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Doctoral Dissertations, 2011-2026 Economics

2019

Essays in Public Economics

Brown, Jessica Hedrich

Princeton, NJ : Princeton University

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http://arks.princeton.edu/ark:/88435/dsp01v405sd259

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Essays in Public Economics

Jessica Hedrich Brown

A Dissertation

Presented to the Faculty

of Princeton University

in Candidacy for the Degree

of Doctor of Philosophy

Recommended for Acceptance

by the Department of Economics

Adviser: Alan Krueger

June 2019

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Abstract

This dissertation consists of three chapters in public economics. A common theme throughout is the use of empirical methods to evaluate public policies or programs.

The first chapter studies the unintended impact of public pre-K for 4-year-olds on private child care for infants and toddlers. Private facilities often care for children from infancy through pre-K. A public option for older children could therefore affect availability, prices, or quality of care for younger children. I use administrative data on private facilities and implement a neighborhood-level difference-in-differences design leveraging spatial variation in the locations of new sites during New York City's 2014 Universal Pre-K expansion. I find the pre-K program reduced the availability of center-based care for children under age 2, and the decline occurred entirely in the poorest areas. Furthermore, new public pre-K sites caused an increase in inspection violations and public complaints at nearby day care centers, suggesting decreases in quality due to the increased competition.

The second chapter estimates the effect of the "Own Your Future" information campaign on long-term care planning behaviors. Using Health and Retirement Study data and a difference-in-differences design leveraging staggered campaign adoption by twenty-five states over five years, I find an increase in long-term care insurance coverage of one percentage point, or ten percent, concentrated among high-asset individuals. A back-of-the-envelope calculation indicates present-value Medicaid savings of $400 million. I also find campaign-induced increases in life insurance coverage and self-reported probabilities of leaving a large bequest.

The third chapter considers a change to the standard mandated benefits model to account for imperfect information. When individuals do not know about the benefit, it is equivalent to a payroll tax, increasing deadweight loss. On the other hand, if workers are unaware of benefits until they need them, moral hazard is reduced, lowering program cost and therefore the implicit tax. I establish empirically that many workers are unaware of Temporary Disability Insurance and apply an updated model to this benefit that incorpo-

rates this tradeoff. I find that under most plausible parameter assumptions, an awareness campaign would reduce deadweight loss.

Acknowledgements

I will be forever grateful to my adviser, Alan Krueger, for his support, insights, and advice through the last four years. My work was greatly enhanced by his guidance. He passed away on March 16, 2019, and I have missed his insights and presence since then. I feel fortunate to have had the opportunity to work with him, and I very much wish that he could be here to see the finished product.

I am also especially grateful for the mentorship of Leah Boustan, who not only provided sound research and professional advice but also much-needed understanding of the joys and challenges associated with raising small children amidst it all. I thank Will Dobbie for his insights throughout this project and particularly for his detail-oriented comments on early drafts. My academic writing has been significantly improved by his efforts. I also thank the other members of my committee, Ilyana Kuziemko and Alex Mas, whose comments and suggestions have greatly enriched this dissertation. I thank Ilyana in particular for encouraging me to pursue the idea that became the first chapter of my dissertation. A special thanks to my entire committee for their support through these last two months in Alan's absence.

My research also benefited from the advice and thoughtful comments of many other members of the Princeton faculty. Specifically, thank you to Janet Currie, Hank Farber, Bo Honoré, Adam Kapor, David Lee, and Harvey Rosen. I also thank Linda Belfield, Laura Hedden, Lori Mitrano, Jeannie Moore, and Patti Tracey for their assistance during my time at Princeton and especially on the job market. I gratefully acknowledge financial support provided by the Graduate School at Princeton University, the Richard A. Lester Fellowship for Industrial Relations, and the Louis A. Simpson *60 Graduate Fellowship.

My experience at Princeton has been enriched by the many friends I have made in my graduate school cohort and in the Industrial Relations Section, including David Arnold, Emily Battaglia, Mingyu Chen, David Cho, Julia Fonseca, Felipe Goncalves, Dan Herbst, Elisa Jacome, Andrew Langan, Steve Mello, and Terry Moon. I am particularly grateful

for the friendship of Julia Fonseca, without whom first year would have been much more difficult and graduate school much lonelier. As the only women in our cohort, we were fast friends, and I could not have asked for a better one. I am also especially indebted to Steve Mello for his near-endless appetite for discussing research and for the invaluable insights and advice gleaned therein, many of which are incorporated in this dissertation.

I thank the many teachers and professors who have guided me prior to graduate school, especially Bill Evans and Jim Sullivan. Their courses and mentorship sparked my interest in economics, and I never would have pursued graduate school without their encouragement. I also thank Louise Moses for encouraging my love of math and problem solving from a young age, skills that provided the foundation for my later studies, and Claudia Polini for reigniting that love in the first semester of my undergraduate studies.

Finally, I owe the deepest debt of gratitude to my family for their unending love and support throughout these graduate school years. To my husband, Patrick, for agreeing to this crazy adventure and for being my greatest source of support along the way. And especially for taking a year "off" to be a stay-at-home dad after Ellie was born, allowing me to focus during the critical fifth year. Words cannot do justice to how much your love and support has meant to me. To my son, Teddy, for his earnest spirit that warms my heart, for providing the inspiration for the first chapter of my dissertation, and for, as an infant, having the semi-predictable nap schedule that allowed me to write my third-year paper, which became the third chapter of this dissertation. To my daughter, Ellie, for being an impish ray of sunshine whose smiles keep me going and for (mostly) allowing daddy to put her to bed when I couldn't be there. I am also grateful to my parents, Peter and Michelle Hedrich, for always believing in me and encouraging me to pursue my dreams without limits.

To Patrick, Teddy, and Ellie.

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Chapter 1

Does Public Pre-K Have Unintended Consequences on the Child Care Market for Infants and Toddlers? [1]

1.1 Introduction

In the past 15 years, enrollment in public pre-kindergarten programs has nearly doubled. By 2016-17, state-funded pre-K served about 1.5 million children, enrolling over 30 percent of 4-year-olds and accounting for over $7.6 billion in spending (Friedman-Krauss et al., 2017). Expansions are ongoing and remain part of conversations at the national level, with President Obama's "Preschool for All" initiative, and the local level, with recent expansions in Philadelphia, Seattle, and Albequerque. The expansions are driven by research on the

[1]This chapter was previously presented at the Economic Graduate Students' Conference at Washington University in St. Louis in October 2018 and and at the Association for Education Finance and Policy annual conference in March 2019. I am grateful to my advisor, Alan Krueger, for his guidance and support throughout this project. I am also grateful to Leah Boustan and Will Dobbie for their advice and encouragement. I benefited from helpful comments from Janet Currie, Bill Evans, Chloe Gibbs, Felipe Goncalves, Bo Honoré, Adam Kapor, Ilyana Kuziemko, Alex Mas, Steve Mello, and seminar participants at the University of Notre Dame, Haverford College, and Princeton University. All errors are my own.

benefits of quality pre-kindergarten, particularly for disadvantaged children, and hopes of closing achievement gaps already present when children enter kindergarten.

Although free public pre-K expands access to early childhood education, it also draws some children to the public sector who would have attended private pre-K otherwise (Cascio and Schanzenbach 2013; Fitzpatrick 2010). On the private market, these 4-year-olds are often cared for in facilities that also care for younger children. Crowd out due to the public expansion could therefore affect the availability, price, and/or quality of child care for younger children between the ages of 0 and 3. García et al. (2018) show that investments in human capital development have larger returns for younger ages, suggesting that care environments for children under age 4 may be even more influential than at age 4. Changes in the child care market could thus have a significant impact on the human capital accumulation of affected children.

Without empirical evidence, it is not clear whether public pre-K programs would increase or decrease availability of private child care for younger children. If 4-year-olds shift to the public market from the private market, private day care facilities may be negatively affected by the lost revenue. Reduced 4-year-old enrollment may be especially problematic for the private sector because centers typically cross-subsidize the very labor-intensive and costly care of infants and toddlers with tuition dollars from less costly older children.[2] If the most profitable children leave, day care centers may need to raise prices, shut down, or reduce quality.

On the other hand, if the supply of buildings suitable for child care is relatively inelastic, if 4-year-olds shift out of private care, it could free up classrooms that could be converted for use by younger children, which could increase availability of care for younger children. Additionally, because public pre-K only operates during school hours, it may increase demand for before- and after-school care. The government's emphasis on quality early childhood

[2]For example, in New York City, licensing requirements allow one teacher to care for twelve 4-year-olds but only four infants. Day cares charge more for infants but not enough to make up for the cost difference. For a more detailed explanation, see Section 1.3.

education could also increase parents' demand for quality child care at younger ages, shifting their demand curve through a "crowd in" effect.[3]

Center-based care is often the highest-quality form of non-parental care available, particularly for children from lower-income families, so a reduction in availability would be likely to shift children to lower-quality care at a critical time for human capital development (Bassok et al. 2016b; Bernal and Keane 2010; Loeb et al. 2007). A decrease in availability or increase in price of child care could also decrease female labor force participation (Cascio 2009b; Baker et al. 2008; Gelbach 2002; Berger and Black 1992).

This paper uses a quasi-experiment created by New York City's 2014 Universal Pre-K (UPK) expansion to estimate the effect of new public pre-K sites on availability and quality of care for younger children. In 2014, NYC added over 25,000 new free, full-day public pre-K seats and increased the number of pre-K sites by over 400 on net. The spatial variation in the location of new sites leads some neighborhoods to be more exposed to the expansion than others.[4] To exploit this variation in exposure, I use a difference-in-differences estimator to compare changes in capacity and quality in the child care market before and after 2014 in areas that were more exposed to UPK to areas that were less exposed.

New York City is an ideal setting to examine this question for several reasons. First, due to the large scale of the roll out, there are over 400 new site "experiments" to analyze, many more than would exist in a typical city. Relatedly, the scope of the program, which enrolls 70,000 students each year, means the effects of the program are independently interesting. To give a sense of scale, there are more 4-year-olds in public pre-K in NYC than there are students in the entire Boston Public School system. Second, the structure of the New York

[3]There are a number of other forces that could generate positive or negative effects on the child care market for younger children. These include general equilibrium labor market effects and parental income effects. Also, many public pre-K programs partner with day care centers to provide some of the pre-K classrom space, which could induce entry or reduce exit of the centers. See Section 1.3 for additional discussion.

[4]Parents prefer to send their children to schools that are closer compared to ones that are farther away, and this preference is especially strong for the youngest children (Dinerstein and Smith, 2014). Therefore, the distribution of sites across the city leads to differential exposure to UPK across neighborhoods.

City licensure data allows me to specifically identify the effect of the roll out on younger children, in contrast to data available for previous studies that only allowed for analysis of the market summed across all ages (Bassok et al. 2014; Bassok et al. 2016a). NYC requires separate licenses for classrooms with children under the age of 2 years and classrooms with children ages 2 years to 5 years, so I am able to observe and analyze capacity separately for this younger age group. Finally, NYC is in the process of rolling out UPK for 3-year-olds. Analyzing the response of the child care market to the first expansion is an important step in predicting what might happen to the child care market in light of the additional expansion.

In order to take advantage of spatial variation in exposure, it is necessary to define a relevant market for each UPK. In addition, because the outcome of interest is the stock of day care centers, it is necessary to define areas over which to measure the stock. This area could be the same or different from the market for the UPK. There is no standard reduced-form approach for defining this area or the market for a UPK in this type of setting.[5] I approach this problem by dividing NYC into a grid of tessellated hexagons and allowing the child care market in a given hexagon to be affected by new UPK sites in that hexagon and in neighboring hexagons.[6] There are several advantages to this approach. It is able to handle the spatial density of treatment by aggregating the number of sites to the hexagon level. Also, allowing UPK sites to affect neighboring hexagons implicitly allows child care markets to overlap. Furthermore, the method allows for straightforward robustness checks of the sensitivity of results to geographic boundaries through changing the size of the hexagons and shifting the grid. Sensitivity to boundaries is something that is often ignored but has been shown to be important in other contexts (Foote et al., 2017).

[5]Two approaches for defining markets in the literature include the "ring method" and using pre-existing administrative boundaries. The ring method is not preferred here because the spatial density of treatment causes the rings to overlap substantially. Using administrative boundaries often leads to areas that are irregularly-shaped, and there is no natural way to test robustness to them. See Section 1.5 for more details.

[6]Hexagons are preferred to squares because all of a hexagon's neighbors have the same relationship to the central hexagon, making it more natural to pull in "neighbor effects." Squares have both same-side and diagonal neighbors.

I have three sets of findings. First, the expansion of UPK decreased availability of center-based care for infants and toddlers. A new UPK site reduces capacity at day care centers for children under the age of 2 by 1.6 seats in an average hexagon with an area of ten blocks. Overall, I estimate that there would be 20% more seats in day care centers for children under age 2 in the absence of the Universal Pre-K program. This decrease in child care center seats for younger children does not seem to be offset by an increase in capacity in licensed home day care centers. However, the analysis of the home day care market can only be suggestive as I do not observe the actual age distribution of the children cared for in this market.

Second, heterogeneity analysis reveals that the entire decline in day care center capacity occurs in poorer areas of the city. I split the hexagons into two groups based on the median fraction of people living below 200% of the federal poverty line and find no effect on total capacity in the wealthier hexagons. A likely explanation for the result that the decrease in capacity is entirely in poorer areas is that the price elasticity of demand for child care is higher in poorer areas. Now the effect of the decreased capacity for younger children on total welfare depends critically on parents' outside option for child care, and in poorer areas, children are likely crowded out to home day cares or informal family and friends care. Based on previous literature, children crowded out of the center-based market to home day cares or other informal settings are likely to experience environments that are less conducive to human capital development (Bassok et al. 2016b; Bernal and Keane 2010; Loeb et al. 2007). Given that returns to human capital investment are generally the highest at younger ages (García, Heckman, Leaf and Prados, 2018), this change could harm their future outcomes.

Third, I analyze inspections violations as a proxy for safety and quality changes. Some of the most common violations include failing to conduct on-time background checks, missing staff or child medical clearances, and not maintaining the physical facility. When a day care faces pressure due to a loss of 4-year-olds, they may attempt to reduce their costs by lowering quality. I find that within-center, the number of inspections violations increases by 13% at day care centers that share a 10-block area hexagon with a new UPK site. They are also more

likely to do poorly enough on the initial inspection to trigger a follow-up inspection. Public complaints against centers near new UPK sites also increased, suggesting that these changes in inspection outcomes point to observable decreases in quality. These results suggest that centers are reducing costs as a result of the competition from public pre-K in a way that affects the quality of care available on the private market.

These empirical results build on the literature studying crowd out in early childhood education. So far, the literature has focused on the direct crowd-out effect of 4-year-olds switching from private pre-K to public pre-K. Crowd out is of interest because it lowers the expected return to public pre-K. Significant crowd out of private pre-K attendance has been documented for some demographic groups by Cascio and Schanzenbach (2013). Additionally, Fitzpatrick (2010) finds that the total increase in pre-K attendance in response to Universal Pre-K expansions is smaller in urban areas, suggesting that there is more crowd out in areas like NYC where there are more potential private market substitutes.[7] I contribute to this literature by expanding the discussion to younger children who may be affected by public pre-K's crowd out of private child care facilities. Just as direct crowd out of 4-year-olds lowers the expected return to public pre-K, if public pre-K causes younger children to be crowded out to lower quality care, it would also lower the expected return.

Most closely related to my work, there are two other studies that estimate the effect of public pre-K on the supply of child care. Bassok, Fitzpatrick and Loeb (2014) and Bassok, Miller and Galdo (2016a) find that public pre-K expands, or at least holds fixed, the size of the child care market. This net expansion is likely due to an increase in the number of seats for 4-year-olds due to the new program, and data available for these previous studies did not allow for disaggregating the change in provision by age group. Considering the total capacity

[7]There are additional studies of crowd out in early childhood education more broadly. For example, when public kindergarten was introduced to public schools, it crowded out enrollment in private kindergarten and Head Start (Cascio, 2009a). Bassok (2012) studies the crowd out effect of public pre-K on another public alternative, Head Start, and finds that public pre-K reduced Head Start enrollment for 4-year-olds and increased enrollment for younger children. Kline and Walters (2016) study substitutes for Head Start and develop a framework for evaluating the effect of public programs with close substitutes.

across all ages potentially masks decreases in capacity at younger ages caused by public pre-K expansions. Although they could not separately estimate supply effects for younger children, using state-level ACS data, Bassok, Miller and Galdo (2016a) do estimate that reported preschool attendance for 3-year-olds declines, indicating that separating increases in provision for 4-year-olds from changes in provision at younger ages is important.

I advance this research in three ways. First, my data allow me to separately analyze the effect of public pre-K on child care supply for younger children between the ages of 0 and 2. I am the first to study the unintended consequences of UPK on this non-targeted age group. I also suggest additional mechanisms through which this effect may operate. Second, my identification strategy allows me to examine the sizeable heterogeneity in the supply response by the level of poverty in the neighborhood. Understanding this heterogeneity is important for understanding welfare implications because the next best option for care will be much different for wealthier families than for poorer families. Third, I am the first to consider the unintended impact of UPK on the safety and quality of child care supplied in the private sector. For this analysis, I use longitudinal administrative inspections and complaints data, and to my knowledge, I am the first to use data of this kind in any context.[8]

I also add to a small literature on the effect of public policies on day care center quality. Previous papers have studied whether policies directly targeting improved day care center quality have been successful (Hotz and Xiao 2011; Blau 2007; Chipty and Witte 1999; Chipty 1995). I contribute to this literature by studying the unintended consequences on child care quality of a public policy that does not directly target this outcome.[9]

Furthermore, I add nuance to the discussion of child care pricing. Prior research estimates how child care prices respond to the price of inputs (Helburn and Howes, 1996) and the child care tax credit (Rodgers, 2018), along with estimating the price elasticity of demand (Blau and Robins, 1988) and the the elasticity of demand for quality (Blau and Mocan, 2002)

[8]Doromal et al. (2018) do use *cross-sectional* inspections data in North Carolina to explore differences in safety across provider types.

