3450.641***	-313.606	770.309	2588.168**	127.102
	(1265.644)	(298.479)	(634.212)	(1067.452)	(448.058)
Individuals	24634	24634	24634	24634	24634
Displacements	1969	1969	1969	1969	1969

Note: Data are from the 1996-2012 waves of the Medical Expenditure Panel Survey (MEPS), and includes five rounds of observations in each panel. The sample includes children who were 1-16 years old and whose father was employed with at least one year of job tenure in the first round. In addition to the variables displayed in the left column, all regressions include individual fixed effects, controls for age, number of siblings, year of interview, calendar month of interview, interview round, round length, and state-level controls (simulated Medicaid/CHIP generosity, simulated UI replacement rate, unemployment rate, max AFDC/TANF Benefits, welfare reform or waiver, state EITC as fraction of federal, and per capital spending on Food Stamps, SSI, Welfare, and Retirement & Disability). Estimates are weighted using MEPS sampling weights.

Appendix

A Appendix A: Additional Tables to Chapter 1

Table A1: Reduced-Form Effects on Development Index Components

	Learning Disability Diag.	ADHD Diagnosis	Limitation Speech	Limitation Behavior
Both Genders, 1-4 Years After Pre-K				
Post Pre-K	-0.021	-0.003	0.001	-0.007*
	(0.014)	(0.013)	(0.008)	(0.004)
N	9069	9069	9069	9069
N Treatment States	6171	6171	6171	6171
Both Genders, 5-8 Years After Pre-K				
Post Pre-K	-0.004	-0.008	-0.008	-0.002
	(0.006)	(0.016)	(0.005)	(0.005)
N	9007	9007	9007	9007
N Treatment States	5889	5889	5889	5889
Boys, 1-4 Years After Pre-K				
Post Pre-K	-0.019	0.007	-0.002	-0.011*
	(0.019)	(0.018)	(0.013)	(0.006)
N	4643	4643	4643	4643
N Treatment States	3154	3154	3154	3154
Boys, 5-8 Years After Pre-K				
Post Pre-K	-0.049***	-0.026	-0.006	-0.008
	(0.016)	(0.025)	(0.006)	(0.007)
N	4576	4576	4576	4576
N Treatment States	3015	3015	3015	3015
Girls, 1-4 Years After Pre-K				
Post Pre-K	-0.020	-0.014	0.004	-0.004
	(0.015)	(0.015)	(0.008)	(0.005)
N	4426	4426	4426	4426
N Treatment States	3017	3017	3017	3017
Girls, 5-8 Years After Pre-K				
Post Pre-K	0.047**	0.014	-0.009	0.006
	(0.018)	(0.019)	(0.007)	(0.010)
N	4431	4431	4431	4431
N Treatment States	2874	2874	2874	2874

Notes: Each cell shows results for separate regressions, for the outcome variable indicated in the column heading, and the sample (gender and age–number of years after pre-K age) indicated in each panel heading. All regressions include state and cohort fixed effects, individual-level control variables for maternal education and race/ethnicity (and gender in the first panel), age dummies, and state-level control variables. Robust standard errors (clustered by state) in parentheses. * $p<0.1$, ** $p<0.05$, *** $p<0.01$.

Table A2: Reduced-Form Effects on Health Index Components

	3+ Ear Infections	Asthma Episode	Frequent Headaches	Frequent Diarrhea
Both Genders, 1-4 Years After Pre-K				
Post Pre-K	0.002	0.016	0.007	0.016***
	(0.013)	(0.011)	(0.011)	(0.005)
N	9069	9069	9069	9067
N Treatment States	6171	6171	6171	6170
Both Genders, 5-8 Years After Pre-K				
Post Pre-K	-0.004	0.019*	0.007	0.000
	(0.007)	(0.011)	(0.015)	(0.005)
N	9007	9007	9007	9007
N Treatment States	5889	5889	5889	5889
Boys, 1-4 Years After Pre-K				
Post Pre-K	0.015	0.022	0.015	0.009
	(0.016)	(0.015)	(0.016)	(0.007)
N	4643	4643	4643	4642
N Treatment States	3154	3154	3154	3153
Boys, 5-8 Years After Pre-K				
Post Pre-K	-0.023***	0.006	-0.008	-0.001
	(0.007)	(0.016)	(0.020)	(0.008)
N	4576	4576	4576	4576
N Treatment States	3015	3015	3015	3015
Girls, 1-4 Years After Pre-K				
Post Pre-K	-0.014	0.009	-0.002	0.023***
	(0.019)	(0.015)	(0.013)	(0.007)
N	4426	4426	4426	4425
N Treatment States	3017	3017	3017	3017
Girls, 5-8 Years After Pre-K				
Post Pre-K	0.017*	0.038**	0.027	0.001
	(0.009)	(0.015)	(0.020)	(0.007)
N	4431	4431	4431	4431
N Treatment States	2874	2874	2874	2874

Notes: Each cell shows results for separate regressions, for the outcome variable indicated in the column heading, and the sample (gender and age–number of years after pre-K age) indicated in each panel heading. All regressions include state and cohort fixed effects, individual-level control variables for maternal education and race/ethnicity (and gender in the first panel), age dummies, and state-level control variables. Robust standard errors (clustered by state) in parentheses. * $p<0.1$, ** $p<0.05$, *** $p<0.01$.

Table A3: Heterogeneity of Effects on Health Care Utilization and Insurance by Race/Ethnicity (Both Genders)

	Hospital Stay	Asthma ER Visit	Could Not Afford Care	Any Insurance	Public Insurance	Private Insurance
1-4 Years After Pre-K						
Post Pre-K	0.007	0.001	-0.002	-0.013	-0.020	0.004
	(0.006)	(0.008)	(0.006)	(0.009)	(0.025)	(0.023)
Post Pre-K * Black	0.002	0.010	-0.003	-0.004	0.025	-0.029
	(0.007)	(0.014)	(0.012)	(0.020)	(0.026)	(0.034)
Post Pre-K * Hispanic	0.001	0.018	-0.038**	0.052**	0.099**	-0.054*
	(0.009)	(0.018)	(0.014)	(0.020)	(0.041)	(0.031)
5-8 Years After Pre-K						
Post Pre-K	0.004	-0.004	-0.007	0.004	0.027	-0.024
	(0.006)	(0.006)	(0.013)	(0.014)	(0.020)	(0.021)
Post Pre-K * Black	-0.002	0.003	-0.007	0.013	0.057*	-0.037
	(0.015)	(0.016)	(0.012)	(0.013)	(0.034)	(0.041)
Post Pre-K * Hispanic	0.008	-0.006	-0.016	0.041	0.109***	-0.063
	(0.006)	(0.013)	(0.012)	(0.035)	(0.035)	(0.044)

Notes: Each panel shows results for separate regressions, for the outcome variable indicated in the column heading, and the age group (number of years after pre-K age) indicated in each panel heading. The regressors are the indicator for Post Pre-K, and its interaction with dummies for black and Hispanic. All regressions include state and cohort fixed effects, individual-level control variables for maternal education, gender, and race/ethnicity, age dummies, and state-level control variables. Sample sizes are shown in Panel A of Table 1.5. Robust standard errors (clustered by state) in parentheses. * p<0.1, ** p<0.05, *** p<0.01.

Table A4: Heterogeneity of Effects on Development and Health Outcomes by Race/Ethnicity (Both Genders)

	Special Education	Development Index	Health Fair/Poor	Health Index	Missed School Days
1-4 Years After Pre-K					
Post Pre-K Expansion	-0.015	-0.063	0.006	0.122**	0.647***
	(0.012)	(0.064)	(0.007)	(0.049)	(0.167)
Post Pre-K Exp. * Black	-0.011	-0.002	0.008	-0.078	-0.237
	(0.018)	(0.069)	(0.008)	(0.089)	(0.358)
Post Pre-K Exp. * Hispanic	0.014	0.112**	0.018	0.072	0.425
	(0.013)	(0.044)	(0.020)	(0.082)	(0.347)
5-8 Years After Pre-K					
Post Pre-K Expansion	0.019	-0.076	0.012*	0.055	0.114
	(0.017)	(0.053)	(0.006)	(0.047)	(0.284)
Post Pre-K Exp. * Black	-0.018	0.036	-0.011	-0.012	-0.088
	(0.029)	(0.094)	(0.011)	(0.068)	(0.303)
Post Pre-K Exp. * Hispanic	-0.011	0.139	-0.009	-0.064	0.662
	(0.027)	(0.084)	(0.008)	(0.072)	(0.508)

Notes: Each panel shows results for separate regressions, for the outcome variable indicated in the column heading, and the age group (number of years after pre-K age) indicated in each panel heading. The regressors are the indicator for Post Pre-K, and its interaction with dummies for black and Hispanic. All regressions include state and cohort fixed effects, individual-level control variables for maternal education, gender, and race/ethnicity, age dummies, and state-level control variables. Sample sizes are shown in Panel A of Table 1.4. Robust standard errors (clustered by state) in parentheses. * $p<0.1$, ** $p<0.05$, *** $p<0.01$.

