THE UNIVERSITY OF CHICAGO

THE IMPACT OF INCOME SUPPORT POLICIES ON CHILD DEVELOPMENT: EVIDENCE FROM SUPPLEMENTAL NUTRITION ASSISTANCE PROGRAM AND EARNED INCOME TAX CREDIT

A DISSERTATION SUBMITTED TO

THE FACULTY OF THE CROWN FAMILY SCHOOL

OF SOCIAL WORK, POLICY, AND PRACTICE

IN CANDIDACY FOR THE DEGREE OF

DOCTOR OF PHILOSOPHY

BY

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CHICAGO, ILLINOIS

AUGUST 2024

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ACKNOWLEDGEMENTS

My academic journey and this dissertation would not have been possible without the guidance and support from many people. I would first like to thank my advisor, Julia Henly, who has provided me with invaluable mentoring and showed me relentless support not only to this dissertation project but to any challenges I was going through in my life. She is a great exemplar of a brilliant, hardworking, and passionate scholar and has taught me how to think critically and thoughtfully as an academic. I feel very fortunate to have a mentor like her. I am deeply grateful to Marci Ybarra, who I have been working with since my very first year in the master's program at Crown. She has been a wonderful and a kind mentor to me, providing intellectual guidance not only in my dissertation work but also in other projects. I also want to thank my other dissertation committee members. Jane Waldfogel has given me invaluable feedback in this work. Conversations with her have pushed me to think further on this project, conceptually and methodologically, which have significantly enhanced the quality of my dissertation. I have also learned a lot from Manasi Deshpande. She has taught me how to think more deeply and creatively as a scholar and was always willing to read over my draft and provide feedback. I cannot emphasize enough how much I am grateful for the time and energy Aaron Gottlieb has put into reviewing my work. He has always been there to talk through any questions or concerns I have had, and his guidance was instrumental in conducting this project. I am also thankful to numerous others, both at Crown and at other schools, who have generously supported my work by responding to my emails, providing suggestions on methodologies, and sharing datasets with me.

I am greatly indebted to my family and friends. I would not have been able to complete my doctoral degree without their prayers, support, and love. I am grateful for my parents, Jong

Ho Hong and Seong Yeon Park, who, as my role models, have shown me how to passionately and diligently strive for the betterment of our society. I have no doubt that my value in social justice and my passion for social policy research have been significantly impacted by the life and faith they have shown me. I am thankful to my younger sister, Young Suh, who has been my comfort zone throughout this time. I give special thanks to my husband, Jitak Ahn, who has supported me in countless ways and has always believed in me. I could never have finished my doctoral journey without his love and support. I look forward to the next chapter of our life. I am grateful for my friends: my life in Chicago would not have been fulfilling without all the laughter and joy I shared with them. And most importantly, I dedicate this work to my Lord and Savior, Jesus Christ. I give glory to God who has guided my path, has given me countless blessings in Chicago, and has filled me with unfailing love and strength.

This dissertation was supported by the Horowitz Foundation for Social Policy Award, the Grand Challenges for Social Work Doctoral Award, and the Crown Family Dissertation Research Grant.

ABSTRACT

Childhood poverty, particularly in early childhood and early school years, can take a toll on the well-being of children, who are the future workers and leaders of our society. The overarching question that this dissertation attempts to answer is whether and how income matters for child development. I examine the role of two U.S. income support programs, Supplemental Nutrition Assistance Program (SNAP) and Earned Income Tax Credit (EITC), in mitigating the negative effects of poverty on child development across various dimensions in preschool to kindergarten-entry. In Chapter 2, I provide a conceptual framework that explicates the mechanisms through which income may affect child development. In doing so, I consider parental investment, family stress, and biological pathways. In Chapter 3, I provide empirical evidence on the effects of SNAP benefit generosity on children's cognitive, socioemotional, and health outcomes. Partly due to lack of program rule variations in SNAP, there has been limited research on how SNAP benefits affect child well-being, particularly among young children. I draw on a novel approach to addressing the endogeneity of SNAP benefit amount, which employs the variation in the purchasing power or real value of SNAP benefits across regions and over time. Considering different timings of exposure to SNAP purchasing power in a child fixed-effects model, I find plausibly causal evidence that greater SNAP purchasing power has positive effects on cognitive and socioemotional development. In Chapter 4, I consider a combination of two income support programs, EITC and SNAP. Despite the growing evidence of high joint participation rates in these programs, until now no research has examined their potential interaction effects on child development. Using variations in state EITC policies and SNAP purchasing power across regions and over time within a child fixed-effects framework, this study provides new evidence that EITC and SNAP produce complementary effects on the cognitive and socioemotional development of children. Specifically, the effects of EITC on these outcomes increase as the level of SNAP benefits increases, indicating that the EITC is more effective at reducing the developmental gaps across socioeconomic status when it is coupled with larger SNAP benefits. This dissertation makes contributions to developmental science by showing the plausibly causal effects of income on cognitive and socioemotional development in a critical developmental stage. The dissertation also contributes to advancing social work and public policy knowledge, by demonstrating that two of the largest income support programs that impact millions of families with children in the U.S. could reduce the detrimental effects of poverty on young children's development.

CHAPTER I: INTRODUCTION

In the United States, 14.4 percent of children under the age of 18 lived in poverty in 2019, which is one-and-a-half times higher than the poverty rate among adults aged 18-64 (9.4 percent). This equates to 10.5 million children living in poverty (Semega, Kollar, Shrider, & Creamer, 2020). The prevalence of childhood poverty is concerning, considering its negative impact on children, particularly during early childhood and early school years (Duncan et al., 1998; Smith, Brooks-Gunn, & Klebanov, 1997). Research shows significant income-based disparities in children's development, such as math, literacy, and language (Waldfogel, & Washbrook, 2011), socioemotional skills (e.g., approaches to learning and externalizing behavior) (Fletcher, & Wolfe, 2016), and health (Pearce et al., 2019). These developmental disparities tend to widen throughout childhood and often translate into larger gaps in adult outcomes, such as earnings (Cunha, Heckman, Lochner, & Masterov, 2005; Duncan, Ziol-Guest, & Kalil, 2010).

While early childhood and early school years represent the most sensitive periods to the negative consequences of poverty, they also offer a window of opportunity for children to thrive despite enduring economic hardship. Recognizing the importance of this developmental stage, the U.S. government invests in human capital through early care and education programs (e.g., Early Head Start and Head Start), specifically targeting young children among socio-economically disadvantaged families. Furthermore, there are other types of social safety net programs made available to socioeconomically disadvantaged families and children, including childcare subsidies and means-tested income support programs. The childcare subsidy program serves the dual goals of improving families' economic well-being – by providing work support – and promoting children's development. Income support programs are primarily aimed at alleviating poverty and economic hardships. While income supports take different forms, in the United States they are typically structured as work contingent in-kind benefits and cash transfers (Pilkauskas, 2023). For instance, the Supplemental Nutrition Assistance Program (SNAP) provides near-cash transfers for the purchase of food, conditional on certain work requirements. Exemptions to work requirements are granted for SNAP recipients in cases such as pregnancy and caregiving responsibilities for a young child under age six and for senior citizens. Another income support program is the Earned Income Tax Credit (EITC), which is a work contingent lump sum cash transfer through the tax system. Other income support policies that provide cash to eligible families include the Child Tax Credit (CTC), Supplemental Security Income (SSI), Social Security Disability Insurance (SSDI), and Temporary Assistance for Needy Families (TANF). TANF is contingent on fulfilling work requirements, SSDI is made available to workers with a history of paid employment, and CTC has been mostly available to families with earnings (with the exception of the 2021 expansion), whereas SSI does not require employment as a condition of benefit receipt.

Increasingly, research shows that income support policies not only alleviate poverty but also have longrun benefits for children (Hoynes, & Schanzenbach, 2018). For instance, studies find that exposure to SNAP during pregnancy – specifically in the third trimester – improves birth outcomes (Almond et al., 2011; East, 2020), and participating in SNAP in preschool to kindergarten-entry period has been associated with improved school-readiness skills (Hong, & Henly, 2020). Also, research shows that prenatal to early childhood exposure to SNAP positively influences childhood health (East, 2020) and health and economic outcomes in adulthood (Hoynes et al., 2016). In addition, research finds positive effects of exposure to EITC on birth outcomes (Strully et al., 2010; Hoynes et al., 2015; Markowitz et al., 2017), test scores (Dahl, &

Lochner, 2012; Chetty et al., 2011; Maxfield, 2015), education attainment (Maxfield, 2015; Bastian, & Michelmore, 2018), and employment outcomes (Bastian, & Michelmore, 2018). All of these studies provide important evidence on how income support programs can shape children's short run and long run well-being.

However, the literature is still limited. There is lack of quasi-experimental evidence on the impact of income support programs on child development across various domains, including cognitive, socioemotional, and health outcomes, in early childhood to early school years. Furthermore, while there has been an increasing trend of multiple program participation (Jackson, & Fanelli, 2023), most previous research focused on a single program's effects on child well-being. Recognizing the investment component of income support programs as well as the existing research gaps, this dissertation examines the effect of means-tested income support policies on the development of young children in socioeconomically disadvantaged families. I focus on two programs, SNAP and EITC, which are two of the largest income support programs in the U.S.[1]

Overview of Dissertation

The overarching question that this dissertation attempts to answer is whether and how income matters for child development. In Chapter 2, I provide a conceptual framework that considers three pathways through which income may affect children's development: biological, parental investment, and family stress. This framework describes how through these mechanisms

[1] In terms of government spending, SNAP and EITC are the first two largest cash or near cash means-tested programs. Although Medicaid has even larger government spending than SNAP and EITC, it is much more illiquid than those two programs and thus is not treated as near cash.

poverty in early life may lay a weaker foundation for children's cognitive, socioemotional, and health outcomes.

Then, through two standalone empirical papers, this dissertation presents empirical evidence regarding the developmental benefits of SNAP alone and in combination with the EITC among families with low socioeconomic status. In both papers, I use the Early Childhood Longitudinal Study-Birth Cohort data and quasi-experimental methods to identify the plausibly causal effect of income support programs on preschool to kindergarten-entry aged children's developmental wellbeing, including cognitive, socioemotional, and health outcomes.

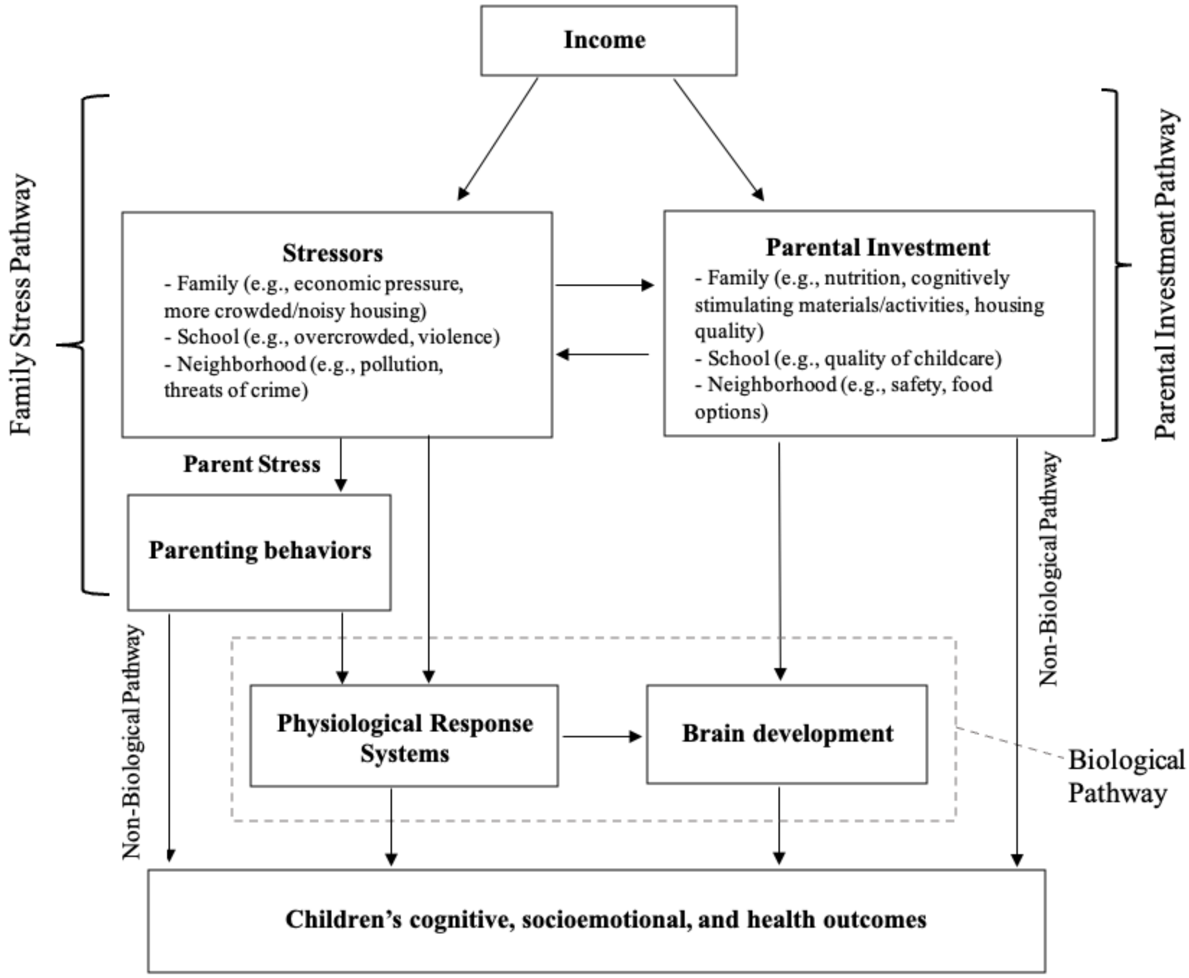
In the first empirical paper, I focus on SNAP benefits and examine their plausibly causal effect on child development. There is limited quasi-experimental research on the benefits of SNAP generosity on child-wellbeing, especially in early childhood to school-entry period. This is partly because there is lack of variation in SNAP program rules across regions and over time, which is typically used to estimate the causal effect of programs. I use a novel approach that takes advantage of variations in the purchasing power (i.e., real value) of SNAP benefits across regions and over time, a method that has not been used in most previous research. This study provides new evidence that an increase in SNAP purchasing power has a statistically significant, strong positive effect on cognitive and socioemotional development. Such effects were only significant when there was a longer time lag between the SNAP purchasing power exposure and the assessment date of child outcomes (i.e., statistically insignificant effects with a shorter time lag), suggesting that the benefits of SNAP may not immediately impact child development. I also provide some suggestive evidence on the mechanisms through which the effect of SNAP purchasing power may occur. This study contributes to the gap in the SNAP literature by showing the non-health benefits of SNAP benefit generosity, which has been an understudied area in the literature.

In the second empirical paper, I investigate the plausibly causal interaction effects between EITC and SNAP benefits on child development. I identify their interaction effects, leveraging the fact that children are exposed to different levels of maximum state and federal EITC benefits and SNAP purchasing power over time based on place and time, as well as on the number of children in the household in the case of EITC. Despite the high joint participation rate in the EITC and SNAP, to my knowledge, no study has yet examined whether EITC and SNAP benefit generosity interact to affect child well-being among families who likely participate in both programs. This paper provides the first known evidence on the complementary relationships between EITC and SNAP, finding that the marginal effect of maximum EITC benefits on cognitive and socioemotional development increases as the level of SNAP purchasing power increases. I also provide suggestive evidence on the mechanisms through which such complementary effects may occur. By demonstrating that the EITC is more effective at improving child development when supported by greater real value of SNAP benefits, this study contributes to a small, but emerging literature that examines the complementarity between multiple public investments in children.

CHAPTER II: OVERARCHING CONCEPTUAL FRAMEWORK

The conceptual framework explicated in Figure 2.1 motivates the overarching question guiding this dissertation – whether and how income and poverty matter for child development. It shows the hypothesized mechanisms through which income may influence children's cognitive, socioemotional, and health outcomes, focusing on parental investment, family (or parent) stress, and biological changes at the child level. In both empirical papers of this dissertation, I examine the role of parental investment and family stress mechanisms, but do not test the biological pathways due to data limitations. Thus, this conceptual framework takes a more comprehensive approach than the empirical work that I undertake by considering how biological pathways (specifically stress response systems and brain development) may explain the mediating role of parental investment and family stress mechanisms in the effect of income on child development. In the following subsections, I first explain the parental investment theory and the family stress theory. Then, I describe the role of biological mechanisms.

Figure 2.1. Conceptual framework of the income effect on child development: Inclusion of parental investment pathway, family stress pathway, and biological pathway.



Parental Investment Theory

As indicated in the "Parental Investment Pathway" in Figure 2.1, the parental investment perspective maintains that children from lower income families lag behind their more affluent counterparts in part because they have limited access to financial and time resources that parents can use to invest in their development (Duncan et al., 2014; Yeung et al., 2002). The parental investment model is based on the economist Gary Becker's household production theory (Becker, 1991), which asserts that children's success is produced from a combination of endowments (e.g., genetic predispositions, parental preferences such as how much they value education and the future) and parental investments in the form of money and time. The level of parental investment can shape children's living environments at multiple levels. For instance, at the family level, evidence has linked low income to insufficient nutrition and less access to cognitively stimulating materials (e.g., books, age-appropriate toys) and activities for children (e.g., reading with the child, visiting a library/museum, receiving lessons to enhance their skills) (Bradley, & Corwyn, 2002; Evans, 2004). In addition, low-income families are less able to afford high quality childcare and schools and to live in safe and healthy neighborhoods where they have access to necessary resources, such as full-service supermarkets with large selections of healthy foods (Evans, 2004; Cohen et al., 2010; Duncan et al., 2014).

Empirical studies support the mediating role of money and time investment in children. In particular, numerous studies have found that cognitively stimulating materials and engagement in stimulating activities strongly mediate the relationship between family income and cognitive skills such as reading and math scores (Gershoff et al., 2007; Yeung et al., 2002; Mistry et al., 2008). Research also supports that parental investment mediates the income effect on socioemotional development, although evidence is weaker compared to cognitive development. For example, Bradley and colleagues (2001) document that access to stimulating materials and activities mediates the association between socioeconomic status and children's behavioral problems. In addition, Ashiabi and O'Neal (2007) found that food insufficiency and inadequate medical care access mediate the effect of poverty on child health status.

Family Stress Theory

The family stress perspective, which stems from the "family stress model" (Conger et al., 1992; Conger, & Elder, 1994), holds that family income affects child development through its effect on parental stress and family functions, e.g., parenting behaviors, as indicated in "Family

Stress Pathway" in Figure 2.1. In contrast to the parental investment perspective, the family stress perspective focuses on the psychological consequences of poverty. As poor families struggle to buy necessary goods and services and to pay bills on time, they are faced with the pressure to reduce their daily expenditures. This economic pressure is often coupled with other stressful life circumstances, thereby creating greater psychological distress for low-income parents (Duncan et al., 2014). The stress may manifest as depression or marital conflict, and this can result in less responsive and supportive parenting practices, which can have detrimental effects on parent-child interactions and children's development (Conger, & Elder, 1994; Cohen et al., 2010; Evans, 2004; Mistry et al., 2008; Masarik, & Conger, 2017).

Empirical studies have shown that parental psychological distress mediates the relationship between economic hardship and harsh, unresponsive parenting behavior and that such parenting behaviors are the key mechanism by which poverty affects children's development, particularly socioemotional outcomes (McLoyd, 1990; Conger et al., 2002; Yeung et al., 2002; Yoshikawa et al., 2012; Mistry et al., 2008). For example, Yeung and colleagues (2002) demonstrate that maternal emotional distress is an important mediator in the income effect on externalizing behavior problems. Studies also lend support to the role of family stress pathways for other child outcomes, although to a lesser extent. For instance, Ashiabi and O'Neal (2007) found that parental depression and parenting behaviors partially mediate the effect of poverty on child health, while Blair and colleagues (2011) showed that negative parenting behaviors are the mediator in the relationship between income and executive functions, which play a critical role in the development of academic skills such as reading and math (Cartwright, 2012; Mazzocco, & Kover, 2007).

Furthermore, this family stress model has been extended to consider psychosocial and physical stressors in the broader environments, recognizing that stress can be multidimensional (indicated in the box named as “Stressors” in Figure 2.1). At the housing level, low-income families are more likely to live in housing that is crowded (i.e., insufficient space) and noisy, has poorer indoor air quality, and contains harmful toxins such as lead-based paint (Bradley, & Corwyn, 2002; Cohen et al., 2010; Duncan et al., 2014). Stressors may also exist at the neighborhood level, as low-income families are more likely to live in neighborhoods with high threats of crime (Evans, 2004; Cohen et al., 2010) and violence (Levy et al., 2016; Cohen et al., 2010) and with excessive noise, poor air/water quality, and hazardous wastes (Evans, 2004; Cohen et al., 2010). In addition, the schools that low-income children attend are more likely to be overcrowded (Duncan et al., 2014), have physical violence (Evans, 2004; Cohen et al., 2010), and lack coherent educational environment due to irregular attendance and less consistent enrollment of students (Cohen et al., 2010). Research has found that exposures to these environmental stressors also serve as mediators in the relationship between income and child development (Evans, & English, 2002; Evans et al., 2005).

Biological Mechanisms

The above two perspectives suggest that income may influence child development by altering the level of parental investment in the child, parental stress level, as well as physical and psychosocial stressors. The biological pathway – indicated as “Biological Pathway” in Figure 2.1 – provides complementary insights into these relationships by explaining how parental financial and time investments as well as stressors associated with poverty may influence child development.

Brain Development

One possible biological mechanism includes brain development. Studies have found a significant relationship between poverty and brain development, and the potential for the mediating role of brain development in the association between poverty and child development in various domains. For example, Noble and colleagues (2015) show that greater family income is associated with larger total cortical surface area, which has been associated with higher intelligence. They also found that the cortical surface area partially mediates the association between family income and executive functions, the cognitive ability central to early academic achievement. Another study found that childhood socioeconomic status is associated with differences in the volumes of the hippocampus and the amygdala, the parts in brain that support language, memory, and socio-emotional processing (Noble et al., 2012). Furthermore, by conducting randomized experiments, Troller-Renfree and colleagues (2022) demonstrate that income positively affects brain activity. In their study, compared to infants who were randomly assigned to receive a small monthly unconditional cash transfer, infants who were randomly assigned to receive a large cash transfer showed the patterns of neural activity that have been correlated with higher language, cognitive, and socio-emotional scores in previous research (Benasich et al., 2008; Brito et al., 2019).

There can be numerous ways in which poverty affects brain development. As one possibility, the parental investment theory suggests that children in low-income families can have fewer opportunities than those in higher income families to be exposed to cognitively stimulating learning environments. Evidence well documents that there exist disparities by income in the amount of cognitive stimulation that a child receives in and outside of their home, such as quantity and quality of language-stimulation that a child is exposed to (Nobel et al., 2012; Hart, & Risley, 1995; Adams, 1990). In addition, research shows that such differences in cognitive stimulation could result in differences in brain development. For example, studies have found that exposures to language stimulation are associated with the development of cortical regions in the left hemisphere that support language skills (Conboy, & Kuhl, 2007; Kuhl, Tsao, & Liu, 2003). Therefore, the above evidence suggests that poverty may induce reduced opportunity for stimulation and learning, which may be related to the development of the brain in regions that support specific skills that have been stimulated.

In addition to the effect of cognitively stimulating environments on brain development, the parental investment theory also suggests inadequate child nutrition as another factor that may engender changes in the brain. When parents do not have enough resources to purchase healthy, nutritious foods for their children, children may not be able to consume adequate nutrition. Inadequate caloric energy or nutrients can alter neural development (e.g., neuronal cell proliferation and differentiation, axonal and dendritic growth, synaptogenesis, and myelination) as well as key neurotransmitters that underlie cognitive processing and mood (Jensen et al., 2017).

Stress Response System

Another possible biological mechanism involves stress response systems. According to the family stress theory, children in low-income families may confront a wide array of stressors, including those from family, school, and neighborhood. When these stressors become chronic, they can be particularly harmful. Chronic stress in the early years of life is detrimental because it can lead to biological impairments and in turn have long-term consequences on cognitive skills, behavior, and physical and mental health of children. When our body is under stress, physiological stress response systems are altered to reestablish homeostasis, i.e., the state of stability where vital physiological systems (essential for survival) are maintained. This activation of stress response is called "allostasis," which is the process of maintaining homeostasis through active change (McEwen, 1998; McEwen, 2000). The commonly studied physiologic stress response systems are hypothalamic-pituitary-adrenocortical (HPA) axis and the sympathetic-adrenomedullary system, which, once activated, secrete stress hormones, such as cortisol and norepinephrine. Although normal and transient stress response is protective and necessary for survival, it becomes a problem when stress response is activated for a prolonged duration and at an excessively high level (McEwen, 1998). Under chronic activation of stressors associated with poverty, therefore, children can experience chronic overactivity or inactivity of physiological response systems. As indicated in Figure 2.1, stressors may directly lead to dysregulation of children's stress response systems, particularly if they are physical or psychosocial stressors, or stressors may indirectly affect their dysregulation through parental stress and parenting behaviors.

Several empirical studies support the link between poverty, stress exposure, and dysregulation of children's stress response systems. For instance, Evans and English (2002) found that poverty is associated with higher levels of physiological stress markers – including cortisol, epinephrine, systolic, diastolic, and resting blood pressure – among children in grades three to five. Furthermore, they demonstrate that cumulative stress exposure (e.g., family turmoil, community violence, substandard housing, noise, crowding) partially mediates the relationship between poverty and physiological stress markers. Blair and colleagues (2011) found that beginning at as early as 7 months of age, children in poverty have higher levels of salivary cortisol (indicative of elevated stress response) compared to their generally better off counterparts.

Importantly, such dysregulation of physiologic response systems can result in the wear and tear effect (referred to as "allostatic load") on the brain and body (McEwen, 1998). As indicated in the arrow that goes from physiologic response systems to brain development in Figure 2.1 ("Physiological Response Systems → Brain Development"), it has been theoretically argued and empirically shown that poverty related chronic stressors can tune physiologic stress response in ways that alter brain development and function (Blair, & Raver, 2016). Empirical studies have found that poverty or its associated stress may influence the brain in regions that underlie the development of executive functions and self-regulation of behavior, thereby resulting in reduced gray matter volumes in the frontal and parietal regions (Hanson, et al., 2013), loss of neurons and neural connections in the hippocampus and medial prefrontal cortex (PFC), reduced volume of hippocampus and PFC, and hyperactivation of amygdala (Evans, & Kim, 2013; Shonkoff et al., 2012; Nobel et al., 2012). Overall, brain development and physiologic stress response suggest the two potential biological pathways by which poverty in early life may lay a weaker foundation for cognitive skills, learning, behavior, and psychological and physical health, thereby altering lifetime developmental trajectories in cognitive, socioemotional, and health outcomes.

Furthermore, in addition to physiologic stress response systems, chronic stressors may also influence physical health through their influences on other response systems. For example, chronic stressors can alter immune functions and increase inflammatory markers, which are known to have long-term consequences on health, such as cardiovascular disease (Araújo et al., 2009; Galkina et al., 2009; Ward et al., 2009). But in this conceptual framework, I do not go in-depth to explain the biological mechanisms involving other response systems.

I also recognize that the biological mechanism is not the only pathway through which parental investments and family stress may influence child development. There can be non-biological pathways – for instance, the child-level psychological and emotional pathway through which elevated parents' stress level influences child development by increasing the child's psychological and emotional stress. There could also be a child behavioral pathway. For example, limited opportunities for learning at home, in childcare, and at school, due to lower parental investments, may decrease the child's engagement in learning, which may have negative consequences on child development (Jensen et al., 2017). These additional pathways are indicated as "Non-Biological Pathway" in Figure 2.1.

Taken together, the connections between poverty, parental investment, and stress, and their potential impacts on biological impairments underscore the significance of intervention in addressing poverty early in children's lives. Through my empirical papers in Chapter 3 and Chapter 4, I provide strong evidence that income support policies in the U.S. social safety net system can play a crucial role in reducing the negative consequences of poverty on development among children in preschool to kindergarten-entry period.

CHAPTER III: THE EFFECT OF PURCHASING POWER OF SUPPLEMENTAL NUTRITION ASSISTANCE PROGRAM ON CHILD DEVELOPMENT OUTCOMES

Introduction

The Supplemental Nutrition Assistance Program (SNAP) is the largest cash or near cash means-tested transfer program and the largest federal food assistance program in the United States. By providing monthly food vouchers to families, SNAP offsets food costs and frees up a family's economic resources, which can be spent on purchasing other needs. Thus, SNAP offers economic and nutritional benefits to millions of low-income working and non-working individuals, while children make up the largest share of SNAP beneficiaries (United States Department of Agriculture, 2022). SNAP has also played a significant role in reducing poverty; in 2015, the program lifted 8.4 million people from poverty, as measured by the Supplemental Poverty Measure, decreasing the poverty rate from 15.4 percent to 12.8 percent (Wheaton, & Tran, 2018).

Despite such evidence on the anti-poverty effects of SNAP and that children are the largest group of SNAP recipients, there remains limited quasi-experimental research to date on the role of SNAP benefits in promoting children's well-being across various developmental domains. Although a few research has found that SNAP participation is beneficial for reading and math test scores among school-age children (Frongillo, Jyoti, & Jones, 2006; Gassman-Pines, & Bellows, 2018), there is only one study that focused on its effects on cognitive development among younger children in early childhood or early school years (Hong, & Henly, 2020). Hong and Henly find that SNAP participation is positively associated with early math and attention skills. Furthermore, to my knowledge, there has been no research that examined how SNAP influences children's behavioral outcomes. There is a larger body of SNAP literature regarding health outcomes, such as children's general health (Bronchetti, Christensen, & Hoynes, 2019; Miller, & Morrissey, 2021) and birth weight (Almond, Hoynes, & Schanzenbach, 2011; East, 2020; Currie, & Moretti, 2008). However, these findings are somewhat mixed and there is still a lack of evidence regarding the impact of SNAP on child health during early childhood or early school years.

Taken together, there are a few important gaps in the SNAP literature. First, few of the quasi-experimental studies examining the effect of SNAP have focused on young children. Considering that prior literature has reached consensus that early childhood and early school years are critical developmental stages that are most sensitive to family income’s effects on child development (Duncan et al., 1998; Smith, Brooks-Gunn, & Klebanov, 1997), further research is necessary to examine the effect of SNAP benefits during early childhood or early school years. Second, existing evidence is largely limited to health-related outcomes, and little is known regarding the potential benefits of SNAP on other domains of child development – such as cognitive and socioemotional development – which are known to be critical for later academic achievement and long-term success (Carneiro, & Heckman, 2003; Duncan et al., 2007; Feinstein, & Duckworth, 2006). The current study contributes to these research gaps by investigating the plausibly causal short-run effect of SNAP benefit generosity on cognitive, socioemotional, and health development among preschool to kindergarten-entry aged children.