[9]Although UPK was intended to provide quality early childhood education in public pre-K classrooms, the rest of the private child care market was not targeted.

and how these factors have affected the evolution of prices over time (Herbst, 2018). This literature models the price of child care as one singular price. I advance this literature by considering differences in prices and profit margins across age groups. These differences are important for thinking about the impact of a public policy that provides a child care substitute for just one age group. I provide empirical evidence showing that day care centers use infants as loss leaders and cross-subsidize care for younger children with tuition from older children. I also provide a model to rationalize this pricing behavior. I believe I am the first to document this fact within the economics literature.

More broadly, my work is connected to research on the effects, often unintended, of public programs on non-targeted individuals. Public interventions often target a subset of the population served by private markets and can therefore have spillovers on the rest of the market. For example, the government provides vouchers and housing targeted toward the subset of the market comprised of low-income households. But housing vouchers increase rents for non-voucher recipients (Susin, 2002) and government-built low-income housing affects nearby housing prices (Diamond and McQuade, 2016). Medicare provides health insurance for the elderly but affects private-pay prices (Clemens and Gottlieb, 2017) and spending (Finkelstein and McKnight, 2005) for the non-elderly. I contribute to this literature by evaluating the impact of public pre-K, a substitute for child care for 4-year-olds, on other ages served in the child care market.

The remainder of the paper is structured as follows. Section 3.3 provides additional institutional details about New York City's UPK expansion and on the landscape of licensed child care facilities in the city. Section 1.3 introduces a conceptual framework for understanding UPK's effect on child care facilities that serve both younger and older children. The data are described in Section 3.4. The empirical strategy is further described in Section 1.5, and results are presented in Section 2.6. Section 3.9 concludes.

1.2 Institutional Background

1.2.1 Background on Universal Pre-K in New York City

Universal Pre-K (UPK) launched in New York City in 2014 and provides the option of free public pre-K to all 4-year-old residents of NYC for 6 hours and 20 minutes per day and 180 days per year. From 1998 to 2014, NYC ran a much smaller public pre-K program with only 19,000 children enrolled in full-day pre-K in the 2013-14 school year. In the 2014-15 school year, the first year of the expansion, nearly 52,000 students were enrolled, representing 60% of the future first grade public school enrollment for this cohort.[10] The program further expanded to enroll 67,000 students in 2015-16, the first year the city was able to guarantee every 4-year-old a spot, and its enrollment and capacity has held steady since then.

NYC's pre-K program is "universal" in the sense that all children are eligible if they are NYC residents and have turned or will be turning 4 years old in that calendar year. Universal programs are in contrast to "targeted" pre-K programs that are targeted to more vulternable children, and only children who meet certain income limits, for example, are eligible.

Parents who want their child to participate in UPK fill out an application ranking their preferred schools. There are no restrictions on where they can apply, though preference is given for students with siblings at the school, and for public schools, children who are zoned for that school. Next priority is given to children who live in the same school district as the site, followed by students in the same borough.

The Universal Pre-K expansion came together rapidly and was largely unexpected. Bill de Blasio came from behind to win NYC's mayoral primary in September 2013 on a platform that included the public pre-K expansion. In July of 2013, Bill de Blasio sat in fourth place in

[10]Part of the increased full-day enrollment was driven from a shift from half-day to full-day programs. A total of 36,000 children were enrolled in half-day programs in 2013-14 compared to 14,000 in 2014-15. Some is also due to a conversion of 12,700 seats for low-income children, which were previously designed as half-day pre-K coupled with Head Start or child care funding to round out the full day, to full-day pre-K options.

the polls at 15.4%, eight percentage points behind the leader, City Council Speaker Christine Quinn. Just two months later, he had surged ahead to first place following a much-lauded campaign ad featuring his son. One of de Blasio's signature proposals was a grand plan to implement free Universal Pre-K for 4-year-olds in NYC with a 0.5 percentage point tax increase on earners making over $500,000. Any tax increase would need to be approved by the state, and de Blasio's closest competitors, Quinn and Bill Thompson, criticized his plan as infeasible because Governor Cuomo would never approve a tax increase while facing reelection in 2014. But their critiques didn't stop de Blasio from emerging victorious in the September primary, and he went on to win the general election in November in a landslide.

His next challenge was to gain approval for his tax plan from Albany and, in parallel, to prepare logistically to implement UPK in case the funding was approved. De Blasio wanted a secure, earmarked funding stream and insisted that the tax on the wealthy was the only way to accomplish it. Cuomo was equally opposed to raising taxes. A budget deal was reached in late March of 2014 for New York state to contribute $300 million from the state budget toward the pre-K expansion in NYC, bringing the total spending on UPK in NYC that year to $575 million.

In addition to funding, de Blasio and his team had to find space for the new UPK classrooms by soliciting proposals from two types of sites, public schools and community-based organizations (CBOs). They began soliciting proposals as early as mid-December 2013 with initial submission deadlines in February 2014, with additional solitation rounds later in the year. Public school sites were almost exclusively housed in existing elementary schools, and public school principals were invited to submit proposals for new classrooms.[11] Additionally, private schools, day care centers, Head Start programs, and other providers of child care and education in NYC were eligible to apply to host Universal Pre-K classrooms.

[11] Although not part of this paper, in 2015, the district opened new "Pre-K Centers", public schools that exclusively housed pre-K classrooms, many of which opened in buildings already owned by the Department of Education, such as closed public schools.

The Department of Education had to vet over 3,000 new classrooms for this effort and work with the Department of Health and Mental Hygeine to ensure any new sites had the proper license. The first round of new sites was announced in April of 2014, with an additional round of sites announced in June of 2014 and some additional seats continuing to be announced throughout the summer and even into the beginning of the school year.

In order to qualify to host one or more UPK classrooms, CBOs must meet the same minimum criteria as public schools. The only exception is that CBO teachers are not required to have a New York State Birth-Grade 2 certification, though they are required to have a written plan to obtain the certification within five years. Public school UPK teachers are paid higher salaries than typical child care workers. In order to create more equity between public-school-based UPK and CBO-based UPK teachers, de Blasio set aside $10 million to increase UPK teacher salaries in CBOs.

With the funding and infrastructure in place, NYC opened applications for pre-K students. The first round of applications for students was due in late April with results announced in June, and applications for available seats, including those at new sites, continued to be open until they were filled. For the first year, parents had to use one application for public school sites and apply separately to each CBO site they were interested in. Admission for public school sites was done centrally, with priority given to students with siblings at the school and to students zoned for that school. Most CBO sites admitted students on a first come, first served basis. As a result of the efforts of de Blasio and his staff, there were 25,000 more full-day pre-K seats in 2014 than in 2013.

1.2.2 Background on the Child Care Market

This paper focuses on the effect of UPK on licensed child care providers who care for children younger than pre-K age. There are two types of licensed child care providers: day care centers and home day cares, which I collectively refer to as child care facilities. Home day cares care for children in residential spaces, while day care centers care for them in non-residential

spaces. Children may also be cared for by individuals without a license, such as friends and family or nannies, and I will not be able to speak to this informal segment of the market with this paper.

Home day cares in New York City are licensed, regulated, and inspected by the New York State Office of Children and Familly Services (OCFS). Home day cares are required to obtain a license if they regularly serve three or more children younger than 13 who are unrelated to the provider.[12] Home day cares without additional employees, which I will refer to as single home day cares, can care for up to six children, of whom two can be under the age of 2. Home day cares with additional employees, which I will call group home day cares, can care for up to twelve children, with a maximum of two under age 2 for each care provider present (for example, an owner with one additional employee can have four children under 2).[13][14][15] OCFS also imposes programming and facility requirements and inspects home day cares for compliance.[16]

Day care centers in New York City are licensed, regulated, and inspected by the New York City Department of Health and Mental Hygiene (DOHMH). Day care centers must have a license if they regularly provide care to three or more children under age 6 in a non-residential space.[17] Centers typically provide care in classrooms separated by age with minimum staff-to-child ratios and maximum group sizes determined by the DOHMH. For

[12] "Regularly" is defined as three or more hours per day on a regular basis.

[13] Home day cares without additional employees can also have up to eight children if at least two are enrolled in Kindergarten or a later grade, and home day cares with additional employees can have up to sixteen children if at least four are school age. Some home day cares are licensed for slightly lower capacities due to square footage requirements.

[14] When they are present, a provider's own children under age 13 are included when determining maximum capacity unless they are enrolled in kindergarten or a higher grade.

[15] Because two providers is the minimum required to use the maximum capacity allowed for a home day care (additional employees do not increase total allowed capacity, only the allowed capacity of infants and young toddlers), throughout the paper, I will assume that group home day cares can care for up to four children under age 2.

[16] There are no minimum education requirements for providers, though they must pass a background check and meet continuing education requirements.

[17] More specifically, they are required to have a license if care is provided for five or more hours per week for more than thirty days in a twelve-month period.

infants under 12 months of age, there can be at most four children per teacher and eight children per room. Young toddlers ages 12 to 24 months can be cared for in groups of at most ten and must have one teacher for every five children. For 4-year-olds, the maximum is twelve children per teacher and twenty children in a room.[18] The ratios and group sizes for all ages can be found in Table A-1. In general, bachelor's degrees are required for lead teachers and education requirements are lower for aides.[19] DOHMH also imposes programming and facility requirements.

Day care centers undergo unannounced inspections annually. Results of those inspections are used in supplementary analysis in this paper as suggestive evidence on day care safety and quality. A full inspection consists of two separate visits, a facility/environmental initial inspection and a programmatic initial inspection. These can occur anywhere from one day to several months apart. Inspections are also conducted to investigate complaints against the day care or to reinspect to ensure compliance with violations discovered in the initial inspection. Any violations that require reinspection can result in a fine of $200 to $2,000. Facilities can also be temporarily shut down until violations are corrected, and repeated or very serious violations can lead to the revocation of their license.

1.3 Conceptual Framework

Universal Pre-K extends free public schooling and school-day child care to a single age cohort of children. On the private market, centers that care for these 4-year-olds often also care for younger children as well. Therefore, a change in the market for care for pre-kindergarten-aged children could affect care for younger children. Whether this effect is positive, negative, or zero is an empirical question. There are different possible consequences of Universal Pre-K

[18] Note that the maximum for UPK classrooms is slightly lower than for child care centers. UPK sites can have at most 18 children if there are two teachers or 20 children if there are three teachers.

[19] All programs are required to have an education director with a bachelor's degree and experience working with children. Each classroom must have a lead teacher who must have a bachelor's degree, except for infant/toddler classrooms where the lead teacher can have a high school diploma plus experience or an approved education plan. Assistant teachers must have a high school diploma.

that could affect the supply or demand for private child care in different directions. And if there is little crowd out of 4-year-old care, then there may be little effect on the child care market. The following subsections discuss some reasons why the effect might be positive or negative. When I say it could have a positive effect, I am referring to increasing capacity, reducing prices, and/or increasing quality and vice versa for a negative effect.

1.3.1 UPK could have a negative impact on child care for younger children

In this section, I will discuss three reasons UPK could have a negative impact on child care for younger children. I describe each briefly in this paragraph and elaborate on these mechanisms in the remainder of the section. First, the viability of child care facilities could be threatened by an inability to attract enough 4-year-olds. For day care centers, 4-year-olds are the least costly to care for, and day cares use them to cross-subsidize younger ages. When these children are crowded out by UPK, day care centers are losing their most profitable children. For home day cares, legally only about one-third of total capacity is available for children under 2. The remaining two-thirds is only available for children ages 2 and older. If home day cares are not able to find enough older children to fill those slots, they may close. Second, UPK increases the demand for early childhood educators, which may make it more difficult for day care centers to attract and retain teachers. Finally, there may be restrictions in the supply of buildings suitable for early childhood education. If more of these classrooms are used for 4-year-olds, there may be less space for younger children.

I will begin by establishing the empirical fact that day cares cross-subsidize younger children with older children. Day care tuition data is not available for New York City, so I will instead look at prices for another large East Coast city, Philadelphia, Pennsylvania.

In Pennsylvania, one teacher can care for at most four infants or at most ten 4-year-olds. With labor accounting for approximately seventy percent of operating costs (Helburn and

Howes, 1996), we would expect tuition to be about twice as high for infants as it is for 4-year-olds.[20]

Figure A-1 plots the actual cumulative density functions of the ratio of tuition for younger children of a given age to 4-year-olds in Philadelphia and its surrounding counties as of November 2017.[21] The red line represents the expected tuition ratio given the required staff ratios and assuming labor costs are seventy percent of total costs. In subfigure (a), we can see that less than one percent of centers charge at least the expected premium for infant care, and the average center charges only a 22 percent premium. Subfigures (b) and (c) show that ages 1 and 2 are cross-subsidized by 4-year-olds as well. For both, less than one percent of day cares charge at least this expected tuition ratio. For one-year-olds, the average tuition premium is fifteen percent compared to the expected premium of seventy percent, and for age 2, the mean is 8 percent versus the expected 45 percent, including twenty percent of day cares that charge the same amount for age 2 and age 4. Thus, younger children are substantially cross-subsidized by older children.

The cross-subsidization found in the microdata in Philadelphia is seen in aggregate national data as well. States generally require two to three times as many staff members to care for a group of infants as for a group of 4-year-olds of the same size (Vandell and Wolfe, 2000). Using the approximation that labor costs are about 70 percent of day care facility costs, we would expect infant care to be 70 percent to 140 percent more expensive than care for 4-year-olds. However, according to a national survey, they are on average only 12

[20]Each teacher can care for 2.5 times as many 4-year-olds as infants, so assuming similar wages for teachers in the two classrooms, labor costs are approximately 2.5 times higher for infants. If labor accounts for seventy percent of total costs, then accounting just for increased labor costs, we would expect infant tuition to be 1.5*0.7=2.05 times higher than 4-year-old tuition.

[21]This data was acquired from the Pennsylvania Department of Human Services through a Right to Know Law request and includes Bucks, Chester, Delaware, Montgomery, and Philadelphia counties. The pricing information includes the prices as the day cares reported it for their licenses. Day cares are supposed to report the tuition that they charge absent any discounts. Similar data is not available for New York as facilities are not required to report their prices to the licensing agencies in New York.

percent higher (Schulte and Durana, 2016). Thus, if 4-year-olds are crowded out, day cares are losing their most profitable children, potentially affecting their viability.

The question of why day cares price in this way is addressed with a model in Appendix .2. I provide a summary of the intuition here. Demand for infant care and pre-K care is interrelated. Because infant care is more costly to provide, day cares cross-subsidize infants with pre-K students in order to attract more pre-K students. One possible mechanism is switching costs. Parents have to pay a switching cost in order to change child care arrangements, so day care choices are sticky. Day cares charge less profitable infant prices in order to attract infants who will become profitable pre-K children. Another mechanism is sibling effects. Parents may value sending their children to the same facility, so if the day care can attract the infant, they may be able to attract their older sibling as well.

Figure B-1 shows what happens to the private day care market when Universal Pre-K is introduced within a simplified supply and demand framework where the demands for pre-K care and infant care are interrelated. When free Universal Pre-K is introduced, parents decrease their demand for private pre-K care, which reduces the equilibrium price and quantity of private pre-K care. The cross-subsidization of infant care with pre-K care means that the supply curve for infant care depends not only on the price of infant care but also on the equilibrium price and quantity of pre-K care. When the price and quantity of private pre-K care fall, day cares reduce the quantity of infant care they are willing to supply for a given price, shifting their supply curve. This shift reduces the equilibrium quantity of infant care and increases its price.

In this simplified model, the size of the decrease in the equilibrium quantity of infant care depends on the elasticity of demand for infant care. If parents are very inelastic in their demand for child care centers for infants, then most of the adjustment will come in the form of increased prices, and there will be little adjustment on quantity. On the other hand, if parents are very elastic, most of the effect will be in the form of a decrease in quantity. Demand by parents who can only pay for child care with government-provided vouchers may

be almost perfectly elastic so that day cares that take a large number of subsidies may be very responsive on the quantity margin since they cannot respond by increasing prices.

The above model considers market-driven changes to the price and quantity of child care, but child care facilities could also make adjustments to the cost margin by changing their quality. When faced with decreased pre-K enrollment, centers could also respond by lowering their quality in order to lower their costs.

A similar argument holds for home day cares, which are restricted in the number of infants and toddlers they can legally care for. In New York City, a maximum of about one-third of total capacity can be filled by children under age 2.[22] If a home day care cannot find enough children to fill the "age 2 and up" slots, they may shut down, increase prices, or reduce quality.

Another reason UPK might reduce the equilibrium quantity of care available for younger children is because of constraints in the supply of suitable classrooms for early childhood education. In New York City, sixty percent of UPK enrollment is in community-based organizations, many of which are day care centers. If these centers are using classrooms for 4-year-olds that otherwise would have been used for younger children, it would reduce the quantity of center-based care supplied for younger children. Note that due to facility regulations, it is easier to substitute a 4-year-old classroom for a 3-year-old classroom than for a classroom for even younger children.[23] A substitution of 4-year-old classrooms for 3-year-old classrooms is something I will not be able to observe in my data because I have capacity by age separated only into two groups, under age 2 and ages 2 to 5. But any effect from conversions of 4-year-old classrooms to classrooms for children under age 2 would be included in my estimate.

[22]Home day cares in New York City can have at most two children under two for each teacher who is present. The total maximum capacity across all ages is six for a home day care with no additional employees and twelve if they do have additional employees, so slots available to younger children make up approximately one-third of total capacity.

[23]For example, NYC regulations require that there be 30 square feet of space in a classroom for each child. A classroom of infants can have at most 8 children, while a classroom of 4-year-olds can have at most 20 children, so infant classrooms tend to be smaller than pre-K classrooms.