Table A5: Alternative Specifications First Stage Results

	(1) Preschool [Both]	(2) Preschool [Both]	(3) Preschool [Both]	(4) Preschool [Both]
Post Pre-K	0.079^{***}	0.073^{***}	0.067^{**}	0.043^{**}
	(0.023)	(0.020)	(0.025)	(0.021)
State FE	Yes	Yes	Yes	Yes
Cohort FE	Yes	Yes	Yes	Yes
Individual controls	No	Yes	Yes	Yes
State controls	No	Yes	Yes	Yes
State trends	No	No	Yes	No
Observations	8880	8880	8880	15541
F(Post Pre-K)	11.46	13.40	7.06	4.34

Notes: All columns show estimates of the first-stage effect of a pre-K expansion on preschool attendance of 4-year-olds. The sample includes children of both genders of age 4 in the October CPS 1997-2005. The samples used for the first three columns include only Treatment and Control States, while the sample in column (4) includes the Excluded States in the control group. Individual controls include indicator variables for race, maternal education, and indicators for female and Hispanic female. State controls include Head Start enrollment, SCHIP/Medicaid eligibility, and economic conditions. Robust standard errors (clustered by state) in parentheses. * $p<0.1$, ** $p<0.05$, *** $p<0.01$.

B Appendix B: Appendix to Chapter 2

B.1 Additional Tables

Table B1: Effects of Parental Job Loss on Child Health, Business Sold or Closed Only

Father's Job Loss						
	Physical Health				Mental Health	
	Fair/Poor	Acute Index	Chronic Index	Trauma Index	Fair/Poor	Mental Index
Dad Firm Closure	0.007	0.036	-0.011	-0.068**	0.001	0.058*
	(0.008)	(0.029)	(0.029)	(0.027)	(0.005)	(0.031)
Naive p-value	0.368	0.211	0.701	0.012	0.905	0.064
Adj. p-value	0.749	0.590	0.911	0.068	0.914	0.243
Mother's Job Loss (Mother Employed Sample)						
	Physical Health				Mental Health	
	Fair/Poor	Acute Index	Chronic Index	Trauma Index	Fair/Poor	Mental Index
Mom Firm Closure	-0.001	-0.009	-0.003	0.025	0.000	-0.032
	(0.004)	(0.027)	(0.029)	(0.037)	(0.005)	(0.025)
Naive p-value	0.749	0.728	0.925	0.491	0.973	0.211
Adj. p-value	0.996	0.996	0.996	0.963	0.996	0.723

Notes: Data are from the 1996-2012 waves of the Medical Expenditure Panel Survey (MEPS). The sample includes children who were 1-16 years old and whose father (top panel) or mother (bottom panel) was employed with at least one year of job tenure in the first round. Construction of health indices is described in Appendix B. All regressions include individual fixed effects, dummies for age, calendar year of interview, month, and survey round, a control for the length of the round in days, and linear time trends specific to the health status reported in the first round. Standard errors (in parentheses) are clustered at the household level (* $p < .10$, ** $p < .05$, and *** $p < .01$). Adjusted p-values reflect familywise error control as discussed in Section 5 and Appendix C. Estimates are weighted using MEPS sampling weights.

Table B2: Effects of Parental Job Loss on Child Health, Two-Earner Families Only

	Physical Health				Mental Health	
	Fair/Poor	Acute Index	Chronic Index	Trauma Index	Fair/Poor	Mental Index
Post dad's displacement	0.003	0.013	0.015	-0.006	0.008	0.110**
	(0.005)	(0.021)	(0.025)	(0.023)	(0.006)	(0.044)
Post mom's displacement	-0.002	-0.038	-0.025	0.015	0.003	-0.025
	(0.003)	(0.024)	(0.022)	(0.025)	(0.003)	(0.062)
Individuals	9613	9613	9613	9613	9613	9613
DadDisp	593	593	593	593	593	593
MomDisp	534	534	534	534	534	534
BothDisp	82	82	82	82	82	82

Notes: Data are from the 1996-2012 waves of the Medical Expenditure Panel Survey (MEPS). The sample includes children who were 1-16 years old and whose father and mother were both employed with at least one year of job tenure in the first round. Construction of health indices is described in Appendix B. All regressions include individual fixed effects, dummies for age, calendar year of interview, month, and survey round, a control for the length of the round in days, and linear time trends specific to the health status reported in the first round. Standard errors (in parentheses) are clustered at the household level (* $p < .10$, ** $p < .05$, and *** $p < .01$). Adjusted p-values reflect familywise error control as discussed in Section 5 and Appendix C. Estimates are weighted using MEPS sampling weights.

Table B3: Effects of Parental Job Loss on Components of Health Indices

Father's Job Loss								
	Acute Index					Chronic Index		
	Acute Resp	Otitis	Flu/Pneum	Intestinal	Other Infect	Chronic Resp	Asthma/COPD	Nutrit/Metab
Father Displaced	-0.012	-0.002	-0.008	0.011	0.011	0.006	0.001	0.004
	(0.012)	(0.007)	(0.006)	(0.010)	(0.008)	(0.008)	(0.005)	(0.004)
	Trauma Index					Mental Health Index		
	Fract/Disloc	Sprains	Wounds	Burns/Pois	Other	Fair/Poor	Depr/Anx	Headache
Father Displaced	-0.005	-0.000	-0.003	-0.001	-0.002	0.006*	0.007**	0.007**
	(0.004)	(0.005)	(0.004)	(0.002)	(0.003)	(0.004)	(0.003)	(0.003)

Mother's Job Loss (Mother Employed Sample)								
	Acute Index					Chronic Index		
	Acute Resp	Otitis	Flu/Pneum	Intestinal	Other Infect	Chronic Resp	Asthma/COPD	Nutrit/Metab
Mother Displaced	0.008	-0.014**	0.004	-0.022*	-0.019**	-0.006	0.002	0.001
	(0.012)	(0.006)	(0.005)	(0.011)	(0.009)	(0.008)	(0.006)	(0.002)
	Trauma Index					Mental Health Index		
	Fract/Disloc	Sprains	Wounds	Burns/Pois	Other	Fair/Poor	Depr/Anx	Headache
Mother Displaced	-0.001	-0.003	-0.001	-0.001	0.003	0.002	-0.006	-0.003
	(0.004)	(0.005)	(0.003)	(0.002)	(0.004)	(0.004)	(0.005)	(0.005)

Notes: Data are from the 1996-2012 waves of the Medical Expenditure Panel Survey (MEPS). The sample includes children who were 1-16 years old and whose father (top panel) or mother (bottom panel) was employed with at least one year of job tenure in the first round. Health condition definitions are outlined in Appendix B. All regressions include individual fixed effects, dummies for age, calendar year of interview, month, and survey round, a control for the length of the round in days, and linear time trends specific to the health status reported in the first round. Standard errors (in parentheses) are clustered at the household level (* $p < .10$, ** $p < .05$, and *** $p < .01$). Estimates are weighted using MEPS sampling weights.

B.2 Description of Health Variables

This appendix provides additional details on the health outcome variables used in our analysis and describes the construction of the summary health indices.

Perceived Health

Our measures of a child's general physical and mental health are based on responses to questions about perceived health and mental health status that are part of the MEPS full-year consolidated data files. In these questions, the respondent (typically the child's mother) is asked to rate the health and mental health of each person in the family according to the following categories: excellent, very good, good, fair, and poor. Subjective categorical ratings of health like these are common in survey data, and have previously been found to be correlated with the incidence of specific health conditions among children (Case, Lubotsky, and Paxson, 2002). In our setting, these variables are useful because they are available for every child in the sample and are likely to pick up changes in health status that may not result in observed medical conditions, either because they are associated with health conditions that are not included in our set of outcome variables or because they are not associated with a specific medical event, and thus do not show up in the MEPS conditions file.