One reason the developmental benefits of SNAP are understudied is likely because of a lack of variation in program rules and requirements, which are typically used to conduct causal analyses. SNAP has historically shown relatively little variation in eligibility rules and benefit amounts across geographic locations and over time, compared to other means-tested programs such as the Earned Income Tax Credit (EITC), leaving fewer opportunities for quasi-experimental research. Existing research examining the plausibly causal effect of SNAP on family- or child-wellbeing outcomes has taken a few different approaches. One set of studies has leveraged variation in state-level application and recertification policies after Welfare Reform in 1996 to instrument for SNAP participation (e.g., Miller, & Morrissey, 2021; Shaefer, & Gutierrez, 2013; Yen et al., 2008; Meyerhoefer, & Pylypchuk, 2008). Another set of studies used variation in the adoption timing of the Food Stamps program (a former name of SNAP) in the 1960s to 1970s to examine the effect of Food Stamps exposure in prenatal period or early childhood (Almond et al., 2011; Hoynes et al., 2016). The change in documented immigrants' eligibility as part of Welfare Reform in 1996 was also used as a source of variation in SNAP participation (East, 2020). A few other studies have assessed the effects of SNAP participation with individual fixed effects models, leveraging longitudinal data on a family's SNAP participation and using changes in SNAP participation status within a family (Hong, & Henly, 2020; Frongillo, Jyoti, & Jones, 2006).

A smaller set of studies have leveraged variation in benefit levels. A few studies have utilized 2009 American Recovery and Reinvestment Act (ARRA) benefit enhancements, which was a temporary SNAP benefit expansion at the federal level (Morrissey, & Miller, 2020; Nord, & Prell, 2011). One study by Collins et al. (2015) examined the provision of USDA Summer Electronic Benefits Transfer for Children, a program that provides additional SNAP benefits in summer to randomly selected families with children who are eligible for free or reduced-price school meals.

In this paper, I use a relatively new approach to address the endogeneity of SNAP benefit levels. I draw on a novel measure of SNAP purchasing power that was first used by Bronchetti, Christensen, and Hoynes (2019) and use a child fixed effects approach with the nationally representative longitudinal survey of a 2001 birth cohort, the Early Childhood Longitudinal Study-Birth Cohort (ECLS-B) data. Although the SNAP benefit formula is fixed in contiguous U.S. (48 states and D.C.), there are substantial differences in local costs of living such as food prices, which has resulted in large variations in the purchasing power, or real value, of SNAP benefits across place and over time. I consider different timings of exposure to SNAP purchasing power, given that the effect of SNAP may manifest over time (even in a short term) rather than immediately after receipt.

Leveraging within child variation in exposure to different levels of SNAP purchasing power over time from 2003 to 2007, this study finds large effects of SNAP purchasing power. A 10 percent increase in SNAP purchasing power improved cognitive development (early reading and math skills) by 0.23 – 0.25 standard deviation (SD) and socioemotional development (approaches to learning and externalizing behavior) by 0.43 – 0.47 SD, but not general child health. Subgroup analyses show similar effects on cognitive development for males and females. Furthermore, results from the test of mechanisms suggest that the positive effect of SNAP purchasing power on cognitive and socioemotional development may be partly driven by a reduction in maternal depression and increases in daily reading time with the child. Importantly, a series of robustness and falsification tests provides strong support for the causal interpretation of the study's findings. I confirm that the estimated effects are not present in placebo samples (children with college educated married mothers). I also do not find statistically significant effects of SNAP purchasing power on household income variables and my results are robust to controlling for a host of other regional prices, suggesting that my findings reflect the impact of variation in SNAP generosity rather than that of variation in local food prices or that of living in a different labor market.

Background

Supplemental Nutrition Assistance Program: Summary of the program

SNAP was first piloted in 1961 and became a nationwide program by 1975. As the U.S. social safety net shifted its focus from out-of-work assistance to work-contingent programs after the 1996 Welfare Reform (Hoynes, & Schanzenbach, 2018), government spending on unrestricted traditional cash assistance was reduced substantially, while in-kind assistance such as SNAP, refundable tax credits linked to paid work (e.g., EITC), and public health insurance (e.g., Medicaid) have significantly expanded. In particular, SNAP has become a crucial component of the safety net. During the 2000s, it experienced significant growth in caseloads and demonstrated its importance as a countercyclical program during the Great Recession (Hardy, Smeeding, & Ziliak, 2018). Furthermore, states were granted substantial flexibility in administering SNAP. This allowed states to adopt various application, recertification, and eligibility policies, including simplified reporting, longer recertification intervals, and Broad-Based Categorical Eligibility (BBCE), which contributed to reducing administrative burdens of SNAP access and increasing SNAP caseloads in the 2000s (Ganong, & Liebman, 2018).

SNAP is currently the largest means-tested income support program in the U.S., excluding Medicaid. In Fiscal Year (FY) 2020, about 20.5 million households (39.9 million individuals) participated in SNAP in an average month and 65 percent of those individuals lived in a household with children. The average monthly SNAP benefit across all participating households was $230 in the pre-pandemic period of FY 2020 and $302 in both the pre- and post-pandemic period of FY 2020. Eligibility for SNAP is typically determined by three tests, although there are exceptions to some types of households (e.g., households in which a member is disabled or is aged 60 or above) and people who are categorically eligible for SNAP: (1) gross income test, which requires households to have monthly gross income below 130 percent of the federal poverty line (FPL); (2) net income test, which requires gross income minus deductions (e.g., deductions related to dependent care, earned income, child support, medical and excess shelter expenses) to be below 100 percent of FPL; (3) and asset test that requires households' assets to be below a certain value. For households who are certified to be eligible, their SNAP benefits are calculated by subtracting 30 percent of a household's net income from the maximum benefit amount to which they are entitled. The maximum benefit is fixed across states and varies only by household size in the contiguous U.S. (United States Department of Agriculture, 2022).

Conceptual Framework

SNAP is considered an income support, similar to other cash-based means-tested programs, such as EITC. Although SNAP is an in-kind food benefit, empirical research supports that SNAP is treated as near-cash. For example, Hoynes and Schanzenbach (2009) and Bruich (2014) conclude from their study that households respond similarly to one dollar in cash income and one dollar in SNAP, although a few studies report that a marginal propensity to consume food out of SNAP benefit income is higher than it is out of cash income (Beatty, & Tuttle, 2015; Hastings, & Shapiro, 2018). From the viewpoint that SNAP increases economic resources among low-income families, the current study grounds the relationship between SNAP benefit generosity and children's development within the parental investment perspective and the family stress perspective, which have emerged as the two main frameworks in the literature on income effects on child development (Duncan, Magnuson, & Votruba-Drzal, 2014). (See Chapter 2. for descriptions of these frameworks).

Drawing on these two perspectives, the current study expects three primary mechanisms through which SNAP benefits can influence children's cognitive, socioemotional, and health development. Based on the parental investment perspective, the effect of SNAP may go through the nutrition pathway and the learning investment pathway. The nutrition pathway would occur if greater SNAP benefits increase the quality or quantity of food and allow children to consume better nutrition. The learning investment pathway may be at play if SNAP benefits increase parental educational inputs into their children, such as by creating stimulating home environments (e.g., reading to the child). Lastly, based on the family stress perspective, the stress pathway could be at play if SNAP benefits alleviate parental stress and depressive symptoms, thereby improving parent-child interactions and child development. I test these mechanisms and present the findings in a later section.

Prior Research on SNAP and Child Development

Despite such potential links between SNAP and children's non-health and health outcomes, there is a very small literature on the effect of SNAP on cognitive and socioemotional development. A few studies demonstrate positive effects of SNAP on school-aged children's cognitive and socioemotional outcomes. Frongillo, Jyoti, and Jones (2006), using an individual fixed effects model, found that SNAP participation has positive relationships with reading and math scores among kindergarten to third-grade children, but only among female students. They did not find a statistically significant relationship between SNAP participation and social skills. In addition, Gassman-Pines and Bellows (2018) studied how the recency of the SNAP benefit transfers affects school-aged children's end-of-grade math and reading test scores. They found a roughly curvilinear relationship; Test scores consistently increased until the third week following the benefit transfer, after which they started to decrease.

The effects of SNAP benefits are not limited to the short-term. Hoynes, Schanzenbach, and Almond (2016) provide evidence that access to SNAP between conception and age 5 positively affects adult economic outcomes. Using a county-level roll-out in the introduction of SNAP program, they found that access to SNAP leads to a significant increase in economic self-sufficiency, but only for women.

Thus, prior research, although there are only a few, supports a positive role of SNAP in non-health outcomes[1], with some evidence of larger effects among females. However, evidence is limited in that there is a lack of research on the effect of SNAP on cognitive and socioemotional development among children in early childhood to school-entry years, during which children may show most sensitivity to families' socioeconomic conditions. To my knowledge, there is only one study by Hong and Henly (2020) that investigated the non-health developmental benefits of SNAP among children in these periods. Using the same dataset as the current study, they found that receiving SNAP (compared to not receiving it) is positively associated with early math skills and one construct of socioemotional development – i.e., approaches to learning (attention) skills – among children who are likely eligible for SNAP. Hong and Henly also found greater effects of SNAP receipt on early reading, early math, and attention skills among poorer children compared to those who are slightly better off but are still low-income. The current study builds upon Hong and Henly (2020) by focusing on the effect of SNAP benefit generosity rather than SNAP participation, examining more diverse constructs of socioemotional development including externalizing behavior and interpersonal skills, and assessing the possible mechanisms through which SNAP benefit generosity may affect child development. I also analyze whether the effect of SNAP benefits is statistically significantly different between male and female children, based on the evidence from Frongillo and

[1] See Miller and Morrissey (2021) for one exception on emotional problems.

colleagues (2006) and Hoynes and colleagues (2016) that SNAP effects seemed to be larger among female students and women, respectively.

Compared to non-health outcomes, there is a larger body of literature on the effect of SNAP on children's health. However, previous research finds somewhat mixed results and there is a paucity of research that examined the health benefits of SNAP in early childhood to school-entry years. A few studies examined the effect of SNAP exposure during prenatal period on birth weight, which is an early health marker associated with outcomes in a later period, such as math and reading test scores (Boardman et al., 2002) and adult health outcomes (Allin et al., 2004; Madzwamuse et al., 2015). For instance, using a roll-out in the introduction of SNAP program, Almond, Hoynes, and Schanzenbach (2011) showed that access to the SNAP program in the third trimester (i.e., 3 months prior to birth) has positive effects on birth weight, and similarly, East (2020) also found positive effects of mother's SNAP eligibility in the third trimester on birth weight. However, using a similar analytical approach, Currie and Moretti (2008) found negative or mixed effects of having access to SNAP in an earlier period during pregnancy, i.e., 9 months prior to birth, on birth weight in California. Looking at adult health, Hoynes, Schanzenbach, and Almond (2016) found that access to SNAP between conception and age 5 leads to a significant reduction in the incidence of metabolic syndrome in adulthood.

In addition to infant and adult health, several studies have examined the effect of SNAP on children's general health status, specifically the probability of excellent or very good health (vs. good, fair, or poor) reported by their parent. For example, East (2020) focused on parental eligibility status when a child was in early childhood (before age 5) and found that an additional year of parental SNAP eligibility during this period increases the probability of excellent or very good health at age 6-16. In contrast, Bronchetti et al. (2019) and Miller and Morrissey (2021) found statistically insignificant effects of SNAP purchasing power and SNAP participation, respectively, on the same outcome, although they both found positive effects on health care utilization. One of the differences between the two studies and East (2020) is that they did not focus on the effect of receiving or generosity of SNAP in early childhood. Their analysis included children spanning wider age ranges, which may be one potential explanation for their diverging results. In the current study, I examine the health benefits of SNAP purchasing power among children in their preschool to kindergarten-entry age, adding further evidence to the literature on the role of SNAP in child health among young children.

Current Study

The study contributes to an understudied area in the SNAP literature, by examining the plausibly causal effect of SNAP benefit generosity (measured as purchasing power of SNAP benefits) on cognitive (early reading and early math), socioemotional (approaches to learning, interpersonal skills, externalizing behavior), and health outcomes (probability of excellent or very good health) among preschool to kindergarten-entry aged children. Based on the three primary mechanisms as explained above, I hypothesize that greater SNAP purchasing power will lead to improvements in these developmental domains. I first analyze the effect of SNAP purchasing power on developmental outcomes and whether there are differential effects by female and male children (only on cognitive outcomes; see the Results section for details). Then, I test the potential mechanisms of the effect of SNAP purchasing power on child development.

Data and Measures

Early Childhood Longitudinal Study – Birth Cohort data

The ECLS-B follows a nationally representative cohort of over 10,000 children born in 2001 from birth through kindergarten entry. 10,700 children were surveyed in the first wave of data collection (9 months old, interviewed in 2001-2022), and then they were followed through wave 2 (2 years of age, interviewed in 2003–2004), wave 3 (the preschool year, i.e., 4 years of age, interviewed in 2005–2006), wave 4 (kindergarten-entry age, interviewed in 2006–2007), and wave 5 (kindergarten-entry age, interviewed in 2007–2008). In wave 5, specific groups of children were only invited to take the survey, which included children who did not enter kindergarten in wave 4, children who were repeating kindergarten in this wave, and twins of these children (Snow et al., 2009). ECLS-B is particularly suitable for this study since it contains a rich set of family and child characteristics, regional information such as state of residence and county of birth, as well as cognitive and socioemotional development outcomes that were directly assessed by the home interviewer and were reported by teachers or childcare providers. Although the ECLS-B does not contain the most recent birth cohort, there are no other nationally representative survey datasets that provide these similar advantages.[2] As recommended by the ECLS-B (Najarian, Snow, Lennon, & Kinsey, 2010; Snow et al., 2009), the current study combines wave 4 and wave 5 to create a kindergarten-entry wave ("wave k"), which is nationally representative of the 2001 birth cohort children at their kindergarten entry period. The primary

[2] For instance, Fragile Families and Child Wellbeing Study follows children born in 1998 to 2000, until they reach age 22, in U.S. 20 cities (15 states). Given that my empirical strategy exploits the variation in SNAP purchasing power across place (market group that consists of a set of counties), having only 15 states will limit the amount of variations. Also, Panel Study of Income Dynamics-Child Development Supplement 2014 (PSID-CDS 2014) is a nationally representative dataset that conducts an in-home assessment to collect information on reading and math achievements; however, the in-home assessment was conducted only among a random 50 percent of households (Institute for Social Research University of Michigan, 2017). Thus, compared to the ECLS-B, PSID-CDS 2014 has a smaller sample of children who have information on child development outcomes (N=1498) and the sample size becomes even smaller when it is restricted to a certain group of children (e.g., whose parents have some college or less). This will substantially limit the precision of estimates. National Health Interview Survey (NHIS) and Early Childhood Longitudinal Study-Kindergarten cohort (ECLS-K) cannot be used as well, since NHIS only provides health-related outcomes and ECLS-K does not collect information on state of birth (or residence).

analysis sample for this study comprised children from wave 3 and wave k, as most of the outcomes of interest were measured only in those waves.

To limit the study sample to families who have likely received SNAP, I focus on children with unmarried mothers (age 19 or above) without a college degree. This is a high intent-to-treat sample and Table 3.1 demonstrates that this sample consists of socioeconomically disadvantaged families. Numerous studies on social safety nets have also used similar sample restriction criteria (Bronchetti et al., 2019; Hoynes et al., 2015; Schmidt et al., 2023; Hardy et al., 2018). An alternative sample I consider is children in families who *report* having received SNAP in wave 3 and wave k, but no other cash-based benefits that may have some purchasing power variation like SNAP (e.g., Temporary Assistance for Needy Families (TANF), SSI, and SSDI)[3]. I exclude families who participated in other cash-based programs to better isolate the effect of SNAP generosity. After making this exclusion, the alternative sample represents 43 percent of households who reported that they received SNAP in both waves.[4] Although children in this sample are likely most affected by SNAP, there are a few downsides of using this sample. Considering that program participation status is self-reported, this may cause bias in my estimates to the extent that it suffers from endogenous underreporting issues (Bitler, 2020). In addition, families may non-randomly select into SNAP, which can question the causal interpretation of the study's estimates. Sample sizes are also reduced significantly in this alternative sample, which may lead to imprecise and less reliable estimates. Due to these

[3] Sine TANF benefits are set at the state level, SSDI is based on earnings (which will vary across place), and SSI has a state supplement, these programs likely do not have the same level of purchasing power variation as SNAP.

[4] Considering that SSI recipients and TANF recipients, in most circumstances, are categorically eligible for SNAP, it is not surprising that families who report having participated in SNAP but not in other cash benefits represent only 43 percent of the total SNAP families.

concerns, I use unmarried mothers who do not have a college degree as my primary analysis sample, but I use this alternative sample to check the sensitivity of my results.

I restrict my primary sample in a few ways (N=3000). First, in the ECLS-B, the county of residence is not collected during interviews, although information on the county of birth and state of residence is available. Since I need to merge the ECLS-B data with other county-level datasets, including SNAP purchasing power data, I drop children whose family moved to different states between the focal child's birth and wave k (N=300). This is to minimize measurement error of the focal child's county of residence during wave 3 and wave k.[5] To account for the possibility that moving may be endogenous to a family's exposure to SNAP purchasing power, I perform a robustness check by analyzing the main model among families who did not move to different states between birth and wave 2 (which is before the study period starts). According to Table A.7 in Appendix A, for most outcomes, results generated from this less restrictive sample with full controls were qualitatively similar to what I found in the main results. Second, I limit my sample to children with birth county information and living in contiguous U.S. states (thereby excluding Alaska and Hawaii) (N=2550), since the SNAP purchasing power measure cannot be estimated in Alaska and Hawaii using existing data (see the SNAP Purchasing Power Measure section for details). Lastly, I restrict to children who have non-missing information on covariates (N=2450) and key dependent variables, resulting in a study sample of 2100-2150 observations (1050-1100 children) for cognitive outcomes, 2350 observations (1200 children) for health, and 1100 observations (550 children) for socioemotional

[5] By restricting to families who did not move across states since the focal child's birth, this study is assuming that families who did not move across states would have remained in the same market group. Families could have moved to a different county since the child's birth, but as long as they moved to a county that falls into the same market group, it would not affect the accuracy of capturing families' exposure to SNAP purchasing power.

outcomes. The sample size is smaller for socioemotional outcomes since not all children were eligible to be assessed on teacher- or childcare provider-reported socioemotional development. In keeping with NCES guidelines for the ECLS-B, sample sizes were rounded to the nearest 50.

Child Development Outcomes

To measure cognitive development, early reading and early math scores are used, which were measured through direct assessments by the home interviewer. The longitudinal scale of early reading and early math scores was created in wave 3 to wave 5 but not in earlier waves. The early reading measure was developed based on validated, standardized instruments such as the Preschool Language Assessment Scale (PreLAS) 2000, Peabody Picture Vocabulary Test (PPVT), and Preschool Comprehensive Test of Phonological and Print Processing (PreCTOPPP). Early reading skills capture both language and literacy skills, such as English language skills, word recognition, letter knowledge, letter-sound knowledge, vocabulary, and developing interpretation. Early math skills, also drawn from validated, standardized instruments such as the Test of Early Mathematics Ability-3, measure knowledge of number sense, operations, measurement, data analysis, patterns, and geometry (Snow et al., 2009). Some of the items in early reading and early math assessments were taken from the Early Childhood Longitudinal Study, Kindergarten Class of 1998–99 (ECLS-K) (Najarian et al., 2010). Overall scale scores for early reading and early math were calculated using Item Response Theory (IRT) procedures. The IRT scale scores represent estimates of the number of correct answers that would have been expected if a child had answered all items in math/reading assessment. The reliability of the IRT based early reading and math scores was tested and was proven to be high enough (Najarian et al., 2010).

As socioemotional development, I consider three constructs of teacher-reported socioemotional outcomes: approaches to learning, interpersonal skills, and externalizing behavior, which have been validated in the previous study (Riser et al., 2024). Higher scores indicate a better outcome for all measures. The socioemotional items were measured as indirect assessments by teachers and childcare providers, who evaluated each item on a 5-level scale from 0 (never) to 4 (very often). The approaches to learning scale is composed of 5-6 items that assess children's eagerness to learn, attentiveness, and task persistence (alpha: 0.83-0.89 depending on survey wave). The interpersonal scale is composed of 3 items that assess children's relationships with others and whether children seem happy (alpha: 0.64-0.76), while the externalizing behaviors scale is composed of 8 items that assess children's aggressive, disruptive, and impulsive behaviors (alpha: 0.88-0.92). To create each construct, I took the average of the total value of all the items included in a construct. For ease of interpretation, cognitive and socioemotional scores are standardized to have a mean of 0 and SD of 1.

Lastly, to measure health status, I use the parent-reported (mostly mothers) general child health status (excellent, very good, good, fair, or poor) and create an indicator for excellent/very good health (=0 if good, fair, or poor). The indicator was often used in the literature on SNAP and child health (e.g., Bronchetti et al., 2019; East, 2020; Miller, & Morrissey, 2021) and research suggests there is a fairly high correlation between parent's reports of child health status and doctor's reports of child health status (Case, Lubotsky, & Paxson, 2002). The study uses a linear probability model for this outcome.

SNAP Purchasing Power Measure

I measure SNAP generosity as the purchasing power of SNAP benefits. While there are significant differences in the local cost of living, SNAP benefits are not adjusted for such geographic variations. Following the approach used by Bronchetti and colleagues (2019), I measure SNAP purchasing power as the ratio of the maximum SNAP benefit for a family of four (which does not vary across all states plus District of Columbia (D.C.) in the contiguous U.S.) to the regional cost of the TFP.[6] TFP is the least expensive nutrition plan, established by the USDA, that contains recommended amounts of foods in 29 food categories for a "reference" family, defined as a family of four comprised of an adult male and female (age 20-50) and two children (age 6-8 and age 9-11). Given that maximum SNAP benefits are legislated based on the national average cost of the TFP, although SNAP recipients are not limited to purchasing items from the TFP, I use the regional TFP price as a standardized price measure to identify changes in prices across different regions and time (Bronchetti et al., 2019).

The regional TFP price is estimated using the Quarterly Food-at-Home Price Database (QFAHPD). The QFAHPD, constructed by USDA Economic Research Service researchers using Nielsen Homescan data, has quarterly prices for 52 food-at-home categories (e.g., 12 vegetables groups, 3 fruit groups, 6 dairy groups) for each of the 35 market groups from 1999 to 2010.[7] 35 market groups are exhaustive of the contiguous U.S. (48 contiguous states and D.C.) and each market group comprises a set of counties. A market group includes one metropolitan area, e.g., Boston, Chicago, San Francisco, urban New York, Los Angeles, when there are no sample size concerns, while in otherwise instance, a few metropolitan areas are aggregated into one market

[6] The TFP price data come from Bronchetti, Christensen, & Hoynes (2019).

[7] There are two versions of QFAHPD data. Version 1 of the QFAHPD (QFAHPD-1) contains prices for 52 food groups in 1999-2006 and version 2 of the QFAHPD (QFAHPD-2) contains prices for 54 food groups in 2004-2010. To combine the two versions, I follow Bronchetti et al. (2019) and estimate the average ratio of the price in QFAHPD-1 to the price in QFAHPD-2 for 2004-2006 in each market group. Then, to put all price data into same units, I divide the price for 1999-2003 by this ratio. See this website for further details on QFAHPD-1 and QFAHPD-2: https://www.ers.usda.gov/data-products/quarterly-food-at-home-price-database/

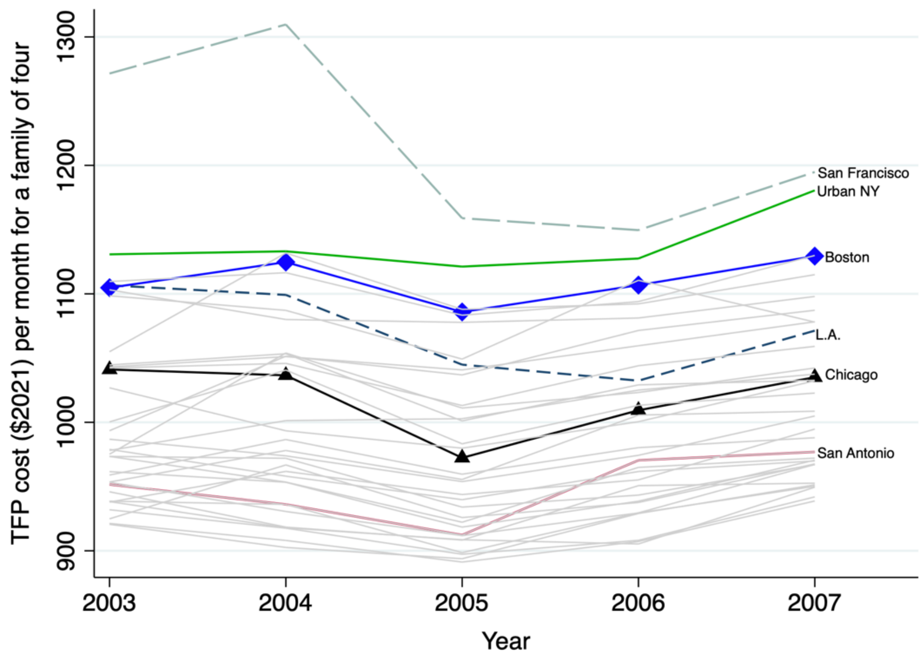
group (e.g., Indianapolis, Detroit, Milwaukee, and Grand Rapids constitute “Metro Midwest 1”). For nonmetro areas, they are aggregated based on 9 census divisions. Thus, this resulted in 26 market groups for metropolitan areas, and 9 market groups for nonmetropolitan areas (Todd et al., 2010). Figure A.1 in Appendix shows a map of 35 market groups, reprinted from Todd and colleagues (2010).

Gregory and Coleman-Jensen (2013) developed a method to create a single price estimate of TFP for each market group and quarter. The first step is to map the individual QFAHPD food categories into a TFP food category (in most cases, a TFP food category consists of multiple QFAHPD food categories). The second step is to compute the price of each TFP food group. In order to do so, they use a weighted average of the quarterly prices for the QFAHPD foods within a TFP food category, where the weights are yearly national expenditure shares for the QFAHPD food in the TFP category. By averaging the quarterly price of TFP food categories across four quarters and then aggregating the TFP prices for all food groups, a single estimate of total TFP price can be calculated by market groups and years (see Gregory, & Coleman-Jensen (2013) and Bronchetti et al. (2019) for further details).

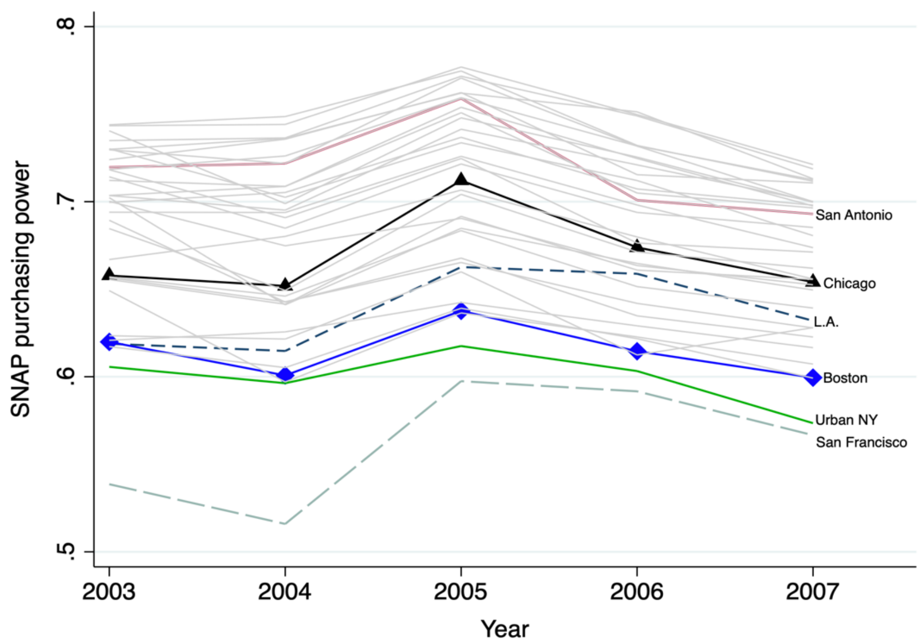
Figure 3.1 shows the trends in TFP price and SNAP purchasing power by each market group, with relatively large cities highlighted as colored lines. This illustrates that there are large variations in TFP price and therefore, SNAP purchasing power, not only across market groups but also over time within each market group.

Figure 3.1. TFP price and SNAP purchasing power by market groups from 2003 to 2007.

Panel A. TFP price (in $2021) over time by market groups



Panel B. SNAP purchasing power over time by market groups



Notes: In panel A and panel B, each line indicates a market group's trend in TFP price and SNAP purchasing power, respectively.
Data source: Bronchetti, Christensen, and Hoynes (2019)

I assign each family in the ECLS-B to this market group-year-level SNAP purchasing power based on the county of birth (hereafter, county of residence) of the focal child and the assessment (i.e., interview) year. In the ECLS-B, children and families were interviewed and assessed at various time points within each wave. For instance, assessment months vary from August, 2005 to June, 2006 in wave 3, and assessment months vary from September, 2006 (November, 2007) to March, 2007 (March, 2008) in wave 4 (wave 5). Thus, to ensure that SNAP purchasing power is measured before their month of assessment, I consider lagged measures of SNAP purchasing power. I lag them by one year and two years relative to the year of assessment given that the effect of SNAP purchasing power may appear over time, rather than immediately after one's exposure to SNAP purchasing power.[8] By examining the effect on child development within two years of the change in SNAP purchasing power, I provide a short-run estimate of the impact of SNAP purchasing power.

State- and County-Level Control Variables

In addition to the ECLS-B and QFAHPD data, I use multiple publicly available sources to construct state- and county-level control variables. As state-year level variables, I adjust for unemployment rate (from Bureau of Labor Statistics (BLS)), poverty rate (from U.S. Census Bureau's Small Area Income and Poverty Estimates), maximum TANF benefit for a family of four (from University of Kentucky Center for Poverty Research), per-capita income (from Bureau of Economic Analysis), state minimum wage (from BLS and Tax Policy Center), and upper income eligibility limit of Medicaid/SCHIP for children (from Kaiser Family Foundation and National Governor's Association). I also control for a summary index of state-level SNAP

[8] I have also conduct analyses using a three-year lagged measure of SNAP purchasing power. Results are no longer statistically significant in the three-year lagged period.

administrative policies, including call centers, online applications, simplified reporting, telephone interview instead of a face-to-face interview at recertification, exclusion of an at least 1 vehicle from the asset test, BBCE, fingerprinting requirement (reverse coded), Supplemental Security Income Combined Application Project, and whether fewer than 50% of SNAP recipients recertify within 1-3 month intervals (from SNAP Policy Database). By averaging across these policy variables (each coded as a binary variable), this summary index indicates the number of policies adopted by each state in a given year, which is similar in spirit to what Ganong and Liebman (2018) used. Moreover, to reduce concerns that the SNAP purchasing power measure may be picking up the effect of food prices rather than the generosity of SNAP benefits, I control for other prices that are correlated with TFP price. I specifically use housing price estimates from the U.S. Department of Housing and Urban Development's (HUD's) Fair Market Rent (FMR) for 2-bedroom units (from HUD Office of Policy Development and Research). FMRs estimate 40^{th} percentile gross rents for standard quality units by county. As a robustness check, I additionally control for prices of other goods, using regional Consumer Price Index (CPI) for apparel, transportation, education, and recreation (from U.S. Census Bureau and BLS). Table B.7 in Appendix B shows the correlation between local TFP price and other local prices, including FMR and regional CPIs. During the study period, regional CPIs are available in 27 metro core based statistical areas, while in other regions, they are available in 11 sampling units that consist of different census regions by population sizes (<50,000; 50,000-1.5 million; >1.5million). Both FMR and regional CPIs are merged with the ECLS-B by county and year.