Finally, with the abrupt increase in demand for early childhood education teachers, day cares may find it difficult to hire enough staff members to remain open, particularly because salaries in the public school pre-Ks, and to a lesser extent the UPKs in the CBOs, are substantially higher than that of the average day care teacher. Day cares may also find it difficult to retain high-quality teachers. These general equilibrium effects are unlikely to be a large part of the estimated effects in this paper because my estimation strategy estimates the effect of placing one new UPK site in a given area holding fixed the general equilibrium effects of the expansion as a whole. There could certainly be small labor market effects from the addition of one new UPK (i.e. a day care worker leaves to go work at the UPK that opened next door), but overall, labor market effects are likely to be a small part of what I am estimating. It is useful to separate the general equilibrium labor market effects from the partial equilibrium effects because the policy prescriptions are different. A decrease in day care center supply due to labor market effects can be ameliorated by increasing the supply of early childhood educators, but that solution will not be as relevant if the cause of the decline is cross-subsidization across age groups or inelasticity of building supply.

1.3.2 UPK could have a positive effect on child care for younger children

This section discusses a few reasons why there might be a positive effect of UPK on child care for younger children. First, Universal Pre-K may increase demand for sibling care and/or for before- and after-school care. Parents who would otherwise have stayed home with their 4-year-old and younger child due to high day care expenses for two children may decide to enter the labor force when their 4-year-old becomes eligible for UPK so that they would only need to pay for one child in day care. Similarly, parents who enter the labor force because of UPK may also require before- and after-school care for their child since public pre-K does not last for the entire work day. In both of these cases, the increase in demand is likely to be very geographically close to new UPK sites since parents will want to be able to drop off

siblings at nearby locations and before- and after-school care would need to be offered nearby for ease of transportation. Home day cares in particular may be able to provide before- and after-school care because they have more flexibility to be open for only a few hours before and after school than day care centers do.

Another reason UPK could have a positive effect is the flipside of the builiding constraints story from the previous section. If there is an inelastic supply of buildings suitable for early childhood education and UPK draws students from day care centers to classrooms in places that would not have hosted day care center classrooms otherwise, such as public schools or libraries, it could increase the supply of classrooms available for younger children. Note that it is more difficult to retrofit a 4-year-old classroom to be used for infants than it is to switch it to a 3-year-old classroom, so again converting 4-year-olds classes to be used by younger children may be a less relevant margin when studying care for children under age 2.[24]

Day care centers may also benefit from hosting UPK classrooms. Depending on how well they are compensated by the city, the possibility of hosting a UPK classroom could encourage entry of new day cares or prevent closures of existing ones that also serve younger children.

Finally, there could be a "crowd in" effect where the city's emphasis on quality early childhood education increases demand for quality early childhood education at younger ages, increasing demand for center-based care (or licensed home day care) for younger children. Similarly, through an income effect, UPK could also increase demand for child care at younger ages by decreasing the lifetime costs of sending a child to day care.

[24]Infant and toddler classrooms must have sprinkler systems, which is not required in classrooms for older children. They also must be on the ground floor of buildings to ease evacuation, which at that age is usually done by putting infants in rolling cribs and rolling them outside. Classrooms for older children can be on the second or third floors.

1.4 Data

1.4.1 Child Care Facility Data

The primary analysis focuses on the number and capacity of child care facilities in New York City using licensure data obtained through Freedom of Information Law requests to New York City's Department of Health and Mental Hygeine (DOHMH) and New York State's Office of Children and Family Services (OCFS). The DOHMH data covers years 2008 to 2018 and contains licensure information for day care centers, while the OCFS data covers years 2010 to 2017 and contains licensure information for home day cares. Both data sets contain the name, address, opening date, and capacity of the facilities.[25] The OCFS data also contains closing date for home day cares that are no longer in operation. The DOHMH data contains the most recent permit expiration date for the two-year license, and an indicator for whether the facility was closed as of January 2018, when the data was extracted. It also includes an indicator for whether the license is for younger children (ages 0 to 2) or older children (ages 2 to 5). For both data sets, I geocoded the addresses in order to determine the spatial locations of the facilities.

Additionally, data on the results of day care center inspections from the NYC DOHMH are used in complementary analysis as a suggestive indicator of day care safety and quality. These data cover all day care center inspections from 2008 to 2017 with indicators for the date of the visit, the type of visit (routine annual inspection, response to a complaint, or to follow up on violations), and a record of whether any violations were discovered, and if so, what they were.

There are three types of inspection violations: public health hazards, critical, and general. Public health hazards are the most serious. They present an imminent threat to the health and safety of children and must be corrected within one business day. Critical violations are

[25] Per a late 2009 law, home day care providers can opt to remove their street address from public availability. For these observations only zip code is available. Less than 5% of the sample does not have a full address, and they were dropped from the analysis.

serious violations that do not present imminent threats and must be corrected within two weeks. All other violations are considered general violations and must be corrected within one month. Facilities are reinspected for compliance if they have one or more public health hazard or critical violations or six or more general violations.

The day care center inspections data is also used to estimate a closing date for out-of-business facilities to within approximately one year instead of the two years allowed by the licensure data. Licenses are issued for two years, while inspections are required every year. In the main specification, I consider a provider to be open for their full license term if they had an inspection at any point in the second year of their license. If they did not, then I set their closure date to one year before the expiration date. Results are robust to alternative definitions.

As of January 1, 2014, the last year before the UPK expansion, there were 2,228 licensed day care centers in New York City with capacity for 133,000 children and 7,887 licensed home day cares with capacity for 77,842 children (see Table 1.2). Of the day care centers, 17 percent were licensed to serve younger children while the remaining were licensed for older children. Facilities licensed for younger children almost always also have a license to care for older children either in the same building or as part of a larger chain.

Figure 1.2 depicts the trends in number and capacity of day care centers from 2008 to 2018, the period over which I have data.[26] The number and capacity of day care centers serving younger children has been steadily increasing over time with a slight slowdown from 2014 to 2017, followed by a jump in 2018, the most recent year. Day care centers serving older children have also increased in size over the sample period with a leveling off in city-wide capacity and slight decline in number of facilities beginning in 2016.

Capacity of home day cares in New York City peaked in 2014 and has dropped by about ten percent since then, from about 78,000 in 2014 to about 70,000 in 2017. There is a steeper

[26]Although I have day care center data back to 2008, I only have home day care data going back to 2010. Therefore, most analysis begins with 2010 both for consistency and to restrict to a reasonably short pre-period.

drop in number of home day cares than capacity of home day cares since 2014 due to an increase in the fraction of home day cares that are group home day cares, which have at least one additional employee and are therefore licensed for a higher capacity, versus single home day cares, which do not have additional employees.

The raw data point to a slowdown in the growth of the child care market in New York City beginning just after 2014, when UPK was implemented. These, of course, are only correlations. Causality will have to wait until Section 2.6, where I discuss results.

1.4.2 Universal Pre-K Data

The locations of new Universal Pre-K sites are used to determine whether and how intensely a given area is "treated" by Universal Pre-K. The locations of the sites are taken primarily from the UPK viewbooks provided to parents to inform them about their options for UPK. I use the viewbooks from 2013-14, the year before the expansion, and 2014-15, the first year of the expansion, both of which I found through the Wayback Machine.[27]

Viewbooks provide the name, address, and number of available full-day and half-day seats, except for the 2013-14 community-based organizations, for which number of seats in 2013-14 is not available (but an indicator for whether there is full-day UPK does exist). I extracted this data from the PDFs and geocoded the addresses to determine the spatial locations of the UPKs. Part of the UPK expansion included increasing capacity in existing sites, but because I do not have capacity data for the 2013-14 CBOs, treatment will be based on an indicator for whether a site is offering full-day UPK for the first time in 2014-15.[28] Exposure is therefore based on new *sites* not new *capacity*.

[27]There were multiple versions of the 2014-15 viewbook due to UPK sites being added. The viewbook I use is the latest I could find and is from June 27, 2014, just over two months before the first day of school.

[28]So if a UPK offered some full-day seats in 2013-14 and added more full-day seats in 2014-15, that would not be counted as an expansion since I do not have the data to see that there were fewer seats in 2013. However, if they only offered half-day UPK in 2013-14 and switched to full-day UPK in 2014-15, then that would count as a new site and is treated the same way as a site that did not have any UPK in 2013 that added full-day UPK in 2014.

The number of sites and total capacity of those sites are provided in Table 1.1. In 2013-14, there were 514 CBO full-day pre-K sites and 438 public sites, for a total of 952.[29] In the expansion, the number of CBO sites in my data increased by 363 and CBO capacity increased by 19,397.[30] The number of public school sites increased by 69 and the capacity by 5,590.[31] The total combined capacity of the public and CBO sites in the viewbook data is 57,151 in the year of the expansion, which is consistent with enrollment figures of about 53,000 as some seats were not filled.

1.5 Empirical Strategy

To identify how a new UPK location affects the local day care market, I compare availability of care in areas very close to new UPK sites to areas not as close to new UPK sites before and after the 2014 UPK expansion in NYC. Exposure is measured intensively, so the comparison is between areas with more new UPKs and those with fewer new UPKs. The assumption is that areas that are closer new UPKs will be more affected by the expansion than those that are farther away. Evidence that there is a utility cost of traveling farther to school, particularly at younger ages, supports the claim that the relevant market for child care will be fairly local to the new site (Dinerstein and Smith, 2014). The identifying assumption is that changes in the day care market in areas with fewer new UPKs provide a good counterfactual for areas with more new UPKs. This assumption is discussed in more detail in Section 1.5.2.

[29] Of these CBO sites, only 59 did not have any income requirements in any of its classrooms. Classrooms with income requirements are primarily contracted with the Administration for Children's Services (ACS) and provide half-day pre-K coupled with child care and/or Head Start to make it a full-day option. The Department of Education does not include these classrooms in its count of full-day pre-K seats in the pre-period since they are not exclusively full-day pre-K. However, I do include them since when they were converted to UPK seats in the following year, it was primarily a change in name coupled with a programmatic change, and it provides essentially the same substitute for private child care as it did before.

[30] The total capacity number is from Shroff et al. (2014) since site-level CBO capacity is not available.

[31] About forty percent of the capacity expansion in public schools is at current sites. Although expansions are not counted as new sites in my analysis, results are robust to including these public school expansions as new sites.

The main outcome of interest is the total number of seats in day care center seats for younger children. Note that if I were only interested in closings, day care centers that were open when UPK rolled out would be a natural unit of observation and treatment could be based on some measure of distance to new sites. However, the most important outcome is not the change in the closure rate for currently-open centers but how UPK affected the total stock of day care centers, which incorporates changes in both closings and openings. Stock must be defined over a geographic area, so it is necessary to define these areas.

In addition to defining areas over which to measure the stock of child care facilities, it is necessary to define child care markets. These may be the same or different from the areas used to measure stock. The ideal experiment would be to have a set of markets with uncrossable boundaries where everyone living within a given area worked and attended school and day care within that boundary. Then, if we randomly dropped UPKs in some of them, we could observe what happens to the market. However, in the real world, people do not stick to one clearly-delineated area with uncrossable boundaries, particularly in large and densely-populated areas like NYC, so we must find a way to approximate markets, preferably one that allows markets to overlap.

Defining markets is challenging, especially with little information. The industrial organization literature solves this problem by using structural estimation. The added value of this approach is usually that it can generate counterfactuals, but in this case, where I observe the market before and after a policy change, a simpler reduced-form approach should be sufficient and is more straighforward.

There are two primary reduced-form approaches to this problem: the "ring method" and using administrative boundaries. Neither of these is ideal in this case, and they are discussed in more detail in Appendix .3.

Briefly, the "ring method" is not ideal due to the spatial concentration of treatment. This method would involve drawing a ring around each new UPK site to delineate the market affected by that UPK and comparing changes in the stock of child care centers in that ring

to changes in the stock of child care centers in a larger "unaffected" ring. The ring method is not ideal here because even if the child care market area affected by a given UPK (and therefore the relevant ring) is very small, the markets (and rings) would still overlap, and "treated" areas will overlap with areas that were supposed to be "untreated," biasing the estimates. And the direction of the bias is not clear ex-ante, as explained in Appendix .3. Furthermore, areas with more than one UPK would be overweighted in the data.

Using administrative boundaries and measuring changes in the child care market within those boundaries based on their level of exposure to new sites would solve the problem of overlapping areas of observation. However, it creates a new problem because these boundaries are aribtrary and irregularly-shaped, and there is no natural way to test robustness to them. Instead, I divide New York City into a grid of tessellated hexagons. All hexagons are the same size, and because the researcher creates the grid, it is straightforward to test robustness to shifting the grid and changingthe size of the hexagons.

1.5.1 Tessellated Hexagons improve upon alternative methods

Using a grid of tessellated hexagons allows me to address the concerns of using areas of observation that overlap and using arbitrary boundaries. In order to account for potentially-overlapping markets, I allow the child care market in one hexagon to be influenced by the change in the number of UPK sites in that hexagon and the hexagon's neighbors. Although this is still an approximation of the true markets, it does improve upon existing methods by better handling dense treatment and allowing for robustness checks to the market boundaries. Dividing space into a grid allows exposure to be measured as the number of nearby UPKs and accurately identifies areas with and without new sites. The ring method, on the other hand, does not provide unbiased estimates when treatment is spatially dense. The definition of the grid is also very flexible, which allows me to easilty test for robustness to different hexagon grids.

I choose hexagons for the shape of the grid areas for two primary reasons. First, neighboring hexagons are easy to define and all have the same relationship to the central hexagon. There are three options for a grid of regular polygons: triangles, squares, and hexagons. Squares have four neighbors with whom they share a side, and they also have four additional neighbors with whom they share a vertice but not a side. Equilateral triangles similarly have same-side neighbors and vertice-sharing neighbors. For hexagons, on the other hand, all neighbors are same-side neighbors.[32] Put another way, the centroids of all of a hexagon's six neighbors are the same distance from the middle hexagon's centroid, whereas for a square, the four diagonal neighbors are farther away than the four same-side neighbors. This property of hexagons makes including in the effect of UPKs in neighboring hexagons more natural, and no adjustment is needed to account for different types of neighbors. The inclusion of neighbor effects is necessary to account for overlapping markets. Not accounting for neighbor effects assumes that a UPK in a given hexagon only influences the child care market in that hexagon and not neighboring hexagons.

A second reason for choosing hexagons is that of the three options, hexagons have the lowest area to perimeter ratio, so the average point in a hexagon is closer to its centroid than for a same-area triangle or square, reducing boundary effects. In other words, hexagons are preferred because they are the closest to circles.[33] Therefore, I will use a grid of tessellated hexagons.[34][35]

[32] See Figure A-2 for a visual.

[33] In fact, hexagons are the least-perimeter way to divide a plane into regions of equal area (Hales, 2001).

[34] Squares do have the advantage that they are easier to work with using longitude and latitude. See Harari and La Ferrara (Forthcoming) for an application using a grid of squares.

[35] There are other possible approaches to this problem, primarily from the literature on structural industrial organization. Due to the setting and data here, a structural approach is not necessary. Although it is certainly possible to approach the question in this way, it is not necessary in this case since I observe the outcomes of interest both before and after the policy change. Additionally, a structural approach would require some additional assumptions due to the lack of individual-level data. Another approach is continuous difference-in-difference, as pioneered by Diamond and McQuade (2016). In the application in their paper, there are few enough overlapping markets that they are able to run their estimation assuming zero overlap. In order to apply to this case, the method would have to be adjusted for overlapping markets. Furthermore, the beauty of the hexagon ap-

1.5.2 Estimation and Identifying Assumptions: Day Care Center Capacity

Now that the geographic boundaries have been chosen, I can implement a difference-in-differences design that leverages the natural experiment of the child care market in some hexagons being more exposed to the 2014 UPK expansion than others.[36] The idea is to compare changes to the day care market before and after the expansion in hexagons that got more sites to ones that got fewer (or none). Under a set of identifying assumptions discussed below, differential changes in day care availability in areas with more new UPK sites can be attributed to differential exposure to UPK, and the change is the causal effect of a new UPK site.

Level of exposure to the expansion is measured by the number of new full-day UPK sites on net in that hexagon and the hexagon's neighbors. Exposure is defined as the number of full-day UPK sites in 2014 minus the number of full-day UPKs in 2013 and is allowed to be negative.[37] I could instead use the number of 2014 sites that did not exist in 2013. I have chosen this definition instead primarily because if a UPK site from 2013 closes but a new one opens next door, the hexagon's exposure to UPK does not change even though there is a new UPK location.[38]

Because the day care market is likely to take time to adjust to the new sites, I focus on a semi-dynamic specification that allows us to observe how the effect of UPK changes with time since exposure. Day care facilities that will close as a result of UPK probably take at least a year or two to make that decision. There also may be child care facilities that would

proach vis-a-vis these alternative approaches is in its simplicity, transparency, and minimal required computing power while still providing plausible identification in this setting.

[36]Because children are more likely to attend school at a location that is closer to them than one that is farther away (Dinerstein and Smith, 2014), differences in the locations of new sites leads the day care market in some hexagons to be more affected by the expansion than others.

[37]A better measure of exposure may be the change in the number of full-day pre-K *seats* instead of the change in the number of full-day pre-K *sites*. However, micro-level data on the capacity of community-based organization sites is not available for 2013.

[38]I also do not have identifiers to easily link 2013 sites to 2014 sites. Linking them without identifiers would introduce noise in the measure.

have opened in the absence of UPK. The decrease in openings may have a compounding effect on stock that takes time to evolve. There are some that would have opened in the first year after UPK, some that would have opened in the second year after UPK, etc.[39] If there are dynamic effects, an estimate of the average effect during the post-period would understate the impact of UPK.