Our choice to focus on an indicator for fair or poor health is consistent with previous studies (Case, Lubotsky, and Paxson, 2002; Schaller and Stevens, 2015) and is motivated by a desire to identify changes in health at the lower end of the health distribution that are potentially costly to families, both financially and in welfare terms. Descriptive analysis of our data (available from the authors upon request) supports this choice. In particular, the differences in the average frequency of diagnostic health visits and prescription drug use, as well as the incidence of several specific health conditions, are larger between children reported to be in "good" health and children reported to be in "fair" health than those between any of the other adjacent categories.

One potential issue with the use of parent-reported health ratings as a proxy for child health is that these reports are subjective and may be influenced by the state-of-mind of the respondent. In fact, several studies have found that responses to these types of questions by mothers in particular are correlated with the respondent's own health and mental health. For example, Waters et al. (2000) find that a mother's self-reported health is strongly associated with her reporting of her child's health, and Pastor and Reuben (2011) find that the relationship between mother-reported child health status and objectively-measured child health conditions is weaker among children whose mothers were in worse health. Focusing on mental health, Davis et al. (2008) find that maternal depression is negatively associated with maternal reports of child health. Interestingly, these same relationships do not hold when fathers are reporting. Given these findings, we acknowledge the possibility that declines in our parent-reported health measures may be due to the direct effects of job displacement on maternal health status and rely also on health measures that are likely to be more objectively measured.

Health Conditions

Our measures of specific health conditions come from the MEPS Medical Conditions data files. Medical conditions are reported by respondents when there is an event related to this condition, such as a doctor visit, hospital stay, disability day, or prescription drug purchase. Conditions are reported verbatim by the interviewer and then coded by professional coders to ICD-9-CM codes. These codes were then mapped to clinically meaningful categories using the Clinical Classification System (CCS) software and also collapsed to 3-digit ICD-9-CM conditions codes, so that each record in the MEPS file has two different codes associated with it. We use the clinical classification codes to define most of our condition variables, using the ICD-9-CM codes only to identify some smaller subgroups of conditions (for example, to isolate certain mental health conditions) and to help determine how to assign the clinical classification codes to even broader groups.

Table B1 provides the classification codes associated with each health condition that we consider. We focus on health conditions that are prevalent among children and are likely to vary in the short-run in response to economic shocks. Though the health conditions are arguably more objective than parent ratings of child health, there is an important source of potential bias in these measures as well. In particular, because a medical condition is identified in the data when a health event related to the condition occurs, changes in the observed incidence of certain health conditions may be related to changes in the consumption of health care. Though we are reassured by the relatively minimal changes in insurance status and routine health care utilization, we interpret our results with this caveat in mind.

Summary Health Indices

Because we have a large number of outcomes, we follow Deming (2009), Hoynes, Schanzenbach and Almond (2016), and Katz et al. (2007) in constructing standardized summary indices that aggregate information from multiple outcome variables. Using the health conditions described above, we create four summary health indices. The first includes acute (infectious) conditions. Collectively these make up the most common diagnosis category among children in our sample by far, with over 20 percent of children experiencing an acute upper respiratory condition in round one and 13 percent of children experiencing an intestinal infection. The second index combines chronic respiratory and nutritional/metabolic conditions, including asthma, COPD, diabetes, and anemia. The third includes trauma-related conditions such as injuries, burns, and poisoning. The final index combines mental health conditions including depression, anxiety, and acute responses to stress, with headaches, malaise and fatigue (physical symptoms that, when they present independently, are often associated with emotional distress), and the mental health fair/poor indicator. We exclude developmental disorders and other mental health conditions that are unlikely to respond to contemporaneous shocks from the mental health index. The health conditions that contribute to each summary index are outlined in Table B1. To create the indices we standardize each variable by subtracting the round 1 (pre-displacement) mean for the treated group, divide by the standard deviation, and then take the simple average across the standardized variables.

Health Insurance and Health Care Utilization

Health insurance information is available at the monthly level in the MEPS full-year consolidated files for each individual in the survey. We construct variables indicating coverage (any, private, or public) at any time during the month of the interview. Health care utilization variables are from the Hospital Inpatient Stays, Emergency Room Visits, Outpatient Visits, and Office-Based Medical Provider Visits files. Each observation in each of these files represents a single visit or hospital stay. We use responses to questions identifying the reason for each visit or stay to categorize each visit or stay as a checkup, diagnostic visit, emergency visit, or mental health visit. We additionally use the Prescribed Medicines files to create an indicator for the use of any prescription drug during the round.

Table B4: Health Conditions - Sample Means and Classification Codes

Condition	Round 1 Sample Mean	Classification Codes
Acute Index		
Acute Respiratory	0.202	125, 126
Otitis	0.071	92
Flu/Pneumonia	0.044	122, 123
Intestinal	0.130	135, 140, 141, 154, 155, 250, 251
Other Infectious	0.075	3, 4, 7, 8, 90, 246
Chronic Index		
Chronic Respiratory	0.081	133, 134
Asthma/COPD	0.057	127, 128
Nutritional and Metabolic	0.010	48-59
Trauma Index		
Fractures and Dislocations	0.013	225-231
Sprains, Strains, and Superficial	0.020	232, 239
Open Wounds	0.011	235, 236
Burns and Poisoning	0.003	240, 242, 243
Other Injuries	0.013	233, 234, 240
Mental Health		
Depression/Anxiety	0.015	ICD: 296, 298, 300, 308, 309, 311-313
Headache, Malaise, and Fatigue	0.016	84, 252

Notes: Clinical classification codes are used to categorize all conditions except for depression/anxiety, which is defined by ICD-9 CM codes. Sample means are estimated on the union of the father employed and mother employed samples, with observations weighted using MEPS sample weights.

B.3 Calculation of Adjusted P-Values for Multiple Hypothesis Testing

This appendix describes the procedure we follow for calculating p-values adjusted for multiple hypothesis testing. When there are several measured outcomes, significant coefficients may emerge by chance even if there are no treatment effects. If a single-hypothesis test statistic rejects a true null hypothesis at a significance level α, the probability of rejecting a single null hypothesis out of a number of null hypotheses increases with the number of hypotheses being tested. The most common approach to adjusting p-values for multiple hypothesis testing is to control the family-wise error rate (FWER). Suppose a family of S hypotheses is tested, of which J are true. The FWER is the probability that at least one of the J true hypotheses in the family is rejected. FWER control procedures adjust the test statistic (or p-value) of each test to reduce the probability of rejecting a true hypothesis. A method provides a *strong* control of the FWER when it assumes that all of the S hypotheses are true. The adjusted p-value can be interpreted as the probability that a result as extreme as the observed individual test statistic (or p-value) will appear when there is no causal basis for any effect (Westfall and Young, 1993).

Recent papers in the program evaluation literature have incorporated step-down algorithms to control for the FWER in the context of randomized control trials. Step-down methods order the observed p-values in a group of hypothesis tests from lowest to largest (or test statistics from largest to lowest, where the hypothesis with the lowest p-value or the largest test statistic is the one more likely to be rejected). The first p-value is adjusted for the FWER under the null hypothesis that all S coefficients are zero. If this hypothesis cannot be rejected, none of the other $S - 1$ hypotheses will be. If this hypothesis is rejected, then we take this result as true and continue to the second lowest p-value, comparing it to the minimum p-value under the null hypothesis that the remaining $S - 1$ treatment effects are zero, and the process continues.

Westfall and Young (1993) and Romano and Wolf (2005) have developed step-down algorithms for strong control of the FWER that are less conservative than the traditional

Bonferroni and Holm methods.[15] Westfall and Young's step-down procedure is most adequate for data from a randomized experiment, as their method relies on permutations of the treatment assignment. Some examples of recent applications are Anderson (2008), Finkelstein et al. (2012), Barrow et al. (2014). Kling, Liebman and Katz (2007) use a bootstrap adaptation of Westfall and Young's algorithm.

Romano and Wolf (2005) developed a similar step-down algorithm whose main difference with Westfall and Young (1993) is that they do not require the assumption of subset pivotality, which is not always satisfied. Romano and Wolf instead require a monotonicity condition for theoretical critical values. An advantage of this algorithm is that it can be applied using the bootstrap as well as permutation tests. This is particularly useful when the data do not come from a randomized experiment and one does not want to assume a distribution of the treatment variable under the null hypothesis, as is required in permutation tests. Some examples of recent papers that analyze data from randomized controlled trials use the FWER control algorithm proposed in Romano and Wolf (2005) using permutation tests are Conti, Heckman and Pinto (2015), Heckman et al. (2010a), and Attanasio et al. (2015). In this paper we use the bootstrap construction of the Romano and Wolf (2005) algorithm.