Identification Strategy

The current study uses a child fixed-effects framework as its main empirical strategy. Specifically, I analyze the following specification.

$$Y_{itrm} = \alpha_0 + \delta_1 \ln\left(\frac{\max SNAP}{TFP\ price}\right)_{m,t-1} + \delta_2 \ln\left(\frac{\max SNAP}{TFP\ price}\right)_{m,t-2} + X_{it}\beta + \phi_{r,t-1}\mu_1 + \phi_{r,t-2}\mu_2 + \lambda_t + \gamma_i + \varepsilon_{itrm}$$

i indicates child, *t* indicates wave 3 and wave k (or the year of assessment in each wave), *r* indicates either a state or a county of residence, and *m* indicates a market group of residence. For ease of interpretation, I take a natural log of one-year and two-year lagged SNAP purchasing power measures. By multiplying the coefficients on log SNAP purchasing power, δ_1 and δ_2, by log (1.10) (= 0.1), I obtain the one-year lagged and two-year lagged effects of a 10 percent increase in SNAP purchasing power on the outcome of interest.[9] Throughout the paper, I discuss the effect of a 10 percent increase in SNAP purchasing power.

In the model, I include child fixed effects γ_i and year (or survey wave) fixed effects λ_t. Child fixed effects control for stable child or family specific characteristics that may confound the relationship between SNAP purchasing power and child outcomes, such as child innate cognitive ability, permanent disability, child temperament, and parental fixed preferences or approach to early child learning. Year fixed effects account for time-varying confounders that affect all children in the same survey wave (e.g., macro-economic shocks or federal-level policy changes). Although child and year fixed effects account for these important confounders, they cannot address confounding regional, and child and family characteristics that vary over time. Thus, as I described in the previous section, I adjust for a rich set of state- and county-year level characteristics, lagged by one and two years (ϕ_{rt-1}, ϕ_{rt-2}). Moreover, as time-varying child and family characteristics (X_{it}), I adjust for child age at the survey assessment (in months) and its squared term, mother's age (in years) and its squared term, parent's highest education attainment

[9] I obtain a very similar result without taking a natural log of SNAP purchasing power.

(below high school, high school degree, some college), number of children (one, two or more), household size, mother's immigrant status (immigrant, US citizen), and urbanicity (rural, urban, urban cluster) (see Table 3.1 for their descriptive statistics). The first category within the parenthesis is the reference category for categorical variables. To adjust for survey nonresponse and disproportionate sampling, and to allow for making inferences about the national population, the ECLS-B survey weights were applied to all the analyses. Standard errors are clustered at the market-group level.

The child fixed effects approach examines whether changes in TFP cost (therefore, SNAP purchasing power) across two waves are driving differences in children's developmental outcomes from their mean outcome score (i.e., averaged across two waves). The variation in TFP cost comes from two sources: (i) change in TFP cost within a market group that a child lives in; and (ii) the years in which a child was assessed in wave 3 and wave k. Regarding the first source of variation, as illustrated in Figure 3.1, market groups have shown differential trends in the TFP cost and SNAP purchasing power during the study period – some areas showing larger changes (increases or decreases) while others experiencing smaller changes. With respect to the second source of variation, children living in the same market group may be assigned SNAP purchasing power from different years since the year of assessment varies across children (2005-2006 in wave 3, and 2006-2008 in wave k). For instance, children who were assessed in 2005 and 2006 in wave 3 and wave k, respectively, are assigned one-year lagged SNAP purchasing power from 2004 and 2005, while children assessed in 2005 and 2007 are assigned with 2004 and 2006 SNAP purchasing power in each wave.[10] I exploit this likely idiosyncratic variation, derived

[10] All possible combinations of assessment years in wave 3 and wave k include 2005-2006, 2005-2007, 2006-2006, 2006-2007, 2005-2008, 2006-2008. 75% of the study sample fall into the 2005-2006 or 2005-2007 categories. For children who were assessed in 2006 in both wave 3 and wave k, I use 2005 instead of 2004 in wave k to merge the

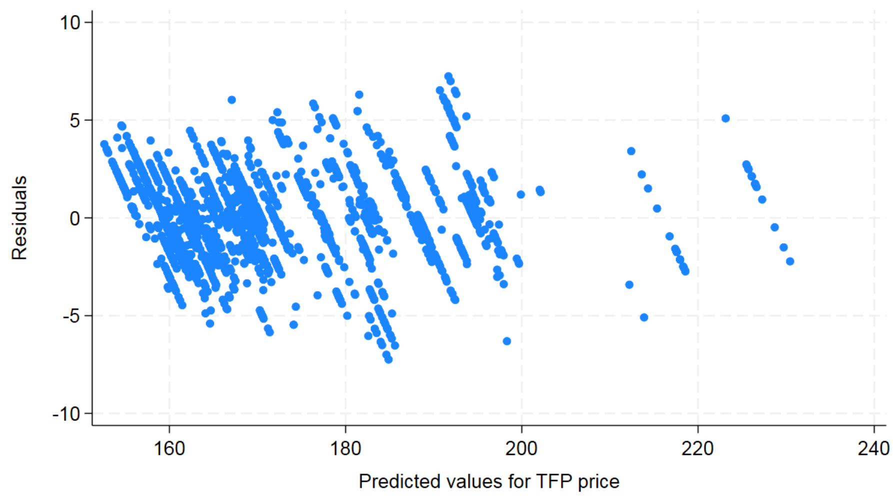
from different assessment timings. Based on my consultation with the ECLS-B's data team, the assessment timing was determined solely by a family's availability and there is no record of field interviewers scheduling the assessments by nonrandom child or family characteristics (e.g., child age, expected year of kindergarten entry). Moreover, descriptive analysis that compares demographic characteristics between different assessment timing groups show that there are no statistically significant differences across these groups for most characteristics (see Table A.1 in Appendix).

We can interpret the key coefficients as causal based on the assumption that changes in the TFP cost or SNAP purchasing power are exogenous – that is, not driven by changes in unobserved correlates of child development outcomes – after adjusting for other covariates in the model. One thing to consider is how much variation remains in the TFP cost after adjusting for covariates. Similar to Bronchetti et al. (2019), I find an R^2 of 0.94 when regressing the TFP cost on housing prices captured by FMR and fixed effects, and an R^2 of 0.98 after adding individual characteristics, regional economic and policy characteristics, and regional CPI variables. Figure 3.2 plots residuals from the regressions of TFP cost on covariates included in the main specification and with regional CPI measures. They illustrate a fair amount of idiosyncratic variation, which may plausibly allow me to obtain a causal effect of SNAP purchasing power. Moreover, I perform a series of robustness and falsification tests to assess the plausibility of the above assumption (see the Falsification and Robustness Tests section). The results are overall not sensitive to these tests, and I interpret the results as plausibly causal evidence on whether and to what extent greater real value of SNAP benefits improves children's development.

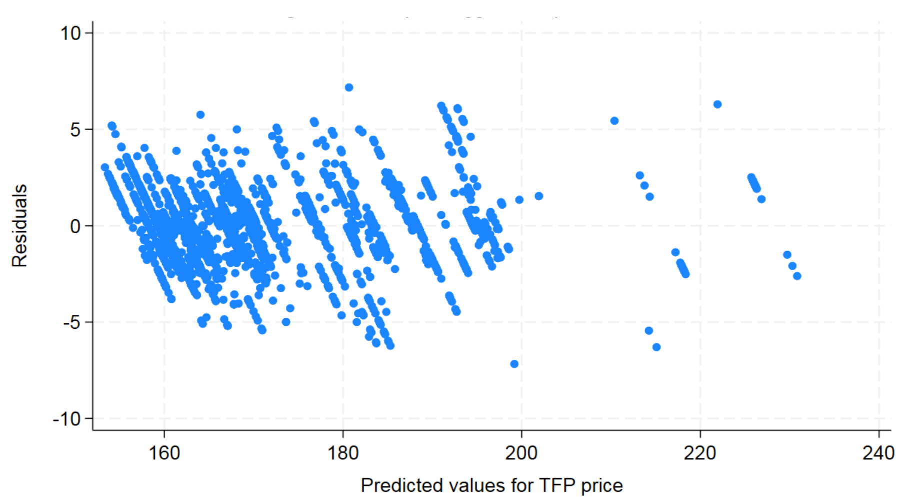
ECLS-B data with the two-year lagged SNAP purchasing power, while I use 2006 instead of 2005 in wave k to merge with the one-year lagged SNAP purchasing power.

Figure 3.2. Residuals from a regression of the TFP cost on control variables.

Panel A.



Panel B.



Notes: In panel A, I included covariates in the main specification, while in panel B, I further added regional CPI measures to those covariates. In both panels, I used two-year lagged SNAP purchasing power.
Data source: Bronchetti, Christensen, and Hoynes (2019)

Note that the current study's identification strategy relies on a *change* in SNAP purchasing power between wave 3 and wave k, *conditional on* prior levels of SNAP purchasing power that children were exposed to. To further investigate this, I divided children into two groups – i.e., those in market groups that showed (i) an increase and (ii) a decrease in SNAP purchasing power from wave 3 to wave k – and compared their prior purchasing power levels in wave 1 and wave 2. Table A.2 shows that the first group only had slightly higher SNAP purchasing power, by 0.01-0.02, compared to the second group at various points along their distribution, including mean, 25th percentile, median, and 75th percentile. Although suggestive, this result alleviates concerns that children who experienced an increase from wave 3 to wave k were potentially exposed to significantly different SNAP purchasing power levels in prior years compared to those who experienced a decrease between the two waves.

Results

Sample Characteristics

Table 3.1 shows sample characteristics of the primary treatment sample (i.e., children whose mothers are unmarried and do not have a college degree) by survey waves and in the full sample, restricted to families with no missing values in the reading outcome (N=2150). Results do not change qualitatively when the sample is restricted to families without missing values in other child outcomes. Table 3.1 supports that my primary treatment sample comprises socioeconomically disadvantaged families who have likely received SNAP. The negative values in the standardized child outcomes demonstrate that this sample represents children whose cognitive and socioemotional skills are lower than the average in the nationally representative ECLS-B children. In the full sample (see "Total" column), the majority of families reported that they received SNAP (54.8 percent) since the prior wave[11] and close to 45 percent of families

[11] To be more exact, the ECLS-B interview asked whether the respondent or any family members received SNAP since a child turned age 2 (in wave 3) and age 4 or age 5 in wave k, depending on whether the child first entered kindergarten at wave 4 or 5, respectively.

comprise parents with a high school degree, followed by parents with some college (34.4 percent) and no high school degree (21.4 percent). The sample is equally divided by male and female children, and Black children constitute the largest share of the sample (37.3 percent), followed by Hispanic (31.5 percent), White (25.3 percent), and other race/ethnicity (5.9 percent) children. Child race/ethnicity and biological sex are not controlled in models as they are constant over time and thus are absorbed by child fixed effects. In addition, several variables show statistically significant differences between wave 3 and wave k. The study's independent variable (i.e., SNAP purchasing power) and most of the state/county covariates show statistically significant changes between the two waves. On the other hand, there are only few child/family covariates and outcomes that show statistically significant changes, including the number of children, SNAP receipt status (marginally significant), child and mother age, and interpersonal skills (marginally significant).

Table 3.1. Sample characteristics of primary study sample (Unmarried mothers, no college)

	Wave 3	Wave K	Total	Sig.
	Mean (SD) / Percent			
Outcomes:				
Std. early reading	-0.40	-0.40	-0.40	
	(0.82)	(0.91)	(0.87)	
Std. early math	-0.44	-0.41	-0.43	
	(0.85)	(0.90)	(0.87)	
Std. approaches to learning	-0.23	-0.27	-0.25	
	(0.94)	(1.01)	(0.98)	
Std. interpersonal skills	-0.01	-0.11	-0.06	+
	(0.96)	(0.99)	(0.98)	
Std. externalizing behavior	-0.22	-0.28	-0.25	
	(1.02)	(1.07)	(1.05)	
Excellent/Very Good (Ref.: Poor/Fair/Good) (%)	81.29	83.6	82.44	
Independent variables:				

(Table 3.1 continued)

1-year lagged SNAP purchasing power	0.68	0.70	0.69	***
	(0.05)	(0.05)	(0.05)	
1-year lagged monthly TFP cost	703.04	721.83	712.44	***
	(55.69)	(53.43)	(55.36)	
1-year lagged Max SNAP benefit for a family of 4	476.43	502.88	489.65	***
	(11.07)	(5.08)	(15.79)	
2-year lagged SNAP purchasing power	0.69	0.69	0.69	**
	(0.05)	(0.05)	(0.05)	
2-year lagged monthly TFP cost	684.00	705.62	694.81	***
	(51.47)	(54.75)	(54.21)	
2-year lagged Max SNAP benefit for a family of 4	466.16	484.07	475.12	***
	(2.37)	(14.49)	(13.71)	
Child/family characteristics:				
Two or more children (Ref.: One child) (%)	67.83	73.13	70.48	***
Urbanicity (%)				
Rural	13.74	14.06	13.9	
Urban	75.63	74.95	75.29	
Urban-cluster	10.63	11	10.81	
US-citizen mother (Ref.: Immigrant mother) (%)	87.82	88.27	88.05	
Parent's highest education attainment (%)				
No high school degree	21.22	21.59	21.4	
High school degree	44.24	44.11	44.17	
Some college	34.54	34.31	34.43	
Child age at the interview (in months)	52.73	67.53	60.13	***
	(4.09)	(4.16)	(8.48)	
Mother's age (in years)	28.54	30.09	29.32	***
	(6.72)	(7.24)	(7.02)	
Household size	4.36	4.39	4.38	
	(1.60)	(1.70)	(1.65)	
Received SNAP (%)	56.03	53.47	54.75	+
Child race/ethnicity (%)				N/A
White	25.3	25.3	25.3	
Black	37.32	37.32	37.32	
Hispanic	31.49	31.49	31.49	
Others	5.89	5.89	5.89	
Male focal child (Ref.: Female focal child) (%)	50.86	50.86	50.86	N/A

(Table 3.1 continued)

State or county level characteristics:				
Unemployment rate	5.56	5.08	5.32	***
	(0.90)	(1.02)	(0.99)	
Percent of poverty	13.07	13.54	13.30	***
	(2.69)	(2.81)	(2.76)	
Per-capita income ($2021 in thousands)	48.64	49.79	49.21	***
	(6.98)	(7.20)	(7.11)	
Maximum TANF benefits for a family of four ($2021 in hundreds)	6.75	6.52	6.64	***
	(2.93)	(2.87)	(2.90)	
Medicaid income eligibility limit as a percent of FPL	2.31	2.32	2.32	+
	(0.48)	(0.48)	(0.48)	
Minimum wage ($2021)	7.86	7.91	7.88	
	(1.01)	(1.09)	(1.05)	
Index for adopting inclusive SNAP administrative policies (0-1)	0.42	0.47	0.44	***
	(0.13)	(0.15)	(0.14)	
Fair market rent for 2-bedroom ($2021)	1054.81	1035.17	1044.99	**
	(344.46)	(307.10)	(326.39)	
Regional CPI for apparel costs	106.47	106.00	106.23	+
	(16.10)	(17.61)	(16.87)	
Regional CPI for education costs	110.85	113.73	112.29	***
	(4.27)	(5.09)	(4.92)	
Regional CPI for recreation costs	109.17	110.54	109.85	***
	(4.88)	(5.47)	(5.22)	
Regional CPI for transportation costs	144.58	155.25	149.91	***
	(26.89)	(28.23)	(28.07)	

Notes: Table 3.1 shows sample characteristics of the study sample for cognitive outcomes (N=2150). Given that sample size is smaller in socioemotional outcomes, fewer number of observations are used to calculate the descriptive statistics of socioemotional outcomes. For state/county level characteristics, their 1-year lagged values are shown in this table. 'Sig.' column shows whether a covariate is statistically significantly different between wave 3 and wave k. N/A in the 'Sig.' column indicates that the corresponding variable is a time-invariant variable. *** p<0.001, ** p<0.01, * p<0.05, + p<0.1

Main Results

I estimated the equation shown in the identification strategy section. Table 3.2 shows consistent results across all child outcomes that one-year lagged SNAP purchasing power does not have a statistically significant effect on child development (see the first row in Table 3.2). However, when log SNAP purchasing power is lagged by two years, I find that greater purchasing power of SNAP benefits has statistically significant positive effects on both cognitive and socioemotional outcomes. Columns 1 and 2 in Table 3.2 (see the 4^{th} row) show that a 10 percent increase in two-year lagged SNAP purchasing power improved early reading and early math skills by 0.25 SD and 0.23 SD, respectively, although the result was marginally significant for early reading skills. With respect to socioemotional outcomes, columns 4 and 6 (see the 4^{th} row) show that a 10 percent increase in two-year lagged SNAP purchasing power improved approaches to learning by 0.43 SD and externalizing behavior by 0.47 SD, respectively. The result on interpersonal skills was statistically insignificant although the coefficient was not small (0.19 SD), while I found null effects on very good/excellent health status. Importantly, these results are mostly robust to adding different control variables. Results from stepwise regressions demonstrate that the effects on all outcomes (except for early reading skills) show relatively small changes in terms of their statistical significance and magnitudes between models, as I added two-year-lagged regional covariates, one-year-lagged regional covariates, and child/family covariates step by step (see Table A.3 in Appendix A). Furthermore, the effects of two-year lagged SNAP purchasing power stay substantively similar when I exclude one-year lagged SNAP purchasing power (see Table A.6 in Appendix A).

I also analyzed whether the effect of SNAP purchasing power is statistically significantly different between male and female children by estimating the same equation I used for the full sample separately by male and female. Due to small sample sizes in approaches to learning and externalizing behavior, I conducted this subgroup analysis only on early reading and math outcomes (excluding interpersonal skills and general health status since their effects are not statistically significant in the full sample). To conduct a statistical test for the equality of female/male coefficients, I used a formula suggested by Paternoster, Brame, Mazerolle, and Piquero (1998). As shown in Table A.4 in Appendix A, I fail to reject the null hypothesis that the effect of SNAP purchasing power on early reading and early math skills is similar between male and female students. Hence, I conclude that SNAP purchasing power has similar effects on cognitive development for male and female children.

To better understand the results, it is helpful to gauge the magnitudes of a 10 percent increase in SNAP purchasing power. Relative to the mean level, a 10 percent increase in SNAP purchasing power is approximately comparable to an additional $850 to $1130 per year (in 2021 dollars) for an average family.[12] To put it in another way, a 10 percent increase from the mean is equivalent to moving from the market group with 0.69 purchasing power (e.g., In 2005, this is "North Florida" (market group 7) which comprises Jacksonville and Orlando) to the market group with 0.76 purchasing power (e.g., In 2005, this is "San Antonio" (market group 18)). In terms of within market group change, experiencing a 10 percent increase in purchasing power is considered a large change. For example, from 2003 to 2005, 15 market groups showed a 5-10 percent increase in their SNAP purchasing power, with three of them showing more than a 7 percent increase ("Los Angeles" (market group 15): 7.1 percent; "Chicago" (market group 16): 8.3 percent; "San Francisco" (market group 23): 10.9 percent). Another 15 market groups

[12] As indicated in Table 3.1, the mean SNAP purchasing power (two-year lagged) is 0.69 in the study sample. 0.69 is calculated by dividing the average maximum SNAP benefit per person – i.e., $118.8 – by the average monthly TFP cost per person – i.e., $173.6. Therefore, to increase the average SNAP purchasing power by 10 percent, it would require an increase in maximum SNAP benefits by approximately $12 or a decrease in TFP cost by $16 per person-month. Translating this into annual dollar amounts for an average household size (4.4) in the study sample, a 10 percent increase would indicate an additional $634 to $845 per year (approximately $850-$1130 in 2021 dollars) for an average family.

showed a 0-5 percent increase in their SNAP purchasing power, while five market groups showed a decrease in their purchasing power.

Table 3.2. Effect of SNAP purchasing power on child development

	Reading	Math	Health	AL	IP	EB
1-year lagged log(SNAP purchasing power)	-1.53	-0.61	-0.40	-2.30	-0.05	-3.17
	(0.98)	(0.78)	(0.58)	(1.67)	(1.81)	(1.90)
2-year lagged log(SNAP purchasing power)	2.46+	2.33*	-0.11	4.30*	1.85	4.72*
	(1.34)	(0.92)	(0.68)	(2.05)	(1.99)	(1.96)
1-year lagged: 10% increase	-0.15	-0.06	-0.04	-0.23	-0.01	-0.32
2-year lagged: 10% increase	0.25	0.23	-0.01	0.43	0.19	0.47
Mean of outcome	-0.40	-0.42	0.81	-0.31	-0.09	-0.31
N	2150	2100	2350	1100	1100	1100
Child fixed effects	Y	Y	Y	Y	Y	Y
Year fixed effects	Y	Y	Y	Y	Y	Y
Child and family characteristics	Y	Y	Y	Y	Y	Y
Regional economic and policy conditions	Y	Y	Y	Y	Y	Y
Housing price	Y	Y	Y	Y	Y	Y

Notes: 'AL' indicates approaches to learning; 'IP' indicates interpersonal skills; 'EB' indicates externalizing behavior. *** p<0.001, ** p<0.01, * p<0.05, + p<0.1

Robustness Tests

Then, I conducted a series of robustness and falsification tests to assess whether the above findings on early reading, early math, approaches to learning, and externalizing behavior are plausibly causal. I focused on these outcomes where I found statistically significant results in Table 3.2. Overall, results from the following tests support that my main results show causal evidence on whether and to what extent the real value of SNAP benefits improves children's cognitive and socioemotional development. First, as a falsification test, I analyzed the effect of SNAP purchasing power among children who are likely to have low rates of SNAP participation: children of college-educated married mothers. If my main results reflect the effect of generosity of SNAP benefits, rather than that of local food prices, I expect to see a smaller or a null effect of SNAP purchasing power in this placebo sample. Reassuringly, Panel 1 in Table 3.3 demonstrates that I found small and statistically insignificant effects of SNAP purchasing power across the four child outcomes among children of college-educated married mothers, adding support to a causal interpretation of my main results.[13]

Second, to provide further evidence that my main result is not picking up the effect of local prices or the broader effect of living in a different market, I tested the effect of SNAP purchasing power on household income (that does not include public benefits) and income to needs ratio as an indication of poverty status. To the extent that I am picking up the effect of local prices or that of living in a different labor market (which is likely to be reflected in wages and income), SNAP purchasing power may be statistically significantly related to families' income. Reassuringly, columns 1 and 2 in Table A.5 in Appendix show that I find statistically insignificant effects of two-year lagged SNAP purchasing power on income and income to needs ratio. As an additional test, I controlled for prices of other goods (in addition to housing prices), using regional CPIs for apparel, education, recreation, and transportation costs. If the effect of SNAP purchasing power reflects the effect of TFP cost, rather than the generosity of SNAP benefits, these additional price controls – which are correlated with the TFP cost – could reduce the magnitude of the effect of SNAP purchasing power on child outcomes. Panel 2 in Table 3.3 shows that my main results are qualitatively similar with these additional price measures. The

[13] An alternative placebo sample may include children in equally disadvantaged families as those in my primary sample (i.e., unmarried mothers without a college degree) but who do not report having received SNAP. However, this placebo group is limited by the potential endogenous underreporting of SNAP participation (Bitler, 2020; Meyer, Mittag, & Goerge, 2022) and small sample sizes (N=700 for cognitive outcomes and N=350 for socioemotional outcomes), which make it difficult to obtain precise and reliable estimates. Thus, I present the results from college educated married mothers as my falsification test.

effects of a 10 percent increase in two-year lagged SNAP purchasing power on early math, approaches to learning, and externalizing behavior stayed statistically significant, and the magnitudes of their effects became a bit larger relative to my main results without regional CPIs. Similarly, for early reading skills, the magnitude of the two-year lagged coefficient stayed almost the same, although it became statistically insignificant while it was marginally significant in my main results. Overall, these tests add confidence that my main results are not reflecting the effect of local food prices or the broader effect of living in a more or less expensive market or a different labor market.

Third, I tested the effect of SNAP purchasing power on families' SNAP participation to evaluate whether changes in SNAP purchasing power lead families to systematically select into the SNAP program (e.g., a family deciding to participate in the SNAP program when the level of SNAP purchasing power increases). Such selection effects can bias the study's estimates of the effect of SNAP purchasing power on child development. Column 3 in Table A.5 in Appendix demonstrates statistically insignificant effects of the two-year lagged SNAP purchasing power on SNAP participation.

Table 3.3. Robustness test results

	Reading	Math	AL	EB
Panel 1. Placebo sample: Children of married college-educated mothers				
1-year lagged log(SNAP purchasing power)	-0.80	-0.14	1.82	-1.10
	(1.37)	(1.01)	(1.51)	(1.34)
2-year lagged log(SNAP purchasing power)	0.89	0.14	2.07	1.41
	(1.14)	(1.12)	(1.59)	(1.11)
2-year lagged: 10% increase in purchasing power	0.09	0.01	0.21	0.14
N	3100	3100	1850	1850
Panel 2. Controlling for regional CPIs				

(Table 3.3 continued)

1-year lagged log(SNAP purchasing power)	-0.92	-1.32	-2.19	-2.93
	(1.32)	(0.98)	(2.41)	(2.20)
2-year lagged log(SNAP purchasing power)	2.41	2.66**	4.57*	4.86*
	(1.53)	(0.92)	(1.93)	(2.08)
2-year lagged: 10% increase in purchasing power	0.24	0.27	0.46	0.49
N	2150	2100	1100	1100

Notes: 'AL' indicates approaches to learning; 'EB' indicates externalizing behavior. Key coefficients of interest are only shown in this table; but the same set of covariates that was controlled in my main model (child fixed effects, year fixed effects, child/family time-varying covariates, county/state time-varying economic and policy conditions, and housing price, i.e., HUD's FMR) was controlled in these falsification tests. *** p<0.001, ** p<0.01, * p<0.05, + p<0.1

Mechanisms

In this section, I examined several possible mechanisms through which SNAP purchasing power may influence children's cognitive and socioemotional development. As described in the conceptual framework section, I considered three possible pathways through which the real value of SNAP benefits may improve child development: nutritional pathway, parental learning investment pathway, and stress pathway. The ECLS-B allows for testing these pathways, but it is methodologically challenging to do a formal mediation analysis. This is because there is no (or very little) time lag between mediators and child outcomes measured in the same wave and I am unable to use mediators and child outcomes from different survey waves with the child fixed-effects model. Thus, as a suggestive test of mediation, I tested these mechanisms by analyzing the direct effects of SNAP purchasing power on potential mediators.

To test the nutritional pathway, I used two indicators for whether a household is food insecure and whether a household is very food insecure. Then, to test the parental learning investment pathway, I used the number of books at home (standardized to have a mean of 0 and a standard deviation of 1), how many minutes per day a parent or a family member reads to the focal child, and a cognitive activity index, which is constructed by averaging the values of three items on how often in a typical week a parent or a family member i) reads to the focal child, ii) sing songs with the child, and iii) tell stories to the child. Each of these items is scaled from 1 (not at all) to 4 (every day). Lastly, to test the stress pathway, I used two indicators for whether a mother has depressive symptoms (which include both moderate and severe conditions) and whether a mother has severe depressive symptoms. To obtain information about depression from the mother, the ECLS-B used a 12-item version of the Center for Epidemiological Studies' Depression Scale (CES-D) (Radloff, 1977), which is a self-reported depression scale and assesses depressive feelings and behavior during the past week. Each item is coded on a 4-point scale between 0 and 3, and the range of total scores of all items (i.e., raw symptom score) is 0 to 36. The raw symptom score can be categorized into no (0-4), mild (5-9), moderate (10-14), or severe depressive symptoms (15+) (Paulson, Keefe, & Leiferman, 2009; Silverstein et al., 2006). I could not use parenting behavior variables (e.g., negative parenting, such as spanking the child or hitting the child back when the child gets so angry and yells at parents) since they are not fully available in wave 3 and wave k.

As Table 3.4 shows, findings lend support to the stress pathway and the parental learning investment pathway. The first row shows that a 10 percent increase in one-year lagged SNAP purchasing power led to a 16 percentage point decrease in the probability of severe depressive symptoms (i.e., the coefficient -1.59 multiplied by 0.1, which is -0.16)[14]. With respect to the

[14] The significant reduction seems to occur when SNAP purchasing power increases from below the mean to the mean, rather than when it increases above the mean. When SNAP purchasing power moves from 0.64 to 0.7 (a 10 percent increase to the mean), there is a 62.2% reduction in the probability of severe depressive symptoms relative to the mean. When SNAP purchasing power moves from 0.7 to 0.77 (a 10 percent increase from the mean), the reduction in the probability of severe depressive symptoms is not statistically significant.

probability of depressive symptoms, the effect of SNAP purchasing power was not statistically significant although the direction of the coefficient was still negative (see the second row). Taken together, these results imply that the effect of SNAP purchasing power occurs at the high end of the depression scale, alleviating depressive symptoms among mothers who are in a severe condition. Regarding the parental learning investment mechanism, I found statistically significant effects on daily reading time (in minutes), but not on the number of books and cognitive activity index. A 10 percent increase in one-year lagged SNAP purchasing power led a parent or a family member to increase their reading time with the child by more than 6 minutes (i.e., the coefficient 60.95 multiplied by 0.1) per day, which is equivalent to 25 percent increase based on the mean. It is possible that the reduction in mothers' depression symptoms and the increase in daily reading time are related, since mothers with high depression may have less capacity to engage in stimulating activities, such as reading to their child (McLennan, & Kotelchuck, 2000). Lastly, I did not find evidence that the nutritional pathway is at play. The coefficients on food insecurity variables were statistically insignificant, although I cannot totally rule out the possibility of this pathway given that those effects were estimated imprecisely.

Of note, I found statistically significant effects of one-year lagged SNAP purchasing power on severe depression symptoms and reading time, whereas two-year lagged SNAP purchasing power was playing a role in child development outcomes. Although I cannot make conclusive claims about the mechanisms, such differential findings for potential mediators and development outcomes based on the timing of SNAP purchasing power (one-year lagged vs. two-year lagged) may be suggesting that a longer time lag is needed for SNAP benefits to have a significant impact on development outcomes compared to a mediator.

Table 3.4. Mechanisms: Effects of SNAP purchasing power on potential mediators

	1-year lagged log(SNAP purchasing power)	2-year lagged log(SNAP purchasing power)	N
Severe depression	-1.59**	-0.21	2250
(Mean: 0.11)	(0.51)	(0.47)	
Depression	-0.77	-0.83	2250
(Mean: 0.27)	(0.55)	(0.62)	
Books	-0.12	0.67	2350
(Mean: -0.42)	(0.42)	(0.40)	
Cognitive activity	0.12	0.74	2350
(Mean: 2.78)	(0.72)	(0.75)	
Read time (min.)	60.95*	-5.28	2200
(Mean: 24.14)	(28.83)	(37.98)	
Food insecure	0.03	0.37	2350
(Mean: 0.22)	(0.43)	(0.63)	
Very food insecure	0.39	0.07	2350
(Mean: 0.06)	(0.29)	(0.19)	

Notes: Each row indicates one model that analyzes the effect of SNAP purchasing power on a given mediator. Key coefficients of interest are only shown in this table; but the same set of covariates that was controlled in my main model (child fixed effects, year fixed effects, child/family time-varying covariates, county/state time-varying economic and policy conditions, and housing price, i.e., HUD's FMR) was controlled in these models.
*** p<0.001, ** p<0.01, * p<0.05, + p<0.1

Results from an Alternative Sample: SNAP Recipients

Next, I analyzed whether my main results are sensitive to an alternative treatment sample: families who reported in both wave 3 and wave k that they have received SNAP but no other cash-based benefits (TANF and SSI/SSDI). Compared to my primary treatment sample (i.e., unmarried mothers without a college degree), this alternative sample, on average, are more economically disadvantaged. Table 3.5 demonstrates this, showing that a smaller portion of them lived above 130 percent of FPL (17.1 percent vs. 35.4 percent), which is the gross income criterion for SNAP eligibility, and a much higher percentage of them received SNAP (100 percent vs. 54.8 percent). It is possible that a 10 percent increase in SNAP purchasing power may be more meaningful for families with lower economic resources and therefore children in those families may show larger changes in response to the increase in SNAP purchasing power. For this reason, I expect to see larger effects of SNAP purchasing power on child development among the SNAP recipient sample, compared to my primary sample.