Specifically, I estimate the following semi-dynamic equation:

$$Y_{ht} = \beta_0 + \sum_{j=2015}^{2018} [\beta_j * \mathbb{1}_{X_t=j} * New_h + \beta_{nj} * \mathbb{1}_{X_t=j} * NewNB_h] + X_t + \sigma_h + X_t * h + \epsilon_{ht} \tag{1.1}$$

Observations are at the hexagon-year level. In the main regressions, Y_{ht} is the total capacity across day care centers for younger children ages 0 to 2 years in hexagon h as of $1/1/t$. I also consider the total number of day care centers serving younger children, the capacity of home day cares, and the number of home day cares. $\mathbb{1}_{X_t=k}$ is an indicator for year k, New_h is the net number of new full-day UPK sites in the hexagon in 2014, and $NewNB_h$ is the net number of new sites in hexagon h's immediate neighboring hexagons. The regression includes year and hexagon fixed effects. I include hexagon-specific linear time trends to account for potential heterogeneity in pre-expansion trends. The data spans years 2010 to 2018. Because the outcome of interest is the stock as of January 1st of each year, the observation from year 2014 is the last one before the expansion. Standard errors are clustered at the hexagon level. The coefficients of interest are the β_js and β_{nj}s. These estimate the effect of one new Universal Pre-K site in the same hexagon (β_j) or a neighboring hexagon (β_{nj}) on the number of seats for younger children in day care centers in that hexagon in year j.

[39] A positive effect could also take time to evolve. There are facilities that would eventually close in the absence of UPK who remain open. In this case, the effect on stock would theoretically be seen in the year they would have closed. Similarly for openings, it can take time for new firms to enter the market.

Identification requires that day care center capacity would have followed a similar trend (net of the hexagon-specific linear time trend) in all hexagons in the absence of the UPK expansion. This assumption would be violated if they are not trending similarly in the pre-period, which I will check for empirically. It can also be violated if there is another shock to the day care market that occurs in 2014 and is correlated with hexagon exposure status. The presence of such a shock is not something I can definitively test for empirically and is an assumption that will be required for causality.

To check for pre-trends, I estimate the fully-dynamic equation:

$$Y_{ht} = \beta_0 + \sum_{k=2011}^{2013} [\beta_k * \mathbb{1}_{t=k} * New_h + \beta_{nk} * \mathbb{1}_{t=k} * NewNB_h] + \sum_{j=2015}^{2018} [\beta_j * \mathbb{1}_{t=j} * New_h + \beta_{nj} * \mathbb{1}_{t=j} * NewNB_h] + X_t + \sigma_h + X_t * h + \epsilon_{ht} \tag{1.2}$$

This is the same regression as equation (1.1) except that it also includes interactions of the treatment variable with year indicators for the pre-period.[40]

1.5.3 Descriptive Statistics at the Hexagon Level

In the main specification, the hexagons are fairly small (covering approximately 10 city blocks, or about three blocks wide). In two alternative specifications, I consider hexagons that are medium-sized (covering about 20 city blocks) and large-sized (covering about 40 city blocks). In all three cases, I allow the child care market in a given hexagon to be affected by new UPKs in that hexagon and in the hexagon's neighbors.

See Figure 1.1 for maps displaying the number of full-day sites in an example hexagon grid in 2014 compared to 2013. In this medium-sized hexagon grid with 998 hexagons, 372 hexagons had more full-day UPK sites in 2014 than they did in 2013. Figure A-3 displays

[40]These additional interactions with year indicators are necessary to check for pre-trends. It is not the main specification because the semi-dynamic specification is more efficient since it uses the whole pre-period to estimate the hexagon-specific linear time trends, while this specification just uses the trend between the left-out years 2010 and 2014.

the exposure variable, the number of 2014 UPK sites in the hexagon minus the number of 2013 UPK sites, for the primary hexagon grid. In order to eliminate areas such as parks, water, and airports, hexagons are included in the analysis only if they had a family day care home, day care center, or elementary school at some point in the pre-period.[41] Of the 2,658 hexagons in this main hexagon grid, 449 had more UPK sites in 2014 than 2013 and 82 had fewer.

Random assignment of UPKs to hexagons is not necessary for identification, and there is no reason to expect exposed and unexposed hexagons to be observably similar. However, comparing these hexagons on observables is still informative in order to better understand the characteristics of the treatment and control groups. Table A-3 compares the number of UPK sites and child care facilities in hexagons that gain one or more UPKs ("expansion" hexagons) versus ones that do not ("non-expansion" hexagons). The number of facilities is measured as of 1/1/2014, just before the UPK expansion. There is no statistical difference between the number of 2013-14 full-day UPK sites between expansion and non-expansion hexagons. The average expansion hexagon had 1.13 more full-day UPK sites in 2014 than it did in 2013. They also had slightly more UPK sites in neighboring hexagons: 0.82 vs. 0.68 for the non-expansion hexagons.

Given the speed of the UPK expansion, UPK sites were primarily located in pre-existing child care and school facilities since there was not time to build or significantly renovate new buildings. So as expected, expansion hexagons have more day care centers serving children ages 2 to 5 since a hexagon is more likely to gain a UPK site if it has more potential sites. And because having more day care center licenses for classrooms for children ages 2 years to 5 years is correlated with having classrooms licensed for ages 0 to 2 years, expansion hexagons also have a higher licensed day care center capacity for children under the age of 2. There are approximately three more seats for children ages 0 to 2 in expansion hexagons than non-

[41] I have run robustness checks including the entire sample, and regression results are virtually unchanged by this restriction.

expansion hexagons, and the difference is statistically significant. Differences across hexagon types in the number of home day cares are small and not statistically significant.

Table A-2 presents a regression table showing what characteristics of a hexagon predict the number of full-day UPK sites it had in the 2014-15 school year. The characteristics are based on Census block data from the 2010 Census that I mapped to hexagons based on which hexagon the block's centroid fell in. The most important predictors for the number of 2014 sites are the number of 2013 full-day UPK sites and proxies for the number of potential sites, the number of public elementary schools and the number of day care centers serving children ages 2 to 5 as of 1/1/2014. Since public elementary schools and day care centers are the primary providers of UPK, it makes sense that they are strong predictors of where new UPKs are located. New UPKs were also significantly more likely to be in areas with higher fraction of non-white residents. Interestingly, the number of day care centers for children under age 2 and the number of home day cares do not have any additional predictive power on whether a given hexagon has a new UPK. Although these near-zero point estimates and statistical insignificance are not necessary for the identification strategy, this regression provides suggestive evidence that the child care markets for infants and toddlers in hexagons that do not get a UPK provide a good counterfactual for the ones that do.

1.5.4 Estimation and Identifying Assumptions: Inspections

To complement the results on the change in availability of child care, I estimate the effect of nearby UPK sites on day care center inspection results and complaints, potential indicators of quality. Day cares have three primary margins on which to adjust when faced with crowd out from UPKs: price, quantity, and quality.[42] One reason changes in day care center capacity are important is because day care centers are on average the highest quality form of non-parental care (Bassok et al., 2016b), so a change in day care center capacity could affect children's quality of care and therefore future human capital development. But if day

[42]For more details on mechanisms and a model, see Appendix A.

care centers reduce their quality, then even children who are able to stay in the generally higher-quality day care market may suffer from reduced quality of care.

To estimate the impact of public pre-K on private day care center quality, I use the same identification stratgey as before, but now the unit of observation is at the inspection level for the inspection results and the day care-year level for the complaints outcome. In addition, I dropped the data from the implementation year (2014) since it is not clear exactly what point in the year treatment begins, and that point may even be different for different hexagons depending on how early a given site was announced. Dropping the data from the year 2014 leaves 2013 as the last year before the expansion, so that will be the left out year. Finally, here there is no reason to restrict to inspection results for day care centers for younger children, so I pool inspection results and complaints for licenses for both younger children and older children. I do exclude day care centers that I spatially match to new UPK sites since the treatment of having a new UPK classroom in the day care center is different from having a new UPK site at a different organzation nearby.[43]

For consistency across outcomes, I will use the same measures of exposure as in the analysis of day care center capacity changes, which is the net number of new sites in the hexagon. The regression allows day care centers in a given hexagon to be affected by competition from new UPK sites in that hexagon and neighboring hexagons. Specifically, for complaints, I estimate:

$$Y_{iht} = \beta_0 + \sum_{k \neq 2013} [\beta_k * \mathbb{1}_{t=k} * New_h + \beta_{nk} * \mathbb{1}_{t=k} * NewNB_h] + X_t + \sigma_i + \epsilon_{iht} \tag{1.3}$$

Where Y_{iht} is an indicator for whether day care i located in hexagon h had any complaints during year t, New_h is the net number of new UPKs in hexagon h, and $NewNB_h$ is the net number of new UPKs in hexagon h's neighbors. I also added a day care center fixed effect,

[43] More specifically, I first spatially matched the 2014 pre-K sites to the 2013 sites to determine which sites were new. I then spatially matched these new sites to the day care center data to determine which day care centers hosted full-day pre-K for the first time in 2014.

σ_i, so that all estimates are *within-center* changes in quality. Standard errors are clustered at the hexagon level.

I also ran this same regression with an outcome variable of the number of inspections for a day care center per year. The addition of a new UPK site could increase the caseload of the inspectors and lead to a decrease in inspections. A decrease in inspections could affect the violations observed at a given inspection. I do observe a slight decrease in the number of inspections for day care centers that are closer to new UPK sites, so I will control for months since the last inspection in my regressions on inspection outcomes to try to control for any mechanical effect of reduced inspections.

To estimate changes in the number of violations and the probability of requiring a reinspection, I ran regressions at the inspection level with day care center and year fixed effects:

$$Y_{jiht} = \beta_0 + \sum_{k \neq 2013} [\beta_k * \mathbb{1}_{t=k} * New_h + \beta_{nk} * \mathbb{1}_{t=k} * NewNB_h] + Months_t + X_t + \sigma_i + \epsilon_{jiht} \tag{1.4}$$

Where Y_{jiht} is the outcome of interest from inspection j of day care i located in hexagon h during year t and $Months_t$ is the number of months since the last inspection. This regression is the same as equation (1.3) except that it is run at the inspection level and includes the control for the number of months since the last inspection. Inspections are included only if they were the initial annual inspection and not if they were due to a complaint or were a reinspection. The outcomes considered are the total number of violations found in the inspection and whether the inspection was poor enough to trigger a reinspection. Facilities are reinspected if they have any of either of the two most serious violations (public health hazard or critical) or if they have six or more violations total.

The identification assumptions for the capacity regressions are also required for these regressions. It must be the case that the day care centers would have followed similar trends

in the absence of the UPK expansion. I will again use the fully-dynamic specification to confirm that there are no pre-trends in the data.[44]

1.6 Results

1.6.1 UPK reduces availability of infant and toddler care

Figure 1.3 plots the coefficients from the fully-dynamic specification (equation (1.2)) of the year indicators interacted with level of exposure, as measured by the net change in the number of UPK sites in the hexagon. With all pre-period coefficients not statistically significant from zero, the parallel trends assumption appears to be reasonably satisfied, and we can see a decrease in the capacity of day care centers for infants and young toddlers as a result of own-hexagon UPKs beginning with the observations from 1/1/2015, the first post-expansion observation. As expected, it takes some time for the child care market to adjust as the effect is growing over time. There appears to be no effect of UPKs in neighboring hexagons, implying that the effect of a new UPK site on child care availability is very local.

Coefficients from the semi-dynamic speficiation (equation (1.1)) are in Table 1.3. By 2018, there was a decline of 1.3 day care center seats for children under the age of 2 years for each new UPK site in the hexagon, a decline of 20% on the pre-expansion base of 6.33 (see Table A-3). With 432 net new UPK sites in 2014, I estimate that the 2014 NYC UPK expansion resulted in 562 fewer day care seats for infants.

The 2014 expansion increased the number of sites by 432 and enrollment by 34,000, but the full UPK program as of 2018 includes four times as many UPK sites. If the results here can be extrapolated to whole program, they imply that there are 2,248 fewer infant and young toddler seats as a result of the UPK program. These seats represent 24% of the total

[44] Another potential problem for identification is differential attrition. If lower quality day cares are closing (or not opening), it would affect the composition of day cares in the sample over time. Day care center fixed effects alleviate some of this concern, but differential attrition on quality could still impact the year effects. In order to address this concern, I verified that results are qualitatively similar when using a balanced panel. Results available upon request.

capacity of this sector as of 1/1/2014. To put these numbers in context relative to effects on the child care market found in other papers, Hotz and Xiao (2011) finds that the number of child care centers in an average area falls by about 10% in response to reducing by one the number of infants who can be cared for by one staff member or to increasing the number of years of education required to be a center director by three.

Day care centers are generally regarded as providing the highest quality non-parental care (Bassok et al., 2016b), so these children may be crowded out to lower-quality environments. Furthermore, these regressions measure the effect of adding one new UPK site to a given area holding everything else fixed. With an expansion as large and fast as this one, there are likely to be general equilibrium effects on the labor market for day care teachers that could make the full effect larger. It is important to separate labor market effects from effects due to the structure of the market because increasing the number of early childhood educators would help with the labor market effects but not the structural ones. Because there seems to be a reduction in day care center seats as a result of the structure of the child care market and not only due to labor market effects, increasing the supply of early childhood workers will not be sufficient to counteract the negative impact of UPK on the day care market.

1.6.2 Robustness of reduction in infant and young toddler seats

One advantage of using a hexagon grid to define the geography is that it is straightforward to test robustness to the original grid placement. I will consider what happens to the estimates when the grid is shifted and when the hexagons are medium size or large size versus the small size in the main specification.

First, I will look at robustness to shifting the grid. If results are robust and not just an artifact of where the grid happened to be placed, we would expect to see similar results across shifts. I will consider a total of six shifts including the original placement. The shifts are of a "half hexagon" each, and there are three "half hexagons" moving east-west and two "half hexagons" moving north-south, for a total of six shifts. See Figure A-4 for a visual.

Figure A-5 plots the coefficients from the fully-dynamic model and demonstrates that the results are largely robust to shifting the grid. Table A-4 contains the point estimates from the semi-dynamic specification for the interactions of the treatment variable with indicators for the years 2017 and 2018. Note that "East 0, South 0" and column (1) in the table are the original grid. All of the 2018 coefficients indicate a decline of about one to two day care center seats for younger children for each new UPK in the hexagon. Although in one of the shifts the point estimate is not significant at the 95% confidence level, in two others, the effect is over two seats per UPK location by 2018.

The average decline is 1.6 seats per new UPK site as of 2018. Using the average of 1.6 seats, I estimate that the entire UPK program leads to a reduction in the number of infant and young toddler seats of 2,700. With a decline of 1.3 seats per site, the original grid represents a result near the median. It is worth noting, however, that it could have happened that the "East 2, South 1" shift (column (6)) with the smallest and least statistically significant results could have been the original boundaries. If these had been the given administrative boundaries, it would be easy to think that perhaps UPK did not have a significant effect on nearby day care centers serving younger children when in reality, the remaining boundary shifts demonstrate that is not the case for the average grid position. Being able to test for this robustness is the value of this approach.

Next, I will consider medium-size and large-size hexagons. The medium-size hexagons cover about 20 blocks, while the large ones cover about 40 blocks. In general, as a grid gets larger, we would expect the effect of neighboring hexagons to become less important since they represent UPKs that are farther away from the average point in the hexagon. In this case, the neighbor effects are already zero, so we would expect them to stay zero. When the hexagon size is initially increased, it could be the case that more of the relevant market is being included in the hexagon. In this case, we would expect that the effect of own-hexagon UPKs would be larger as a level since the area being incorporated is also affected by the UPKs in the smaller hexagons, but we might expect the estimate to fall as a percent if

the effect dissipates with distance. However, if the area becomes too large and begins to incorporate areas that are not part of the same market, then even the own-hexagon UPKs may not have a large effect on the area as a whole, so the estimate will be attenuated and, for large enough hexagons, eventually go to zero. We might also expect that for larger hexagons, edge effects could be more important, which could make point estimates vary more across shifts.

Figure A-7 plots the coefficients from equation (1.2) for these two larger grids. The effect is slightly attenuated and has much larger standard errors for the medium-size grid with an area of 20 blocks, and it goes to zero for the large-size grid with an area of 40 blocks. These results indicate that at least for the margin of the opening and shutdown decision, the market that is affected by a given UPK is fairly local. Figure A-8 demonstrates that, as expected, the point estimates for the medium-size grid vary much more across shifts, possibly indicating that edge effects have become more important.

1.6.3 Heterogeneity: Reduction entirely in high-poverty neighborhoods

The welfare consequences of the reduction in day care center seats depends on the outside option for the marginal families who would have used a day care center in the absence of Universal Pre-K but no longer will due to the spillovers from UPK on the child care market for younger children. Even if the day care center would have been the highest quality option available, in the absence of the center, wealthier families will have more alternative options available to them and are therefore likely to have a higher quality outside option than poorer families. Welfare losses will therefore be larger if the crowd out affects poorer families more than wealthier families.

In order to address this question, I estimate a model to determine whether the effect of Universal Pre-K sites on availability of child care for younger children is stronger in relatively wealthier or poorer areas. I split the hexagons at the median fraction of individuals living

below 200% of the federal poverty line. The median hexagon has 30% of individuals with income below this level. I interact indicator variables for the hexagon having above median or below median poverty with the Universal Pre-K exposure variable and year indicators:

$$Y_{ht} = \beta_0 + \sum_{k \neq 2014} [\beta_{ak} * \mathbb{1}_{t=k} * New_h * AboveMed_h + \beta_{bk} * \mathbb{1}_{t=k} * New_h * BelowMed_h] + X_t + \sigma_h + X_t * h + \epsilon_{ht} \quad (1.5)$$

This is the same regression as equation (1.2) except for the additional interactions with $AboveMed_h$ and $BelowMed_h$ and the exclusion of effects from UPKs in neighboring hexagons. The latter are excluded for the sake of brevity since there was little effect from these in the main specification. $AboveMed_h$ and $BelowMed_h$ are indicators for whether the hexagon has above median or below median fractions of people living below 200% of the federal poverty line. This fraction is determined by applying the American Community Survey 5-year estimates at the block group level to each block within the block group and calculating for each hexagon the average poverty level for the blocks whose centroids are in that hexagon weighting by Census 2010 population at the block level. There are 17 hexagons in the main hexagon grid that have zero population, or 0.6% of the 2,658 hexagons, and these are dropped from the analysis.