Description of the algorithm

We start by presenting the description of the algorithm we use, which is based on Algorithm 1 and the bootstrap construction of critical values of Section 4.2 in Romano and Wolf (2005). We construct adjusted p-values based on this algorithm.

Denote an individual hypothesis by H_j, and its corresponding test statistic T_j (we use the t-statistic). Suppose we want to test a total of S hypotheses, with an intersection of K hypotheses being denoted by H_K, with $K \subset \{1, ..., S\}$.

The step-down method proceeds as follows:

[15] For a description of the Bonferroni and Holm methods, see Westfall and Young (1993) or Heckman et al. (2010b).

0. After estimating the regression coefficients of interest and their corresponding t-statistics ($\hat{\beta}_1, ..., \hat{\beta}_S$ and $\hat{T}_1, ..., \hat{T}_S$), order the test statistics from largest (most significant) to smallest:

$$T_{r_1} \geq T_{r_2} \geq ... \geq T_{r_S}$$

Perform N bootstrap replications of the S regressions (we use $N = 1000$), each time saving the estimated coefficient of interest for each hypothesis ($\hat{\beta}^{b_n}_{r_1}, ..., \hat{\beta}^{b_n}_{r_S}$). Calculate the simulated test statistics for each individual hypothesis based on the bootstrap:

$$T^{b_n}_{r_j} = \frac{|\hat{\beta}^{b_n}_{r_j} - \hat{\beta}_{r_j}|}{se(\hat{\beta}^{b_n}_{r_j})}$$

1. Let $H_{K_1} = \cap_{j \in K_1} H_j$, where $K_1 = r_1, ..., r_S$, be the intersection of the S hypotheses. Calculate $T^{b_n}_{m_1} = \max_{j \in K_1} T^{b_n}_{r_j}$, and order the N maximum bootstrap statistics from smaller to largest to construct the bootstrap approximation of the distribution of $T_{r_1} = \max_{j \in K_1} T_j$. Calculate s_1, the number of times that $T^{b_n}_{m_1} \geq T_{r_1}$, and compute the adjusted p-value for the hypothesis with the largest test statistic (smaller p-value):

$$p^{adj}_{r_1} = \frac{s_1}{N}$$

2. Let $H_{K_2} = \cap_{j \in K_2} H_j$, where $K_2 = r_2, ..., r_S$, be the intersection of the $S - 1$ hypotheses excluding H_{r_1}. Calculate $T^{b_n}_{m_2} = \max_{j \in K_2} T^{b_n}_{r_j}$, and order the N maximum bootstrap statistics from smaller to largest to construct the bootstrap approximation of the distribution of $T_{r_2} = \max_{j \in K_2} T_j$. Calculate s_2, the number of times that $T^{b_n}_{m_2} \geq T_{r_2}$, and compute the adjusted p-value for the hypothesis with the second largest test statistic (second smaller p-value):

$$p^{adj}_{r_2} = \max\{\frac{s_2}{N}, p_{r_1}\}$$

Note that this last step imposes the monotonicity of the individual hypothesis p-values to the adjusted p-values.

⋮

k. Let $H_{K_k} = \cap_{j \in K_k} H_j$, where $K_k = r_k, ..., r_S$, be the intersection of the $S - (k-1)$ hypotheses excluding $H_{r_1}, ..., H_{r_{k-1}}$. Calculate $T_{m_k}^{b_n} = \max_{j \in K_k} T_{r_j}^{b_n}$, and order the N maximum bootstrap statistics from smaller to largest to construct the bootstrap approximation of the distribution of $T_{r_k} = \max_{j \in K_k} T_j$. Calculate s_k, the number of times that $T_{m_k}^{b_n} \geq T_{r_k}$, and compute the adjusted p-value for the hypothesis with the k_{th} largest test statistic (k_{th} smaller p-value):

$$p_{r_k}^{adj} = \max\{\frac{s_k}{N}, p_{r_{k-1}}\}$$

⋮

S. Let $H_{K_S} = H_S$ be the last hypothesis. Compute $T_{m_S}^{b_n} = T_{r_S}^{b_n}$, and order the N bootstrap statistics from smaller to largest to construct the bootstrap approximation of the distribution of T_{r_S}. Calculate s_S, the number of times that $T_{m_S}^{b_n} \geq T_{r_S}$, and compute the adjusted p-value for the hypothesis with the smallest test statistic (largest p-value):

$$p_{r_S}^{adj} = \max\{\frac{s_S}{N}, p_{r_{S-1}}\}$$

C Appendix C: Additional Tables to Chapter 3

Table C1: Effects of Father's Job Loss

			Health Insurance		
	Any	Private	Public	Med/CHIP	Gap
Father Displaced	-0.055***	-0.109***	0.056***	0.056***	0.068***
	(0.010)	(0.012)	(0.009)	(0.010)	(0.011)
Sim Med/CHIP Generosity	-0.007	-0.020	0.006	0.005	0.004
	(0.014)	(0.020)	(0.009)	(0.010)	(0.016)
Sim UI Repl. Rate	0.387*	0.230	0.340	0.363*	-0.339
	(0.208)	(0.197)	(0.215)	(0.206)	(0.248)
			Health Care Utilization		
	Checkup	Diagnosis	ER	Mental	Dental
Father Displaced	-0.010	0.004	-0.007	0.007**	-0.028**
	(0.011)	(0.012)	(0.006)	(0.003)	(0.014)
Sim Med/CHIP Generosity	0.002	0.019	-0.016	0.014**	-0.041
	(0.023)	(0.037)	(0.012)	(0.007)	(0.041)
Sim UI Repl. Rate	-0.122	-0.114	0.180	0.092	-0.081
	(0.250)	(0.235)	(0.136)	(0.059)	(0.305)
			Health Care Expenditures		
	Total	OOP	Med/CHIP	Private Ins.	Dental
Father Displaced	13.232	-16.990	57.685***	-31.008	-9.187
	(53.558)	(12.512)	(14.596)	(52.683)	(20.745)
Sim Med/CHIP Generosity	-10.873	-114.299	7.596	94.540	-129.422**
	(155.743)	(77.919)	(22.764)	(133.378)	(56.026)
Sim UI Repl. Rate	515.819	-220.434	337.595	574.411	-92.011
	(824.924)	(263.334)	(237.778)	(775.680)	(318.633)
Individuals	24634	24634	24634	24634	24634
Displacements	1969	1969	1969	1969	1969

Note: Data are from the 1996-2012 waves of the Medical Expenditure Panel Survey (MEPS), and includes five rounds of observations in each panel. The sample includes children who were 1-16 years old and whose father was employed with at least one year of job tenure in the first round. In addition to the variables displayed in the left column, all regressions include individual fixed effects, controls for age, number of siblings, year of interview, calendar month of interview, interview round, round length, and state-level controls (unemployment rate, max AFDC/TANF Benefits, welfare reform or waiver, state EITC as fraction of federal, and per capital spending on Food Stamps, SSI, Welfare, and Retirement & Disability). Estimates are weighted using MEPS sampling weights.

Table C2: Effects of Father's Job Loss, by Source of Insurance

	Health Insurance				
	Any	Private	Public	Med/CHIP	Gap
Father Displaced	0.077***	0.070***	0.012	0.020	-0.071***
	(0.028)	(0.024)	(0.020)	(0.019)	(0.023)
Father disp*Employer Ins	-0.192***	-0.262***	0.064**	0.053**	0.204***
	(0.032)	(0.027)	(0.026)	(0.023)	(0.028)
Sim Med/CHIP Generosity	-0.008	-0.022	0.006	0.005	0.005
	(0.014)	(0.020)	(0.009)	(0.010)	(0.016)
Sim UI Repl. Rate	0.380*	0.221	0.342	0.365*	-0.332
	(0.205)	(0.194)	(0.217)	(0.207)	(0.245)
	Health Care Utilization				
	Checkup	Diagnosis	ER	Mental	Dental
Father Displaced	-0.042**	0.044**	0.010	0.001	-0.025
	(0.019)	(0.021)	(0.009)	(0.005)	(0.016)
Father disp*Employer Ins	0.047*	-0.059**	-0.025*	0.009	-0.004
	(0.027)	(0.026)	(0.013)	(0.008)	(0.029)
Sim Med/CHIP Generosity	0.002	0.018	-0.017	0.014**	-0.041
	(0.023)	(0.037)	(0.012)	(0.007)	(0.041)
Sim UI Repl. Rate	-0.121	-0.116	0.179	0.092	-0.081
	(0.250)	(0.235)	(0.136)	(0.059)	(0.305)
	Health Care Expenditures				
	Total	OOP	Med/CHIP	Private Ins.	Dental
Father Displaced	40.617	-3.962	82.688***	-43.747**	9.151
	(39.932)	(7.249)	(28.277)	(20.821)	(17.343)
Father disp*Employer Ins	-39.909	-18.986	-36.437	18.565	-26.724
	(73.983)	(18.212)	(37.655)	(65.457)	(31.074)
Sim Med/CHIP Generosity	-11.105	-114.409	7.385	94.648	-129.577**
	(155.674)	(77.934)	(22.843)	(133.397)	(55.996)
Sim UI Repl. Rate	514.316	-221.149	336.222	575.110	-93.018
	(825.203)	(263.108)	(237.611)	(775.299)	(318.145)
Individuals	24634	24634	24634	24634	24634
Displacements	1969	1969	1969	1969	1969