Findings are presented in Table 3.6. For early reading, approaches to learning, and externalizing behavior outcomes, I found substantively similar effects of SNAP purchasing power but with larger magnitudes. The effect of a 10 percent increase in two-year lagged SNAP purchasing power on reading increased to 0.29 SD (from 0.25 SD). For approaches to learning, the effect increased to 1 SD-1.18 SD (from 0.43 SD) across one-year lagged and two-year lagged SNAP purchasing power, while they increased to 1 SD to 1.25 SD (from 0.47 SD) for externalizing behavior. The effect of two-year lagged SNAP purchasing power on externalizing behavior was estimated more imprecisely (thus, became statistically insignificant), but this might be an artifact of sensitivity to small sample sizes since they are reduced to N=400. It is noteworthy that the effect sizes increased in the SNAP recipient sample, particularly for socioemotional outcomes; however, given the small sample sizes, I am cautious about concluding that a 10 percent increase in SNAP purchasing power causes more than a 1 SD increase in approaches to learning and externalizing behavior. I interpret this result as evidence of more socioeconomically disadvantaged families benefiting from greater SNAP purchasing power potentially more so than less disadvantaged families, rather than focusing on the estimated number itself.

On the other hand, unexpectedly, I found weaker results for early math skills using the SNAP recipient sample. The effect magnitude of two-year lagged SNAP purchasing power was reduced to 0.14 SD and it was imprecisely estimated, becoming statistically insignificant. With larger standard errors, however, the 95 percent confidence interval of the math coefficient encapsulates the estimate I found in my primary sample (0.23 SD).

Table 3.5. Comparison between the primary treatment sample and SNAP recipients.

	Unmarried mothers without college	SNAP recipients
Parent's highest education attainment (%)		
No high school degree	21.40	22.20
High school degree	44.17	42.62
Some college	34.43	31.21
BA or higher	0.00	3.97
Poverty status (%)		
<50% of FPL	22.59	22.94
50%-130% of FPL	42.01	60.01
>130% of FPL	35.39	17.06
Not married (Ref. Married) (%)	100	54.50
Family structure (%)		
Two biological parents	24.78	48.45
1 biological parent, 1 other parent	8.41	10.98
Single biological parent	63.2	40.17
Received SNAP (%)	54.75	100
N	2150	850

Notes: Data come from the study sample for cognitive outcomes in both wave 3 and wave k. The “other” category of family structure variable is not shown in this table.

Table 3.6. Effects of SNAP purchasing power on child development among SNAP recipients.

	Reading	Math	Health	AL	IP	EB
1-year lagged log(SNAP purchasing power)	-2.33	-0.35	0.39	10.08*	2.65	12.46**
	(1.82)	(1.70)	(0.92)	(4.06)	(5.40)	(4.36)
2-year lagged log(SNAP purchasing power)	2.86*	1.43	0.24	11.79+	5.56	10.00
	(1.29)	(1.91)	(1.15)	(6.28)	(6.87)	(6.11)
1-year lagged: 10% increase	-0.23	-0.04	0.04	1.01	0.27	1.25
2-year lagged: 10% increase	0.29	0.14	0.02	1.18	0.56	1.00
Mean of outcome	-0.38	-0.42	0.79	-0.13	0.01	-0.01
Child fixed effects	Y	Y	Y	Y	Y	Y

(Table 3.6 continued)

Year fixed effects	Y	Y	Y	Y	Y	Y
Child and family characteristics	Y	Y	Y	Y	Y	Y
Regional economic and policy conditions	Y	Y	Y	Y	Y	Y
Housing price (HUD's FMR)	Y	Y	Y	Y	Y	Y
N	850	800	900	400	400	400

Notes: 'AL' indicates approaches to learning; 'IP' indicates interpersonal skills; 'EB' indicates externalizing behavior. *** p<0.001, ** p<0.01, * p<0.05, + p<0.1

Discussion

In this paper, I provided new evidence on the short-run effect of the real value, i.e., purchasing power, of SNAP benefits on the cognitive, socioemotional, and health outcomes of children during the early childhood to kindergarten-entry period. I found that greater SNAP purchasing power, when lagged by two years, increases early math and reading scores by 0.23 – 0.25 SD (although the effect on early reading skills was less robust than early math skills), as well as approaches to learning and externalizing behavior by 0.43 – 0.47 SD in the main specification. I found statistically insignificant effects of SNAP purchasing power on those outcomes when it was lagged by one year.

These results suggest that greater purchasing power of SNAP benefits contributes to reducing the developmental gap across socioeconomic status by meaningful amounts. For instance, Reardon and Portilla (2016) report that there is a 1.16 SD gap in early math scores at kindergarten entry between children in the bottom and the top 10 percent income groups. Thus, the current study’s finding is comparable in size to closing one fifth (20 percent) of the size of the early math gap between the lowest and the highest income groups. In addition, when compared to other research that examined the effect of income support policies among older school-aged children, the current study finds larger effect sizes. For example, several studies that investigated the effect of EITC on school-aged children's cognitive test scores (math and reading) or behavioral problems consistently demonstrate a 0.1 SD or lower improvement in math and reading skills and behavioral problems as a result of a $1000 increase in simulated tax credit amount, family income instrumented by eligible EITC amount, or maximum or eligible EITC amount (Chetty, Friedman, & Rockoff, 2021; Dahl, & Lochner, 2012; Hamad, & Rehkopf, 2016; Maxfield, 2015). Such differences in effect magnitudes might reflect that younger children are more sensitive to changes in a family's economic resources. It is also possible that such differences in effect sizes indicate the differential effects of SNAP and EITC or they may be due to the different methods used in the current paper and past studies.

Moreover, compared to one of the few existing studies on the effect of SNAP participation on early reading, early math, and approaches to learning among young children (Hong, & Henly, 2020), I found similar effect sizes for early math, but larger effect sizes for early reading and approaches to learning. Hong and Henly used the same dataset as the current study but different sample restriction criteria for capturing socioeconomically disadvantaged families. In addition, the effect sizes are not directly comparable between these two studies due to their different treatments (generosity vs. participation), and potentially, the underreporting of SNAP receipt in the ECLS-B could have underestimated the effect of SNAP participation in Hong and Henly.

Of note, the current study found a larger effect of SNAP purchasing power on socioemotional development, specifically approaches to learning and externalizing behavior (effect size varies from 0.43 to 0.47 SD), compared to cognitive development, i.e., early reading and math (effect size varies from 0.23 to 0.25 SD). This closely aligns with the study's finding on the mechanisms, which shows that SNAP purchasing power decreases severe depressive symptoms among mothers, lending a strong support to the stress pathway. On the contrary, findings provide only partial support to the parental learning investment pathway, given that SNAP purchasing power does not statistically significantly increase the number of books at home while improving daily reading time with the child. Based on empirical research that shows that parental stress mediates the effect of income or material hardship on socioemotional development more than cognitive development (Gershoff, Aber, Raver, & Lennon, 2007; Mistry et al., 2008), I may have found greater effects on socioemotional development because of the significant reduction in maternal severe depression, which could have improved parent-child interactions. This aligns with recent evidence from Schmidt, Shore-Sheppard, and Watson (2023) who also found that greater simulated SNAP benefits reduce severe psychological distress. For cognitive development, my results from the test of mechanisms suggest that the effect of SNAP purchasing power may be mediated by increases in daily reading time with the child and/or by reductions in maternal stress (see Price (2010) and Villena-Roldan, & Rios-Aguilar (2012) as examples of studies that found positive effects of increased parental time with the child on cognitive outcomes). Although I did not find that greater SNAP purchasing power leads parents to purchase more children's books, it is still possible that SNAP purchasing power increases parental investment in other educational materials, such as toys, games, school supplies, and in non-educational goods, including clothing and children's furniture. Recent research supports this possibility. For instance, Jackson and Schneider (2022) found that public spending on income support programs (including SNAP) is strongly associated with increased parental investment in children's items, including furniture, clothing, toys, games, educational books, arts and crafts, among families with low-socioeconomic status.

As opposed to socioemotional development and cognitive development, the current study did not find statistically significant effects of SNAP purchasing power on the probability of excellent or very good health status. This result closely aligns with Bronchetti et al. (2020) study, as they also found statistically insignificant effects of SNAP purchasing power on the likelihood of a child's health status being excellent or very good, with the same magnitude as the current study (i.e., -0.01). The plausible explanation for this result can be similar to that suggested by Miller and Morrissey (2021) – that is, it might be a result of a "ceiling effect" given that around 82 percent of children were in very good or excellent general health status in the ECLS-B (see Table 1). Therefore, a marginal increase in SNAP purchasing power may not have been enough to improve child health (i.e., being reported by mothers as having very good or excellent health) in the infrequent cases where children were in poor, fair, or good health. Moreover, the null results could be attributed to the possibility that parents shield young children from the direct effect of food insecurity or reduced food intake. Thus, a marginal increase in SNAP purchasing power may not significantly impact children's general health status. The statistically insignificant effects of SNAP purchasing power on food insecurity, shown in the Mechanisms section, support this possibility as well. Another explanation might be that the effect of SNAP purchasing power on health manifests in a longer term than in a short-term.

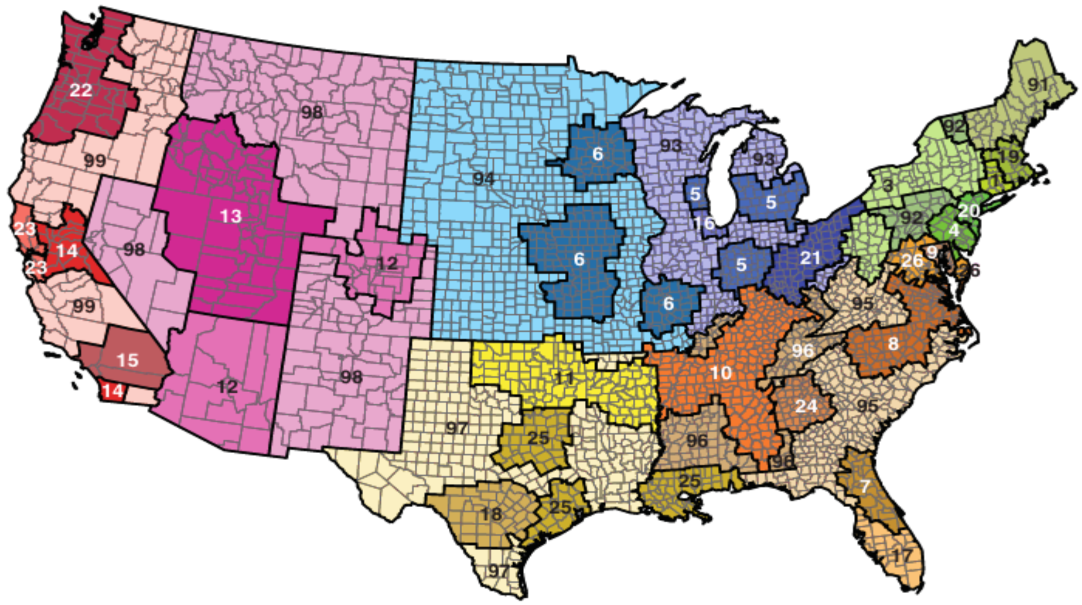
In addition, I show in my main results that two-year lagged, but not one-year lagged, SNAP purchasing power positively affects cognitive and socioemotional development. As mentioned previously, this result may indicate that the impact of SNAP purchasing power on child development takes longer to emerge than what is captured by the one-year lagged measure. However, I do not focus on the specific duration or conclude that it necessarily takes more than one year for SNAP purchasing power to significantly impact child development. Instead, I approach this issue from a measurement perspective. While children's outcomes were assessed across the 12-month period, the SNAP measure captures the average purchasing power in a given year. Hence, compared to children assessed in September to December (which represent 77.5 percent of the primary sample), the time gap between the measurement of the SNAP purchasing power and child outcomes will be shorter for those assessed in January to March (which represent 18.4 percent of the primary sample), suggesting that this group may have diluted any observable one-year lagged effect due to their shorter time lag. However, as some previous studies (e.g., Dahl, & Lochner, 2012) have found contemporaneous effects of family income on children's test scores among older school-age children, it would be important for future research to study the effects of income supports on young children by the timing of their receipt. An alternative reason that I found two-year, but not one-year, lagged effects may concern the child age of the exposure. The two-year lagged measure indicates the level of SNAP purchasing power that a child was exposed to at a younger age than the one-year lagged measure. Since children can be more malleable to economic resources when they are younger, it is possible that my main results were reflective of the age at which a child was exposed to greater SNAP purchasing power.

There are several limitations to this study. First, estimates provided by this paper are short-run effects of SNAP purchasing power on child development. Thus, although prior literature suggests a link between school readiness and later academic achievement and long-term success (Carneiro, & Heckman, 2003; Duncan et al., 2007; Feinstein, & Duckworth, 2006), there is a possibility that the effect of SNAP purchasing power in early childhood to kindergarten-entry period may fade out in later childhood. This is an important area for future research. In addition, the study leaves open the question of estimating full benefits of the SNAP program. I provide a more circumscribed interpretation of what happens to children's development when we make the program more generous by increasing its real value. Nonetheless, given the relatively large magnitude of the observed effect, this study provides hopeful results on the effectiveness of SNAP in improving low-income children's cognitive and socioemotional development and thereby reducing the inequality in child development across socioeconomic gradients. Furthermore, although a series of robustness checks does not find evidence of threats to internal validity, my results may be subject to bias if time-varying correlates of child development – that are also related to changes in SNAP purchasing power – were remaining in the fixed-effects model.

In spite of these limitations, the current study finds a novel insight that greater purchasing power of SNAP benefits improves cognitive and socioemotional development of children living in families with low socioeconomic status. This study also highlights that there are large variations in SNAP purchasing power across regions, which causes a concern for equity. The varying levels of SNAP purchasing power can result in heterogeneous effects of SNAP on child development depending on geography, with children in high-priced areas receiving lower developmental benefits. Hence, by increasing SNAP benefit levels to account for local prices, especially in high priced regions, we can promote equitable effectiveness of SNAP in reducing the negative effects of poverty on children's cognitive and socioemotional development.

APPENDIX A: CHAPTER III APPENDIX

Figure A.1. 35 market groups in Quarterly Food-at-Home Price Database (Reprinted from Todd et al., 2010).



Notes: For 1999-2001, markets 91 and 92 are combined as market 81; markets 93 and 94 are combined as market 82; markets 95, 96, and 97 are combined as market 83; and markets 98 and 99 are combined as market 84.

1	Hartford	19	Boston
2	Urban NY	20	Other NY
3	Western NY/PA	21	Metro Ohio
4	Philadelphia	22	North Pacific
5	Metro Midwest 1	23	San Francisco
6	Metro Midwest 2	24	Atlanta
7	North Florida	25	Metro South 4
8	Metro South 1	26	Washington, DC
9	Baltimore	91	Nonmetro New England
10	Metro South 2	92	Nonmetro Middle Atlantic
11	Metro South 3	93	Nonmetro East North Central
12	Metro Mountain	94	Nonmetro West North Central
13	Salt Lake City	95	Nonmetro South Atlantic
14	Metro California	96	Nonmetro East South Central
15	Los Angeles	97	Nonmetro West South Central
16	Chicago	98	Nonmetro Mountain
17	South Florida	99	Nonmetro Pacific
18	San Antonio		

Notes: This figure is not subject to copyright.

Table A.1. Demographic characteristics by different assessment timing groups.

	2005-2006	2005-2007	2006-2007	2006-2006	2005-2008	2006-2008	Sig.
Two or more children (Ref.: One child) (%)	70.01	70.04	65.57	75.48	66.2	90.47	
Urbanicity (%)							
Rural	14.47	12.34	11.46	13.69	20.47	12.29	

(Table A.1 continued)

Urban	73.19	75.35	84.12	78.84	73.37	85.41	
Urban-cluster	12.34	12.31	4.42	7.48	6.16	2.3	
US-citizen mother (Ref.: Immigrant mother) (%)	88.96	89.42	88.05	80.06	92.91	77.81	
Parent's highest education attainment (%)							+
No high school degree	17.59	22.34	33.28	29.97	21.71	28.19	
High school degree	47.23	42.08	40.08	40.39	33.41	38.82	
Some college	35.19	35.59	26.64	29.64	44.88	32.99	
Child age at the interview (months)	59.12	60.05	63.40	62.25	61.61	62.94	
	(6.86)	(11.21)	(7.44)	(4.87)	(13.96)	(11.34)	
Mother's age (years)	29.34	29.50	29.64	29.25	27.88	28.68	
	(6.84)	(7.62)	(7.27)	(7.28)	(5.33)	(5.75)	
Household size	4.42	4.23	4.21	4.52	4.21	4.91	
	(1.70)	(1.55)	(1.48)	(1.78)	(1.37)	(1.74)	
Received SNAP (%)	57.02	56.44	46.06	43.07	56.11	61.93	
Child race/ethnicity (%)							**
White	29.2	22.63	17.38	19.81	16.95	20.43	
Black	35.97	40.22	32.2	30.34	69.64	31.18	
Hispanic	29.44	27.49	48.01	45.01	9.75	47.38	
Others	5.39	9.65	2.4	4.84	3.67	1.01	
Male focal child (Ref.: Female focal child) (%)	51.93	52.63	37.76	48.35	53.68	58.29	
Sample size of each group	1150	500	150	200	100	50	

Notes: I used the study sample for cognitive outcomes (N=2150). *** p<0.001, ** p<0.01, * p<0.05, + p<0.1

Table A.2. Prior SNAP purchasing power levels (wave 1 and wave 2) by market groups that show an increase and a decrease in SNAP purchasing power from wave 3 to k.

	Market groups with decrease in purchasing power from wave 3 to k	Market groups with increase in purchasing power from wave 3 to k
Average	0.66	0.67
25^{th} pct.	0.62	0.64
Median	0.66	0.67
75th pct.	0.70	0.71
N	900	1250

Notes: I used the study sample for cognitive outcomes (N=2150).

Table A.3. Stepwise regressions in the primary treatment sample.

	M1	M2	M3
Panel 1. Early reading			
1-year lagged log(SNAP purchasing power)	-1.72+	-2.24+	-1.53
	(0.97)	(1.16)	(0.98)
2-year lagged log(SNAP purchasing power)	1.63	3.35+	2.46+
	(1.22)	(1.67)	(1.34)
Panel 2. Early math			
1-year lagged log(SNAP purchasing power)	0.68	-0.52	-0.61
	(0.99)	(0.95)	(0.78)
2-year lagged log(SNAP purchasing power)	2.66*	3.35*	2.33*
	(1.29)	(1.51)	(0.92)
Panel 3. Excellent/very good health			
1-year lagged log(SNAP purchasing power)	0.13	0.07	-0.40
	(0.59)	(0.62)	(0.58)
2-year lagged log(SNAP purchasing power)	-0.25	-0.24	-0.11
	(0.65)	(0.69)	(0.68)
Panel 4. Approaches to learning			
1-year lagged log(SNAP purchasing power)	-1.41	-1.72	-2.30
	(1.23)	(1.41)	(1.67)
2-year lagged log(SNAP purchasing power)	3.66*	4.78*	4.30*
	(1.50)	(2.07)	(2.05)
Panel 5. Interpersonal skills			
1-year lagged log(SNAP purchasing power)	1.91	0.66	-0.05
	(1.53)	(1.90)	(1.81)
2-year lagged log(SNAP purchasing power)	0.98	2.25	1.85
	(1.57)	(2.13)	(1.99)
Panel 6. Externalizing behavior			
1-year lagged log(SNAP purchasing power)	-1.89	-2.62	-3.17
	(1.57)	(1.66)	(1.90)
2-year lagged log(SNAP purchasing power)	3.41+	5.20**	4.72*
	(1.85)	(1.75)	(1.96)
Lag2 state/county and price controls	Y	Y	Y
Lag1 state/county and price controls		Y	Y
Child/family controls			Y

Notes. In M1, only 2-year lagged state, county, and housing price control variables are adjusted; in M2, their 1-year lagged terms are added; and in M3, child/family covariates are added. In all models, child and year fixed effects are controlled. *** p<0.001, ** p<0.01, * p<0.05, + p<0.1

Table A.4. Effects of SNAP purchasing power on cognitive development by female and male children.

	Reading		Math	
	Female	Male	Female	Male
1-year lagged log(SNAP purchasing power)	-2.50	-0.80	-0.02	-1.05
	(1.56)	(2.27)	(1.43)	(1.77)
2-year lagged log(SNAP purchasing power)	3.00*	2.65	2.19+	2.49*
	(1.41)	(1.62)	(1.21)	(1.04)
Z-score on 2-year lagged SNAP purchasing power	0.016		0.19	
Child fixed effects	Y	Y	Y	Y
Year fixed effects	Y	Y	Y	Y
Child and family characteristics	Y	Y	Y	Y
Regional economic and policy conditions	Y	Y	Y	Y
Housing price (HUD's FMR)	Y	Y	Y	Y
N	1100	1050	1100	1050

Notes: Z-score is derived from the statistical test for the equality of female/male coefficients. I use a formula suggested by Paternoster, Brame, Mazerolle, & Piquero (1998). *** p<0.001, ** p<0.01, * p<0.05, + p<0.1

Table A.5. Falsification tests: Effect of SNAP purchasing power on income/poverty and SNAP participation.

	Income to needs ratio	Household income ($2021)	SNAP participation
2-year lagged log(SNAP purchasing power)	0.02	2,004.94	0.55
	(1.92)	(53,835.56)	(0.41)
N	2150	2150	2100

Notes: I used the study sample for cognitive outcomes. Income to needs ratio and household income indicate a family's income/poverty status in a year prior to the year of interview. The coefficient on 2-year lagged log(SNAP purchasing power) is only shown in this table; but the same set of covariates that was controlled in my main model (child fixed effects, year fixed effects, child/family time-varying covariates, county/state time-varying economic and policy conditions, and housing price, i.e., HUD's FMR) and 1-year lagged log(SNAP purchasing power) were controlled in these models. *** p<0.001, ** p<0.01, * p<0.05, + p<0.1

Table A.6. Effect of two-year lagged SNAP purchasing power on child development.

	Reading	Math	Health	AL	IP	EB
2-year lagged log(SNAP purchasing power)	3.17*	2.61**	0.07	5.30*	1.88	6.10***
	(1.35)	(0.92)	(0.67)	(1.97)	(2.11)	(1.57)
2-year lagged: 10% increase	0.32	0.26	0.01	0.53	0.19	0.61
Mean of outcome	-0.40	-0.42	0.81	-0.31	-0.09	-0.31
N	2150	2100	2350	1100	1100	1100
Child fixed effects	Y	Y	Y	Y	Y	Y
Year fixed effects	Y	Y	Y	Y	Y	Y
Child and family characteristics	Y	Y	Y	Y	Y	Y
Regional economic and policy conditions	Y	Y	Y	Y	Y	Y
Housing price	Y	Y	Y	Y	Y	Y

Notes: 'AL' indicates approaches to learning; 'IP' indicates interpersonal skills; 'EB' indicates externalizing behavior. *** p<0.001, ** p<0.01, * p<0.05, + p<0.1. In these models displayed in Table A.6, one-year lagged SNAP purchasing power has been excluded from the models shown in Table 3.2.

Table A.7. Effect of SNAP purchasing power on child development using a sample of families who did not move between birth and wave 2.

	Reading	Math	Health	AL	IP	EB
1-year lagged log(SNAP purchasing power)	-0.83	-1.35	-0.04	-1.71	-1.52	-3.90+
	(1.31)	(0.98)	(0.70)	(2.26)	(2.28)	(2.13)
2-year lagged log(SNAP purchasing power)	2.21	1.92*	-0.12	4.96*	2.55	5.69**
	(1.45)	(0.92)	(0.69)	(1.88)	(2.33)	(1.90)
2-year lagged: 10% increase in SNAP purchasing power	0.22	0.19	-0.01	0.50	0.26	0.57
N	2200	2200	2450	1150	1150	1150
Child fixed effects	Y	Y	Y	Y	Y	Y
Year fixed effects	Y	Y	Y	Y	Y	Y
Child and family characteristics	Y	Y	Y	Y	Y	Y
Regional economic and policy conditions	Y	Y	Y	Y	Y	Y
Housing price	Y	Y	Y	Y	Y	Y
Other local prices (regional CPIs)	Y	Y	Y	Y	Y	Y

Notes: 'AL' indicates approaches to learning; 'IP' indicates interpersonal skills; 'EB' indicates externalizing behavior. *** p<0.001, ** p<0.01, * p<0.05, + p<0.1

CHAPTER IV: COMPLEMENTARY EFFECTS OF INCOME SUPPORT POLICIES ON CHILD DEVELOPMENT: EVIDENCE FROM EITC AND SNAP

Introduction

In the United States, 14.4 percent of children (under age 18) lived under poverty in 2019. This is one-and-a-half times higher than the adult (age 18-64) poverty rate (9.4 percent in 2019), reflecting 10.5 million children living in poverty (Semega, Kollar, Shrider, & Creamer, 2020). This is concerning because poverty, particularly in early childhood and early school years, can have negative consequences on children (Duncan et al., 1998; Smith, Brooks-Gunn, & Klebanov, 1997). Descriptive evidence suggests that there are large income-based gaps in children's development, including math, literacy, and language (Waldfogel, & Washbrook, 2011), socioemotional skills such as approaches to learning and externalizing behavior (Fletcher, & Wolfe, 2016), as well as health (Pearce et al., 2019). Such developmental gaps widen over the course of childhood and translate into larger gaps in adult outcomes, such as earnings (Cunha, Heckman, Lochner, & Masterov, 2005; Duncan, Ziol-Guest, & Kalil, 2010).

There are public investments to offset economic hardships and improve the well-being of low-income families and children. The largest means-tested cash or near cash program in the U.S. is the Supplemental Nutrition Assistance Program (SNAP). SNAP is a monthly in-kind benefit that allows low-income (working and non-working) populations to purchase food. By offsetting food costs, SNAP frees up a family's economic resources, and empirical research supports that SNAP is treated as near-cash[1] (Hoynes, & Schanzenbach, 2009; Bruich, 2014).

[1] While a few studies report that a marginal propensity to consume food out of SNAP benefit income is higher than it is out of cash income (Beatty, & Tuttle, 2015; Hastings, & Shapiro, 2018), Hoynes and Schanzenbach (2009) and Bruich (2014) conclude from their study that households respond similarly to one dollar in cash income and one dollar in SNAP.

Following SNAP, the second largest cash or near cash program is the Earned Income Tax Credit (EITC). [2] The EITC, a refundable tax credit for low-to-moderate income working populations, provides cash benefits in a lumpsum payment when taxpayers file their income tax returns. Since the EITC is "refundable," the Internal Revenue Service (IRS) refunds the remaining portion of the EITC benefits that exceed a taxpayer's tax liability. A low-wage worker with no tax liability can also receive the benefit (Marr et al., 2015). As such, EITC and SNAP are one of the largest, differently structured, anti-poverty programs that provide income supports to families.

Despite their different program structures, EITC and SNAP share several commonalities. First, EITC and SNAP are both liquid programs that increase economic resources among families (SNAP may be less liquid than EITC due to its near-cash nature). This quality differentiates them from Medicaid, which is another key component of the safety net but with less liquidity (Hardy et al., 2018). Second, children are the largest beneficiaries of the EITC and SNAP. In Fiscal Year (FY) 2020, 65 percent of the SNAP recipients were households with children (United States Department of Agriculture, 2022), while in tax year 2020, 71 percent of the EITC returns were received by households with children (Internal Revenue Service [IRS], 2023). Third, EITC and SNAP have overlapping target populations, and therefore, there is a high joint participation rate in these programs. For instance, based on the 2008-2009 Survey of Income and Program Participation data, Moffitt (2020) shows that among the major tax and transfer programs, the most common receipt among SNAP participants is of EITC benefits, excluding Medicaid. Of the non-elderly and non-disabled SNAP families, 53 percent likely

[2] Program costs can be found in United States Department of Agriculture (2021) and Internal Revenue Service (2021).

received EITC benefits.[3] Moffitt finds that this percentage further increases to 95 percent for SNAP families with income between 50 percent and 100 percent of the poverty line, which is likely because the EITC, by design, excludes the lowest-income families (Hoynes, & Patel, 2018; Moffitt, 2020). Similarly, using more recent data in 2019, Macartney and Ghertner (2023) demonstrate that EITC and SNAP represent one of the most common program bundles received by people who participate in multiple safety net programs In addition, EITC and SNAP are unique program bundles in that participating in one program does not *directly* affect one's eligibility and benefit amounts for the other program. This is because EITC benefits are refundable tax credits – which are not generally countable income and resources considered in SNAP eligibility and benefit determination (Moffitt, 2020) – and SNAP benefits are not earned income and therefore are not included in calculation of EITC eligibility and benefit amounts.[4]

Taking these into account, the current study considers how a combination of these two important income support policies in the U.S. – EITC and SNAP – contributes to reducing the negative effects of poverty on child development during preschool to kindergarten-entry period.[5] I consider various child outcomes, including cognitive and socioemotional development, and general health status. To evaluate this question, I test a plausibly causal interaction effect

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[3] Since SIPP does not report EITC participation status, Moffitt (2020) used NBER TAXSIM to calculate the percentage of EITC eligibility, assuming 100 percent participation.

[4] As a more indirect pathway, it is possible that EITC and SNAP benefits affect one another through the labor supply increase or decrease. I discuss this issue more in-depth in the discussion section.

[5] Other safety net programs that can be important for children include Child Tax Credit (CTC), WIC as well as SSI. CTC was significantly expanded through 2021 American Rescue Plan, albeit temporarily; however, CTC was a relatively small program within the study period and was not fully refundable at that time, thereby limiting people's ability to benefit from this program (Hoynes & Schanzenbach, 2018). In addition, although WIC and SSI play an important role for low-income families by providing food vouchers and cash benefits, respectively, they are smaller programs in terms of budget costs (Hoynes & Schanzenbach, 2018). Thus, this paper focuses on EITC and SNAP as primary programs of interest.

between the *generosity* of EITC and SNAP benefits among families who likely participate in both programs – that is, whether the EITC is more effective at improving children's development when it is coupled with greater SNAP benefits and vice versa, i.e., whether the SNAP is more effective at improving children's development when it is supported by greater EITC benefits. There can be three possibilities in the relationship between EITC and SNAP: complementary (i.e., the effect of receiving larger EITC benefits on child development *increases* when families receive larger SNAP benefits and vice versa); substitution (i.e., the effect of EITC on child development *decreases* as the level of SNAP benefits increases and vice versa); and no interactions (i.e., SNAP and EITC independently affect child development).