Observations are at the hexagon-year level. Y_{ht} is the capacity of day care centers serving younger children in hexagon h as of $1/1/t$, $\mathbb{1}_{X_t=k}$ is an indicator for year k, and New_h is the net new number of full-day UPK sites in the hexagon in 2014. The regression includes year and hexagon fixed effects and hexagon-specific linear time trends. The data spans years 2010-2018; 2014 is the left out year because 1/1/2014 is the last observation before the expansion, and 2010 is left out in order to identify the linear time trends. Standard errors are clustered at the hexagon level.

As seen in Figure 1.4, which plots the β_{ak} and βbk coefficients from equation (1.5), this heterogeneity analysis reveals that the entire decline in seats from the Universal Pre-K

introduction occurs in the relatively poorer areas. There is no discernible effect of new UPK sites in the relatively wealthier areas. In poorer areas, by 2.5 years after the expansion, each new UPK site led to just over two fewer seats at day care centers for younger children, a reduction of over one-third.[45] This result is very robust across the six shifts of the hexagon grid (see Figure A-6). The effect on day care centers in poorer areas is even apparent in the one shift that had less clear results in aggregate ("East 2, South 1"). The addition of Universal Pre-K sites led to a decrease in child care availability for younger children concentrated in the poorest areas.

A likely explanation for the disparate response across areas of different income levels is that families in poorer areas have higher price elasticities, so their demand for child care is more price sensitive. When day care centers in wealthier areas had reduced enrollment of 4-year-olds due to the Universal Pre-K expansion, they may have been able to raise their prices and still attract enough families to stay in business. On the other hand, day care centers in poorer areas may not have been able to attract enough families if they raised prices, so they closed (or did not open) instead. An additional factor in poorer areas is that there are likely more day cares serving children using child care subsidies. Since the subsidy rates are fixed and set by the state, there is no room to adjust prices for these infants and toddlers, leaving only the quantity and quality margins available for adjustment.

1.6.4 New UPK Sites increase licensed home day cares very nearby

I now turn to the effect of UPK sites on home day cares. Given that in the absence of UPK, some 4-year-olds might instead go to home day cares for child care, we would expect that some home day cares would close and care might be consolidated. In this scenario, and in the

[45]This reduction is relative to the 5.8 seats in 2014 in day care centers serving children between the ages of 0 and 2 in hexagons with over 30% of people living below 200% of the federal poverty line that had an increase in the number of Universal Pre-K sites. Hexagons with new UPK sites had higher average day care capacity relative to ones that did not get a new UPK site.

absence of other regulations, even though the total number of home day care seats declines, the only effect on the availability of care for younger children may be the distance parents need to travel to find care. However, New York regulations require that there be only two children under the age of 2 years for each care provider, so a consolidation of seats could lead to fewer slots available for infants and toddlers. As an extreme example, if all home day cares were at capacity for children under the age of 2 years and some of them closed down, it would directly decrease availability and use of care for children under age 2. Because I do not know actual enrollment by age, the results here can only be suggestive indications of possible increases or decreases in availability of care for younger ages.

As seen in Figure 1.5, which plots the coefficients from equation (1.2), UPK may increase the total capacity of home day cares very close to new UPKs. Two explanations for an increase in the number of home day cares are that they provide before- and after-school care or sibling care for siblings of 4-year-olds in the UPK. Home day cares may have more flexibility than day care centers to be open for just a few hours a day before and after school or to provide care to just a few children. This result is not especially robust across shifts (see Figure A-9), but at the very least, a new UPK does not appear to decrease the supply of home day cares in its immediate vicinity.

Considering a larger market with a medium-size grid, Figure 1.7 shows that there may be a decrease in capacity of home day care centers in some areas when a larger market is considered. Part (a) of this figure plots the coefficients on the interaction terms between year and exposure intensity from the main specification, and it appears that UPKs have little effect on the supply of home day cares.

However, the aggregate result hides important heterogeneity. Part (b) of Figure 1.7 shows that in wealthier areas, there is a statistically-significant increase in home day care capacity, even when considering this larger market. By 2017, there are about 7 more seats in home day cares for each new UPK site in wealthier areas. Based on licensing regulations, approximately one-third of these seats, or just over 2 seats, would be available to children

under age 2. On the other hand, in the poorer areas, if anything, there is a decrease in the capacity of home day care centers close to new Universal Pre-K sites. Therefore, it does not appear that the decrease in day care center capacity is being offset by an increase in licensed home day cares. These results are confirmed by an analysis of robustness to shifting the grid, shown in Figure A-11.

Even if the decrease in day care center capacity in poorer areas were offset by an increase in licensed home day care capacity, we would still be concerned about a decrease in the quality of care as day care centers generally provide the observably highest quality of care. For example, children at licensed home day cares watch on average 1.5 hours of television per day, compared to only a few minutes on average for children in centers (Bassok et al., 2016b). But *without* an increase in licensed home day cares, either younger children are being shifted to informal care arrangements, which are typically even lower quality than home day cares, home day cares are taking more children per home, or their parents are adjusting their labor force participation.

1.6.5 Evidence on declines in day care center quality

This section considers whether existing day care centers adjusted their quality in response to a new UPK site nearby. To do so, I consider whether there were any changes in safety that can be observed in inspections data. The three outcomes I consider are whether there were any public complaints filed against the day care center in a given year, the number of violations at their initial inspection, and whether reinspection was required.

As seen in Panel (d) of Figure 1.8, there may be a drop in the number of inspections per year in day cares closer to new UPK sites. Inspectors are assigned to regions, so this finding is consistent with increased case loads due to the new UPK site. I show this result because it could cause some correlation between time between inspections and number of violations. In order to focus on quality decreases due to increased competition from UPK and not quality

decreases due to a decrease in inspection frequency, I include the control for time since last inspection in the regressions analyzing inspection results.

Figure 1.8 plots the coefficients on the interactions between the year indicators and hexagon exposure status. Although none of the coefficients are individually statistically significant at the 5% level, there does appear to be an increase in the total number of violations at each inspection, in the probability reinspection will be required, and whether there were any complaints in a given year. In all cases, day cares are affected by new UPK sites in their hexagon but not very affected by new sites in neighboring hexagons. The lack of an effect from UPK sites in neighboring hexagons is consistent with the prior results indicating that the relevant markets for day cares are likely relatively small.

Table 1.5 presents the coefficients from a difference-in-differences regression. These estimates pool the post-period coefficients to test whether they are jointly signficant. Since the effects are growing in the post-period with only small effects in the first year after implementation, pooling likely underestimates the total effect. For each new UPK in the hexagon, the number of inspection violations for a day care center increased by 0.28 per inspection, which is an increase of 13% from a mean of 2.2 violations. There was also a modest increase in the probability of requiring reinspection. For each new UPK in its hexagon, a day care center was 2.3 percentage points more likely to require reinspection after an initial inspection, about a 5% increase compared to the mean reinspection rate. Together, these results show that day care center inspection results declined for centers near new UPK sites, consistent with the theory that day care centers may cut corners in order to cut costs when faced with reduced enrollment due to UPK.

The results on complaints can help clarify whether the worse inspection results translated into meaningful decreases in quality. If the public made more complaints, then that may indicate that the day care center has declined in quality enough that parents or other members of the public were concerned. Because parents may be a significant group of complainants, a null result on complaints is difficult to interpret since a decline in enrollment means there

are fewer parents to make complaints. The point estimate from Column (3) of Table 1.5 indicates that the probability of a complaint in a given year increased by 2 percentage points for each new UPK site in the same hexagon as the day care center, which is an increase of 15% relative to the mean, though the estimate is not quite significant at the 10% level (p=0.11).

These inspections results provide suggestive evidence that day cares may have responded to crowd out from UPK in other ways in addition to closing or not opening. When faced with lower enrollment, they may have reduced quality in order to lower their marginal costs and stay in operation.

1.6.6 Number of pre-K switchers to crowd out one infant seat

This section goes through a back-of-the-envelope calculation to estimate the ratio of 4-year-olds who switch from private to public pre-K to the reduction in infant and toddler seats. This parameter could be applied to other pre-K expansions to estimate the number of infant and young toddler seats that will be crowded out based on the expected number of students switching from private to public pre-K.

First, I will estimate the number of students who switch from private pre-K to public pre-K in the presence of Universal Pre-K. There were 15,793 more public pre-K students in NYC in 2015-16 than in 2013-14, and the number enrolled has held pretty steady since 2015-16. This number is smaller than the increse in the number of full-day students because there were more half-day slots in 2013-14 than in 2015-16.

Next, I need to estimate how much of the increased enrollment was from students who would not have enrolled in pre-K otherwise versus how much is from students who would have enrolled in private pre-K. Fitzpatrick (2010) estimates that UPK increases enrollment in pre-K in urban areas by 5 percentage points. There were approximately 100,000 4-year-olds in NYC as of the 2010 Census, so a five percentage point increase translates into approximately 5,000 children who would not have attended pre-K otherwise.

Subtracting the 5,000 children switching from no pre-K to public pre-K from the total increase in public pre-K students leaves 10,793 who switch from private to public. Some of these children would have attended pre-K at private schools, not day care centers. To calculate the desired parameter, I need to isolate those who switched from *day care centers* only. Using enrollment data from the Private School Survey and the capacity data from my data set, I estimate that 81% of the private pre-K students are at day care centers.[46] Assuming crowd out is proportionally distributed between centers and schools, taking 81% of the 10,793 switchers yields 8,742 who switch from private pre-K in day care centers to public pre-K as a result of the implementation of Universal Pre-K, which is the numerator in the calculation.

Now I need to calculate the number of infant and young toddler seats crowded out. Using the average estimate from the main specification, there were 1.6 fewer day care center seats for children under age 2 for each new full-day UPK site. The number of new full-day sites added by the Universal Pre-K expansion was approximately 776.[47] Then the decrease in the number of center-based seats for children under age 2 due to the expansion was 1,242. Putting these numbers together yields the result that one under 2 seat was lost for every seven pre-K children who switched from private pre-K to public pre-K.[48]

[46]Because capacity data for pre-K in the Private School Survey lumps together private pre-K for 4-year-olds with public pre-K and also private pre-K for age 3, not just age 4, I use kindergarten enrollment as a proxy for age 4 private preschool enrollment at the school. This yields an estimated 12,398 seats. For day care centers, I assume that seats are allocated by age in the same ratio as regulated maximum group size and that every day care includes each age group. Centers can have 12 two year olds in a group, 15 three year olds, or 30 four year olds. Then the fraction of day care center seats available for 4-year-olds is 20/(20+15+12)=43%. There were 123,351 day care center seats for children between the ages of 2 and 5 as of 1/1/2014, so I estimate that 52,490 of these were available to 4-year-olds.

[47]The 776 new sites uses the number of full-day sites in 2018 (1,728) and subtracts from it the number of full-day sites in 2013 (952). I use 2018 data because the number of full-day sites was not readily available for years 2015, 2016, or 2017. Since the total number of students has held relatively steady since 2015, the number of sites in 2018 should be a good approximation of the effect of the expansion.

[48]This is from the calculation of 8,742 switchers divided by 1,242 lost infant and young toddler seats.

1.7 Conclusion

As a result of research demonstrating the importance of early childhood education, there has been a push to expand public schooling to include pre-kindergarten for 4-year-olds. Decreasing the starting age of free public schooling to 4 years old from 5 years old is proposed as a way to reduce inequality, and it is particularly appealing in light of recent research that human capital investments have larger impacts the earlier that they occur. By that same token, investments at ages 0 to 3 may be even more important than investments at age 4, but less attention has been given to this age group.

Extending schooling to 4-year-olds may be more tractable given the existing infrastructure and means of delivering services. However, given the importance of early childhood education, it is important to determine whether this focus on 4-year-olds has had any unintended consequences for even younger children ages 0 to 3.

This paper finds that public pre-K for 4-year-olds had unintended consequences on the child care market for younger children. New UPK sites reduced the number of center-based day care seats for children under age two, with the whole program leading to a decline of about 2,700 seats. This estimate is an underestimate of the overall effect if there are general equilibrium effects on the labor market that caused some day cares to close due to a shortage of early childhood educators. It also only includes children under 2 years. There may also be fewer seats for children ages 2 years and 3 years.

Children displaced from center-based care to home day cares or other types of care are likely to experience a decrease in quality of care that could affect their human capital accumulation. I further find evidence of a decline in quality at day care centers that remain in operation, suggesting that even those children who are still able to attend a day care center may experience lower quality care.

I have shown that public pre-K can decrease the availability of care for children younger than two and suggestive evidence of a decline in day care center quality. Important areas for future research include documenting effects on children ages 2 and 3 and more thoroughly

examining other channels through which public pre-K can affect the child care market, such as price and quality of care.

A primary reason for these spillovers may be due to the pricing model in the child care market where older children cross-subsidize more costly-to-care-for younger children. I rationalize this pricing behavior with a model of switching costs. Day cares with local monopoly power have an incentive to attract infants at a lower initial profit because once the infants are at the center, it increases their chance of remaining at that center for pre-kindergarten, where the day care can extract a higher monopolist profit. Note that for this mechanism to be at play, there must be crowd out of 4-year-olds from the private market. Areas with less crowd out will experience less of a decline in care for younger children.

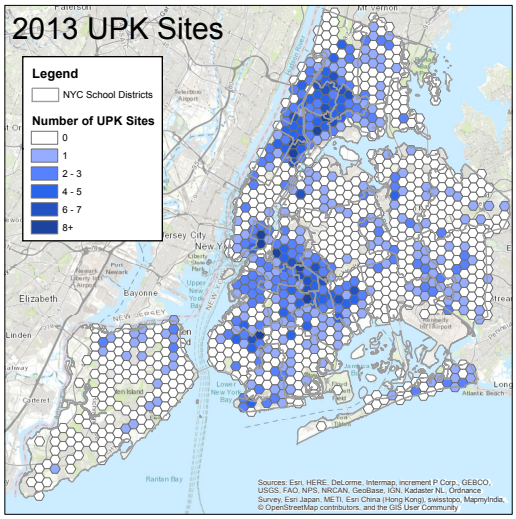
There are some additional explanations for why public pre-K for 4-year-olds could reduce availability of care for younger children. There are likely labor market effects from increased demand for early childhood educators for better-paying jobs in the public sector. Although changes in the child care worker labor market is a likely channel through which public pre-K may affect the child care market, it is likely a small part of the effect I measure as my identification strategy estimates the impact of one new UPK site on the nearby area holding the city-wide labor market fixed. Potentially more relevant for NYC, where space is at a premium, there could be an inelastic supply of buildings and rooms suitable for day care centers, and more of those places may have been shifted to 4-year-old care from care for younger children. Estimating the relative importance of these different channels is an important area for future research.

Although I find negative effects in the context of New York City, there are some forces from public pre-K that could lead to improvements in the child care market for younger children. Perhaps in a different context, these could outweigh the aforementioned forces pushing in the opposite direction. For example, a decrease in lifetime child care expenses induce an income effect that increases demand for child care at younger ages. Also, if public pre-K can be in day care classrooms and reimbursement rates are high enough, it may help

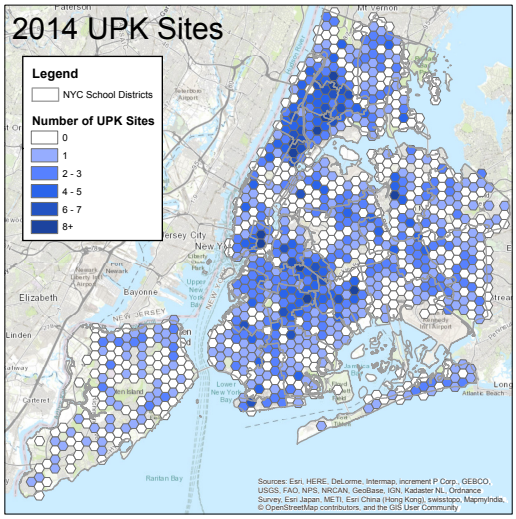
day cares stay afloat and continue to offer care for younger children (or begin to offer care for younger children, particularly if there is a sibling-demand effect). Finally, there could be a "crowding in" effect where parents, seeing the emphasis the government is placing on quality early childhood education, may increase their demand for quality early childhood education at younger ages, which could lead to an expansion in the number of seats for younger children. Understanding these effects and what could mitigate the impact of public pre-K for 4-year-olds on the child care market for younger children is becoming increasingly important as more cities and states push to expand the availability of public pre-K to more 4-year-olds and even to 3 year olds. Given the result that the crowd out takes place entirely in areas with higher levels of poverty, policy makers could consider increasing the subsidy rate for infants so it is more in line with the actual cost of providing care, as opposed to the market rate, to reduce market exit and increase entry.

Figure 1.1: Expansion of Full-Day Public Pre-K Sites in New York City in 2014

(a) Number of Full-Day Sites in 2013-14



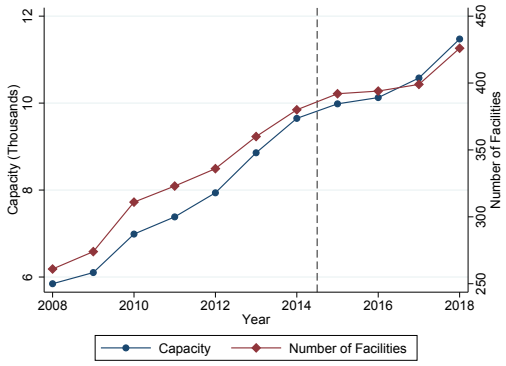
(b) Number of Full-Day Sites in 2014-15



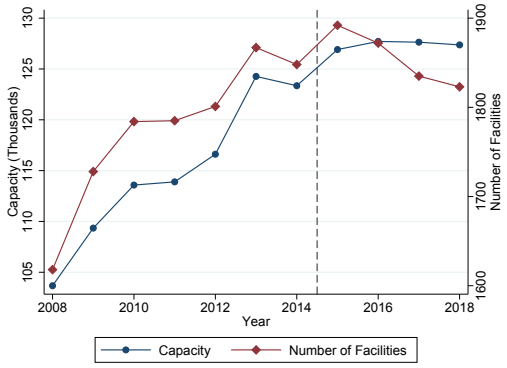
Notes: Maps display the number of full-day Universal Pre-K sites in each hexagonal area in the 2013-14 (a) and 2014-15 (b) school years. New York City expanded public pre-K for 4-year-olds rapidly in the 2014-15 school year, adding 432 new sites and 25,000 seats in one year. Of the 998 hexagons displayed here, 372 had more public pre-K sites in 2014 than they did in 2013. The hexagons in this grid cover approximately 20 city blocks and are the same size as the hexagons in the medium-size grid referred to in the text.