Note: Data are from the 1996-2012 waves of the Medical Expenditure Panel Survey (MEPS), and includes five rounds of observations in each panel. The sample includes children who were 1-16 years old and whose father was employed with at least one year of job tenure in the first round. In addition to the variables displayed in the left column, all regressions include individual fixed effects, controls for age, number of siblings, year of interview, calendar month of interview, interview round, round length, and state-level controls (unemployment rate, max AFDC/TANF Benefits, welfare reform or waiver, state EITC as fraction of federal, and per capital spending on Food Stamps, SSI, Welfare, and Retirement & Disability). Estimates are weighted using MEPS sampling weights.

Table C3: Effects of *Primary Earner*'s Job Loss

	Health Insurance				
	Any	Private	Public	Med/CHIP	Gap
Parent Displaced	-0.054***	-0.115***	0.063***	0.062***	0.065***
	(0.008)	(0.010)	(0.008)	(0.009)	(0.009)
Sim Med/CHIP Generosity	-0.011	-0.035**	0.020**	0.020**	0.003
	(0.013)	(0.017)	(0.009)	(0.010)	(0.013)
Sim UI Repl. Rate	0.370*	0.225	0.292	0.311*	-0.303
	(0.189)	(0.157)	(0.182)	(0.161)	(0.228)
	Health Care Utilization				
	Checkup	Diagnosis	ER	Mental	Dental
Parent Displaced	-0.012	-0.002	-0.007	0.005*	-0.029**
	(0.010)	(0.012)	(0.006)	(0.003)	(0.011)
Sim Med/CHIP Generosity	-0.004	0.012	-0.014	0.015**	-0.028
	(0.022)	(0.034)	(0.011)	(0.007)	(0.040)
Sim UI Repl. Rate	-0.197	-0.130	0.164	0.107	-0.213
	(0.226)	(0.201)	(0.116)	(0.103)	(0.288)
	Health Care Expenditures				
	Total	OOP	Med/CHIP	Private Ins.	Dental
Parent Displaced	23.412	-14.643	60.173***	-16.694	-12.343
	(47.517)	(9.768)	(16.860)	(41.372)	(16.478)
Sim Med/CHIP Generosity	-69.808	-99.881	0.648	28.505	-123.084**
	(143.993)	(67.191)	(22.740)	(121.581)	(48.291)
Sim UI Repl. Rate	-40.359	-192.651	21.756	345.116	-178.974
	(757.208)	(239.363)	(192.614)	(665.318)	(280.878)
Individuals	30745	30745	30745	30745	30745
Displacements	2596	2596	2596	2596	2596

Note: Data are from the 1996-2012 waves of the Medical Expenditure Panel Survey (MEPS), and includes five rounds of observations in each panel. The sample includes children who were 1-16 years old and whose father or single mother was employed with at least one year of job tenure in the first round. In addition to the variables displayed in the left column, all regressions include individual fixed effects, controls for age, number of siblings, year of interview, calendar month of interview, interview round, round length, and state-level controls (unemployment rate, max AFDC/TANF Benefits, welfare reform or waiver, state EITC as fraction of federal, and per capital spending on Food Stamps, SSI, Welfare, and Retirement & Disability). Estimates are weighted using MEPS sampling weights.

Table C4: Effects of *Primary Earner*'s Job Loss Interacted with Medicaid/CHIP Generosity, by Source of Insurance

	Health Insurance				
	Any	Private	Public	Med/CHIP	Gap
Parent displaced	0.105**	0.022	0.088***	0.067**	-0.091**
	(0.050)	(0.049)	(0.029)	(0.029)	(0.040)
Parent disp*Employer Ins	-0.256***	-0.213***	-0.048	-0.032	0.233***
	(0.055)	(0.063)	(0.040)	(0.038)	(0.055)
Parent disp*Sim Med/CHIP Gen	-0.080	0.040	-0.118**	-0.071	0.055
	(0.076)	(0.082)	(0.056)	(0.053)	(0.066)
Parent disp*Employer Ins*Sim Med/CHIP Gen	0.120	-0.092	0.213***	0.166**	-0.046
	(0.090)	(0.112)	(0.077)	(0.071)	(0.096)
	Health Care Utilization				
	Checkup	Diagnosis	ER	Mental	Dental
Parent displaced	-0.034	0.048	0.051**	0.022	-0.105**
	(0.028)	(0.043)	(0.022)	(0.015)	(0.044)
Parent disp*Employer Ins	0.045	-0.017	-0.054*	-0.024	0.034
	(0.042)	(0.060)	(0.028)	(0.018)	(0.065)
Parent disp*Sim Med/CHIP Gen	-0.024	-0.058	-0.084**	-0.050	0.154*
	(0.066)	(0.085)	(0.037)	(0.030)	(0.077)
Parent disp*Employer Ins*Sim Med/CHIP Gen	0.022	-0.033	0.060	0.077*	-0.076
	(0.084)	(0.104)	(0.049)	(0.039)	(0.114)
	Health Care Expenditures				
	Total	OOP	Med/CHIP	Private Ins.	Dental
Parent displaced	81.875	7.194	68.727	28.249	-49.549
	(91.437)	(21.762)	(58.711)	(55.020)	(41.788)
Parent disp*Employer Ins	13.451	92.312	-66.586	-57.772	97.796
	(205.182)	(56.910)	(65.566)	(200.216)	(75.523)
Parent disp*Sim Med/CHIP Gen	-91.604	-29.344	4.940	-74.935	71.558
	(170.463)	(34.740)	(135.243)	(95.759)	(66.336)
Parent disp*Employer Ins*Sim Med/CHIP Gen	-62.727	-203.558**	95.135	93.532	-191.834
	(363.766)	(100.968)	(160.700)	(350.309)	(135.240)
Individuals	30745	30745	30745	30745	30745
Displacements	2596	2596	2596	2596	2596

Note: Data are from the 1996-2012 waves of the Medical Expenditure Panel Survey (MEPS), and includes five rounds of observations in each panel. The sample includes children who were 1-16 years old and whose father or single mother was employed with at least one year of job tenure in the first round. In addition to the variables displayed in the left column, all regressions include individual fixed effects, controls for age, number of siblings, year of interview, calendar month of interview, interview round, round length, and state-level controls (simulated Medicaid/CHIP generosity, simulated UI generosity, unemployment rate, max AFDC/TANF Benefits, welfare reform or waiver, state EITC as fraction of federal, and per capital spending on Food Stamps, SSI, Welfare, and Retirement & Disability). Estimates are weighted using MEPS sampling weights.