Previous research has examined the individual effects of EITC and SNAP on children's well-being, finding positive effects on several child outcomes in one or both programs: cognitive development (e.g., test scores) among school-age children (Dahl, & Lochner, 2012; Bastian, & Michelmore, 2018; Chetty et al., 2011; Maxfield, 2015; Frongillo, Jyoti, & Jones, 2006; Gassman-Pines, & Bellows, 2018), short-term improvements in child behavior (Hamad, & Rehkopf, 2016), and education attainment (Bastian, & Michelmore, 2018; Maxfield, 2015). Also, prior research has found that the EITC or SNAP improves birth weight (Almond, Hoynes, & Schanzenbach, 2011; East, 2020; Strully et al., 2010; Hoynes et al., 2015; Markowitz et al., 2017) and child general health status (East, 2020; Hamad, Collin, & Rehkopf, 2018), although a few other studies have found opposite or statistically insignificant results on birth weight and general health (Bronchetti, Christensen, & Hoynes, 2019; Miller, & Morrissey, 2021; Currie, & Moretti, 2008; Bruckner et al., 2013).

Given the existing literature, this study makes two unique contributions. First, to my knowledge, this paper is the first study to examine whether EITC and SNAP benefits interact to affect diverse developmental outcomes. Given the high joint participation rate in EITC and SNAP, it is important to estimate their potential interaction effects, therefore, their overall benefits on children's well-being. To the extent that EITC and SNAP have complementary effects on child development, simply evaluating them, separately, will lead to an underestimation of their effects. Second, there remains limited quasi-experimental research, particularly on the effects of EITC and SNAP on diverse domains of child development (e.g., cognitive and socioemotional, and general health) in early childhood and early school years. These early periods are important to investigate because they are critical developmental stages known to be most sensitive to family income's effects on child development (Duncan et al., 1998; Smith, Brooks-Gunn, & Klebanov, 1997) and early development has the potential to affect later academic achievement and long-term success (Carneiro, & Heckman, 2003; Duncan et al., 2007; Feinstein, & Duckworth, 2006). To my knowledge, fewer studies have focused on the effects of EITC on diverse domains of development during early childhood or early school years (although a few studies examined the effect of EITC on test scores, primarily focusing on older children, e.g., see Bastian, & Michelmore, 2018). Similarly, only few studies have examined the impact of being exposed to SNAP in early childhood to early school years on non-health development (Hong, & Henly, 2020; Hoynes, Schanzenbach, & Almond, 2016).

Identifying the interaction effect between two programs requires exogenous variations in both programs, so that one can credibly identify the individual effect of each program. In addition, it also requires that each of the programs is independent of the other (Almond, & Mazumder, 2013). Considering that observable and unobservable characteristics (e.g., earnings) correlated with the actual SNAP and EITC benefits received by families may also be related to children's developmental outcomes, actual SNAP and EITC benefit amount may be endogenous to child development. Therefore, to satisfy those two conditions, I construct exogenous annual measures of EITC generosity, defined as the maximum federal and state EITC benefits that a child's family could receive given the year, state of residence, and number of children in the household. Then, to address the endogeneity of SNAP benefits, I draw on a novel measure of annual SNAP purchasing power that was first used by Bronchetti and colleagues (2019). Unlike the EITC program, SNAP does not have variation in benefit amounts across states. However, there are large variations in the real value, or purchasing power, of SNAP benefits across place and over time, since the SNAP benefit level is not adjusted for differences in local costs of living, such as food prices, in the contiguous U.S. (48 states and District of Columbia (D.C.)). Using longitudinal data on children, I use a child fixed effects approach by leveraging within child variation in exposure to different levels of SNAP purchasing power and maximum EITC benefits based on place, year, and number of children in household. I conduct multiple falsification and robustness tests, and they support that my findings indicate plausibly causal interaction effects between the EITC and SNAP benefits.

Results show that EITC and SNAP generosity have complementary effects on cognitive development (early reading and early math scores) and socioemotional development, particularly externalizing behavior. I find that the effect of a $1000 increase in the maximum (federal and state) EITC benefits (in $2021) on early reading and early math scores significantly increases from 0.06 SD to 0.14 SD and from 0.01 SD to 0.13 SD, respectively, as the level of SNAP purchasing power increases by 10 percent from the mean. I also find that the effect of a $1000 increase in the maximum EITC benefits on externalizing behavior significantly increases from 0.30 SD to 0.46 SD with a 10 percent increase in SNAP purchasing power from its mean. Results from the test of mechanisms suggest that the complementary effect on child development may be partly driven by complementary changes in daily reading time with the child. Taken together, the current study sheds light on the overall effectiveness of the two largest income support policies in the U.S. in mitigating the adverse effects of poverty on child development, thereby narrowing developmental disparities across socioeconomic status (SES) to some extent.

Background

Earned Income Tax Credit

The EITC is a federal tax credit and a wage subsidy for low-to-moderate income working families with children, though a small EITC is available to working individuals without children. EITC has had strong anti-poverty effects. In 2018, the EITC lifted about 5.6 million people above the poverty line, including nearly 3 million children, based on the Supplemental Poverty Measure (SPM) (Center on Budget and Policy Priorities, 2023). According to the recent IRS data, for tax year 2020 (i.e., 2020 income tax returns filed in 2021), 26 million taxpayers (i.e., the number of returns) received about $59 billion in EITC. In that year, 96 percent of all EITC dollars were received by families with children (Crandall-Hollick, Falk, & Boyle, 2023; IRS, 2023). The average federal EITC benefit was $3099 for families with children and $295 for individuals without children (IRS, 2023). The EITC benefit has a trapezoidal structure, containing a phase-in region (where the credit increases at a certain rate (“credit rate”) as a function of earned income); a plateau region (where the credit reaches the maximum benefits and does not change with earnings); and a phase-out region (where the credit decreases as a function of earned income). Specific parameters of the federal EITC structure further vary by the number of children residing in the household and marital status. For instance, the credit rate and the maximum EITC benefit are higher among families with more children. The maximum earning limit also varies; for instance, in 2023, depending on the number of children and filing status (single vs. joint), the EITC was completely phased out once the taxpayer's adjusted gross income reached the limit of $46,560 to $63,398 (Crandall-Hollick, Falk, & Boyle, 2023).

Enacted in 1975, the EITC started as a modest tax credit. Since then, there have been several expansions in the federal EITC in 1986, 1990, 1993, and 2009. In 1986, the credit rate increased, and then in 1990 and 1993, the credit further increased, with a larger benefit for families with two or more qualifying children than those with one qualifying child. Also, in 1993, a small EITC was created for individuals with no qualifying children. In 2009, a larger credit was introduced for families with three or more children compared to families with fewer children through the American Recovery and Reinvestment Act (ARRA).

In addition to the federal EITC, beginning in 1986, states started to implement and expand (sometimes removed, and reduced) their own EITC programs. As of tax year 2023, 31 states plus D.C. implemented their own EITC programs. In several states, EITCs are non-refundable, while in other states, they provide refundable EITCs. State EITCs also vary substantially in terms of generosity. In tax year 2023, the amount of state credit – which is generally expressed in terms of the percent of federal EITC (i.e., state credit rate) – ranged from 70 percent in D.C. to 3 percent in Montana among states with a refundable state EITC (Urban Institute, 2023). During the years that I examine in this study, spanning tax years from 2002 to 2006, nine states have changed their state EITC generosity by either adopting a state EITC or expanding their state credit rate (see Table B.1 for specific details of these states in Appendix B).

Supplemental Nutrition Assistance Program

SNAP was first piloted in 1961 and became a nationwide program by 1975. SNAP has also played a significant role in reducing poverty; in 2015, the program lifted 8.4 million people from poverty, as measured by the SPM, decreasing the poverty rate from 15.4 percent to 12.8

percent (Wheaton, & Tran, 2018). In FY 2020, about 20.5 million households (39.9 million individuals) participated in SNAP in an average month and 65 percent of those individuals lived in a household with children. The total federal cost of the program was $79.3 billion, $74.2 billion of which went to SNAP benefits. The average monthly SNAP benefit across all participating households was $230 in the pre-pandemic period of FY 2020 and $302 in both the pre- and post-pandemic period of FY 2020. Eligibility for SNAP is typically determined by three tests, although there are exceptions to some types of households (e.g., households in which a member is disabled or is aged 60 or above) and people who are categorically eligible for SNAP. The three tests include (1) gross income test, which requires households to have monthly gross income below 130 percent of the federal poverty line (FPL) (e.g., $39,000 for a family of four in 2023); (2) net income test, which requires gross income minus deductions (e.g., deductions related to dependent care, earned income, child support, medical and excess shelter expenses) to be below 100 percent of FPL; (3) and asset test that requires households' assets to be below a certain value. For households who are certified to be eligible, their SNAP benefits are calculated by subtracting 30 percent of a household's net income from the maximum benefit amount to which they are entitled. The maximum SNAP benefit only varies by household size in the contiguous U.S. (United States Department of Agriculture, 2022). Thus, unlike other means-tested programs whose benefit levels vary across states (such as the EITC), the SNAP benefit formula is fixed in the contiguous U.S.

How might the EITC and SNAP be Used Differently?

Since the EITC and SNAP have different program structures, families might use them for different purposes. As the EITC is received in lump sum once a year, the EITC might be differently spent than monthly transfers like SNAP. For example, evidence shows that lump-sum transfers are more likely to be spent on durables compared to monthly transfers (Haushofer, & Shapiro, 2016). Research demonstrates that the purchase of durable goods, such as furniture, home appliances (e.g., washing machines, dish washers), and cars, or repairing cars, are common uses for EITC refunds (Romich, & Weisner, 2000; Mammen, & Lawrence, 2006). Other common categories of EITC usage are paying off bills and debts, child-related expenses (e.g., clothing, school supplies, toys), asset building (e.g., housing purchase or improvement and savings), human capital investments (e.g., school tuition), and purchase of other non-durables (e.g., food) (Romich, & Weisner, 2000; Mammen, & Lawrence, 2006; Mendenhall et al., 2012; Jones, & Michelmore, 2018). In contrast, SNAP – given its monthly benefit nature – is more likely to increase families' current consumption on non-durables, rather than supporting their purchase of durables or asset building. Previous studies found that SNAP benefits contribute to both food and nonfood essential expenses, including paying off bills related to housing (rent), utilities, and medical costs (Shaefer, & Gutierrez, 2013), while also reducing families' debt burdens (Kim, & Wilmarth, 2016).

How would the EITC and SNAP Affect Child Development?

Although their specific usage may be slightly different, EITC and SNAP are both income supports that increase economic resources among socioeconomically disadvantaged families with children. The parental investment perspective and the family stress perspective, which have emerged as the two main frameworks in the literature on income effects on child development, support the "income pathway" as one potential mechanism through which the EITC and SNAP may benefit child development. The parental investment perspective suggests that greater income would increase the level of parental investment in the child in the form of money and time, such as engaging in enriching activities with the child (e.g., reading with the child) and purchasing cognitively stimulating materials and activities for the child (e.g., books, toys, lessons that can enhance children's skills) (Becker, 1991; Duncan et al., 2014; Yeung et al., 2002). The family stress perspective suggests that greater income would reduce parents' stress, which may result in more responsive and supportive parenting practices and better quality of parent-child interactions (Conger, & Elder, 1994; Duncan et al., 2014; Masarik, & Conger, 2017). According to these theories, income increases due to the EITC and SNAP may positively affect child development in preschool to kindergarten-entry by increasing parental investments in the child and reducing parental stress level.

However, the theoretical effect of EITC on child development in preschool to kindergarten-entry is more ambiguous because of its role as a work incentive. Studies have found particularly strong labor supply effects of the EITC among single mothers (Eissa, & Liebman, 1996; Adireksombat, 2010).[6] Although most empirical research have found positive effects of the EITC on child well-being in school age (e.g., Dahl, & Lochner, 2012), the effect of the EITC's labor supply response on child development is theoretically ambiguous, particularly in early childhood. On the one hand, a working mother brings more income to families (the "income pathway" described in the previous paragraph), and thus, maternal labor market participation can be beneficial for the child. On the other hand, increased working due to the EITC may reduce time spent with the child or may induce the child to attend low-quality childcare. For instance, Bastian and Lochner (2021) find that the EITC reduces time spent with

[6] Unlike EITC, the labor supply effect of SNAP participation is theoretically ambiguous and prior empirical research on SNAP shows mixed findings (Farkhad, & Meyerhoefer, 2018; Bitler, Cook, & Rothbaum, 2021). It is theoretically ambiguous because on the one hand the labor supply theory predicts that SNAP can generate work disincentives, while on the other hand, SNAP's work requirement may create work incentives, especially among those who are subject to general work requirement (Farkhad, & Meyerhoefer, 2018).

the child. However, none of this reduction was related to time devoted to developmentally enriching activities. The paper by Bastian and Michelmore (2018) found a similar result of decreasing effects on time spent with the child due to the EITC, but this effect was statistically insignificant and small. Findings by Michelmore and Pilkauskas (2021) indicate that the labor supply response to EITC is larger among mothers whose youngest child is aged 5 or below (with the largest effect for infants and toddlers), while showing that many infants and toddlers – but not older children – are moved into informal care, which is typically linked with lower quality compared to center care. Moreover, the literature on maternal labor supply suggests that maternal labor supply can have negative consequences on child development during the first few years of a child's life, especially during infancy, while it may be beneficial afterwards (Baum II, 2003; Kopp, Lindauer, & Garthus-Niegel, 2023; Waldfogel, Han & Brooks-Gunn, 2002; Brooks-Gunn, Han, & Waldfogel, 2002). Therefore, the labor supply effect of EITC on child development in preschool to kindergarten entry is not entirely clear, although it appears to lean more towards the positive side. Taken together, compared to the effect of the SNAP, the effect of the EITC on child development is more of an open empirical question.

How would EITC and SNAP Interact to Affect Child Development?

The primary focus of this paper is to test how the effect of one program may change when the other program's generosity increases. Assuming the exogeneity of EITC and SNAP benefits (which I consider in my empirical strategy), i.e., comparing otherwise similar families who receive greater or lower EITC and SNAP benefits, there are three possible scenarios on their interaction effects: complementary, substitution, or additive (independent) relationships. As defined in the introduction, when EITC and SNAP serve as complements, the effect of receiving larger EITC benefits on child development *increases* when families receive larger SNAP benefits and vice versa – i.e., the effect of receiving larger SNAP benefits on child development *increases* with larger EITC benefits. When EITC and SNAP serve as substitutes, the effect of EITC on child development *decreases* as the level of SNAP benefits increases and vice versa. When they are in an additive relationship, they simply work independently from one another. In this section, for the sake of simplicity, I will provide examples that demonstrate the case in which the effect of EITC on child development increases or decreases as the level of SNAP benefits changes. However, a similar logic applies to the other case in which the effect of SNAP benefits on child development changes as the level of EITC changes.

The complementary effects might occur through various mechanisms. In relation to the parental investment perspective, one potential mechanism can include how much and in what way parents would spend the EITC refunds or the EITC-induced earnings (both referred to as "EITC income" in this section) on their children, both directly and indirectly, at different levels of SNAP benefits. It is possible that receiving greater SNAP benefits allows parents to spend a larger portion of EITC income on children, compared to otherwise similar families who receive lower SNAP benefits. The specific ways in which these dynamics occur may depend on families' differential situations and their preferences. For example, research shows that regular government benefits, such as SNAP, improve financial security through a reduction of debt burden (Kim, & Wilmarth, 2016). Thus, if greater SNAP benefits reduce household debt, this may "free up" a greater portion of EITC income that parents can use to purchase children's items that may developmentally benefit children, such as books, games, and toys (Mendenhall et al., 2012), compared to otherwise similar families with smaller SNAP benefits who might have to use that portion of the EITC to pay off bills and debt. Also, with larger SNAP benefits, the "freed up" portion of EITC, especially EITC refunds, may be used to purchase durables, including cars and home appliances such as washing machines and dish washers, or to repair cars, which are common ways to spend the EITC refunds (Romich, & Weisner, 2000; Mammen, & Lawrence, 2013). These purchases can indirectly benefit children by allowing parents to save time and do more investment activities with children, such as reading books together. Morrissey (2023), for example, finds that EITC outlays predicted increases in the time spent reading to or with children, particularly young children. Furthermore, it is possible that SNAP benefits mitigate any potentially negative effects of the EITC on child development, those arising from its positive impact on labor supply. As described in the previous section, the effect of EITC on younger children's development is an empirical question and possibly, may vary at different levels of SNAP value. Given that the EITC may increase working and increased working may reduce time spent with children, the EITC may have a small positive effect or even a negative effect on child development, particularly at low levels of SNAP benefits. However, as the level of SNAP benefits increases, a greater portion of the EITC income may be available to purchase necessary goods that serve as time savers for working mothers (e.g., cars and home appliances). Having access to these goods might partially compensate for the lost time with their children, thereby enhancing the effect of EITC on child development as SNAP benefits increase.

In addition, the complementary effect may also work through the family stress pathway. For example, as the family stress perspective would suggest, receiving SNAP and EITC benefits may reduce parents' stress level. Empirical research also supports the positive effects of EITC and SNAP (individually) on mental health outcomes including reductions in stress and depressive symptoms (Schmidt, Shore-Sheppard, & Watson, 2023; Boyd-Swan, Herbst, Ifcher, & Zarghamee, 2016). Hence, it is plausible that the EITC and SNAP lead to complementary reductions in stress level for families – meaning that the EITC contributes to a *greater* decrease in parental stress levels when families receive larger SNAP benefits, and vice versa.

Moreover, there is a possibility that the complementary effect arises from SNAP and EITC benefiting different domains of child development and creating a dynamic complementarity between them. It might be that public investment in one domain (e.g., health) is more beneficial when coupled with other domains (e.g., education) (Jackson, & Fanelli, 2023). For instance, if SNAP improves health, children whose health improves due to SNAP may derive a greater return from parents' investments in home and childcare environments using EITC income (e.g., reading more to the child and purchasing developmentally enriching toys and better quality childcare). This will increase the effectiveness of EITC in benefiting child development as the level of SNAP benefits improves. However, since children's exposures to these programs are measured in the same year in this study, the possibility of dynamic complementarity is less likely compared to the possibilities I describe above.

Taken together, although not exhaustive, the above examples demonstrate that under the complementarity between EITC and SNAP, which can occur through various mechanisms, one income support program would be particularly effective at improving child development when it is supported by another income support program.

However, it is possible that EITC and SNAP do not work complementarily. Receiving greater SNAP benefits may not necessarily lead parents to spend their EITC income in a way that benefits children to a greater degree (either directly or indirectly). But instead, parents might use the portion of EITC income that are freed up by greater SNAP benefits for their own leisure or activities that do not involve children, or personal needs and household improvements that do not affect child development. In this case, EITC and SNAP may not interact to affect child development (additive relationships), or to a lesser extent, EITC and SNAP may be in a substitution relationship, particularly if parents allocate a smaller portion of their EITC income to children or spend less time with them (e.g., if parents socialize with their friends) when they receive larger SNAP benefits.

Prior Research

Most prior studies have focused on studying a single program's effects on child well-being. There is an emerging literature that investigates how childhood exposure to multiple investments interacts to affect child well-being. However, this literature is scarce and does not examine the complementary effects of EITC and SNAP. The few studies that do exist provide evidence of long-run complementary effects between multiple educational investments. For example, Johnson and Jackson (2019) studied the interaction effects between Head Start and K-12 school spending, induced by School Finance Reform. They found that for poor children, exposure to a typical Head Start center in early childhood has positive effects on education attainment and wages, as well as reduces their likelihood of being incarcerated and poor as an adult, only when the Head Start exposure is followed by greater K-12 spending, and vice versa. Furthermore, Ansari and Pianta (2018) show that there is a positive association between high-quality childcare in early childhood and children's math, language, and literacy skills at age 15 only among children who subsequently experience higher quality classroom environments during the elementary school years. Results from both studies suggest that a given educational investment works best when it is followed by another educational investment and the importance of continued investments in children. On the contrary, there is also a study that finds no clear evidence of complementary effects between an educational investment and an income support program. Carter (2020) found a statistically insignificant interaction effect between childhood exposure to SNAP and preschool-age exposure to Head Start on labor market and health outcomes in adulthood. Carter concludes that there is no clear evidence that the long-run return to childhood access to SNAP varies by the availability of Head Start at preschool age, although Carter could not rule out the possibility of non-zero interaction effects between the two programs with any reasonable degree of certainty. The current study adds a novel contribution to this small, but emerging literature, by examining interaction effects between two income support programs on children's cognitive, socioemotional, and health outcomes in preschool to kindergarten-entry years.

Current Study

The current study tests the interaction effect between EITC and SNAP and conducts a suggestive test of mechanisms through which the interaction effect occurs. As described in the background section, I consider three possibilities in their interaction effects and empirically test which of these three possibilities holds true. Without prior research as guidance, I do not develop a specific hypothesis.

Data and Measures

Early Childhood Longitudinal Study – Birth Cohort data

Data come from the Early Childhood Longitudinal Study – Birth Cohort (ECLS-B) data. The ECLS-B follows a nationally representative cohort of over 10,000 children born in 2001 from birth through kindergarten entry. 10,700 children were surveyed in the first wave of data collection (9 months old, interviewed in 2001-2022), and then they were followed through wave 2 (2 years of age, interviewed in 2003–2004), wave 3 (the preschool year, i.e., 4 years of age, interviewed in 2005–2006), wave 4 (kindergarten-entry age, interviewed in 2006–2007), and wave 5 (kindergarten-entry age, interviewed in 2007–2008). In wave 5, specific groups of children were only invited to take the survey, which included children who did not enter kindergarten in wave 4, children who were repeating kindergarten in this wave, and twins of these children (Snow et al., 2009). ECLS-B is particularly suitable for this study since it contains a rich set of family and child characteristics reported by the parent respondent – mostly mothers (hereafter, the survey respondent and the parent are used interchangeably) – regional information such as state of residence and county of birth, as well as cognitive and socioemotional development outcomes that were measured through direct assessments by the home interviewer and were reported by teacher/childcare provider. Also, there are no other nationally representative survey datasets that have followed a newer birth cohort and provide those similar advantages.[7] As recommended by the ECLS-B (Najarian, Snow, Lennon, & Kinsey, 2010; Snow et al., 2009), the current study combines wave 4 and wave 5 to create a kindergarten-entry wave ("wave k"), which is nationally representative of the 2001 birth cohort children at their kindergarten entry period. I use wave 3 and wave k, as most of the outcomes of interest were measured only in those waves.

As described earlier, the current study's population of interest is families who participate in both EITC and SNAP. Thus, ideally, it would be best to have access to information on EITC and SNAP participation status, but only information on SNAP status is available from the ECLS-B. Based on the evidence that there is a high rate of EITC-eligible families among SNAP families (Moffitt, 2020; see the introduction for details), the primary study sample includes families who reported at both wave 3 and wave k interviews that they received SNAP in the past one to two years.[8] Given that the EITC is only for working families, it is less likely to serve the

[7] See footnote 2 in Chapter 3.

[8] To be more exact, the ECLS-B interview asked whether the respondent or any family members received SNAP

lowest income families (e.g., those in <50 percent of FPL). Thus, as a robustness check, I conduct subgroup analyses among families above 50 percent of FPL and those in 50-100 percent of FPL, and families in which the mother or the resident father was working (see the Falsification and Robustness Tests section). Furthermore, I also consider an alternative sample (children with unmarried parent(s) who have a high school degree or below) and results are reported in Table B.3. In conforming to NCES guidelines for the ECLS-B, the sample sizes were rounded to the nearest 50.

I restrict my primary sample in a few ways (N=2600). First, I drop children whose family moved to different states between the focal child's birth and wave k (N=2250). In the ECLS-B, county of residence is not collected during interviews, while state of residence and county of birth information are available. Thus, I make this exclusion to minimize measurement error of the focal child's county and state of residence during wave 3 and wave k.[9] The county and state information are important as they are used to merge the ECLS-B data with the SNAP and EITC measures, respectively. Second, I limit my sample to children living in contiguous U.S. states (thereby excluding Alaska and Hawaii) and those with birth county information (N=2150). This is because the SNAP purchasing power data cannot be obtained in Alaska and Hawaii, since their regional TFP price cannot be estimated using existing data (see the SNAP and EITC Generosity Measures section for details). Lastly, I restrict to children who have non-missing information on covariates (N=2050) and key dependent variables, resulting in a study sample of 1850

since a child turned age 2 (in wave 3) and age 4 (in wave 4) or age 5 (in wave 5).

[9] By restricting to families who did not move across states since the focal child's birth, this study is assuming that families who did not move across states would have remained in the same market group. Families could have moved to a different county since the child's birth, but as long as they moved to a county that falls into the same market group, it would not affect the accuracy of capturing families' exposure to SNAP purchasing power.

observations (900 children) for cognitive outcomes, 2000 observations (1000 children) for general health, and 850 observations (400-450 children) for socioemotional outcomes. The sample size is smaller for socioemotional outcomes since not all children were eligible to be assessed on teacher- or childcare provider-reported socioemotional development.

Child Development Outcomes

To measure cognitive development, early reading and early math scores are used, which were measured through direct assessments by the home interviewer. The longitudinal scale of early reading and early math scores was created in wave 3 to wave 5 but not in earlier waves. The early reading measure was developed based on validated, standardized instruments such as the Preschool Language Assessment Scale (PreLAS) 2000, Peabody Picture Vocabulary Test (PPVT), and Preschool Comprehensive Test of Phonological and Print Processing (PreCTOPPP). Early reading skills capture both language and literacy skills, such as English language skills, word recognition, letter knowledge, letter-sound knowledge, vocabulary, and developing interpretation. Early math skills, drawn from validated, standardized instruments such as the Test of Early Mathematics Ability-3, measure knowledge of number sense, operations, measurement, data analysis, patterns, and geometry (Snow et al., 2009). Some of the items in early reading and early math assessments were taken from the Early Childhood Longitudinal Study, Kindergarten Class of 1998–99 (ECLS-K) (Najarian et al., 2010). Overall scale scores for early reading and early math were calculated using Item Response Theory (IRT) procedures. The IRT scale scores represent estimates of the number of correct answers that would have been expected if a child had answered all items in math/reading assessment. The reliability of the IRT based early reading and math scores was tested and was proven to be high enough (Najarian et al., 2010).

As socioemotional development, I consider three constructs of teacher-reported socioemotional outcomes: approaches to learning, interpersonal skills, and externalizing behavior, which have been validated in the previous study (Riser et al., 2024). Higher scores indicate a better outcome for all measures. The socioemotional items were measured as indirect assessments by teachers and childcare providers, who evaluated each item on a 5-level scale from 0 (never) to 4 (very often). The approaches to learning scale is composed of 5-6 items that assess children's eagerness to learn, attentiveness, and task persistence (alpha: 0.83-0.89 depending on survey wave). The interpersonal scale is composed of 3 items that assess children's relationships with others and whether children seem happy (alpha: 0.64-0.76), while the externalizing behaviors scale is composed of 8 items that assess children's aggressive, disruptive, and impulsive behaviors (alpha: 0.88-0.92). To create each construct, I took the average of the total value of all the items included in a construct. For ease of interpretation, cognitive and socioemotional scores are standardized to have a mean of 0 and SD of 1.

Lastly, to measure health status, I use the parent-reported general child health status (excellent, very good, good, fair, or poor) and create an indicator for excellent/very good health (=0 if good, fair, or poor). The parent-reported general child health status is often used in the literature on SNAP and child health (e.g., Bronchetti et al., 2019; East, 2020; Miller, & Morrissey, 2021) and research suggests there is a fairly high correlation between parent's reports of child health status and doctor's reports of child health status (Case, Lubotsky, & Paxson, 2002). The study uses a linear probability model for this outcome.

SNAP and EITC Generosity Measures

To employ an exogenous variation in SNAP benefit levels, I measure SNAP generosity as the annual purchasing power of SNAP benefits. This approach is based on the idea that SNAP benefits are not adjusted for geographic variations in the local cost of living. Following Bronchetti and colleagues (2019), SNAP purchasing power is calculated as the ratio of the maximum SNAP benefit for a family of four (which does not vary across regions in all contiguous states plus D.C.) to the regional cost of the Thrifty Food Plan (TFP). TFP is the least expensive nutrition plan, established by the USDA, that contains recommended amounts of foods in 29 food categories for a "reference" family, defined as a family of four comprised of an adult male and female (age 20-50) and two children (age 6-8 and age 9-11). Although SNAP recipients are not limited to purchasing the TFP, I use it as a standardized local price measure. The TFP price is an appealing choice since the maximum SNAP benefits are legislated based on the national average cost of the TFP and food prices tend to be highly correlated with the prices of other goods or services (Bronchetti et al., 2019).

The regional TFP price is estimated using the Quarterly Food-at-Home Price Database (QFAHPD). The QFAHPD, constructed by USDA Economic Research Service researchers using Nielsen Homescan data, has quarterly prices for 52 food-at-home categories (e.g., 12 vegetables groups, 3 fruit groups, 6 dairy groups) for each of the 35 market groups from 1999 to 2010. 35 market groups are exhaustive of the contiguous U.S. (48 contiguous states and D.C.) and each market group comprises a set of counties. A market group includes one metropolitan area, e.g., Boston, Chicago, San Francisco, urban New York, Los Angeles, when there are no sample size concerns, while in otherwise instance, a few metropolitan areas are aggregated into one market group (e.g., Indianapolis, Detroit, Milwaukee, and Grand Rapids constitute "Metro Midwest 1"). For nonmetro areas, they are aggregated based on 9 census divisions. Thus, this resulted in 26 market groups for metropolitan areas, and 9 market groups for nonmetropolitan areas (Todd et al., 2010).

Gregory and Coleman-Jensen (2013) developed a method to create a single price estimate of TFP for each market group and quarter. The first step is to map the individual QFAHPD food categories into a TFP food category (in most cases, a TFP food category consists of multiple QFAHPD food categories). The second step is to compute the price of each TFP food group. To do so, they use a weighted average of the quarterly prices for the QFAHPD foods within a TFP food category, where the weights are yearly national expenditure shares for the QFAHPD food in the TFP category. By averaging the quarterly price of TFP food categories across four quarters and then aggregating the TFP prices for all food groups, a single estimate of total TFP price can be calculated by market groups and years (see Gregory, & Coleman-Jensen (2013) and Bronchetti et al. (2019) for further details).

Figure 4.1 (Panels A and B) shows the trends in TFP price and SNAP purchasing power across time by each market group, with relatively large cities highlighted as colored lines. This illustrates that there are large variations in TFP price and therefore, SNAP purchasing power, not only across market groups but also over time within each market group. Figure 4.2 shows a histogram of the SNAP purchasing power.

Then, to leverage an exogenous variation in EITC benefit levels, I use the maximum federal and state EITC benefits as my measure of EITC generosity. I draw on multiple data sources, including NBER TAXSIM data (National Bureau of Economic Research [NBER], 2019) and the EITC data created by Komro et al. (2020). The maximum federal and state EITC benefits are calculated by multiplying the maximum federal EITC by (1+state credit rate).[10]

[10] This measure includes both refundable and non-refundable state EITCs. Results are nearly unchanged when I only consider states with refundable state EITC (i.e., assign the maximum federal EITC benefit to states with non-refundable state EITC).