Figure 1.2: Stock of Day Care Facilities in New York City as of January 1st

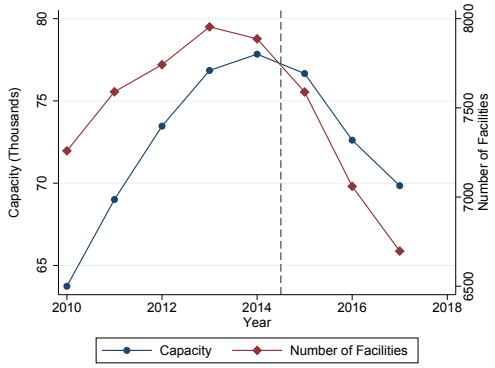
(a) Centers Serving Younger Children



(b) Centers Serving Older Children

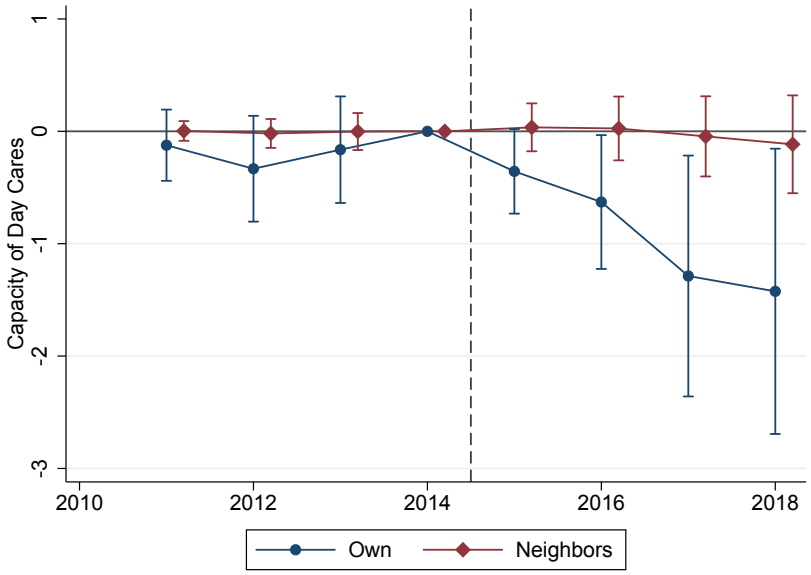


(c) Home Day Cares



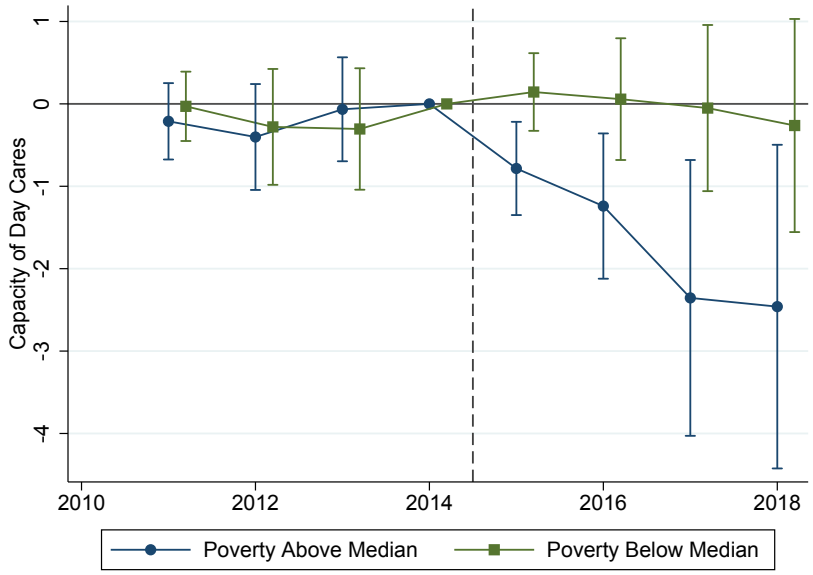
Notes: Figures plot the number and capacity of licensed day care facilities in New York City as of January 1st of each year. Vertical lines indicate the Universal Pre-K expansion in September 2014. Subfigures (a) and (b) are based on day care center licensure data from the NYC Department of Health and Mental Hygiene. Separate licenses are required to care for children under 2 and children ages 2years through 5 years. Closure dates are estimated based on license expiration date and the date of the last inspection. Refer to Section 1.4.1 of the text for details. Subfigure (c) is based on home day care licensure data from the New York State Office of Children and Family Services. Approximately one-third of total capacity is available to children under age 2. Refer to Section 1.2.2 for details.

Figure 1.3: Effect of a New Pre-K Site on Capacity of Day Care Centers Serving Younger Children



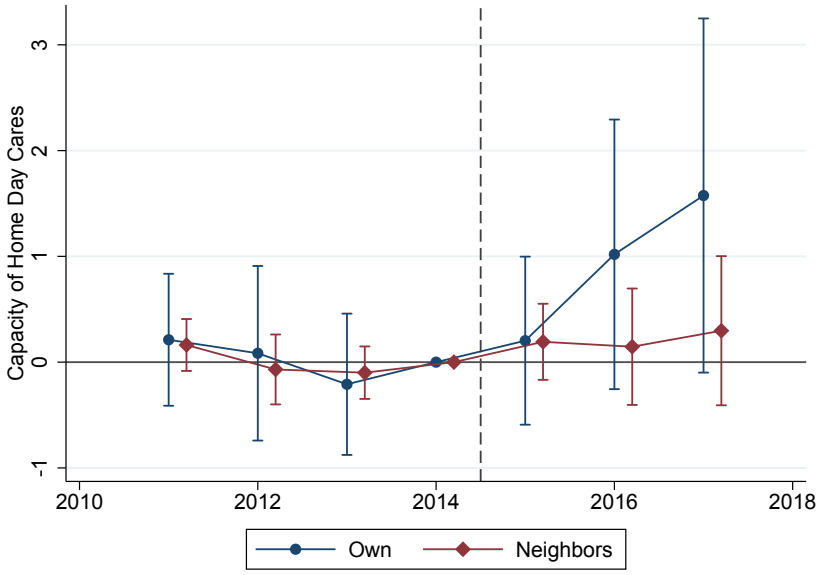
Notes: Figure plots coefficients on interactions between year indicators and the change in the number of Universal Pre-K sites between 2013-14 and 2014-15 in the hexagon ("Own") and the hexagon's immediate neighbors ("Neighbors"). Observations are at the hexagon-year level. Hexagons are approximately ten city blocks in area and form a grid that covers New York City. The outcome variable is the total licensed capacity for children between the ages of 0 and 2 in day care centers in the given hexagon. Includes hexagon fixed effects, time fixed effects, and hexagon-specific linear time trends. Bands show the 95% confidence interval with standard errors clustered at the hexagon level.

Figure 1.4: Effect of a New Pre-K Site on Capacity of Day Care Centers Serving Younger Children: Heterogeneity by Poverty Level



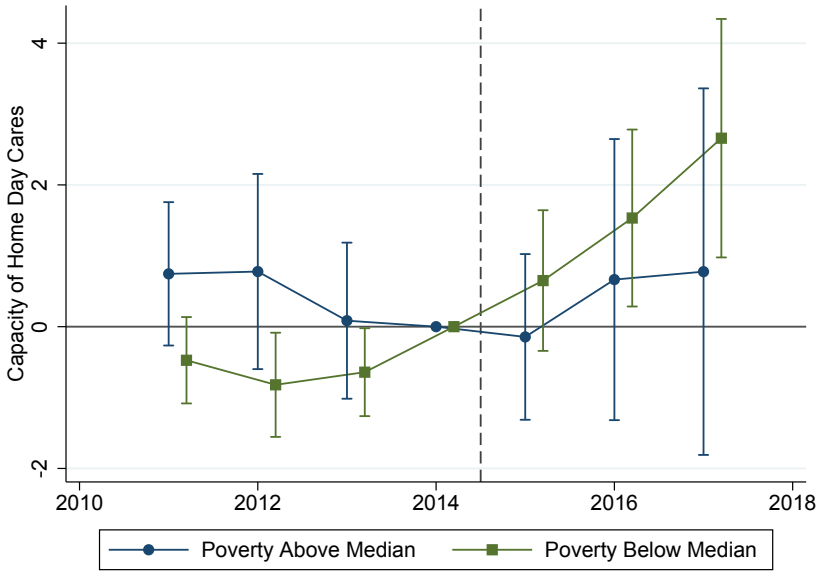
Notes: Figure compares how a new Universal Pre-K site affects the total capacity of day care centers serving ages 0-2 in areas with above-median levels of people living below the 200% poverty line to areas with below-median levels. In the median area, approximately 30% of people live below 200% of the federal poverty line. Figure plots coefficients of interactions of three variables: 1) year indicators, 2) indicators for above-median and below-median fraction of people living below 200% of the federal poverty line as of 2014, and 3) the change in the number of Universal Pre-K sites between 2013-14 and 2014-15 in the hexagon. Observations are at the hexagon-year level. Hexagons are approximately ten city blocks in area and form a grid that covers New York City. The outcome variable is the total licensed capacity for children between the ages of 0 and 2 in day care centers in the given hexagon. Includes hexagon fixed effects, time fixed effects, and hexagon-specific linear time trends. Bands show the 95% confidence interval with standard errors clustered at the hexagon level. Poverty level is determined by applying the American Community Survey 5-year estimates at the block group level to each block within the block group and calculating for each hexagon the average poverty level for the blocks in that hexagon weighting by Census 2010 population at the block level.

Figure 1.5: Effect of a New Pre-K Site on Capacity of Home Day Cares



Notes: Figure plots coefficients on interactions between year indicators and the change in the number of Universal Pre-K sites between 2013-14 and 2014-15 in the hexagon ("Own") and the hexagon's immediate neighbors ("Neighbors"). Observations are at the hexagon-year level. Hexagons are approximately ten city blocks in area and form a grid that covers New York City. The outcome variable is the total licensed capacity of home day cares in the given hexagon. Note that approximately one-third of licensed capacity is available for children under age 2. Includes hexagon fixed effects, time fixed effects, and hexagon-specific linear time trends. Bands show the 95% confidence interval with standard errors clustered at the hexagon level.

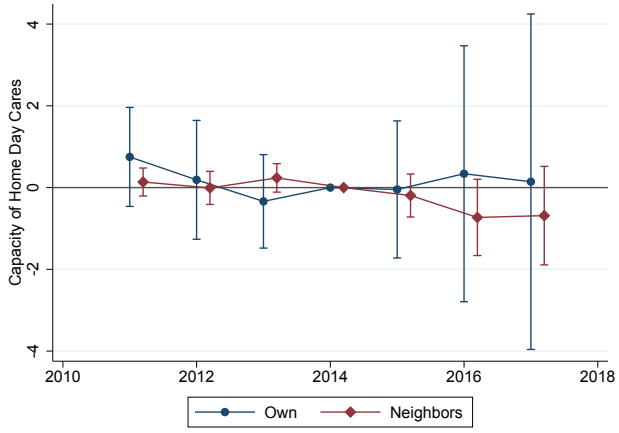
Figure 1.6: Effect of a New Pre-K Site on Capacity of Home Day Cares: Heterogeneity by Poverty Level



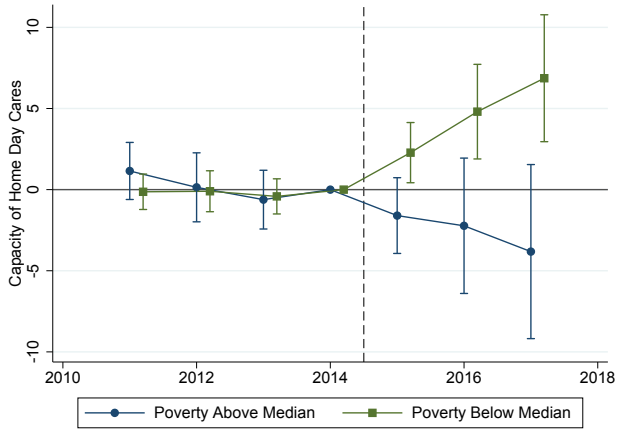
Notes: Figure compares how a new Universal Pre-K site affects the total capacity of home day cares in areas with above-median levels of people living below the 200% poverty line to areas with below-median levels. In the median area, approximately 30% of people live below 200% of the federal poverty line. Figure plots coefficients of interactions of three variables: 1) year indicators, 2) indicators for above-median and below-median fraction of people living below 200% of the federal poverty line as of 2014, and 3) the change in the number of Universal Pre-K sites between 2013-14 and 2014-15 in the hexagon. Observations are at the hexagon-year level. Hexagons are approximately ten city blocks in area and form a grid that covers New York City. The outcome variable is the total licensed capacity of home day cares in the given hexagon. Includes hexagon fixed effects, time fixed effects, and hexagon-specific linear time trends. Bands show the 95% confidence interval with standard errors clustered at the hexagon level. Poverty level is determined by applying the American Community Survey 5-year estimates at the block group level to each block within the block group and calculating for each hexagon the average poverty level for the blocks in that hexagon weighting by Census 2010 population at the block level.

Figure 1.7: Effect of New Pre-K Site on Home Day Care Capacity: Larger Market

(a) Primary Specification



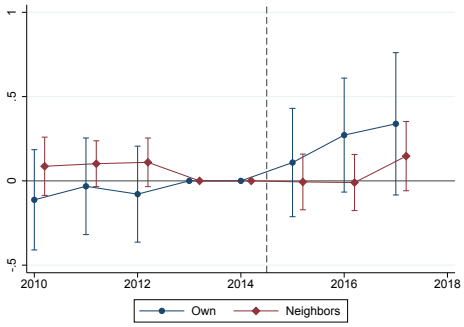
(b) Heterogeneity by Poverty Level



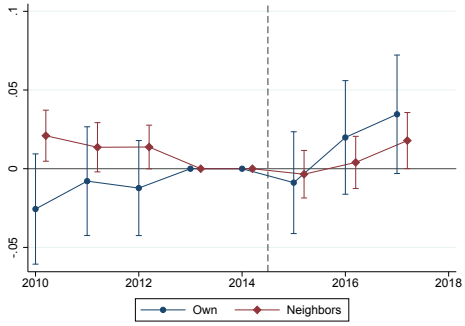
Notes: Figures plot coefficients on interactions between year indicators and a measure of Universal Pre-K exposure in the hexagon ("Own") and the hexagon's immediate neighbors ("Neighbors"). In subfigure (a), the measure of Universal Pre-K exposure is the change in the number of Universal Pre-K sites from 2013-14 to 2014-15. In subfigure (b), the measure of Universal Pre-K exposure is, for each hexagon, an indicator for whether there were more Universal Pre-K sites in that hexagon in 2014-15 than in 2013-14. In this specification, the measure of exposure from neighbors runs from 0 to 6 depending on how many neighbors had more sites in 2014. Observations are at the hexagon-year level. Hexagons are approximately *twenty* city blocks in area and form a grid that covers the city. The outcome variable is the total licensed capacity of home day cares in the given hexagon. Note that approximately one-third of licensed capacity is available for children under age 2. Includes hexagon fixed effects, time fixed effects, and hexagon-specific linear time trends. Bands show the 95% confidence interval with standard errors clustered at the hexagon level.

Figure 1.8: Effect of a New Pre-K Site on Day Care Center Violations

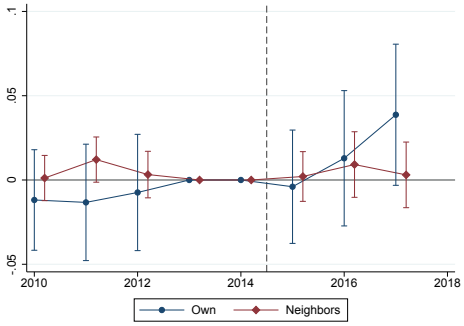
(a) Total Number of Violations



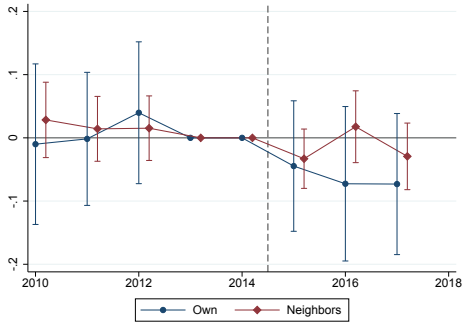
(b) Reinspection Required



(c) Any Complaints from Public



(d) Number of Annual Inspections



Notes: Figures plot coefficients on interactions between year indicators and the change in the number of Universal Pre-K sites between 2013-14 and 2014-15 in the hexagon that the day care is in. Hexagons are approximately ten city blocks in area and form a grid that covers New York City. Observations in subfigures (a) and (b) are at the inspection level. Observations in subfigures (c) and (d) are at the day care center-year level. Figures (a), (b), and (d) include initial annual inspections only. Figure (c) is an indicator for whether the day care center had any inspections in that year that were prompted by a complaint from the public. Inspections in year 2014 are dropped since based on the Universal Pre-K implementation timeline, it is not clear what part of the year would be in the pre-period and what part would be in the post-period. Day care centers that were themselves new UPK sites in 2014 are dropped. Regressions include day care center fixed effects and year fixed effects. Subfigures (a) and (b) include a control for the number of months since the last inspection. Bands show the 95% confidence interval with standard errors clustered at the hexagon level.