Table C5: Effects of *Primary Earner*'s Job Loss Interacted with UI Generosity

	Health Insurance				
	Any	Private	Public	Med/CHIP	Gap
Parent displaced	0.113	-0.068	0.186***	0.220***	-0.110
	(0.069)	(0.104)	(0.055)	(0.062)	(0.066)
Parent displaced*Sim UI Repl. Rate	-0.412**	-0.118	-0.303**	-0.389**	0.429**
	(0.175)	(0.260)	(0.135)	(0.150)	(0.167)
Sim UI Repl. Rate	0.384**	0.229	0.302	0.324**	-0.317
	(0.187)	(0.154)	(0.182)	(0.160)	(0.227)
	Health Care Utilization				
	Checkup	Diagnosis	ER	Mental	Dental
Parent displaced	-0.058	0.134*	-0.044	0.008	-0.001
	(0.071)	(0.069)	(0.054)	(0.020)	(0.079)
Parent displaced*Sim UI Repl. Rate	0.112	-0.336*	0.092	-0.008	-0.068
	(0.182)	(0.172)	(0.129)	(0.049)	(0.190)
Sim UI Repl. Rate	-0.201	-0.120	0.161	0.107	-0.211
	(0.226)	(0.203)	(0.117)	(0.103)	(0.289)
	Health Care Expenditures				
	Total	OOP	Med/CHIP	Private Ins.	Dental
Parent displaced	-447.483	79.460	111.554	-561.768*	90.585
	(318.428)	(54.579)	(90.394)	(300.743)	(101.081)
Parent displaced*Sim UI Repl. Rate	1160.474	-231.909*	-126.625	1343.281*	-253.658
	(742.091)	(134.876)	(223.914)	(700.391)	(226.491)
Sim UI Repl. Rate	-78.085	-185.112	25.873	301.447	-170.728
	(762.645)	(241.032)	(191.467)	(669.131)	(281.929)
Individuals	30745	30745	30745	30745	30745
Displacements	2596	2596	2596	2596	2596

Note: Data are from the 1996-2012 waves of the Medical Expenditure Panel Survey (MEPS), and includes five rounds of observations in each panel. The sample includes children who were 1-16 years old and whose father or single mother was employed with at least one year of job tenure in the first round. In addition to the variables displayed in the left column, all regressions include individual fixed effects, controls for age, number of siblings, interview year and calendar month, round fixed effects, round length, and state-level controls (simulated Medicaid/CHIP generosity, unemployment rate, max AFDC/TANF Benefits, welfare reform or waiver, state EITC as fraction of federal, and per capital spending on Food Stamps, SSI, Welfare, and Retirement & Disability). Estimates are weighted using MEPS sampling weights.

References

Aguiar, M., E. Hurst, and L. Karabarbounis. 2013. "Time Use during the Great Recession, *American Economic Review*, 103 (5), 16641696.

Ananat, E.O., A. Gassman-Pines, D.V. Francis, and C.M. Gibson-Davis. 2011. "Children Left Behind: The Effects of Statewide Job Loss on Student Achievement," *NBER Working Paper* No. 17104. National Bureau of Economic Research.

Anderson, P.M., K.F. Butcher, and P.B. Levine. 2003. "Maternal Employment and Overweight Children," *Journal of Health Economics*, 22 (2003), 577-504.

Anderson, M.L. 2008. "Multiple Inference and Gender Differences in the Effects of Early Intervention: A Reevaluation of the Abecedarian, Perry Preschool, and Early Training Projects," *Journal of the American Statistical Association*, 103 (484), 1481-1495.

Baker, M., J. Gruber, and K. Milligan. 2008. "Universal Child Care, Maternal Labor Supply, and Family Well-Being." *Journal of Political Economy*, 116 (4), 709-745.

Baker, M., J. Gruber, and K. Milligan. 2015. "Non-cognitive deficits and young adult outcomes: The long-run impacts of a universal child care program." *NBER Working Paper* No. 21571. National Bureau of Economic Research.

Ball, T. M., J. A. Castro-Rodriguez, K. A. Griffith, C. J. Holberg, F. D. Martinez, and A. L. Wright. 2000. "Siblings, day-care attendance, and the risk of asthma and wheezing during childhood." *New England Journal of Medicine*, 343 (8), 538-543.

Banthin, J. S., and T.M. Selden. 2003. "The ABCs of children's health care: how the Medicaid expansions affected access, burdens, and coverage between 1987 and 1996." *INQUIRY: The Journal of Health Care Organization, Provision, and Financing*, 40 (2), 133-145.

Barrow, L., L. Richburg-Hayes, C.E. Rouse, and T. Brock. 2014. "Paying for Performance: The Education Impacts of a Community College Scholarship Program for Low-Income Adults," *Journal of Labor Economics*, 32 (3), 563-599.

Beijers, R. J. Jansen, M.Riksen-Walraven, and C. de Weerth. 2011. "Nonparental Care and Infant Health: Do the Number of Hours and Number of Concurrent Arrangements Matter?" *Early Human Development*, 87 (1), 9-15.

Berlinski, S., S. Galiani, and P. Gertler. 2009. "The effect of pre-primary education on primary school performance." *Journal of Public Economics*, 93 (1), 219-234.

Berlinski, S., S. Galiani, and M. Manacorda. 2008. "Giving children a better start: Preschool attendance and school-age profiles." *Journal of public Economics*, 92 (5), 1416-1440.

Bitler, M., J. Gelbach, and H. Hoynes. 2005. "Welfare reform and health." *Journal of Human Resources*, 40 (2), 309-334.

Bitler, M. and H. Hoynes. 2016. "The More Things Change, the More They Stay the Same? The Safety Net and Poverty in the Great Recession." *Journal of Labor Economics*, 34 (S1), S403-S444.

Black, S.E., P.J. Devereux and K.G. Salvanes. 2015. "Losing Heart? The Effect of Job Displacement on Health." *ILR Review*, 68 (4), 833-861.

Blau D. and E. Tekin. 2007. "The Determinants and Consequences of Child Care Subsidies for Single Mothers in the USA." *Journal of Population Economics*, 20 (4), 719-741.

Bradley, R.H. 2003. "Child Care and Common Communicable Illnesses in Children Aged 37 to 54 Months," *Archives of Pediatrics and Adolescent Medicine*, 157 (2), 196-200.

Bratberg, E., O.A. Nilsen and K. Vaage. 2008. "Job Losses and Child Outcomes," *Labour Economics*, 15 (2008), 591-603.

Browning, M. and E. Heinesen. 2012. "Effect of job loss due to plant closure on mortality and hospitalization," *Journal of Health Economics*, 31 (2012), 599-616.

Buchmuller, T., J. Ham and L. Shore-Sheppard. 2016. "The Medicaid Program." In *Economics of Means-Tested Transfer Programs in the United States*, 1 (2016), edited by Robert Moffitt, 20-136. National Bureau of Economic Research.

Carneiro, P., and R. Ginja. 2014. "Long-Term Impacts of Compensatory Preschool on Health and Behavior: Evidence from Head Start." *American Economic Journal: Economic Policy*, 6 (4), 135-173.

Cascio, E. U., and D. W. Schanzenbach. 2013. "The impacts of expanding access to high-quality preschool education." *Brookings Papers on Economic Activity*, 2 (2013), 1-54.

Cawley, J. and K.I. Simon. 2005. "Health Insurance Coverage and the Macroeconomy," *Journal of Health Economics*, 24 (2005), 299-315.

Cawley, J., A.S. Moriya, and K. Simon. 2013. "The impact of the macroeconomy on health insurance coverage: Evidence from the great recession," *Health Economics*, 24 (2), 206-223.

Classen T.J. and R.A. Dunn. 2012. "The Effect of Job Loss and Unemployment Duration on Suicide Risk in the United States: a New Look Using Mass-Layoffs and Unemployment Duration," *Health Economics*, 21, 338-350.

CMS*a*. 2017. *National Health Expenditure Fact Sheet*, U.S. Centers for Medicare & Medicaid Services, retrieved on May 7, 2017 from: https://www.cms.gov/.

CMS*b*. 2017. *FY 2016 Children's Enrollment Report*, U.S. Centers for Medicare & Medicaid Services, retrieved on May 7, 2017 from: https://www.medicaid.gov/.

Cohodes, S., D. Grossman, S. Kleiner, and M. Lovenheim. 2016. "The effect of child health insurance access on schooling: Evidence from public insurance expansions." *Journal of Human Resources*, 51 (3), 727-759.

Coelli, M.B. 2011. "Parental Job Loss and the Education Enrollment of Youth," *Labour Economics*, 18 (2011), 25-35.

Conley, D., and N.G. Bennett. 2000. "Is biology destiny? Birth weight and life chances." *American Sociological Review*, 458-467.

Conley, D., and N.G. Bennett. 2001. "Birth weight and income: interactions across generations." *Journal of Health and Social Behavior*, 450-465.

Côté, S. M., A. Petitclerc, M. F. Raynault, Q. Xu, B. Falissard, M. Boivin, and R. E. Tremblay. 2010. "Short-and long-term risk of infections as a function of group child care attendance: an 8-year population-based study." *Archives of Pediatrics & Adolescent Medicine*, 164 (12), 1132-1137.

Cuellar, A. 2015. "Preventing and treating child mental health problems." *The Future of Children*, 25 (1), 111-134.

Cullen, J., and J. Gruber. 2000. "Does Unemployment Insurance Crowd out Spousal Labor Supply?" *Journal of Labor Economics*, 18 (3), 546-572.

Currie, J. 2001. "Early childhood education programs." *The Journal of Economic Perspectives*, 15 (2), 213-238.