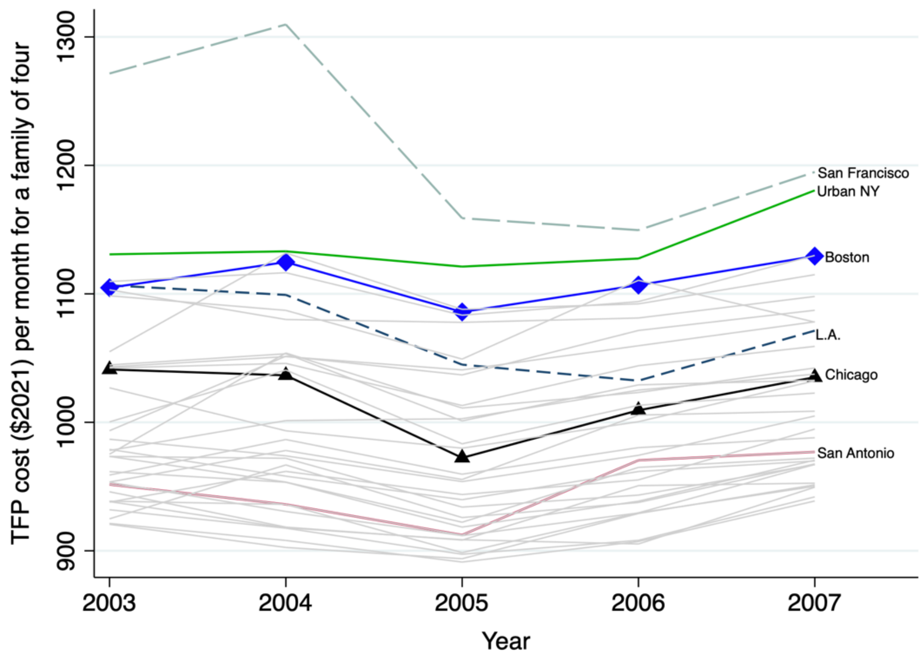
Figure 4.3 illustrates the variations in the maximum federal and state combined EITC benefits across states and over time during tax years 2002-2006 (i.e., 2003-2007 in terms of the actual receipt of EITC). States that changed their state EITC generosity during the study period[11] (states that either implemented the state EITC or changed the generosity of state credit rate) are indicated as thin colored lines, while states without their own EITCs, who are assigned with the maximum federal EITC benefit, are indicated as the black thick line. To simplify the figure, I did not include states who had their state EITC program but did not show any changes in its generosity during the study period.[12]

[11] These states include DC, DE, IN, MD, NE, NJ, NY, RI, VA.

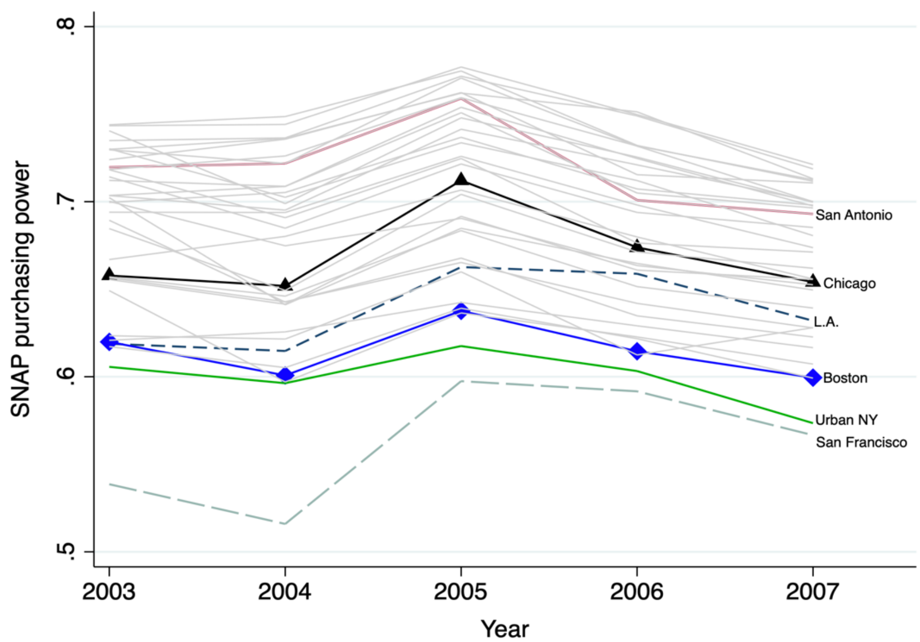
[12] There are 10 states who fall into this category: IA, IL, KS, MA, ME, MN, OK OR, VT, WI.

Figure 4.1. TFP price and SNAP purchasing power by market groups from 2003 to 2007.

Panel A. TFP price ($2021) over time by market groups

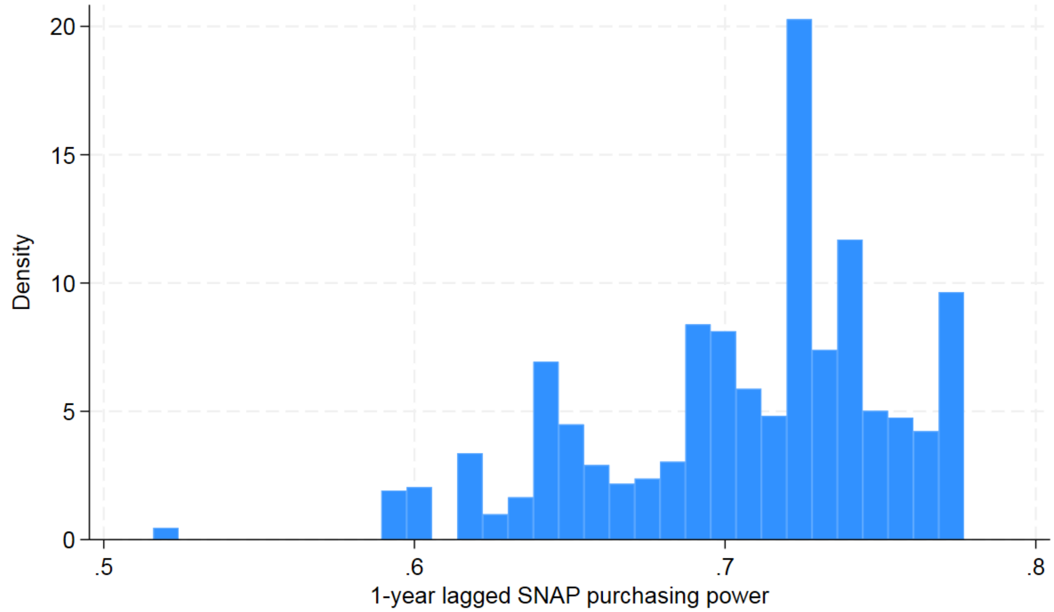


Panel B. SNAP purchasing power over time by market groups



Notes: In panel A and panel B, each line indicates a market group's trend in TFP price and SNAP purchasing power, respectively.
Data source: Bronchetti, Christensen, and Hoynes (2019)

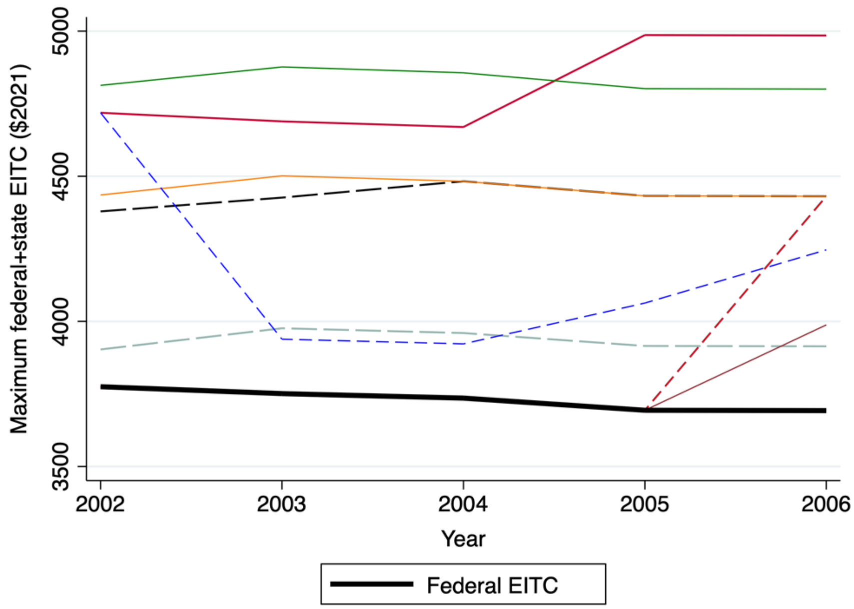
Figure 4.2. Histogram of the SNAP purchasing power.



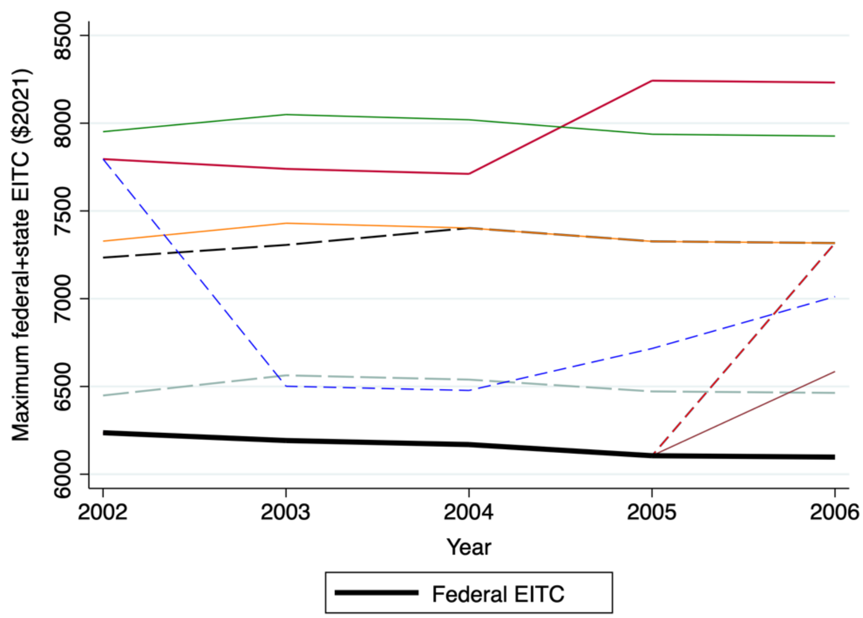
One might wonder why I am not using a similar purchasing power approach to measure EITC generosity. Although the federal EITC is not adjusted for local cost of living (Hanson, & Hawley, 2016; Fitzpatrick, & Thompson, 2010), in contrast with SNAP, which is strictly fixed at the federal level, EITC has state-level supplements. Thus, EITC does not have the same purchasing power problem as SNAP, and therefore the amount of variation in EITC purchasing power is smaller than what is found in SNAP purchasing power. This led me to use the variation in EITC benefits arising from state EITC policy changes, a more common approach in the EITC literature (e.g., Bastian, & Michelmore, 2018; Pilkauskas, & Michelmore, 2019). Furthermore, the county-level price controls in my model help minimize any potential variation in EITC purchasing power, if exists, within a state (see the next section).

Figure 4.3. Maximum state and federal EITC benefits from tax year 2002 to 2006.

Panel A. Maximum EITC benefits (in $2021) over time by states for families with one child.



Panel B. Maximum EITC benefits (in $2021) over time by states for families with two+ children.



Notes: In figures in panel A and panel B, the colored lines indicate the maximum federal and state EITC benefits among states that showed changes in their state generosity during tax years 2002-2006. The black thick line indicates the maximum federal EITC benefits, which are assigned to states without their own state EITC.

After the market group-year-level SNAP purchasing power and the state-year-number of children-level maximum EITC benefit are created, I assign each family in the ECLS-B to these measures. SNAP purchasing power is merged based on the county of birth and the assessment year, while maximum EITC benefits are merged based on the state of residence, the assessment year, and the number of children living in household in the relevant tax year. In the ECLS-B, children and families were interviewed and assessed at various time points within each wave. For instance, assessment months vary from August, 2005 to June, 2006 in wave 3, and assessment months vary from September, 2006 (November, 2007) to March, 2007 (March, 2008) in wave 4 (wave 5). Thus, to ensure that SNAP purchasing power and maximum EITC benefit measures accurately reflect the generosity of SNAP and EITC benefits families received before their month of assessment, I lag those measures by one year relative to the year of child assessment. In terms of tax year, this is equivalent to lagging maximum EITC benefits by two years, because families receive EITC refunds in the following year. Since interaction terms increase model complexity, I only consider one year lagged measures in my primary analysis, rather than including up to two year lagged measures. But as an additional analysis, I consider up to two year lagged measures and present the findings in Appendix C.

State- and County-Level Control Variables

To construct state- and county-level control variables, I use multiple publicly available sources. As state-year level variables, I adjust for unemployment rate (from Bureau of Labor Statistics (BLS)), poverty rate (from U.S. Census Bureau's Small Area Income and Poverty Estimates), maximum TANF benefit for a family of four (from University of Kentucky Center for Poverty Research), per-capita income (from Bureau of Economic Analysis), state minimum wage (from BLS and Tax Policy Center), upper income eligibility limit of Medicaid/SCHIP for children (from Kaiser Family Foundation and National Governor's Association), and a summary index of state-level SNAP administrative policies including call centers, online applications, simplified reporting, telephone interview instead of a face-to-face interview at recertification, exclusion of an at least 1 vehicle from the asset test, Broad Based Categorical Eligibility, fingerprinting requirement (reverse coded), Supplemental Security Income Combined Application Project, and fewer than 50% of SNAP recipients recertifying within 1-3 month intervals (from SNAP Policy Database). These policies are coded as a binary variable (1 or 0). By averaging across these policy variables, this summary index indicates the number of policies adopted by each state in a given year (out of 9 policies), which is similar in spirit to what Ganong and Liebman (2018) used. Moreover, to reduce concerns that the SNAP purchasing power measure may be picking up the effect of food prices rather than the generosity of SNAP benefits, I control for other prices that are correlated with TFP price. Specifically, I use local housing price estimates from the U.S. Department of Housing and Urban Development's (HUD's) Fair Market Rent (FMR) for 2-bedroom units (from HUD Office of Policy Development and Research). FMRs estimate 40th percentile gross rents for standard quality units by county. In the sensitivity analysis, I additionally control for prices of other goods, using regional CPIs for apparel, transportation, education, and recreation (from U.S. Census Bureau and BLS). During the study period, regional CPIs are available in 27 metro core based statistical areas, while in the rest of the regions, they are available in 11 sampling units that consist of different census regions by population sizes (<50,000; 50,000-1.5 million; >1.5million). Both FMR and regional CPIs are merged with the ECLS-B by county and year. Table B.7 in Appendix B shows correlations between TFP price and other local prices, including FMR and regional CPIs.

Identification Strategy

The current study uses a child fixed-effects framework as its main empirical strategy. I analyze the following specification.

$$Y_{itsmc} = \alpha_0 + \delta_1 \text{maxEITC}_{is,t-1} + \delta_2 \frac{\text{maxSNAP}}{\text{TFP price}}_{m,t-1} + \delta_3 \text{maxEITC}_{is,t-1} \times \frac{\text{maxSNAP}}{\text{TFP price}}_{m,t-1} + X_{it}\beta$$

$$+ \phi_{s,t-1}\mu_1 + \phi_{s,t-2}\mu_2 + \theta_{c,t-1}\mu_3 + \lambda_t + \gamma_i + \varepsilon_{itsmc}$$

i indicates child, *t* indicates wave 3 and wave k (or the year of assessment in each wave), *s* indicates state of residence, *m* indicates market group (a set of counties) of residence, and *c* indicates county of residence. I mean center SNAP purchasing power (centered at 0.71; 1-unit = 0.1) and maximum EITC benefits (centered at \$5830, 1-unit=\$1000) in order to obtain a meaningful and realistic interpretation of the main effect of each measure. In the model, I include child fixed effects γ_i and year (or survey wave) fixed effects λ_t.

Child fixed effects control for stable child or family specific characteristics that may confound the relationship between the SNAP and EITC measures and child outcomes, such as child innate cognitive ability, permanent disability, child temperament, and parental fixed preferences or approach to early child learning. Year fixed effects account for time-varying confounders that affect all children in the same survey wave (e.g., macro-economic shocks or federal-level policy changes). Although child and year fixed effects account for these important confounders, they cannot address endogeneity coming from regional, and child and family characteristics that vary over time. Of particular concern are systematic economic or policy changes that would improve child development among low-income families at the same time the state EITC expanded or the SNAP purchasing power increased. Thus, I adjust for a rich set of state-year level characteristics $\phi_{s,t-1}$ $\phi_{s,t-2}$, described in the previous section. I include their one-year and two-year lagged terms to account for the fact that the SNAP and EITC measures are lagged by one year or by two years in terms of tax year. Indicated as $\theta_{c,t-1}$, I also control for local housing prices.[13]

As child and family characteristics that vary over time (X_{it}), I adjust for child age at the survey assessment (in months) and its squared term, mother's age (in years) and its squared term, parent's highest education attainment (below high school, high school degree, some college), number of children (one, two or more), parent's marital status (not married, married), parent's immigrant status (immigrant, US citizen), and urbanicity (rural, urban, urban cluster) (see Table 4.2 for their descriptive statistics). The first category within the parenthesis is the reference category for categorical variables. To adjust for survey nonresponse and disproportionate sampling, and to allow for making inferences about the national population, the ECLS-B survey weights were applied to all the analyses. Two-way clustering method is used to cluster standard errors at the market group- and the state-level (results are robust to clustering standard errors at the market-group-level only and the state-level only).

With child and year fixed effects, identification relies on whether changes in maximum EITC benefits and TFP price (therefore, SNAP purchasing power) are driving differences in children's developmental outcomes from their mean outcome score (averaged across two waves). The variation in TFP price comes from two sources: (i) change in TFP price over time within a market group that a child lives in; and (ii) the fact that children in a given survey wave were assessed in different years. Based on the second source of variation, children living in the same market group may be assigned SNAP purchasing power from different years. For instance,

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[13] For all of the regional level covariates, their one-year lagged and two-year lagged terms are controlled, except for SNAP policy index and local housing price. Since they are included to adjust for the correlates of changes in child development that are also related to changes in SNAP purchasing power, I only include their one-year lagged measures.

children who were assessed in 2005 and 2006 in wave 3 and wave k, respectively, are assigned SNAP purchasing power from 2004 and 2005, while children assessed in 2005 and 2007 are assigned with 2004 and 2006 values in each wave.[14] I exploit this likely idiosyncratic variation, derived from different assessment timings. Based on my consultation with the ECLS-B's data team, the assessment timing was determined solely by a family's availability and there is no record of field interviewers scheduling the assessments by nonrandom child or family characteristics. Moreover, descriptive analysis that compares demographic characteristics between different assessment timing groups demonstrates that there are no statistically significant differences across those groups for many of the characteristics, and no particular trends are found for the characteristics that show statistically significant differences (see Table B.2 in Appendix B).

Similarly, the variation in maximum EITC benefits comes from (i) changes in state EITC policies over time within a state that a child lives in; (ii) the fact that children in a given survey wave were assessed in different years; and (iii) changes in the number of children in household across the two relevant tax years[15]. With the maximum federal and state EITC benefits and child and year fixed effects, my analysis is similar in spirit to a parameterized difference-in-differences approach, which allows for multiple policy changes as well as differences in the magnitudes of each policy change in a difference in differences framework (Pilkauskas, & Michelmore, 2019). This method is particularly useful for this study because multiple states expanded their state EITCs to varying degrees over the study period.

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[14] See Table B.2 for a full list of different assessment timing groups.

[15] To minimize any endogenous changes in family structure and to increase precision, I control for the number of children living in household in the assessment year. The result is nearly unchanged, however, without the number of children in the assessment year.

The key coefficient of interest is δ_3, the interaction effect coefficient. This indicates whether the effect of a $1000 increase in the maximum (federal and state) EITC benefits on child outcomes statistically significantly varies at different levels of SNAP purchasing power or whether the effect of a 0.1 increase in SNAP purchasing power on child outcomes statistically significantly varies at different levels of maximum EITC benefits. A positive value of δ_3 would indicate that there is a complementary effect. δ_1 indicates the main effect of a $1000 increase in the maximum EITC at the mean SNAP purchasing power, while δ_2 indicates the main effect of a 0.1 increase in SNAP purchasing power at the mean maximum EITC benefits. Combining the main effect coefficient on each program with δ_3, I can compute the marginal effect of a $1000 increase in the maximum EITC benefits at varying levels of SNAP purchasing power and vice versa.

We can interpret the main effect and the interaction effect as causal to the extent that (i) changes in SNAP purchasing power and maximum EITC benefits are exogenous – that is, not driven by changes in unobserved correlates of child development outcomes – and that (ii) those changes in SNAP purchasing power and maximum EITC benefits are independent from one another, after adjusting for other covariates in the model above.

To assess the plausibility of the first assumption, I perform a series of robustness and falsification tests. I find that my results are overall not sensitive to these tests, which add confidence to a causal interpretation of my findings. To test the second assumption, I test the independence of SNAP purchasing power and the maximum EITC benefits by regressing the SNAP purchasing power on the maximum EITC as well as the other way around. Table 4.1 presents the results. In either way, the magnitude of the coefficient is very small (close to zero) and it is statistically insignificant, indicating that changes in SNAP purchasing power are independent from changes in maximum EITC benefits.

Table 4.1. Independence of the maximum EITC and SNAP purchasing power.

	SNAP purchasing power	Maximum EITC
Maximum EITC	0.00	
	(0.00)	
SNAP purchasing power		0.15
		(1.42)
N	2000	2000
Child fixed effects	Y	Y
Year fixed effects	Y	Y
Demographic characteristics	Y	Y
Regional economic and policy conditions	Y	Y
Housing price	Y	Y

Notes: *** p<0.001, ** p<0.01, * p<0.05, + p<0.1. All estimates are weighted.

Results

Sample Characteristics

Table 4.2 shows sample characteristics of the study sample by survey waves and in the full sample. I present the findings from the sample restricted to families who do not have missing values in early reading scores, but sample characteristics are qualitatively similar in samples that are restricted to families without missing values in other child outcomes. The negative values in the standardized child outcomes demonstrate that children in the sample have lower scores than the national average, represented by the ECLS-B children. In the full sample (see "Total" column), around 71.5 percent of children are with parents who have a high school degree or some college education, 66.0 percent of children live with unmarried parents, 84.3 percent of children live in families with two or more children, and 79.0 percent of children live in urban or urban-cluster areas. The majority of the sample (66.1 percent) has family income above 50 percent of FPL in the year preceding the assessment year, and Black children constitute the largest share of the sample (38.3 percent), followed by White (33.6 percent), Hispanic (22 percent), and other race/ethnicity (6.1 percent).[16] In addition, several variables show statistically significant changes from wave 3 to wave k. As expected, the study's independent variable (i.e., SNAP purchasing power) and most of the state/county covariates show statistically significant changes between the two waves. However, there are only few child and family covariates and outcomes that statistically significantly differ across waves, including the number of children, child and mother's age, mother's immigrant status (marginally significant), and interpersonal skills (marginally significant).

Table 4.2. Sample characteristics of the study sample (SNAP recipients)

	Mean (SD) / Percent			
	Wave 3	Wave K	Total	Sig.
Std. early reading	-0.47	-0.44	-0.45	
	(0.75)	(0.88)	(0.82)	
Std. early math	-0.54	-0.47	-0.50	
	(0.82)	(0.90)	(0.86)	
Std. approaches to learning	-0.27	-0.27	-0.27	
	(0.93)	(1.02)	(0.98)	
Std. interpersonal skills	-0.05	-0.20	-0.13	+
	(1.01)	(1.04)	(1.03)	
Std. externalizing behavior	-0.15	-0.20	-0.18	
	(1.00)	(1.02)	(1.01)	
Excellent or very good general health (%) (Ref.: Poor, Fair, or Good)	81.00	81.16	81.08	
Family with two or more children (%) (Ref.: Family with one child)	82.48	86.06	84.27	**
Married (%) (Ref.: Not married)	34.13	33.76	33.95	
Urbanicity (%) (Ref.: Rural)				
Urban	64.29	63.23	63.76	

[16] Child race/ethnicity is not controlled in models as it is absorbed by child fixed effects. Also, poverty status is not controlled since it is directly affected by SNAP and EITC (thus, it is potentially endogenous since it is likely related to child development).

(Table 4.2 continued)

Urban-cluster	15.41	15.06	15.23	
Parent is an immigrant (%) (Ref.: US-citizen)	7.83	7.00	7.41	+
Parent's highest education attainment (%) (Ref.: No high school degree)				
High school degree	41.99	41.51	41.75	
Some college	30.07	29.37	29.72	
BA or higher	3.17	4.40	3.79	
Child age at the interview (in months)	52.28	67.92	60.10	***
	(3.98)	(4.41)	(8.88)	
Mother's age (in years)	28.84	30.20	29.52	***
	(6.29)	(6.69)	(6.53)	
Child race/ethnicity (%) (Ref.: White)				N/A
Black	38.29	38.29	38.29	
Hispanic	21.99	21.99	21.99	
Others	6.12	6.12	6.12	
Male (%) (Ref.: Female)	49.06	49.06	49.06	N/A
Poverty status (%) (Ref.: <50% of FPL)				
50%-130% of FPL	53.74	52.69	53.21	
>130% of FPL	12.77	13.03	12.90	
SNAP purchasing power	0.69	0.71	0.70	***
	(0.05)	(0.05)	(0.05)	
Maximum state and federal EITC benefits ($2021)	5745.92	5860.25	5803.08	**
	(1202.94)	(1105.70)	(1156.45)	
Unemployment rate	5.62	5.15	5.39	***
	(0.86)	(1.05)	(0.99)	
Percent of poverty	13.53	14.12	13.83	***
	(2.76)	(2.90)	(2.85)	
Per-capita income ($2021 in thousands)	46.87	48.00	47.43	***
	(6.57)	(6.76)	(6.69)	
Maximum TANF benefits for a family of four ($2021 in hundreds)	6.15	5.94	6.04	***
	(2.69)	(2.63)	(2.66)	
Medicaid income eligibility limit as a percent of FPL	2.24	2.24	2.24	
	(0.45)	(0.45)	(0.45)	
Minimum wage ($2021)	7.69	7.72	7.70	
	(0.89)	(1.04)	(0.97)	

(Table 4.2 continued)

Index for adopting inclusive SNAP administrative policies (0-1)	0.43	0.48	0.46	***
	(0.12)	(0.13)	(0.13)	
Fair market rent for 2-bedroom ($2021)	960.12	943.07	951.59	**
	(318.29)	(278.70)	(299.20)	
Regional CPI for apparel costs	106.65	106.00	106.33	*
	(16.83)	(18.51)	(17.69)	
Regional CPI for education costs	110.58	113.30	111.94	***
	(4.14)	(4.97)	(4.77)	
Regional CPI for recreation costs	108.77	110.04	109.40	***
	(4.15)	(4.77)	(4.52)	
Regional CPI for transportation costs	142.73	154.00	148.36	***
	(26.75)	(28.27)	(28.09)	

Notes: *** p<0.001, ** p<0.01, * p<0.05, + p<0.1. 'Sig.' column shows whether a given variable is statistically significantly different between wave 3 and wave k. 'Std.' stands for standardized. Child race/ethnicity, sex, and poverty status are not controlled in models. Table 4.2 is generated using the early reading sample (N=1850). Mean, standard deviation, and percentage are weighted, while sample size is unweighted.

Main Results

Table 4.3 shows that the main effect of the maximum EITC benefits at the average SNAP purchasing power level is positive across all cognitive and socioemotional outcomes, with larger magnitudes for socioemotional outcomes (Columns 3-5) compared to cognitive outcomes (Columns 1-2). The results are statistically significant for approaches to learning and externalizing behavior (Columns 3 and 4), indicating that a $1000 increase in the maximum EITC benefits improves approaches to learning by 0.20 SD and externalizing behavior by 0.30 SD for children exposed to the average SNAP purchasing power. For cognitive outcomes, the effects are imprecisely estimated and relatively small, while the coefficient for interpersonal skills is approaching significance and comparable in size to the coefficient for approaches to learning. With respect to the main effect of the SNAP purchasing power, however, the coefficients are estimated with very large standard errors and have an opposite sign from my expectation. I provide potential explanations for this finding below. Regarding general health status (Column 6), I find null effects in both the main effects of maximum EITC and SNAP purchasing power.

Regarding the interaction, as Table 4.3 shows, I find strong complementary effects between EITC and SNAP, particularly on early reading and early math scores (cognitive development) and externalizing behavior (socioemotional development). The interaction effect coefficients are statistically significant and positive for these outcomes, indicating that the effect of a $1000 increase in the maximum EITC on those outcomes statistically significantly increases as the level of SNAP purchasing power increases and vice versa – i.e., the effect of a 0.1 increase in SNAP purchasing power statistically significantly increases as the level of maximum EITC increases. Consistent with the above outcomes, the interaction effect coefficient for approaches to learning is positive, although statistically insignificant. With respect to interpersonal skills and the probability of excellent or very good health, I find a statistically insignificant and negative interaction effect (which aligns with the substitution effect). However, since their standard errors are large, I cannot draw conclusions on the interaction effects for approaches to learning, interpersonal skills, and general health status.

To better show the magnitude of the complementary effect of EITC and SNAP, I present a series of graphs that illustrate how the marginal effect of the maximum EITC changes as the level of SNAP purchasing power changes (at the average, 10 percent increase from the average). Relative to the mean (0.71), a 10 percent increase in SNAP purchasing power is 0.78. This is the highest SNAP purchasing power during the study period (see Figure 4.2), and a 10 percent increase from the mean is comparable to an additional $1000 to 1300 (in 2021 dollars) per year for an average family (size of 4.8) in the study sample.[17]

These graphs are presented in Figure 4.4 and the specific values of those marginal effects are displayed in Table 4.3. I focused on the marginal effect of maximum EITC benefits, rather than that of SNAP purchasing power, due to imprecisely estimated main effects of SNAP purchasing power across all outcomes (see the paragraph below for further explanations). Panel 1 in Figure 4.4 shows that, for children exposed to the average SNAP purchasing power, a $1000 increase in the maximum EITC benefit leads to a statistically insignificant 0.06 SD increase in early reading scores (which equals the value of the main effect of EITC in Column 1 in Table 4.3). However, with a 10 percent increase in SNAP purchasing power, a $1000 increase in the maximum EITC benefit results in a statistically significant 0.14 SD increase in early reading scores. The same trend appears for early math scores (see Panel 2). While the effect of a $1000 increase in the maximum EITC on early math is 0.01 SD at the average SNAP purchasing power (see Column 2 in Table 4.3), the EITC effect becomes statistically significant and increases to 0.13 SD, when the level of SNAP purchasing power increases by 10 percent from its mean. Furthermore, Panel 3 shows that the marginal effect of the maximum EITC on approaches to learning increases from 0.20 SD (statistically significant) to 0.22 SD (marginally significant) with a 10 percent increase in SNAP purchasing power, although this change is not statistically significant as indicated by the interaction effect coefficient in Table 4.3. Lastly, Panel 4

[17] 0.71 is calculated by dividing the average maximum SNAP benefit per person – i.e., $122.3 – by the average monthly TFP cost per person – i.e., $173.7. Therefore, to increase the average SNAP purchasing power by 10 percent from the mean, it would require an increase in maximum SNAP benefits by approximately $13 or a decrease in TFP cost by $17 per person-month. If we translate this into annual dollar amounts for an average household size (4.8) in the study sample, a 10 percent increase indicates an additional $745-$979 per year (approximately $1000-$1300 in 2021 dollars).

illustrates that the marginal effect of maximum EITC on externalizing behavior increases from 0.30 SD to 0.46 SD (all statistically significant) as the level of SNAP purchasing power increases by 10 percent from its mean. These graphs consistently show that the EITC is more effective at improving child development when supported by higher SNAP purchasing power.