Table 1.1: Change in Public Pre-K Availability in NYC from 2013-14 to 2014-15

	2013-14		2014-15		Net Increase	
Site Type	Num. Sites	Capacity	Num. Sites	Capacity	Num. Sites	Capacity
CBOs	514[a]	16,177[b]	877	35,574	363	19,397
Public	438	15,987	507	21,577	69	5,590
Total	952	32,164	1,384	57,151	432	24,987

[a] Includes 455 that have income requirements in one or more of their classrooms. Classes with income requirements are primarily Administration for Children's Services (ACS) classrooms that combine half-day pre-K with child care and Head Start.
[b] Approximately 12,700 of these children are enrolled in ACS programs described in footnote a.

Notes: Table provides the total number and capacity of full-day pre-K sites in NYC in 2013-14, the year before the Universal Pre-K expansion, and 2014-15, the year of the expansion. Note that these are capacities, not enrollments. Because some seats are unused, these do not match enrollment numbers. The difference between these numbers and official Department of Education (DOE) numbers is that DOE excludes ACS programs for low-income children that combine half-day pre-K with child care and Head Start to create a full day. I include these as full-day options in the pre-period because the change to Universal Pre-K in 2014-15 is a programmatic one and does not change the hours that care is available in these locations. The 2013-14 CBO capacity number is only available in aggregate as no microdata on the capacity at each site is available in this year.

Table 1.2: Child Care Facilities in New York City as of 1/1/2014

		Total Capacity					Younger Children Only	
Facility Type	Num. Facilities	Total	Mean	Min.	Median	Max.	Total	Mean
Day Care Centers Overall	**2,228**	**133,000**	**59.7**	**4**	**45**	**424**	**9,649**	**4.3**
Licensed for Younger Children	380	9,649	25.4	4	20	136	9,649	25.4
Licensed for Older Children	1,848	123,351	66.7	8	54	424	0	0
Home Day Cares Overall	**7,887**	**77,842**	**9.9**	**3**	**12**	**12**	**31,548**	**3.3**
Single Home Day Cares	2,617	15,310	6.0	5	6	6	5,234	2.0
Group Home Day Cares	5,270	62,532	11.9	7	12	12	26,314	4.0

Notes: Table provides statistics on the licensed child care market in New York City as of 1/1/2014, the last year before the Universal Pre-K expansion. Day care center licenses cover either children under the age of 2 years or ages 2 years through 5 years, and facilities that serve both age groups must have two licenses, one for each age group. All licenses are counted separately in this table. Single home day cares do not have additional employees; care is provided by the owner only. Group home day cares have at least one additional employee. Group home day cares can care for two children under two for each care provider present (up to the total allowed capacity). For these calculations, I have assumed that there is one additional employee since that is the minimum required to care for seven or more children.

Table 1.3: Effect of New Public Pre-K Site on Stock of Day Care Centers Serving Younger Children

	Capacity		Number of Facilities	
	(1)	(2)	(3)	(4)
$\Delta Sites * (Year = 2017)$	-1.144*	-0.967*	-0.0553***	-0.0475***
	(0.588)	(0.528)	(0.0200)	(0.0181)
$\Delta Sites * (Year = 2018)$	-1.276*	-1.060*	-0.0609***	-0.0524**
	(0.693)	(0.619)	(0.0236)	(0.0212)
$\Delta NeighborSites * (Year = 2017)$	-0.0388	-0.181	0.00279	0.000461
	(0.159)	(0.183)	(0.183)	(0.00574)
$\Delta NeighborSites * (Year = 2018)$	-0.109	-0.300	0.00118	-0.00102
	(0.195)	(0.223)	(0.00674)	(0.00665)
Dep. Var. Mean	3.408	3.408	0.137	0.137
Dep. Var. Mean (Exposed)	5.690	5.690	0.249	0.249
Incl. Expansions		X		X
N	23922	23922	23922	23922

Notes: Table provides coefficient estimates on the interaction terms between the 2017 and 2018 indicators and the change in the number of Universal Pre-K sites between 2013-14 and 2014-15 in the hexagon and the hexagon's immediate neighbors from equation (1.1). Dependent variable is the capacity or number of day care centers serving children between the ages of 0 and 2 in the hexagon. Hexagons are approximately ten blocks in area and cover New York City. Columns (2) and (4) include public schools that had full-day pre-K in 2013-14 and added more seats in 2014-15 as new sites. Universal Pre-K classrooms can also be in community-based organizations, and the analogous information about whether an existing site expanded capacity is not available for those organizations. Includes year and hexagon fixed effects and a hexagon-specific linear time trend. Standard errors are clustered at the hexagon level.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 1.4: Effect of New Public Pre-K Site on Stock of Home Day Cares

	Total Capacity		Infant Capacity		Number of Facilities	
	(1)	(2)	(3)	(4)	(5)	(6)
ΔSites*(Year>2014)	1.084*	0.505	0.339	0.171	0.135**	0.0697
	(0.630)	(0.607)	(0.210)	(0.203)	(0.0649)	(0.0614)
ΔNeighbor Sites*(Year>2014)	0.318	-0.151	-0.112	-0.0443	0.0362	-0.00795
	(0.280)	(0.279)	(0.126)	(0.0940)	(0.0287)	(0.0289)
Dep. Var. Mean	27.19	27.19	9.270	9.270	2.803	2.803
Incl. Expansions		X		X		X
N	21264	21264	21264	21264	21264	21264

Notes: Dependent variable is the capacity or number of home day cares in the hexagon. Hexagons are approximately ten blocks in area and cover New York City. Even columns include public schools that had full-day pre-K in 2013-14 and added more seats in 2014-15 as new sites. Universal Pre-K classrooms can also be in community-based organizations, and the analogous information about whether an existing site expanded capacity is not available for those organizations. Estimates are the average of coefficients from post-treatment years from a model in which each post-period year is interacted with the exposure variable: $Y_{ht} = \beta_0 + \sum_{j=2015}^{2017} [\beta_j * \mathbb{1}_{X_t=j} * New_h + \beta_{nj} * \mathbb{1}_{X_t=j} * NewNB_h] + X_t + \sigma_h + X_t * h + \epsilon_{ht}$. Includes year and hexagon fixed effects and a hexagon-specific linear time trend. Standard errors are clustered at the hexagon level.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 1.5: Effect of New Public Pre-K Site on Day Care Center Violations

	(1) Total Violations	(2) Reinspection Req'd	(3) Any Complaints	(4) Num. Inspections
$\Delta Sites * (Year > 2014)$	0.278**	0.0230**	0.0217	-0.0692
	(0.108)	(0.0108)	(0.0132)	(0.0428)
$\Delta NeighborSites * (Year > 2014)$	-0.0299	-0.00564	0.000891	-0.0279
	(0.0631)	(0.00517)	(0.00659)	(0.0173)
Months Since Last Inspection	-0.000503	0.00323***		
	(0.00894)	(0.000884)		
Dep. Var. Mean	2.156	0.403	0.140	2.220
N	29437	29437	13260	13260

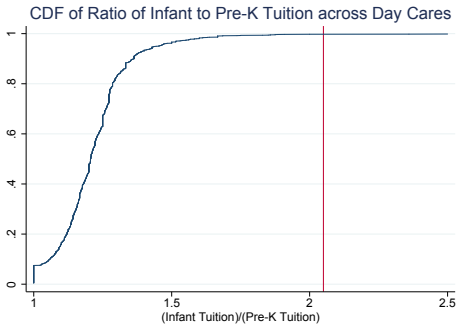
Notes: This table reports difference-in-differences estimates of the effect of one new full-day public pre-K site in a hexagon on the inspection results of day care centers in that hexagon. Hexagons are approximately ten city blocks in area and cover the city. Observations in columns (a) and (b) are at the inspection level and include only initial annual inspections. The dependent variable in column (a) is the total number of violations recorded in that inspection. The dependent variable in column (b) is an indicator for whether the initial inspection triggered a reinspection. Observations in columns (c) and (d) are at the day care center-year level. The dependent variable in column (c) is an indicator for whether the day care center had any inspections in that year that were triggered by a public complaint against the day care. The dependent variable in column (d) is the number of initial inspections done at that day care in that year. Inspections in year 2014 are dropped since it is not clear what part of the year would be in the pre-period and what part would be in the post-period. All regressions exclude day care centers that were themselves new UPK sites in 2014. Regressions include day care center fixed effects and year fixed effects. Standard errors are clustered at the hexagon level.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Appendices

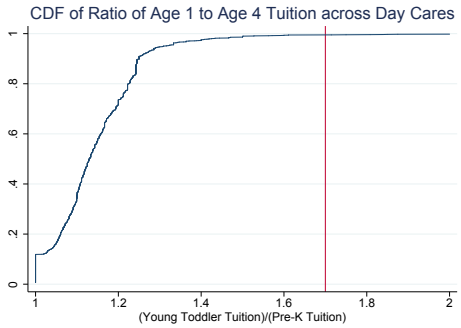
.1 Additional Figures and Tables

Figure A-1: Ratio of Younger Child Tuition to Age 4 Tuition in the Philadelphia Region

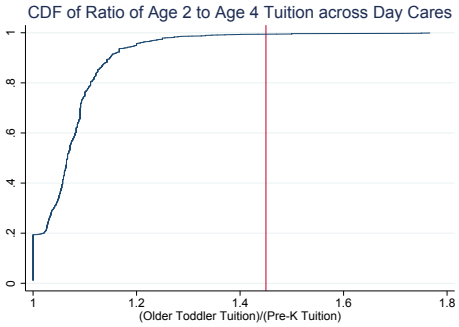
(a) Infant vs. Age 4 Tuition



(b) Age 1 vs. Age 4 Tuition

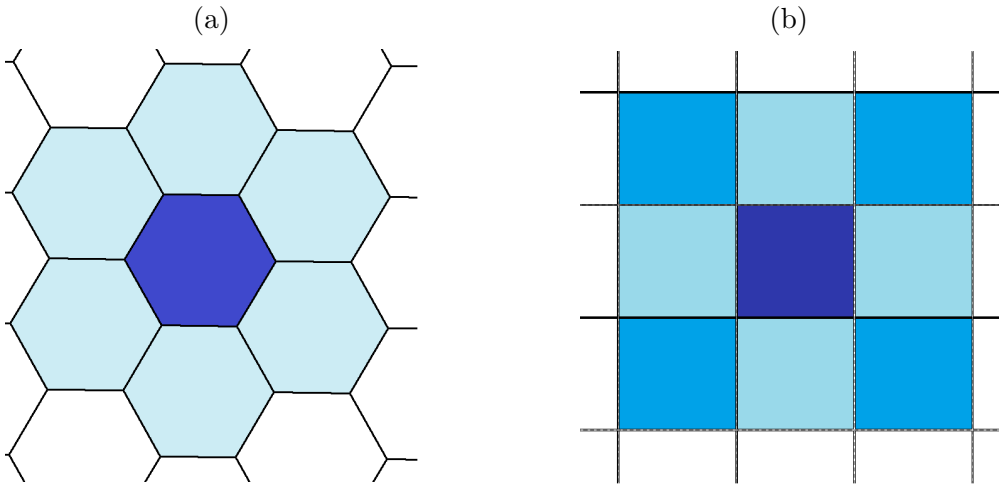


(c) Age 2 vs. Age 4 Tuition



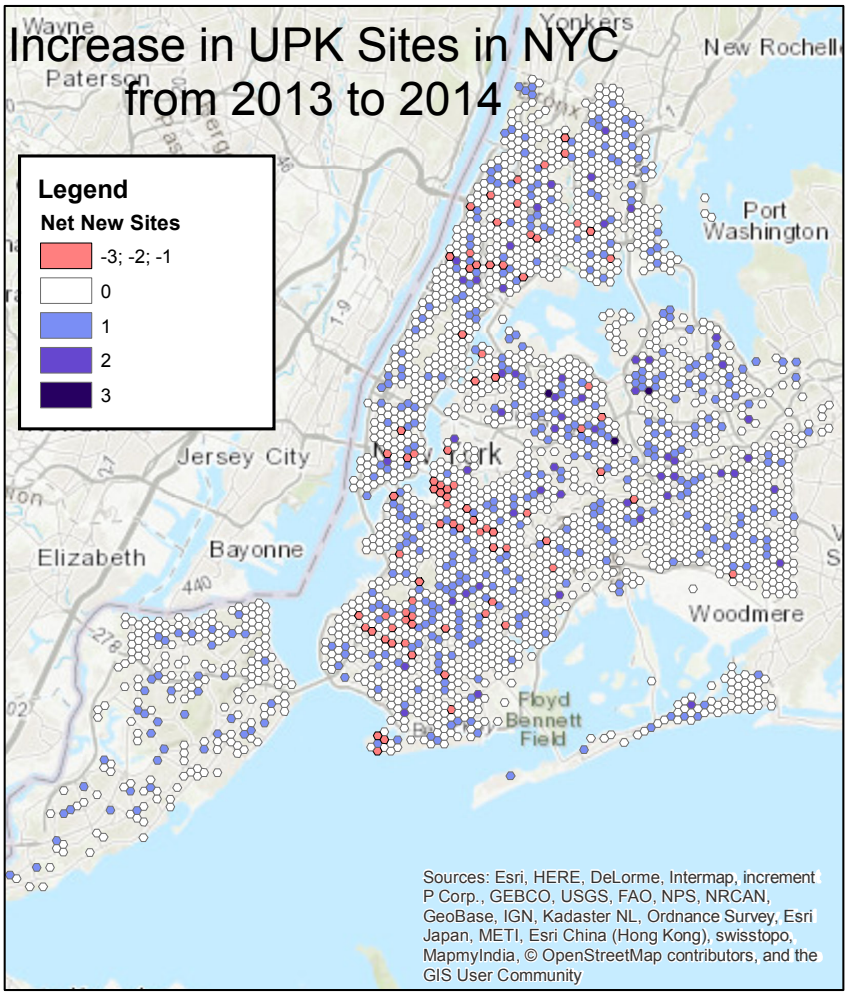
Notes: Using data acquired from the Pennsylvania Department of Human Services (DHS), graphs report the cumulative density function of the ratio of younger child tuition to age 4 tuition as reported by the day cares to DHS, their licensing agency. Data is as of November 2017 and covers Bucks, Chester, Delaware, Montgomery, and Philadelphia counties in Pennsylvania. The vertical red lines indicate the expected ratio based on mandated minimum staff to child ratios and assuming labor accounts for 70% of costs. For example, 1 year olds (shown in (b)) are required to have a 1:5 ratio compared to the 4 year old ratio of 1:10. Therefore, the tuition for 1 year olds should be at least 70% higher, so the tuition ratio should be at least 1.7. The required ratio for infants under 12 months is 1:4 and for 2 year olds it is 1:6.

Figure A-2: Tessellated Hexagons have the same relationship to all of their neighbors



Notes: Figure (a) demonstrates that in a grid of hexagons, all neighbors have the same relationship to the central hexagon (they are same-side neighbors). On the other hand, with a grid of squares, the central square has both same-side (white) neighbors and diagonal (light blue) neighbors, as seen in Figure (b). Because hexagons have the same relationship with all of their neighbors, pulling in neighbor effects is more natural and does not require adjustment for the type of neighbor.

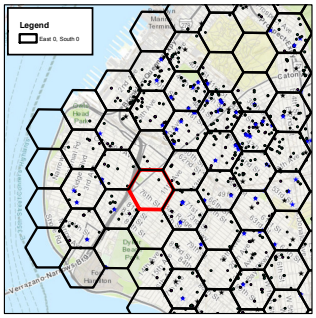
Figure A-3: Change in Number of Universal Pre-K Sites from 2013 to 2014 for Main Hexagon Grid



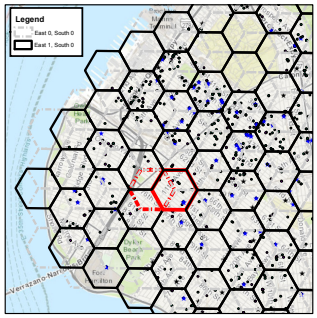
Notes: Map displays the number of full-day UPK sites in each hexagon in 2014-15 minus the number of full-day UPK sites in that hexagon in 2013-14. The change in the number of pre-K sites in the hexagon is the main "exposure" variable and is displayed on the hexagon grid used in the main specification. Hexagons are only included in the regression if they had a family day care home, day care center, or public elementary school at some point from 2008-2013. This restriction is made to make the "control" hexagons more similar to "treated" hexagons by excluding areas that are primarily parks, water, the airport, or other areas that day cares and UPKs cannot operate. Of the 2,658 hexagons shown, 449 had more UPK sites in 2014 than 2013 and 82 had fewer.