Currie, J. 2009. "Healthy, Wealthy, and Wise: Socioeconomic Status, Poor Health in Childhood, and Human Capital Development," *Journal of Economic Literature*, 47 (1), 87-122.

Currie, J., and J. Gruber. 1996 "Health Insurance Eligibility, Utilization of Medical Care, and Child Health." *The Quarterly Journal of Economics*, 111 (2), 431-466.

Currie, J., and M. Stabile. 2006. "Child mental health and human capital accumulation: the case of ADHD." *Journal of Health Economics*, 25 (6), 1094-1118.

Currie, J., and D. Thomas. 1995. "Does Head Start Make a Difference?" *American Economic Review*, 85 (3), 341-364.

Currie, J., and D. Thomas. 1999. "Does Head Start help hispanic children?" *Journal of Public Economics*, 74 (2), 235-262.

Davis, E., B. Davies, E. Waters and N. Priest. 2008. "The relationship between proxy reported health-related quality of life and parental distress: gender differences," *Child:*

Care, Health, and Development, 34 (6), 830-837.

Deb, P., W.T. Gallo, P. Ayyagari, J.M. Fletcher, J.L. Sindelar. 2011. "The Effect of Job Loss on Overweight and Drinking," *Journal of Health Economics*, 30 (2011), 317-327.

Deming, D. 2009. "Early childhood intervention and life-cycle skill development: Evidence from Head Start." *American Economic Journal: Applied Economics*, 1 (3), 111-134.

East, C., and E. Kuka. 2014. "Reexamining the Consumption Smoothing Benefits of Unemployment Insurance: Why Have These Effects Declined?" *Mimeo.*

Eliason, M. and D. Storrie. 2009. "Job loss is bad for your healthSwedish evidence on cause-specific hospitalization following involuntary job loss," *Social Science and Medicine*, 68 (8), 1396-1406.

Engen, E., and J. Gruber. 2001. "Unemployment insurance and precautionary saving." *Journal of Monetary Economics*, 47 (3), 545-579.

Evans, W.N., M.S. Morrill, and S.T. Parente. 2010. "Measuring inappropriate medical diagnosis and treatment in survey data: The case of ADHD among school-age children." *Journal of health economics*, 29 (5), 657-673.

Fairbrother, G.L., A.C. Carle, A. Cassedy and P.W. Newacheck. 2010. "The Impact Of Parental Job Loss On Children's Health Insurance Coverage," *Health Affairs*, 29 (7), 1343-1349.

Farber, H. S. 2011. "Job Loss in the Great Recession: Historical Perspective from the Displaced Worker Survey, 1984-2010." *NBER Working Paper* No. 17040. National Bureau of Economic Research.

Finch, C.E., and E.M. Crimmons. 2004. "Inflammatory Exposure and Historical Changes in Human Life-Spans," *Science*: 305 (5691), 1736-1739.

Finkelstein, A., S. Taubman, B. Wright, M. Bernstein, J. Gruber, J.P. Newhouse, H. Allen, K. Baicker, Oregon Health Study Group. 2012. "The Oregon Health Insurance Experiment: Evidence From the First Year." *The Quarterly Journal of Economics*, 127 (3), 1057-1106.

Fitzpatrick, M.D. 2008. "Starting School at Four: The Effect of Universal Pre-Kindergarten on Childrens Academic Achievement." *The B.E. Journal of Economic Analysis & Policy*, 8 (1).

Fitzpatrick, M.D. 2010. "Preschoolers Enrolled and Mothers at Work? The Effects of Universal Prekindergarten." *Journal of Labor Economics*, 28 (1), 51-85.

Frede, E., K. Jung, S. Barnett, and A. Figueras. 2009. *The APPLES Blossom: Abbott Preschool Program Longitudinal Effects Study (APPLES). Preliminary Results through 2nd Grade.* National Institute for Early Education Research, Graduate School of Education, Rutgers, The State University, June, 2009

Garces, E., D. Thomas,and J. Currie. 2002. "Longer-term effects of Head Start." *The*

American Economic Review', 92 (4), 999-1012.

Gassman-Pines, A., E.O. Ananat, and C.M. Gibson-Davis. 2014. "Effects of Statewide Job Losses on Adolescent and Suicide-Related Behaviors," *American Journal of Public Health* 104 (10), 1964-1970.

Gennetian, L.A., H.D. Hill, A.S. London, and L.M. Lopoo. 2010. "Maternal Employment and the Health of Low-Income Young Children." *Journal of Health Economics*, 29, 353-363.

Gormley Jr., W.T., and T. Gayer. 2005. "Promoting School Readiness in Oklahoma: An Evaluation of Tulsas Pre-K Program." *Journal of Human Resources*, 40 (3), 533-558.

Gormley Jr, W.T., D. Phillips, and T. Gayer (2008): "The early years. Preschool programs can boost school readiness." *Science*, 320 (5884), 1723-1724.

Gross, T. and M. Notowidigdo. 2011. "Health insurance and the consumer bankruptcy decision: Evidence from expansions of Medicaid." *Journal of Public Economics*, 95 (2011), 767778.

Grossman, M. 2000. "The Human Capital Model," in *Handbook of Health Economics*, A.J. Culyer and J.P Newhouse (eds.), Volume 1, Chapter 7, Elsevier Science B. V.

Gruber, J. 1997. "The Consumption Smoothing Benefits of Unemployment Insurance." *The American Economic Review*, 87 (1), 192-205

Gruber, J. and B.C. Madrian. 1997. "Employment Separation and Health Insurance Coverage," *Journal of Public Economics*, 66 (3), 349382.

Gruber, J. and K. Simon. 2008. "Crowd-out 10 years later: Have recent public insurance expansions crowded out private health insurance?" *Journal of Health Economics*, 27 (2008), 201217.

Ham, J., X. Li and L. Shore-Shepard. 2009. "Public Policy and the Dynamics of Children's Health Insurance, 1986-1999." *American Economic Review: Papers & Proceedings*, 99 (2), 522-526.

Hardy, A.M. and M.G. Fowler. 1993. "Child Care Arrangements and Repeated Ear Infections in Young Children," *American Journal of Public Health*, 83 (9), 1321-1325.

Havnes, T., and M. Mogstad. 2011. "No child left behind: Subsidized child care and children's long-run outcomes." *American Economic Journal: Economic Policy*, 3 (2), 97-129.

Herbst, C. and E. Tekin. 2010. "Child Care Subsidies and Child Development." *Economics of Education Review*, 29 (4), 618-638.

Herbst, C. and E. Tekin. 2012. "The Geographic Accessibility of Child Care Subsidies and Evidence on the Impact of Subsidy Receipt on Childhood Obesity." *Journal of Urban Economics*, 71 (1), 37-52.

Hill, C.J., W.T. Gormley, and S. Adelstein. 2015. “Do the short-term effects of a high-quality preschool program persist?” *Early Childhood Research Quarterly*, 32, 60-79.

Hoynes, H. W., D. L. Miller, and D. Simon. 2015. “Income, the earned income tax credit, and infant health.” *American Economic Journal: Economic Policy*, 7 (1), 172-211.

Hoynes, H.W., Schanzenbach, D.W., and D. Almond. 2016. “Long Run Impacts of Childhood Access to the Safety Net,” *American Economic Review*, 106 (4), 903-934.

Hsu, J., D. Matsa, and B. Melzer. 2014. “Unemployment Insurance and Consumer Credit.”

Illi, S., E. von Mutius, S. Lau, R. Bergmann, B. Niggemann, C. Sommerfeld, and U. Wahn. 2001. “Early childhood infectious diseases and the development of asthma up to school age: a birth cohort study.” *BMJ*, 322 (7283), 390-395.

Inoue, A., and G. Solon. 2010. “Two-Sample Instrumental Variables Estimators.” *Review of Economics and Statistics*, 92 (3), 557-561.

Jacobson, L.S., R.J. Lalonde, and D.G. Sullivan. 1993. “Earnings Losses of Displaced Workers,” *American Economic Review*, 83 (4), 685-709.

Kaiser Commission on Medicaid and the Uninsured. 2012. *Performing under pressure: Annual findings of a 50-state survey of eligibility, enrollment, renewal, and cost-sharing policies in Medicaid and CHIP, 2011-2012.* Publication number 8272, available on the Kaiser Family Foundation’s website at www.kff.org.

Kalil, A. and K.M. Ziol-Guest. 2008. “Parental employment circumstances and children’s academic progress,” *Social Science Research*, 37, 500515.