Of note, when compared to results from a model that only includes the main effects of EITC and SNAP without their interaction term (see Table B.4 in Appendix B), these findings from the interaction effect model clearly demonstrate the importance of including the interaction effect. For example, according to Table B.4, with only main effect terms, we would have concluded that the EITC does not statistically significantly improve early reading and early math skills. However, by estimating the interaction effect, I found new evidence that the EITC statistically significantly increases early reading and early math outcomes when it is coupled with greater SNAP purchasing power.

With respect to the marginal effects of SNAP purchasing power, they also statistically significantly vary by different levels of maximum EITC benefits (e.g., $1000 below the mean, mean, $1000 above the mean). However, as they are imprecisely estimated in most cases, I do not draw conclusions on how SNAP purchasing power changes as maximum EITC benefits increase. One potential reason for such statistically insignificant effects of SNAP purchasing power includes the way in which it is calculated. As mentioned previously, children's outcomes were assessed across the 12-month period, while the SNAP purchasing power measure captures the average purchasing power across 12 months in a given year. Hence, compared to children assessed in September to December (which represent 80.6 percent of the primary sample), the time gap between the measurement of the SNAP purchasing power and child outcomes is shorter for those assessed in January to March (which represent 15.1 percent of the primary sample).

This suggests that this group may have diluted any observable effect of SNAP due to their shorter time lag. The EITC measure is less likely to have been affected by this measurement issue, as most families receive EITC refunds in the first three months of the year, particularly in February and March (Aladangady et al., 2018; Rehkopf, Strully, & Dow, 2014). In line with this idea, I show in Appendix C that when SNAP purchasing power is lagged by two years, its main effect is positive on most outcomes and is even larger in magnitude than the main effect of two-year lagged maximum EITC benefits.[18]

Table 4.3. Interaction effects between the maximum EITC and the SNAP purchasing power

	Reading	Math	AL	EB	IP	Health
Regression coefficients						
Maximum EITC (centered)	0.06	0.01	0.20*	0.30***	0.19	-0.00
	(0.05)	(0.04)	(0.09)	(0.07)	(0.11)	(0.02)
SNAP purchasing power (centered)	-0.26	-0.05	-0.08	-0.40	0.12	-0.00
	(0.17)	(0.16)	(0.44)	(0.36)	(0.52)	(0.09)
Maximum EITC X SNAP purchasing power	0.12**	0.18*	0.03	0.23*	-0.20	-0.05
	(0.04)	(0.07)	(0.10)	(0.11)	(0.15)	(0.03)
Mean of outcome	-0.45	-0.51	-0.33	-0.23	-0.14	0.80
N	1850	1850	850	850	850	2000
Child fixed effects	Y	Y	Y	Y	Y	Y
Year fixed effects	Y	Y	Y	Y	Y	Y
Demographic characteristics	Y	Y	Y	Y	Y	Y
Regional economic and policy conditions	Y	Y	Y	Y	Y	Y
Housing price	Y	Y	Y	Y	Y	Y

[18] This aligns with the results in Chapter 3 – except that the coefficients are much more imprecisely estimated in the current study. The likely reason is because the model is much more complex in this paper since the one-year lagged and two-year lagged main effects of EITC and SNAP and their interaction effect terms (six terms in total) have been added, which could have further decreased the remaining variation in SNAP purchasing power. Another potential reason is a different sample definition. In the current study, my primary study sample consists of SNAP recipients, while in Chapter 3, the primary study sample comprises unmarried parents without a college degree.

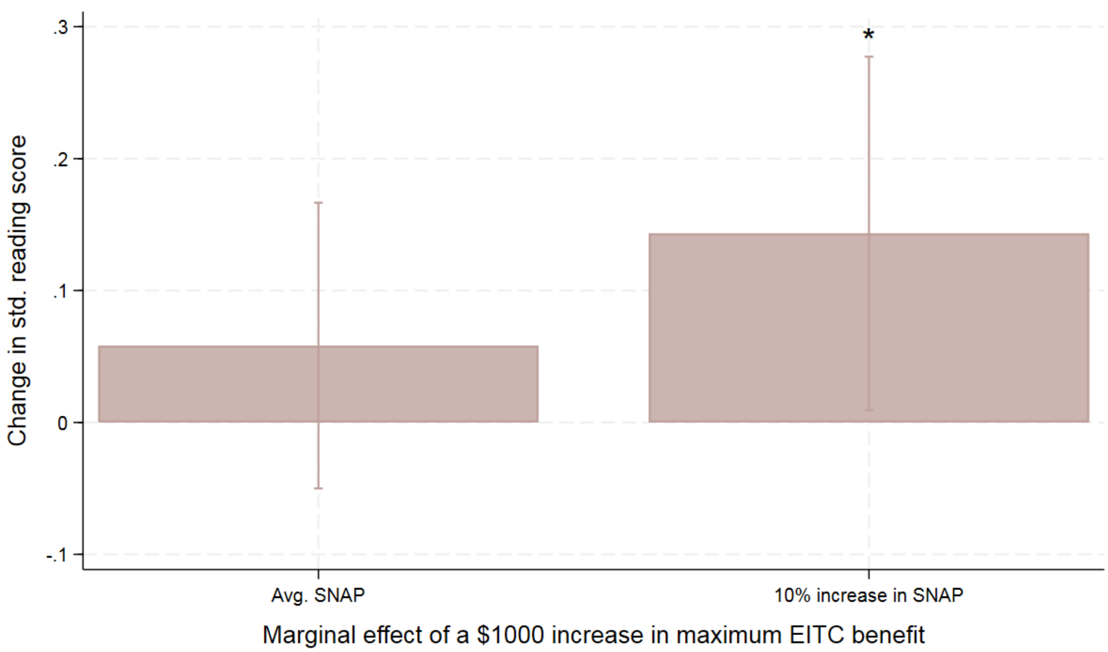
(Table 4.3 continued)

Marginal effects of $1000 increase in maximum EITC by SNAP purchasing power						
With 10% decrease	-0.03	-0.12+	0.18+	0.14	0.33*	0.03
	(0.05)	(0.06)	(0.09)	(0.11)	(0.14)	(0.02)
Average	0.06	0.01	0.20*	0.30***	0.19+	-0.00
	(0.05)	(0.04)	(0.09)	(0.07)	(0.11)	(0.03)
With 10% increase	0.14*	0.13*	0.22+	0.46***	0.05	-0.04
	(0.07)	(0.06)	(0.13)	(0.10)	(0.17)	(0.04)

Notes: 'AL' indicates approaches to learning; 'IP' indicates interpersonal skills; 'EB' indicates externalizing behavior. *** p<0.001, ** p<0.01, * p<0.05, + p<0.1. The delta-method standard error is used to compute the marginal effects of $1000 increase in maximum EITC (the default option in STATA 'margins' command), while the cluster standard error is used to compute regression coefficients. This creates a slight difference in the p-value generated in the regression and the marginal effects. There is only one small difference, which is in the marginal effect of maximum EITC on interpersonal skills at the average SNAP purchasing power. All estimates are weighted.

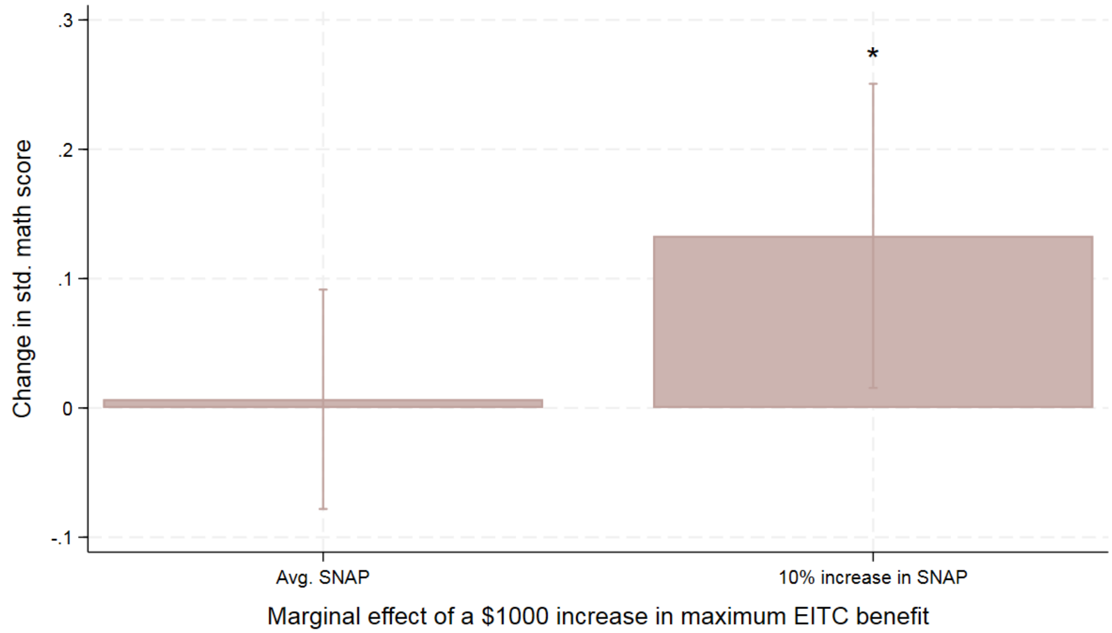
Figure 4.4. Marginal effect of maximum federal and state EITC benefits on child development at different levels of SNAP purchasing power.

Panel 1. Early reading scores.

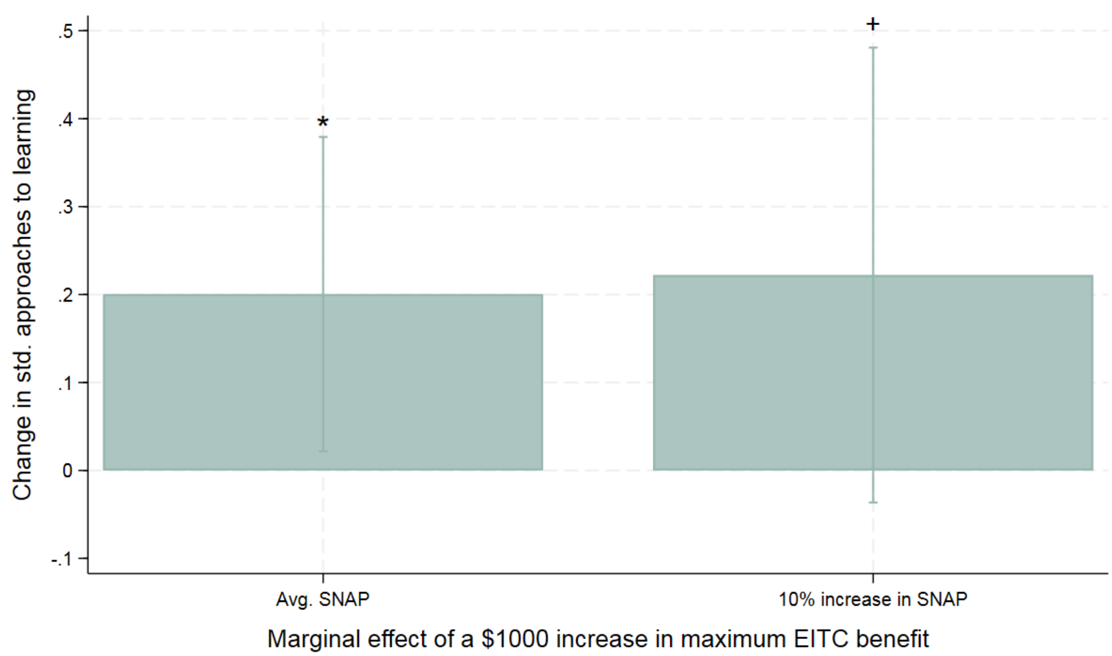


(Figure 4.4 continued)

Panel 2. Early math scores.

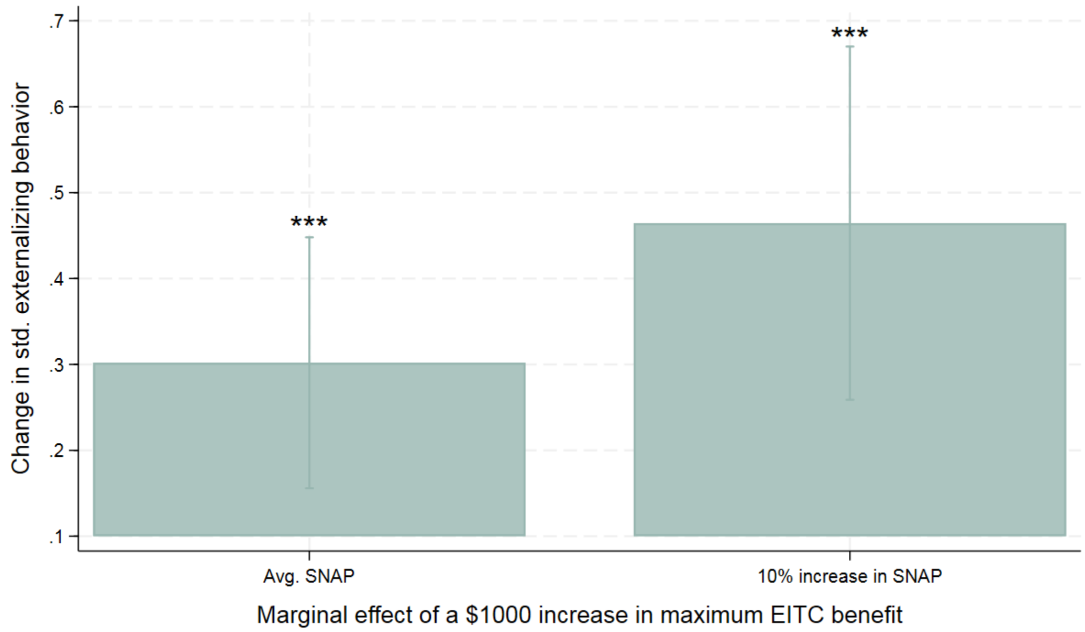


Panel 3. Approaches to learning.



(Figure 4.4 continued)

Panel 4. Externalizing behavior.



Notes: The average SNAP purchasing power is 0.71. A 10 percent increase from the average equals 0.78. *** p<0.001, ** p<0.01, * p<0.05, + p<0.1 placed above the confidence interval show the statistical significance of the marginal effect of a $1000 increase in the maximum EITC at a given SNAP purchasing power level. The interaction effect coefficient shown in Table 4.3 indicates whether the marginal effect of a $1000 increase in the maximum EITC statistically significantly changes when the level of SNAP purchasing power increases by 10 percent from the mean.

Falsification and Robustness Tests

I conducted a series of robustness/falsification tests to assess whether the above findings on early reading, early math, externalizing behavior, and approaches to learning are plausibly causal. For these tests, I do not include interpersonal skills and general health status since both their main effects and interaction effects were not statistically significant.

First, I conducted a falsification test that reruns the main model for a following placebo sample where I expect to see null or very small effects of EITC and SNAP: children whose parent is married and college educated. Panel 1 in Table 4.4 shows the estimates. Reassuringly, most estimates of the main effects of maximum EITC benefits and SNAP purchasing power and their interaction effects are statistically insignificant and they all have small magnitudes or are in the opposite direction from my main results. This suggests that the complementary effect does not exist among the advantaged placebo sample (nor do the main effects of each program), adding support to a causal interpretation of my main results.

Second, I analyzed my main model after excluding families under deep poverty (under 50 percent of the FPL). I expect to find larger complementary effects within this group as compared to the primary sample, since families who participate in both EITC and SNAP are less likely to be in deep poverty because the EITC is provided to families with earnings (Moffit, 2020). I conducted this analysis in two different ways using the wave 2 income information: excluding families in deep poverty from my primary sample (Panel 2); and limiting the primary sample to families in 50-100 percent of the FPL (Panel 3)[19]. The interaction effect coefficients in Panel 2 and Panel 3 align with the main results and expectedly, they have larger effect sizes (largest for the second subgroup), demonstrating that complementary effects are larger among subgroups who are expected to have a greater likelihood of participating in both EITC and SNAP. Given that the EITC is provided to families with earnings, I also analyzed the main model after restricting the sample to families in which the mother or the resident father of the focal child was working. I reached a similar conclusion among this subpopulation (see Table B.5 in Appendix).

Third, I controlled for the family's participation in other cash based means-tested programs (SSI, SSDI and TANF). These programs might also have some purchasing power variation, although not to the same level as SNAP, since TANF benefits are set at the state level, SSDI is based on earnings (which will vary across place), and SSI has a state supplement.

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[19] Since family income is affected by, and therefore strictly endogenous to, the SNAP and EITC benefits, I used the income information from wave 2.

According to Panel 4, results are not sensitive to controlling for these variables. In addition, I examined whether my main results are robust to controlling for families' Medicaid participation status since it is common for SNAP and EITC recipients to receive Medicaid. Results are nearly unchanged with this adjustment (see Table B.6 in Appendix).

Fourth, I investigated whether my SNAP purchasing power measure is adequately reflecting the generosity of SNAP benefits. To provide evidence that the effects of SNAP purchasing power are not capturing the effect of local prices or the broader effect of living in a different labor market, I controlled for a host of local prices of other goods (measured by regional CPIs) in the main model. As Panel 5 indicates, results on the key coefficients did not change substantively. This finding has implications on the EITC effects as well, suggesting that the effects of EITC are not sensitive to possible purchasing power variations. Furthermore, as another validity check of SNAP purchasing power, I controlled for EITC purchasing power in my main model. I created it in a similar spirit to the SNAP purchasing power measure by taking the ratio of the maximum state and federal EITC benefits to the local housing price captured by FMR, which is correlated with the local TFP price. If EITC purchasing power is highly correlated with SNAP purchasing power, which would suggest that SNAP purchasing power is reflecting the effect of local prices, the effect of SNAP purchasing power would be sensitive to the addition of EITC purchasing power. As indicated in Table B.8 in Appendix B, both the main effect of SNAP purchasing power and the interaction effect coefficient were robust to this adjustment. Overall, results from these various sensitivity and falsification tests support that my main results show causal evidence on the complementary effect between the generosity of EITC and SNAP benefits on children's cognitive and socioemotional development.

Table 4.4. Robustness/Falsification test results.

	Reading	Math	AL	EB
Panel 1. Placebo group (married, college-educated)				
Maximum EITC (centered)	-0.05	-0.08*	-0.02	0.06
	(0.04)	(0.04)	(0.08)	(0.05)
SNAP purchasing power (centered)	-0.21	-0.05	0.21	-0.27
	(0.24)	(0.14)	(0.22)	(0.21)
Maximum EITC X SNAP purchasing power	-0.04	0.03	-0.05	-0.08
	(0.07)	(0.05)	(0.08)	(0.07)
N	3100	3100	1850	1850
Panel 2. Excluding families in deep poverty (i.e., above 50 percent of poverty line)				
Maximum EITC (centered)	0.10+	0.02	0.23+	0.29***
	(0.05)	(0.05)	(0.12)	(0.08)
SNAP purchasing power (centered)	-0.29	-0.16	0.03	-0.53
	(0.21)	(0.18)	(0.42)	(0.47)
Maximum EITC X SNAP purchasing power	0.19***	0.24**	-0.11	0.10
	(0.05)	(0.07)	(0.17)	(0.15)
N	1200	1200	600	600
Panel 3. Families above deep poverty, but below the poverty line (50-100 percent of poverty line)				
Maximum EITC (centered)	0.02	0.10	0.14	0.18
	(0.09)	(0.10)	(0.23)	(0.17)
SNAP purchasing power (centered)	-0.23	0.26	-0.14	-0.75
	(0.27)	(0.35)	(0.80)	(0.52)
Maximum EITC X SNAP purchasing power	0.23*	0.26*	0.41	0.73*
	(0.11)	(0.12)	(0.46)	(0.30)
N	550	550	300	300
Panel 4. Controlling for participation in other cash-based programs (SSI/SSDI, TANF)				
Maximum EITC (centered)	0.06	0.01	0.20*	0.31***
	(0.06)	(0.04)	(0.09)	(0.08)
SNAP purchasing power (centered)	-0.23	-0.05	-0.12	-0.33
	(0.17)	(0.16)	(0.42)	(0.35)
Maximum EITC X SNAP purchasing power	0.10*	0.18*	0.03	0.25*
	(0.05)	(0.07)	(0.09)	(0.11)
N	1850	1800	850	850

(Table 4.4 continued)

Panel 5. Controlling for regional CPIs (other prices)				
Maximum EITC (centered)	0.06	0.01	0.20*	0.31***
	(0.06)	(0.04)	(0.09)	(0.07)
SNAP purchasing power (centered)	-0.13	0.02	0.06	-0.57
	(0.20)	(0.17)	(0.52)	(0.49)
Maximum EITC X SNAP purchasing power	0.13**	0.19*	0.05	0.25*
	(0.04)	(0.07)	(0.11)	(0.11)
N	1850	1850	850	850

Notes: 'AL' indicates approaches to learning; 'IP' indicates interpersonal skills; 'EB' indicates externalizing behavior. *** p<0.001, ** p<0.01, * p<0.05, + p<0.1. All estimates are weighted.

Mechanisms

This section examines possible mechanisms through which the complementarity between the EITC and SNAP may occur. The ECLS-B has several variables that may proxy parental investments and parental stress (two of the key mechanisms discussed in the Background section). As a measure of parental investment, I considered the number of books at home (standardized to mean of 0 and SD of 1), cognitive activity index (which is a combination of reading to the child, singing songs to the child, and telling stories to the child; range: 1-4), and daily reading time (in minutes). As a measure of parental stress, I considered mother's depressive symptoms. However, it is methodologically challenging to do a formal mediation analysis, because there is no (or very little) time lag between mediators and child outcomes measured in the same wave and I am unable to use mediators and child outcomes from different survey waves with the child fixed-effects model. Thus, as a suggestive test of mediation, I analyzed the direct interaction effects of maximum EITC benefits and SNAP purchasing power on potential mediators in the ECLS-B.

Among the mediators I tested, the strongest result I found was on daily reading time. As the positive, although marginally significant, interaction effect shows in Column 5 in Table 4.5 (coefficient size: 3.14), the effect of the maximum EITC on daily reading time increases as the level of SNAP purchasing power increases. Specifically, Figure 4.5 illustrates that a $1000 increase in the maximum EITC results in a statistically significant increase in reading time per day by close to 3 minutes (12 percent increase, relative to the sample mean of 25 minutes) for children exposed to a 10 percent increase in the SNAP purchasing power, while there is a statistically insignificant increase in daily reading time at the average SNAP purchasing power. This finding provides a partial support to the parental investment pathway and aligns with some of the examples I described in the "How would EITC and SNAP Interact to Affect Child Development?" subsection of the Background section. As indicated in Columns 1 to 4 in Table 4.5, I did not find statistically significant effects on other variables related to the parental investment pathway and maternal depressive symptoms.

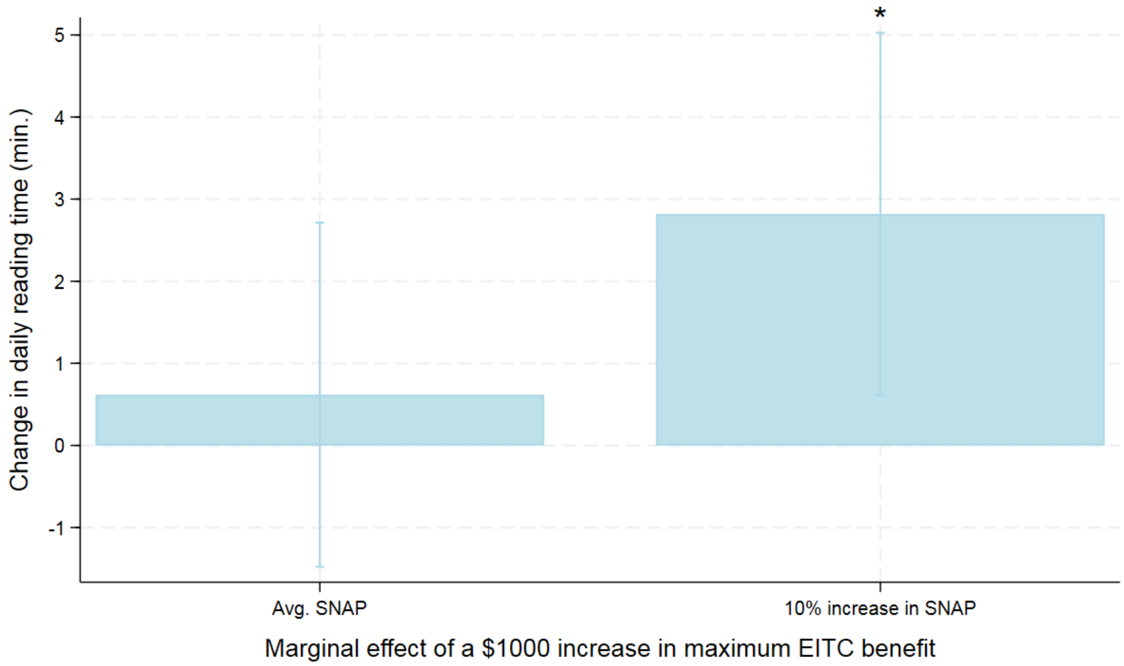
Although the result on maternal depressive symptoms is not statistically significant, I cannot definitively rule out the possibility of the family stress pathway. There may be other potential mediators related to the family stress pathway that I could not test in this study, such as maintaining an orderly household, close parental supervision, parents' emotional support for the child, and quality parent-child interactions. According to the "How would EITC and SNAP Interact to Affect Child Development?" subsection, there could also be other mediators related to the parental investment pathway. They may include purchase of children's items other than books – such as games, toys, and necessary school supplies – as well as expenses related to repairing or purchasing cars and purchasing home appliances like dishwashers, which could save parents' time and in turn affect their time investment in the child. Given these limitations, although my analysis of mechanisms provides helpful insights on how the complementary effect of EITC and SNAP may occur, mechanisms should be further evaluated using a more comprehensive set of mediators.

Table 4.5. Mechanisms: Interaction effects between maximum EITC and SNAP purchasing power on potential mediators.

	Severe depressive symptoms	Depressive symptoms	Books (std.)	Cognitive activity	Read time (min.)
Maximum EITC (centered)	0.01	-0.01	-0.01	-0.01	0.62
	(0.03)	(0.03)	(0.03)	(0.05)	(1.07)
SNAP purchasing power (centered)	-0.07	-0.07	0.05	0.06	12.17*
	(0.08)	(0.12)	(0.07)	(0.12)	(4.84)
Maximum EITC X SNAP purchasing power	0.01	0.04	-0.00	-0.05	3.14+
	(0.03)	(0.04)	(0.04)	(0.07)	(1.61)
N	1850	1850	2000	2000	1850
Mean of outcome	0.14	0.32	-0.38	2.82	24.94
Child fixed effects	Y	Y	Y	Y	Y
Year fixed effects	Y	Y	Y	Y	Y
Demographic characteristics	Y	Y	Y	Y	Y
Regional economic and policy conditions	Y	Y	Y	Y	Y
Housing price	Y	Y	Y	Y	Y

Notes: *** p<0.001, ** p<0.01, * p<0.05, + p<0.1. All estimates are weighted.

Figure 4.5. Marginal effect of maximum federal and state EITC benefits on daily reading time at different levels of SNAP purchasing power.



Notes: The average SNAP purchasing power is 0.71 and thus, a 10 percent increase from the average equals to 0.78. The stars (*** p<0.001, ** p<0.01, * p<0.05, + p<0.1) placed above the confidence interval show the statistical significance of the marginal effect of a $1000 increase in the maximum EITC at a given SNAP purchasing power level. The interaction effect coefficient shown in Table 4.5 indicates whether the effect of a $1000 increase in the maximum EITC statistically significantly changes when the level of SNAP purchasing power increases by 10% from the mean.

Discussion

In this paper, I presented novel evidence on the causal complementary effects between the generosity of EITC and SNAP benefits on cognitive and socioemotional development in the preschool to kindergarten-entry period, using a sample of SNAP recipients. Using children's differential exposure to SNAP purchasing power levels and maximum EITC benefit levels depending on place (market group or state), year of survey assessment, and number of children in household (only for EITC), the current study found that the marginal effect of maximum EITC benefits on early reading and early math skills, and externalizing behavior problems is amplified for children exposed to greater SNAP purchasing power. The effect of a $1000 increase in the maximum EITC benefits on early reading increased from 0.06 SD (statistically insignificant) to 0.14 SD (statistically significant) as SNAP purchasing power increased by 10 percent from its mean, while the effect on early math increased from 0.01 SD (statistically insignificant) to 0.13 SD (statistically significant) with a 10 percent increase in SNAP purchasing power. These are substantial amounts of increase in the EITC effects on cognitive development. To put these magnitudes into context, 0.13 SD – 0.14 SD increase in math and reading scores is comparable to closing 11-14 percent of the gap in math and reading scores at kindergarten-entry between the lowest (the bottom 10 percent) and the highest income (the top 10 percent) children (Reardon, & Portilla, 2016). With respect to externalizing behavior, the effect of a $1000 increase in the maximum EITC benefits increased by 50 percent from 0.30 SD (statistically significant) to 0.46 SD (statistically significant), with a 10 percent increase in SNAP purchasing power. An improvement of 0.46 SD in externalizing behavior is a very large magnitude, which is comparable in size to fully closing the gap in externalizing behavior at kindergarten-entry between children in the bottom and top 10 percent of the income distribution (Reardon, & Portilla, 2016). While a statistically significant complementary effect was not detected for approaches to learning skills, a $1000 increase in the maximum EITC benefits statistically significantly increased approaches to learning skills by 0.20 SD at the average SNAP purchasing power. The marginal effect of EITC on interpersonal skills was not significant, although approaching significance, at the average SNAP purchasing power, with a magnitude of 0.19 SD. Taken together, strong complementary effects are found on cognitive development and one domain of socioemotional development, but not on all outcomes considered in this study. Furthermore, the parental investment pathway was partially supported, suggesting that the complementary effects on child development might be partly driven by complementary increases in parental reading time with the child, in line with the conceptual framework provided in the Background section.

Overall, findings from this study add new evidence to the small but emerging literature on the complementarity of income support programs. The few existing studies have so far focused on dynamic complementarity between early and later educational investments or between educational investments and income support programs. Previous studies have found that an early educational investment works best when it is coupled with a later educational investment (e.g., Head Start program followed by greater K-12 spending or high-quality childcare followed by high quality classroom environments during elementary school) (Johnson, & Jackson, 2019; Ansari, & Pianta, 2018). The current study shows that complementary effects also exist between two income support programs, EITC and SNAP, on early math and reading skills and externalizing behavior in the short run. It is important to note that a large body of child developmental literature documents a close association between early reading, early math, and attention skills and later reading and math achievement (Lee, 2010; Rabiner et al., 2016; Watts et al., 2014; Braak et al., 2022, Duncan et al., 2007). Studies have also found a link between early behavioral problems and later behavioral problems (Lee, 2010), academic achievement (Breslau et al., 2009), and education attainment (Duncan, & Magnuson, 2011). Hence, the short-term estimates found in this study may have longer term implications on children's academic and socioeconomic trajectories, potentially reducing economic and racial inequity in child well-being.

Regarding the probability of excellent/very good health status, I found null results in the main effects of SNAP purchasing power and maximum EITC benefits, respectively, and no evidence of interaction effects. Prior studies that examined the health effects of EITC or SNAP report mixed results. For example, Bronchetti and colleagues (2019) and Miller and Morrissey (2021) provide short-run estimates, finding that SNAP does not statistically significantly affect the probability the child is reported in excellent or very good health by their parent. On the other hand, East (2020) found a positive medium-term effect of SNAP, showing that an additional year of immigrant parents' access to SNAP during utero to age four increases the likelihood that their child is reported in excellent or very good health by their parent. In the EITC literature, one study by Baughman and Duchovny (2016) shows that state EITCs have meaningful effects on the probability that mothers report their child to be in excellent health for children aged 6-14. Also, Hamad, Collin, and Rehkopf (2018) found a short-term (immediate) positive impact of EITC refunds on the general health reported by physicians among children under age 18. Although it is not straightforward as to why the findings vary across previous studies, there can be a few potential reasons. One possibility is that health effects may appear over time among young children rather than immediately after the income boost. The current study estimates a short-term impact of SNAP purchasing power and maximum EITC benefits, which might be the reason for the null results. It is also possible that parents tend to shield children, especially young children, from the direct effect of food insecurity or reduced food intake by reducing their own food consumption during times of scarcity, while maintaining their children's regular diet. Thus, a marginal increase in the generosity of EITC or SNAP might not make much of a difference in general health of young children in early childhood to early school years. In fact, there is evidence that suggests that the positive association between income and health becomes more pronounced as children age (Currie, & Stabile, 2003; Case, Lubotsky, and Paxson, 2002). Another possibility for the null effect is that the vast majority of children in the study sample (81 percent) were reported to have an excellent or very good health. A marginal increase in SNAP purchasing power may not have been enough to improve child health (i.e., being reported by mothers as having very good or excellent health) in the infrequent cases where children were in poor, fair, or good health.

In this study, I found small and statistically insignificant effects of EITC on cognitive outcomes at the average SNAP purchasing power, while its main effect on socioemotional outcomes (externalizing behavior, approaches to learning, interpersonal skills) is relatively large and statistically significant or at least approaching significance. This finding aligns with the results of Hamad, Collin, and Rehkopf (2018), which also found no immediate (short run) effect of EITC refunds on math and reading test scores, although a few other studies found positive effects of EITC on contemporaneous test scores among school-age children (Dahl, & Lochner, 2012; Bastian, & Michelmore, 2018). Although the reason for the statistically insignificant main effect of EITC on cognitive development is unclear, it might be related to the fact that EITC incentivizes working particularly among unmarried mothers and children may be induced to attend low quality childcare. To the extent that cognitive outcomes are more sensitive to maternal employment (less time with mom) and low-quality childcare than socioemotional outcomes in early childhood to early school years, the effect of EITC income on cognitive development could have been diluted. Hence, cognitive outcomes may require consistent income increases over a longer period or larger income increases, than socioemotional outcomes, to show significant improvements. In line with this possibility, when the maximum EITC is lagged by two years instead of one year, it statistically significantly increases early reading and early math scores at the average SNAP purchasing power (i.e., the main effect of EITC), with the magnitudes varying from 0.07 SD to 0.11 SD (see Table C.1 in Appendix C). This possibility also aligns with the study's result that higher SNAP purchasing power, indicative of greater income increases in families, enhances the marginal effect of the maximum EITC on cognitive outcomes (i.e., complementary effect).

There are a few limitations in this paper. First, I could not perfectly capture families who participated in the EITC and SNAP due to data limitations in the ECLS-B. Also, to the extent that SNAP program participation status suffers from endogenous underreporting issues (Bitler, 2020), my decision to determine my sample based on reported participation in SNAP could have biased my estimates. However, since it is critical in this study to capture families who participated in both EITC and SNAP benefits, I view that focusing on SNAP recipients is a better approach than focusing on a population subgroup who has a higher probability of receiving any public benefits, such as unmarried parents with high school degree or below. In Table B.3, I presented findings from this alternative sample and find some evidence of complementary effects, but weaker effects. Another limitation concerns my efforts to assess mediation. Although I provide suggestive evidence on the mechanisms of the complementary effect, using a child fixed-effects approach with the ECLS-B data did not allow me to have sufficient time between the measurement of the mediator and the outcomes to formally test for mediation. Moreover, the ECLS-B data and more generally, quantitative survey data, are limited in their ability to reveal whether and how families perceive these benefits differently and their strategies in using these benefits together. This would be an important area for future qualitative research. Importantly, having this information might provide insights on why the complementary effect was found on one particular domain of socioemotional development (i.e., externalizing behavior), but not on other domains, in the current study. Third, compared to early 1990's or 2009 (ARRA) when there were large federal EITC policy changes, EITC policy changes were relatively modest and occurred only at the state-level during the study period. For instance, the largest increase in maximum federal and state EITC benefits is about $1000 among children who were assessed in 2005 and 2008 in each wave, while the largest increase is about $500 for children assessed in 2005 and 2007 or 2006 and 2007 in each wave. Such modest changes in state EITC policies could have limited my ability to detect the effect of EITC on child development with enough statistical power.

Fourth, although the EITC and SNAP benefit amount and eligibility are not directly affected by one another, it is still possible that they indirectly influence each other through labor supply changes. This is not likely to occur contemporaneously, but their influence on one another may happen over a longer period of time. Research shows that receipt of the EITC refunds serves as a work incentive in the longer-term (Bastian, & Lochner, 2020), although it may serve as a temporary work disincentive immediately after the receipt of refunds (because it increases resources), especially for married women (LaLumia, 2013; Yang, 2017). Thus, in the longer run, labor supply may increase in response to the EITC, which will increase family income. In turn, this could reduce the amount of SNAP benefits a family is eligible for, and certain families may even decide not to participate in SNAP. Similarly, if SNAP benefits reduce labor supply – but importantly, there are mixed findings on the labor supply effect of SNAP (see footnote 6) – this will reduce earnings and can potentially reduce the amount of EITC refunds received in the following calendar year (when they file taxes for the current tax year), depending on the family's earnings level. If families decide to leave the labor market, they will become ineligible for EITC. Importantly, such dynamics between EITC and SNAP are less of a concern at the extensive margin (i.e., the SNAP or EITC *receipt status* being affected by one another). This is because the study sample is restricted to families who reported that they received SNAP in both study waves.

Also, the majority of mothers or resident fathers who worked in wave 2 (which represents, although not perfectly, their work status in the tax year relevant to the EITC received in wave 3) continued to work in later waves (76.5 percent and 80.4 percent of them continued to work in wave 3 and wave K, respectively). Moreover, prior research shows that families with a young child tend to show fairly consistent patterns over time in their welfare benefit receipt status and work status (Slack et al., 2014).

On the other hand, such dynamics between EITC and SNAP are more likely to show up at the intensive margin (i.e., the SNAP and EITC *benefit amount* being affected by one another). Given that the identification strategy of this study comes from relating changes in the maximum EITC benefits and SNAP purchasing power across two study waves to changes in child developmental outcomes, the actual amount of EITC and SNAP benefits received by families in wave K might be lower than what we can expect from the maximum EITC and SNAP purchasing power measures. To the extent that the amount of benefit reduction was substantial, the current study's estimates of the complementary effect could have been underestimated.

Despite these limitations, findings highlight the critical role of two of the largest income support programs in the U.S. in narrowing gaps in child development across SES. The study provides novel insights that greater EITC benefits are more effective at increasing child development when they are combined with more generous SNAP benefits. Although this is a short-run effect, such improvements in child development during critical developmental stages could shape children's longer-term socioeconomic trajectories.

APPENDIX B: CHAPTER IV APPENDIX

Table B.1. States that show changes in state EITC generosity between tax years 2002-2006.

State	Tax year	State credit rate	Refundability
DC	2002	0.25	1
DC	2003	0.25	1
DC	2004	0.25	1
DC	2005	0.35	1
DC	2006	0.35	1
DE	2002	0	0
DE	2003	0	0
DE	2004	0	0
DE	2005	0	0
DE	2006	0.2	0
IN	2002	0.034	1
IN	2003	0.06	1
IN	2004	0.06	1
IN	2005	0.06	1
IN	2006	0.06	1
MD	2002	0.16	1
MD	2003	0.18	1
MD	2004	0.2	1
MD	2005	0.2	1
MD	2006	0.2	1
NE	2002	0	0
NE	2003	0	0
NE	2004	0	0
NE	2005	0	0
NE	2006	0.08	1
NJ	2002	0.175	1
NJ	2003	0.2	1
NJ	2004	0.2	1
NJ	2005	0.2	1
NJ	2006	0.2	1

(Table B.1 continued)

NY	2002	0.275	1
NY	2003	0.3	1
NY	2004	0.3	1
NY	2005	0.3	1
NY	2006	0.3	1
RI	2002	0.25	0
RI	2003	0.05	1
RI	2004	0.05	1
RI	2005	0.1	1
RI	2006	0.15	1
VA	2002	0	0
VA	2003	0	0
VA	2004	0	0
VA	2005	0	0
VA	2006	0.2	0

Notes: Data sources include National Bureau of Economic Research [NBER], 2019) and Komro et al. (2020).

Table B.2. Demographic characteristics by different assessment timing groups.

	2005-2006	2005-2007	2006-2007	2006-2006	2005-2008	2006-2008	Sig.
Two or more children (Ref.: One child) (%)	83.77	83.53	93.6	84.56	79.98	76.9	
Urbanicity (%)							
Rural	20.95	23.07	10.92	24.59	16.64	35.06	
Urban	60.81	62.46	80.26	69.83	72.09	61.39	
Urban-cluster	18.24	14.47	8.82	5.58	11.27	3.55	
US-citizen mother (Ref.: Immigrant mother) (%)	94.77	91	78.16	92.45	97.78	100	*
Parent's highest education attainment (%)							+
No high school degree	20.19	32.75	39.47	17.61	24	17.73	
Highschool degree	46.13	35.35	33.86	44.57	29.07	46.39	
Some college	29.44	28.46	23.14	33.95	45.56	35.88	
BA or higher	4.24	3.44	3.54	3.87	1.37	0	
Household size	4.81	4.60	5.20	4.29	4.73	4.45	+
	(1.65)	(1.60)	(1.65)	(1.42)	(1.82)	(1.60)	

(Table B.2 continued)

Not married (Ref. Married) (%)	65.71	64.02	55.12	76.13	81.91	83.18	
Child race/ethnicity (%)							*
White	34.88	36.25	23.7	26.26	24.32	48.31	
Black	36.87	38.94	27.51	44.99	66.03	36.36	
Hispanic	22.16	17.09	47.35	20.41	6.15	13.78	
Others	6.08	7.73	1.43	8.34	3.5	1.55	
Poverty status							
<50% of FPL	33.48	33.65	37.45	34.85	34.99	27.56	
50%-130% of FPL	53	54.91	58.09	43.36	50.03	59.73	
>130% of FPL	13.52	11.43	4.45	21.79	14.98	12.71	
Male focal child (Ref.: Female focal child) (%)	52.29	46.63	34.59	42.72	56.29	54.94	
Sample size of each group	1050	400	100	150	100	N/A	

Notes: I used the study sample for early reading outcomes (N=1850). Sample size for the '2006-2008' group is N/A because it becomes too small when it is rounded to nearest 50.

Table B.3. Interaction effects between the maximum EITC and the SNAP purchasing power among children with unmarried parents who have a high school degree or below.

	Reading	Math	AL	EB	IP	Health
Maximum EITC (centered)	-0.07	-0.01	0.44**	0.34**	-0.06	-0.01
	(0.07)	(0.06)	(0.13)	(0.12)	(0.16)	(0.03)
SNAP purchasing power (centered)	-0.18	0.00	-0.61+	-0.39	0.01	-0.08
	(0.18)	(0.17)	(0.33)	(0.24)	(0.45)	(0.12)
Maximum EITC X SNAP purchasing power	0.05	0.15+	0.31*	0.09	0.30	-0.01
	(0.06)	(0.08)	(0.14)	(0.16)	(0.20)	(0.03)
Mean of outcome	-0.57	-0.59	-0.39	-0.32	-0.16	0.78
N	1150	1150	550	550	550	1300
Child fixed effects	Y	Y	Y	Y	Y	Y
Year fixed effects	Y	Y	Y	Y	Y	Y
Demographic characteristics	Y	Y	Y	Y	Y	Y
Regional economic and policy conditions	Y	Y	Y	Y	Y	Y
Housing price	Y	Y	Y	Y	Y	Y

Notes: 'AL' indicates approaches to learning; 'IP' indicates interpersonal skills; 'EB' indicates externalizing behavior. *** p<0.001, ** p<0.01, * p<0.05, + p<0.1. All estimates are weighted.

Table B.4. Main effects of maximum EITC benefits and SNAP purchasing power without their interaction term.

	Reading	Math	AL	EB	IP	Health
Maximum EITC (centered)	-0.01	-0.04	0.19*	0.25*	0.24+	0.01
	(0.07)	(0.04)	(0.09)	(0.09)	(0.14)	(0.02)
SNAP purchasing power (centered)	-0.07	-0.00	0.13	-0.42	0.24	-0.05
	(0.19)	(0.17)	(0.38)	(0.39)	(0.51)	(0.11)
N	1850	1850	850	850	850	2000

Notes: 'AL' indicates approaches to learning; 'IP' indicates interpersonal skills; 'EB' indicates externalizing behavior. *** p<0.001, ** p<0.01, * p<0.05, + p<0.1. All estimates are weighted.

Table B.5. Interaction effects between the maximum EITC and the SNAP purchasing power among children whose mother or resident father was working in wave 2.

	Reading	Math	AL	EB	IP	Health
Maximum EITC (centered)	0.08	0.02	0.19	0.35***	0.20	-0.02
	(0.08)	(0.06)	(0.15)	(0.09)	(0.17)	(0.03)
SNAP purchasing power (centered)	-0.17	0.13	0.12	0.05	0.06	-0.08
	(0.19)	(0.20)	(0.49)	(0.44)	(0.59)	(0.07)
Maximum EITC X SNAP purchasing power	0.14*	0.19*	-0.24	0.01	-0.40	-0.01
	(0.06)	(0.08)	(0.16)	(0.16)	(0.25)	(0.05)
N	1200	1200	550	550	550	1250
Child fixed effects	Y	Y	Y	Y	Y	Y
Year fixed effects	Y	Y	Y	Y	Y	Y
Demographic characteristics	Y	Y	Y	Y	Y	Y
Regional economic and policy conditions	Y	Y	Y	Y	Y	Y
Housing price	Y	Y	Y	Y	Y	Y

Notes: 'AL' indicates approaches to learning; 'IP' indicates interpersonal skills; 'EB' indicates externalizing behavior. *** p<0.001, ** p<0.01, * p<0.05, + p<0.1. All estimates are weighted.

Table B.6. Interaction effects between the maximum EITC and the SNAP purchasing power adjusting for Medicaid participation status.

	Reading	Math	AL	EB	IP	Health
Maximum EITC (centered)	0.06	0.01	0.22*	0.32***	0.20+	-0.00
	(0.05)	(0.04)	(0.09)	(0.07)	(0.11)	(0.02)
SNAP purchasing power (centered)	-0.24	-0.05	-0.09	-0.34	0.09	-0.01

(Table B.6 continued)

	(0.17)	(0.16)	(0.42)	(0.35)	(0.52)	(0.09)
Maximum EITC X SNAP purchasing power	0.11*	0.19**	0.04	0.25*	-0.19	-0.04
	(0.04)	(0.07)	(0.10)	(0.12)	(0.15)	(0.03)
N	1850	1800	850	850	850	1950
Child fixed effects	Y	Y	Y	Y	Y	Y
Year fixed effects	Y	Y	Y	Y	Y	Y
Demographic characteristics	Y	Y	Y	Y	Y	Y
Regional economic and policy conditions	Y	Y	Y	Y	Y	Y
Housing price	Y	Y	Y	Y	Y	Y

Notes: 'AL' indicates approaches to learning; 'IP' indicates interpersonal skills; 'EB' indicates externalizing behavior. *** p<0.001, ** p<0.01, * p<0.05, + p<0.1. All estimates are weighted.

Table B.7. Relationship between TFP price and other prices.

	(1) TFP price	(2) TFP price
Regional CPI for apparel costs	-0.15***	0.02***
	(0.01)	(0.00)
Regional CPI for education costs	0.67***	0.00
	(0.02)	(0.01)
Regional CPI for recreation costs	-0.23***	0.07***
	(0.03)	(0.01)
Regional CPI for transportation costs	0.15***	-0.02***
	(0.01)	(0.00)
Fair market rent for 2-bedroom	0.04***	0.00
	(0.00)	(0.00)
Market group FE		Y
Year FE		Y
N	15,486	15,486
R-squared	0.51	0.97

Notes: This table is generated from regressions of TFP price (which is used to construct the SNAP purchasing power measure) on other price measures. County-year level data (2003-2007 years) are used to run these regressions. *** p<0.001, ** p<0.01, * p<0.05, + p<0.1.

Table B.8. Interaction effects between the maximum EITC and the SNAP purchasing power adjusting for EITC purchasing power.

	Reading	Math	AL	EB	IP	Health
Maximum EITC (centered)	0.06	0.01	0.20*	0.31***	0.18	-0.00
	(0.05)	(0.04)	(0.09)	(0.08)	(0.11)	(0.02)
SNAP purchasing power (centered)	-0.27	-0.06	-0.10	-0.43	0.07	-0.00
	(0.17)	(0.17)	(0.44)	(0.37)	(0.54)	(0.09)
Maximum EITC X SNAP purchasing power	0.12**	0.18*	0.03	0.23*	-0.20	-0.05
	(0.04)	(0.07)	(0.10)	(0.11)	(0.15)	(0.04)
EITC purchasing power (Max EITC / FMR)	0.84	0.39	1.06	0.66	3.06*	-0.09
	(0.91)	(0.49)	(1.23)	(1.08)	(1.45)	(0.24)
Observations	1850	1800	850	850	850	2000
Child fixed effects	Y	Y	Y	Y	Y	Y
Year fixed effects	Y	Y	Y	Y	Y	Y
Demographic characteristics	Y	Y	Y	Y	Y	Y
Regional economic and policy conditions	Y	Y	Y	Y	Y	Y

Notes: 'AL' indicates approaches to learning; 'IP' indicates interpersonal skills; 'EB' indicates externalizing behavior. *** p<0.001, ** p<0.01, * p<0.05, + p<0.1. All estimates are weighted.

APPENDIX C: CHAPTER IV SUPPLEMENTARY ANALYSIS

Analysis for One-Year and Two-Year Lagged Effects of Maximum EITC Benefit and SNAP Purchasing Power

I ran an additional analysis that tests the main effects and interaction effects for both one-year lagged and two-year lagged EITC and SNAP measures. Table C.1 shows the results. In the one-year lagged estimates, the complementary effects as well as the main effects of EITC and SNAP were similar to what I found from my main model. There were just small changes in statistical significance and magnitudes of the estimates (e.g., the positive main effect of maximum EITC on interpersonal skills became marginally significant).

In terms of the two-year lagged estimates, a few interesting insights were found. First, while the main effects of SNAP purchasing power in the one-year lagged period had an opposite direction (but statistically insignificant) from my expectation for most outcomes (see Table 4.3), in the two-year lagged period, the main effects of SNAP purchasing power became positive across most outcomes, even with a larger magnitude than the main effects of maximum EITC (e.g., for early reading, the main effect of SNAP purchasing power is 0.32 SD vs. the main effect of maximum EITC is 0.11 SD). This finding on SNAP purchasing power aligns with the results found in the first empirical paper (Chapter 3). However, unlike in Chapter 3, the main effects of two-year lagged SNAP purchasing power were still imprecisely estimated in this study, which may potentially be due to small variation left in SNAP purchasing power after including all the covariates and interaction terms, especially given the modest sample size. The model is more complex in this paper compared to Chapter 3, since the one-year lagged and two-year lagged main effects of maximum EITC and SNAP purchasing power as well as their interaction effect terms have been added to the model (six terms in total), which could have further decreased the remaining variation in SNAP purchasing power. Nevertheless, this result supports the possibility that the measurement issue of the SNAP purchasing power might have led to statistically insignificant, negatively directed main effects of SNAP purchasing power in the one-year lagged period.

Second, the main effects of maximum EITC received in the two-year lagged period – particularly on cognitive outcomes – became larger than the main effects of maximum EITC received in the one-year lagged period (which is not the case for socioemotional development). As Table C.1 indicates, a $1000 increase in the two-year lagged maximum EITC benefits statistically significantly increases early reading skills by 0.11 SD and early math skills by 0.07 SD for children exposed to the average SNAP purchasing power. This might be suggesting that consistent income increases over a longer period or larger income increases are needed to have a significant effect on cognitive development, compared to socioemotional development.

Lastly, the interaction effects between the two-year lagged maximum EITC and SNAP purchasing power were no longer statistically significant, with a smaller magnitude compared to the one-year lagged interaction effects. This shows that the complementary effect no longer exists between the EITC and SNAP benefits received in two years prior to the assessment year.

Table C.1. Interaction effects between the maximum EITC and the SNAP purchasing power with one-year and two-year lags.

	Reading	Math	AL	EB	IP	Health
Maximum EITC (one-year lag, centered)	0.01	-0.02	0.20+	0.27**	0.23+	-0.00
	(0.07)	(0.04)	(0.10)	(0.08)	(0.13)	(0.02)
SNAP purchasing power (one-year lag, centered)	-0.06	-0.01	0.12	-0.40	0.21	-0.04
	(0.19)	(0.17)	(0.35)	(0.43)	(0.50)	(0.12)
Maximum EITC X SNAP purchasing power (one-year lag)	0.11*	0.16*	0.03	0.23*	-0.21	-0.05
	(0.04)	(0.06)	(0.11)	(0.10)	(0.17)	(0.04)

(Table C.1 continued)

Maximum EITC (two-year lag, centered)	0.11*	0.07*	-0.02	0.10	-0.14	0.01
	(0.04)	(0.03)	(0.10)	(0.09)	(0.08)	(0.03)
SNAP purchasing power (two-year lag, centered)	0.32	0.13	0.56	-0.12	0.45	-0.12
	(0.19)	(0.16)	(0.42)	(0.47)	(0.72)	(0.08)
Maximum EITC X SNAP purchasing power (two-year lag)	0.02	-0.03	-0.02	0.12	-0.14	0.02
	(0.06)	(0.06)	(0.17)	(0.16)	(0.18)	(0.05)
N	1850	1850	850	850	850	2000
Child fixed effects	Y	Y	Y	Y	Y	Y
Year fixed effects	Y	Y	Y	Y	Y	Y
Demographic characteristics	Y	Y	Y	Y	Y	Y
Regional economic and policy conditions	Y	Y	Y	Y	Y	Y
Housing price	Y	Y	Y	Y	Y	Y

Notes: 'AL' indicates approaches to learning; 'IP' indicates interpersonal skills; 'EB' indicates externalizing behavior. *** p<0.001, ** p<0.01, * p<0.05, + p<0.1. All estimates are weighted.

CHAPTER V: CONCLUSION

The findings presented in this dissertation expand knowledge on the value of income support policies on children's cognitive and socioemotional development in preschool to kindergarten-entry. The motivation from this study stems from a broader question as to whether and how income matters for child development. In Chapter 2, I provided an overarching conceptual framework of the income effect on child development. Through the two standalone empirical papers, I investigated the impacts of two of the largest income support policies in the U.S., the SNAP and EITC programs, on child development and explored some of the potential mechanisms that may explain their effects.

In the first empirical paper (Chapter 3), I focused on SNAP benefits alone. I found that there are large variations in the purchasing power (real value) of SNAP benefits across regions and over time and employed these variations to estimate the plausibly causal impact of SNAP benefit generosity on child development, while taking into account different timings of SNAP purchasing power exposure. This paper showed that greater SNAP purchasing power, when lagged by two years relative to the assessment of child outcomes, has positive effects on early reading and math scores, as well as approaches to learning and externalizing behavior. I also provided suggestive evidence that these effects may be explained by parental investment and family stress pathways.

In the second empirical paper (Chapter 4), I examined how the combination of EITC and SNAP programs interacts to affect child development. To the extent that they show complementary effects, simply evaluating their effects, separately, on child development may lead to an underestimation of the benefits of these policies on child well-being. This paper provided new evidence that there are complementary effects between EITC and SNAP benefits, showing that the effect of larger EITC benefits (measured as the maximum state and federal combined EITC benefits) on early reading and math scores, as well as externalizing behavior statistically significantly increases when they are coupled with larger SNAP benefits, measured as greater SNAP purchasing power. The effect of maximum EITC benefits on approaches to learning was also positive and statistically significant at the mean level of SNAP purchasing power, but there was less evidence of complementary effects. In terms of mechanisms, results provided partial support for the parental investment pathway. More rigorous analyses should be conducted to evaluate the mechanisms, however, given the limited set of potential mediators in the ECLS-B.

Taken together, these three chapters make conceptual and empirical contributions to the existing literature. In particular, the empirical papers contribute to social work and public policy literature by demonstrating to what extent SNAP benefits alone, as well as in combination with EITC benefits, could mitigate the detrimental effects of poverty on young children's development and in turn help them build resilience to overcome additional hardships. These findings also contribute to developmental science by showing the plausibly causal effects of income on cognitive and socioemotional development during a critical developmental stage.

This dissertation provides a few potential directions for future research. First, given that I estimated short run effects of SNAP benefits and complementary effects between EITC and SNAP benefits, future research should consider evaluating whether these effects persist in longer-run outcomes, such as academic achievements among older children, education attainment, and labor market outcomes in adulthood, using different longitudinal data.

Second, I could not conduct a formal mediation analysis on the parental investment and stress mechanisms due to methodological challenges. To address this limitation, future research could use different datasets or empirical strategies – rather than the child fixed-effects approach – to employ a temporal order of the program generosity measure (i.e., independent variable), mediator, and outcome. Furthermore, future research could test biological pathways, which I was unable to investigate because of data limitations. In Chapter 2, I described how biological pathways may explain the mediating role of parental investment and family stress mechanisms in the effect of income on children's cognitive, socioemotional, and health outcomes. By testing biological mechanisms through which poverty may influence child development, such empirical evidence can inform the development of evidence-based early childhood programs that could contribute to preventing poverty from becoming biologically embedded in the brain and body. For example, if a study finds that poverty leads to a child's elevated stress response through parental stress and parenting behavior, this evidence can form the basis for interventions like home visiting programs that can support parents emotionally and physically, helping them provide more nurturing and responsive care. Other helpful interventions could include programs that remove physical stressors (e.g., neurotoxic exposures) at home and in the neighborhood, as well as programs that support parents in their engagement in cognitively stimulating activities with the child, which could benefit the child's brain development. Moreover, testing *how* small-scale or large-scale anti-poverty programs reduce the negative effects of poverty on biology and subsequently improve child development will also provide critical evidence for expanding government anti-poverty programs, such as EITC, SNAP, Child Tax Credit (CTC), etc.

Third, given the complementary effect found between EITC and SNAP, complementary effects may also exist among other U.S. safety net programs, including Medicaid and CTC, both of which have greatly expanded after the study period. This would also be an interesting area for future research.

Fourth, building on the results of this dissertation, future research, by using larger samples, could explore whether the effects of SNAP purchasing power and the complementary effects of EITC and SNAP vary across demographic characteristics such as race and ethnicity. Such an analysis would enable an explicit assessment of the impacts of multiple program participation on equity.

Moreover, findings from this dissertation provide important policy implications on the current EITC and SNAP policies. In both empirical papers, I showed that more generous SNAP and EITC benefits are beneficial for marginalized children's well-being, enhancing their cognitive and socioemotional development. These results contribute timely evidence to current policy debates about the value of income support programs for child well-being and suggest the importance of expanding the reach and generosity of current SNAP and EITC policies. There have been consistent efforts – even recently – to make budget cuts in the SNAP program (Bergh, & Rosenbaum, 2023; Llobrera, 2024) and there are still many states that have not yet implemented their own state EITC programs (as of 2023, 31 states and D.C. have state EITC programs). Findings from this dissertation can inform the decisions of policymakers who are evaluating the cost-effectiveness of any proposed changes to the EITC and SNAP budgets.

This dissertation makes a few specific implications on the current EITC and SNAP policies. First, using the SNAP purchasing power approach, I showed the value of increasing the real value of SNAP benefits by accounting for local costs of living. This dissertation revealed that there are significant variations in SNAP purchasing power across regions. Increasing SNAP benefit levels to account for local prices, especially in high priced regions, would address unevenness in the value of SNAP by geography, thereby promoting equity so that all low-income families, regardless of where they live, have the same opportunity to benefit from SNAP's beneficial effects on children’s cognitive and socioemotional development. In fact, it is the case that most public programs in the U.S. do not adjust benefit levels based on local costs of living. An analysis of local SNAP purchasing power has provided one clear example of the inequities resulting from not doing so. While implementing this change in the SNAP program may be politically challenging in the near future, this dissertation underscores that the U.S. social safety net system should endeavor to move towards that goal. To reduce regional disparities in SNAP purchasing power, policymakers could also consider implementing state-level SNAP supplements. For example, adopting and expanding existing state-level programs, such as the Summer Electronic Benefit Transfer program, can be a potential option for state policymakers.

Second, using the variation in state EITC policy changes over time, I showed the value of implementing and expanding state EITCs. By increasing the generosity of state EITCs, particularly refundable state EITCs, we can improve the effectiveness of EITC in promoting children’s healthy development during critical developmental stages. Moreover, according to the previous study (Neumark, & Williams, 2020), having a state EITC program also increases the likelihood of families taking up the federal EITC program.

Third, in order to benefit from the complementary effect of EITC and SNAP, it is important that all eligible families and children participate in both programs. Although the take up rates are higher in the EITC and SNAP compared to other safety net benefits[20], not all eligible families are receiving these programs due to administrative burdens (Moynihan, Herd, & Harvey, 2014). Furthermore, lower income people and people of color tend to experience

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[20] Estimates of take-up rates by eligible beneficiaries of SNAP and EITC are not 100%: SNAP individual participation rates ranged from 54 percent to 69 percent between 1997 and 2007 (Leftin, & Wolkwitz, 2009), which continued to increase over time and reached 85 percent in fiscal year 2016 (Vigil, 2019); while take-up rates of federal EITC in tax year 2016 were around 80 percent (Tax Policy Center, 2020).

unequally greater administrative burdens in social safety nets (Jackson, & Fanelli, 2023). Therefore, findings from this dissertation suggest the importance of simplifying the application and recertification process in the EITC and SNAP. Policymakers could consider increasing government funding to community organizations involved in public outreach, such as Volunteer Income Tax Assistance program and SNAP Outreach program. These agencies could encourage families to file taxes and apply for SNAP benefits by helping them complete application forms and fill out tax forms, gather the necessary documentation, submit materials, and go through recertification. Policymakers could also simplify tax filing by creating a “non-filer online portal” for those who are not required to file tax returns – the population subgroup who is less likely to apply for EITC benefits.

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