Kline, P. and C. Walters. 2016. “Evaluating Public Programs with Close Substitutes: The Case of Head Start.” *The Quarterly Journal of Economics*, 131 (4), 1795-1848.

Kling, J.R., J.B.Liebman, and L.F. Katz. 2007. “Experimental Analysis of Neighborhood Effects,” *Econometrica*, 75 (1), 83-119.

Kuhn, A., R. Lalive, and J. Zweimller. 2009. ”The public health costs of job loss.” *Journal of Health Economics*, 28 (6), 1099-1115.

Kuka, E. 2015. “Quantifying the Benefits of Social Insurance: Unemployment Insurance and Health.” *Mimeo.*

Leana, C.R., and D.C. Feldman. 1988. “Individual responses to job loss: Perceptions, reactions, and coping behaviors.” *Journal of Management*, 14, 375389.

Lindo, J.M. 2011. “Parental Job Loss and Infant Health,” *Journal of Health Economics*, 30 (2011), 869 879.

Lindo, J.M., J. Schaller, and B. Hansen. 2013. “Caution! Men Not at Work: Gender-Specific Labor Market Conditions and Child Maltreatment.” *NBER Working Paper*, No. 18994, Revised on October 6, 2016.

Lipsey, M. W., D.C. Farran, and K.G. Hofer. 2015. "A Randomized Control Trial of the Effects of a Statewide Voluntary Prekindergarten Program on Childrens Skills and Behaviors through Third Grade" (Research Report). Vanderbilt University, Peabody Research Institute.

Liu, A.H., and J.R. Murphy. 2003. "Hygiene hypothesis: fact or fiction?" *Journal of Allergy and Clinical Immunology*, 111 (3), 471-478.

Liu H. and Z. Zhao. 2014. "Parental Job Loss and Children's Health: Ten Years after the Massive Layoff of the SOEs' Workers in China." *China Economic Review*, 31 (2014), 303-319.

LoSasso, A. and T. Buchmueller. 2004. "The effect of the state children's health insurance program on health insurance coverage." *Journal of Health Economics*, 23 (2004), 10591082

Luca, D.L. 2014. "The Long-Term Effects of Post-Neonatal Childhood Health: Evidence from Mandatory School Vaccination Laws," *Mimeo.*

Ludwig, J., and D.L. Miller. 2007. "Does Head Start Improve Childrens Life Chances? Evidence from a Regression Discontinuity Design." *Quarterly Journal of Economics*, 122 (1), 159-208.

Magnuson, K., M. Meyers, and J. Waldfogel. 2007. "Public Funding and Enrollment in Formal Childcare in the 1990s." *Social Service Review*, 81 (1), 47-83.

Miller, S. 2012. "The Impact of the Massachusetts Health Care Reform on Health Care Use Among Children." *American Economic Review: Papers & Proceedings*, 102 (3), 502507.

Mörk, E., A. Sjögren and H. Svaleryd. 2014. "Parental Unemployment and Child Health", *CESifo Economic Studies*, 60 (2), 366-401.

Morrill, M.S. 2011. "The Effects of Maternal Employment on the Health of School-Age Children." *Journal of Health Economics*, 30 (2011), 240-257.

Nafstad, P., B. Brunekreef, A. Skrondal, and W. Nystad. 2005. "Early Respiratory Infections, Asthma, and Allergy: 10-Year Follow-up of the Oslo Birth Cohort, *Pediatrics*, 116 (2), e255-e262.

NCES. 2015. *The Condition of Education 2015.* U.S. Department of Education, National Center for Education Statistics, NCES 2015-144.

National Governors Association (NGA). 1997. *MCH Update: State Medicaid Coverage of Pregnant Women and Children*, Health Policy Studies Division, NGA Center for Best Practices, September 1997.

National Governors Association (NGA). 2003. *MCH Update 2002: State Health Coverage for Low-Income Pregnant Women, Children, and Parents*, Health Policy Studies Division, NGA Center for Best Practices, June 2003.

NIEER. 2006. *The State of Preschool 2006: State Preschool Yearbook.* National Institute for Early Education Research, Rutgers Graduate School of Education.

NIEER. 2013. *The State of Preschool 2013: State Preschool Yearbook.* National Institute for Early Education Research, Rutgers Graduate School of Education.

NIEER. 2016. *The State of Preschool 2015: State Preschool Yearbook.* National Institute for Early Education Research, Rutgers Graduate School of Education.

Oreopoulos, P., M. Page and A.H. Stevens. 2008. "The Intergenerational Effects of Worker Displacement," *Journal of Labor Economics*, 26 (3), 455-483.

Page, M., A.H. Stevens and J. Lindo. 2009. "Parental Income Shocks and Outcomes of Disadvantaged Youth in the United States," in *The Problems of Disadvantaged Youth: An Economic Perspective*, J. Gruber (ed.), Conference held April 13-14, 2007, published in October 2009, University of Chicago Press.

Pastor, P.N., and C.A. Reuben. 2011. "Maternal Reports of Child Health Status and Health Conditions: The Influence of Self-Reported Maternal Health Status," *Academic Pediatrics*, 11 (4), 311-317.

Rege, M., K. Telle and M. Votruba. 2011. "Parental Job Loss and Children's School Performance," *Review of Economic Studies*, 78 (2011), 14621489.

Romano, J.P. and Wolf, M. 2005. "Exact and approximate stepdown methods for multiple hypothesis testing." *Journal of the American Statistical Association*, 100 (469), 94-108.

Ruhm, C.J. 2000. "Parental Leave and Child Health," *Journal of Health Economics*, 19 (6), 931-960.

Ruhm, C.J. 2008. "Maternal employment and adolescent development," *Labour Economics*, 15 (5), 958-983.

Schaller, J. and A.H. Stevens. 2015. "Short-run Effects of Job Loss on Health Conditions, Health Insurance, and Health Care Utilization." *Journal of Health Economics*, 43 (2015), 190-203.

Schaller, J. and M. Zerpa. 2016. "Short-run effects of parental job loss on child health." *Mimeo.*

Snyder, T.D., and S.A. Dillow. 2010. *Digest of Education Statistics 2009* (NCES 2010-013). National Center for Education Statistics, Institute of Education Sciences, U.S. Department of Education. Washington, DC.

Solon, G., S.J. Haider, and J. Wooldridge. 2015. "What are we Weighting For?" *Journal of Human Resources,* 50 (2), 301-316.

Stevens, A.H. and J. Schaller. 2011. "Short-run effects of parental job loss on children's academic achievement," *Economics of Education Review*, 30 (2011), 289299.

Sullivan, D. and T. von Wachter. 2009. "Job Displacement and Mortality: an Analysis Using Administrative Data," *Quarterly Journal of Economics*, 124 (3), 1265-1306.

US Department of Education. 2015. *Preschool Development Grants Brochure Years 1 and 2*. U.S. Department of Education, Office of Early Learning. Retrieved from http://www2.ed.gov/programs/preschooldevelopmentgrants/index.html.

US Department of Health and Human Services (DHHS). 1997. *State Welfare Waivers: An Overview*. US Department of Health and Human Services, Office of the Assistant Secretary for Planning and Evaluation. Retrieved from https://aspe.hhs.gov/legacy-page/state-welfare-waivers-overview-152151

US Department of Health and Human Services (DHHS). 2010. *Head Start impact study: Final report.* US Department of Health and Human Services. Washington, DC.

US Department of Health and Human Services (DHHS). 2015. *Head Start Program Facts. Fiscal Year 2015.* US Department of Health and Human Services, Administration for Children and Families, Office of Head Start. Retrieved from https://eclkc.ohs.acf.hhs.gov/hslc/data/factsheets/2015-hs-program-factsheet.html.

Waters, E., J. Doyle, R. Wolfe, M. Wright, M. Wake, and L. Salmon. 2000. "Influence of Parental Gender and Self-Reported Health and Illness on Parent-Reported Child Health," *Pediatrics*, 106 (6).

Waters, L.E., and K.A. Moore. 2002. "Predicting self-esteem during unemployment: The effect of gender, financial deprivation, alternate roles, and social support, *Journal of Employment Counseling*, 39 (4): 171-189.

White House. 2016. *Supporting Children and Youth*. Retrieved from https://www.whitehouse.gov/sites/default/files/omb/budget/fy2017/assets/fact_sheets/Supporting%20Children%20and%20Youth.pdf

Wong, V. C., T.D. Cook, W.S. Barnett, and K. Jung. 2008. "An effectiveness-based evaluation of five state pre-kindergarten programs." *Journal of policy Analysis and management*, 27 (1), 122-154.