Figure A-4: Example of Hexagon Shifting for Robustness

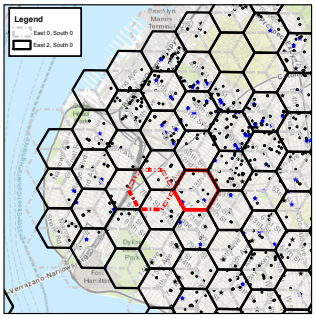
(a) East 0, South 0



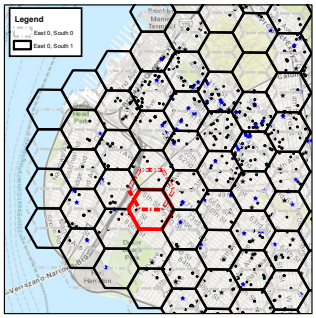
(b) East 1, South 0



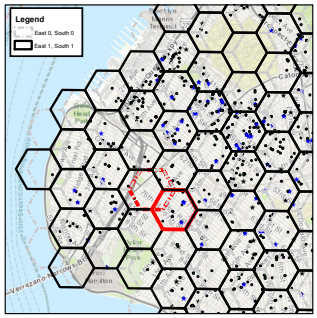
(c) East 2, South 0



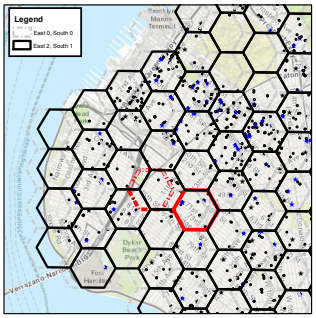
(d) East 0, South 1



(e) East 1, South 1

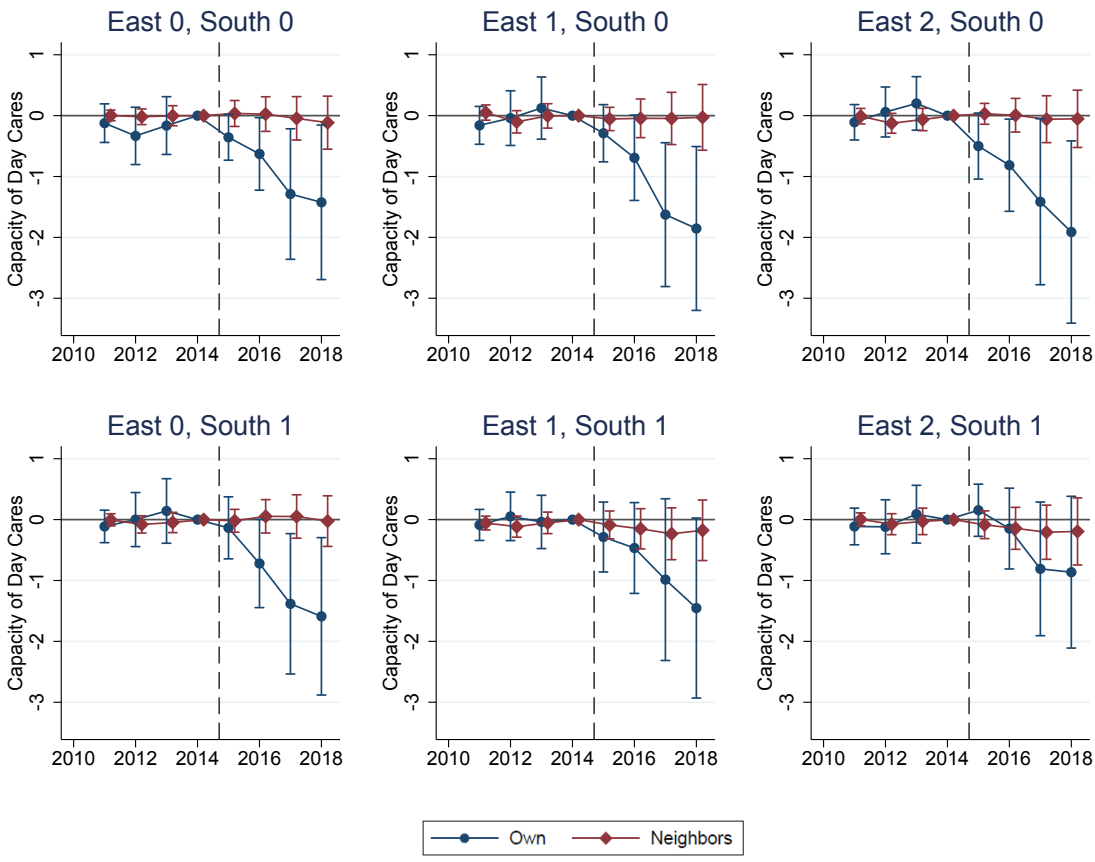


(f) East 2, South 1



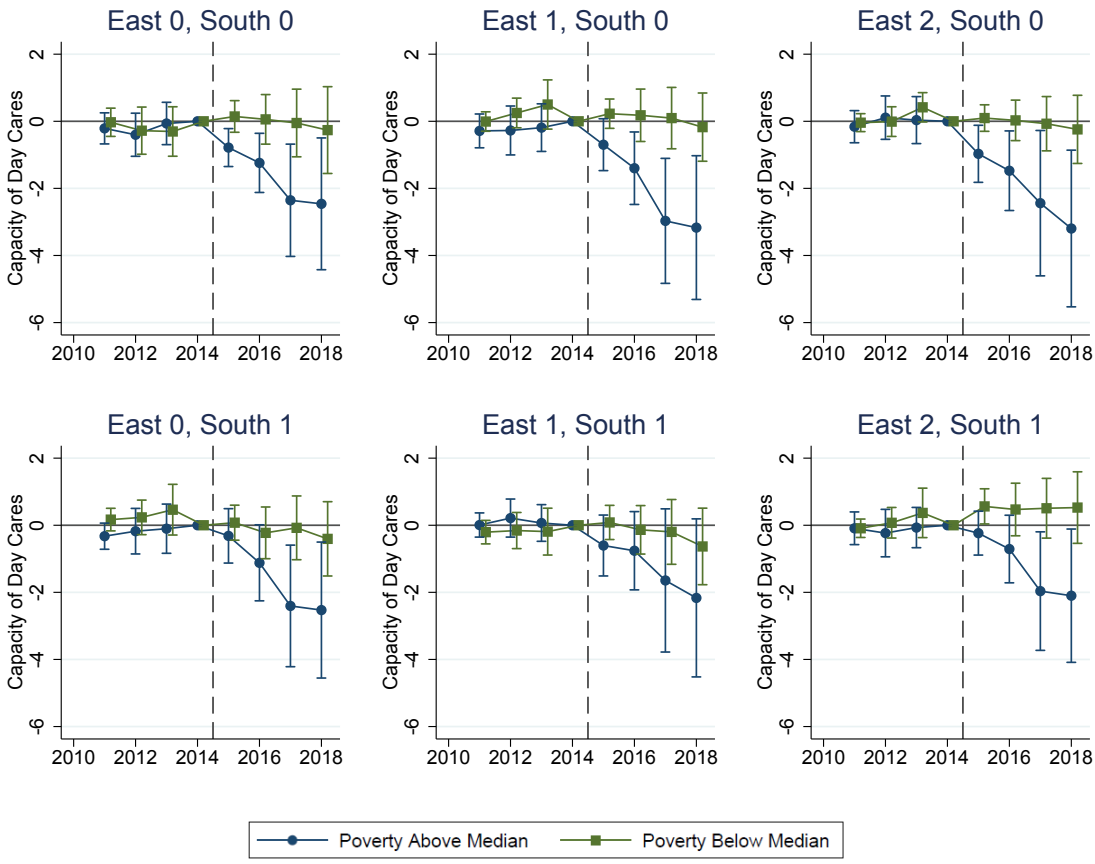
Notes: Figures demonstrate shifting an example hexagon grid. There are three "half hexagon" shifts east-west and two "half hexagon" shifts north-south.

Figure A-5: Effect of New Public Pre-K Site on Capacity of Day Care Centers Serving Younger Children: Robustness to Shifting the Grid



Notes: Figures plot coefficients on interactions between year indicators and the change in the number of Universal Pre-K sites between 2013-14 and 2014-15 in the hexagon ("Own") and the hexagon's immediate neighbors ("Neighbors"). Observations are at the hexagon-year level. Hexagons are approximately ten city blocks in area and form a grid that covers the city. Each figure represents a different shift of the hexagon grid. "East 0, South 0" is the original grid, while "East 1, South 0" shifts the whole grid one half hexagon to the east, and so on. There are three half hexagons going east-west and two going north-south, so these represent the complete set of half-hexagon shifts. The outcome variable is the total licensed capacity of day care centers serving children between the ages of 0 and 2. Includes hexagon fixed effects, time fixed effects, and hexagon-specific linear time trends. Bands show the 95% confidence interval with standard errors clustered at the hexagon level.

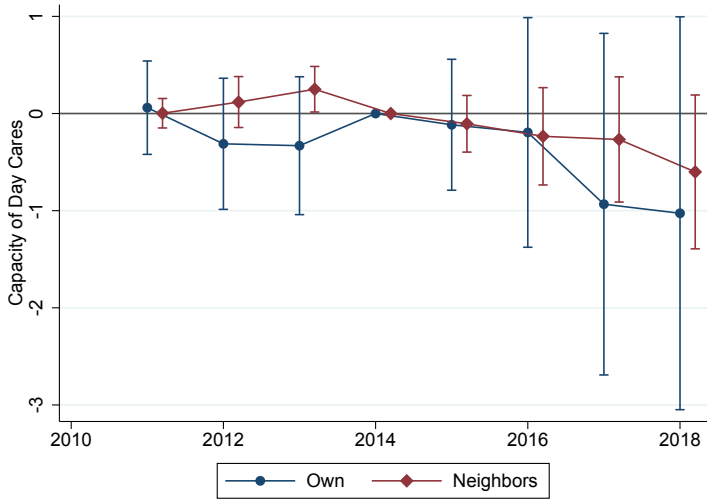
Figure A-6: Effect of New Public Pre-K Site on Capacity of Day Care Centers Serving Younger Children: Heterogeneity by Poverty Level Robustness to Shifting the Grid



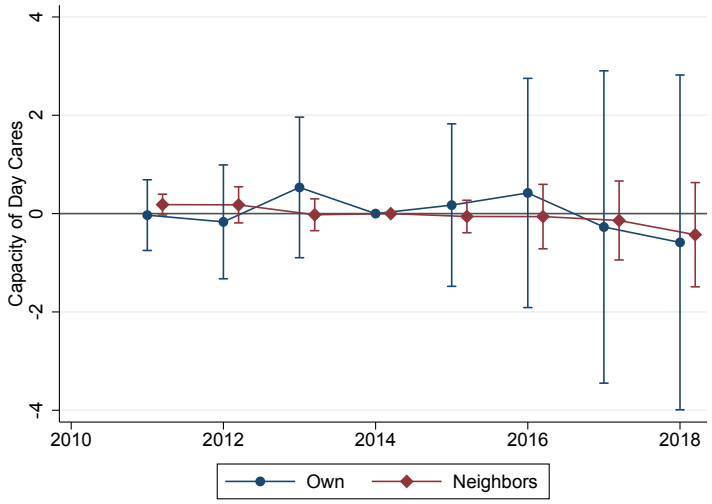
Notes: Figure compares how a new Universal Pre-K site affects the total capacity of day care centers serving children between the ages of 0 and 2 in areas with above-median levels of people living below the 200% poverty line to areas with below-median levels. In the median area, approximately 30% of people live below 200% of the federal poverty line. Figure plots coefficients of interactions of three variables: 1) year indicators, 2) indicators for above-median and below-median fraction of people living below 200% of the federal poverty line as of 2014, and 3) the change in the number of Universal Pre-K sites between 2013-14 and 2014-15 in the hexagon. Observations are at the hexagon-year level. Hexagons are approximately ten city blocks in area. Each figure represents a different shift of the hexagon grid. "East 0, South 0" is the original grid, while "East 1, South 0" shifts the whole grid one half hexagon to the east, and so on. There are three half hexagons going east-west and two going north-south. The outcome variable is the total licensed capacity of home day cares in the given hexagon. Includes hexagon fixed effects, time fixed effects, and hexagon-specific linear time trends. Bands show the 95% confidence interval with standard errors clustered at the hexagon level. Poverty level is determined by applying the American Community Survey 5-year estimates at the block group level to each block within the block group and calculating for each hexagon the average poverty level for the blocks in that hexagon weighting by Census 2010 population at the block level.

Figure A-7: Effect of New Public Pre-K Site on Capacity of Day Care Centers Serving Younger Children: Change in Estimate when Increasing the Size of the Hexagons

(a) Medium size: Area of Approximately 20 blocks

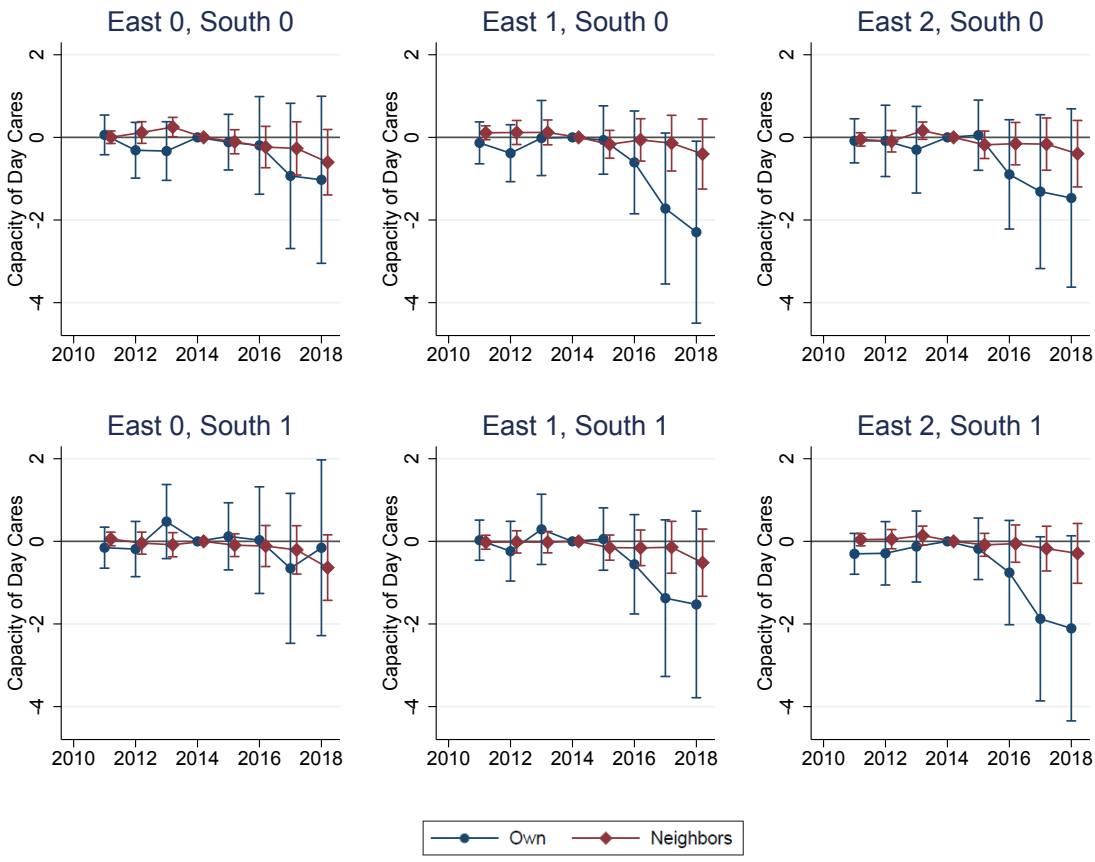


(b) Large size: Area of Approximately 40 blocks



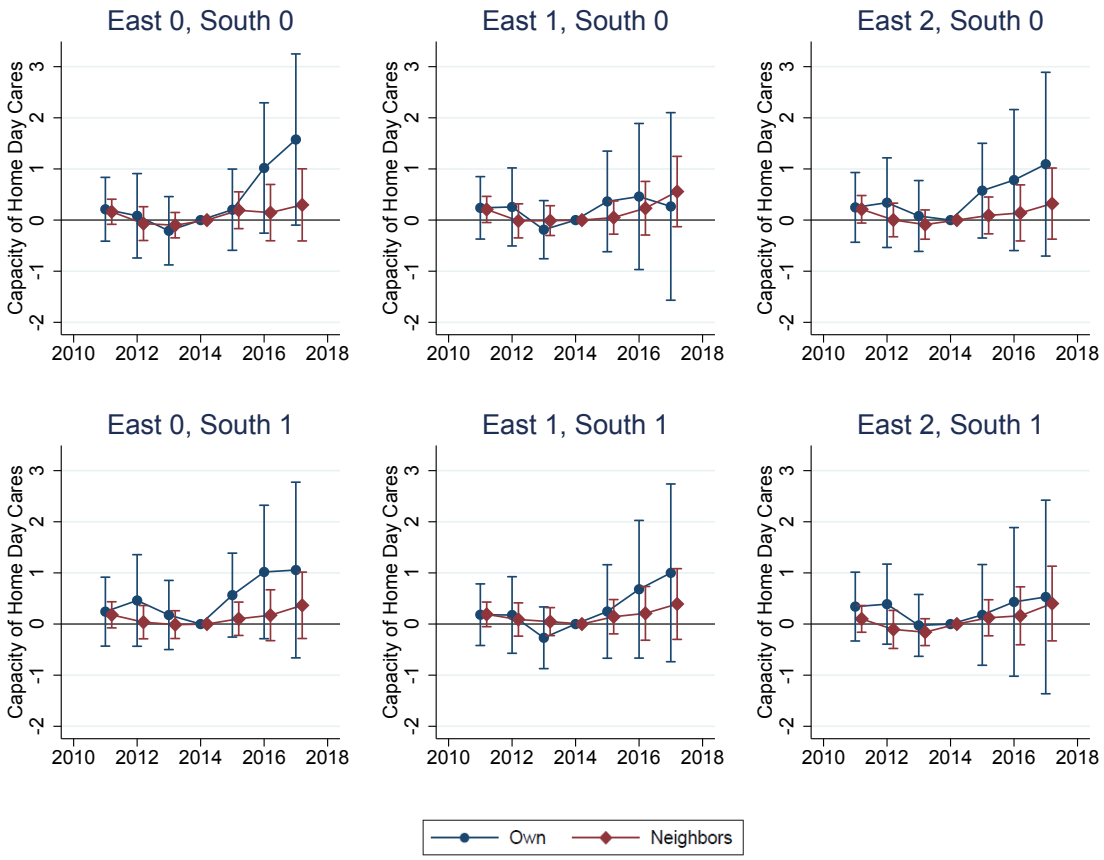
Notes: Plot of regression results from equation 1.2 with 95% confidence bands. "Own" coefficients estimate the effect of one new UPK site in a hexagon on the total capacity of day care centers for children between the ages of 0 and 2 in that hexagon. "Neighbors" coefficients estimate the effect of a new UPK in a neighboring hexagon on the capacity of day care centers serving children between the ages of 0 and 2 in the given hexagon. These figures show how estimates change for two larger grid sizes: areas of 20 blocks and 40 blocks.

Figure A-8: Effect of New Public Pre-K Site on Capacity of Day Care Centers Serving Younger Children: Medium Grid Robustness to Shifting



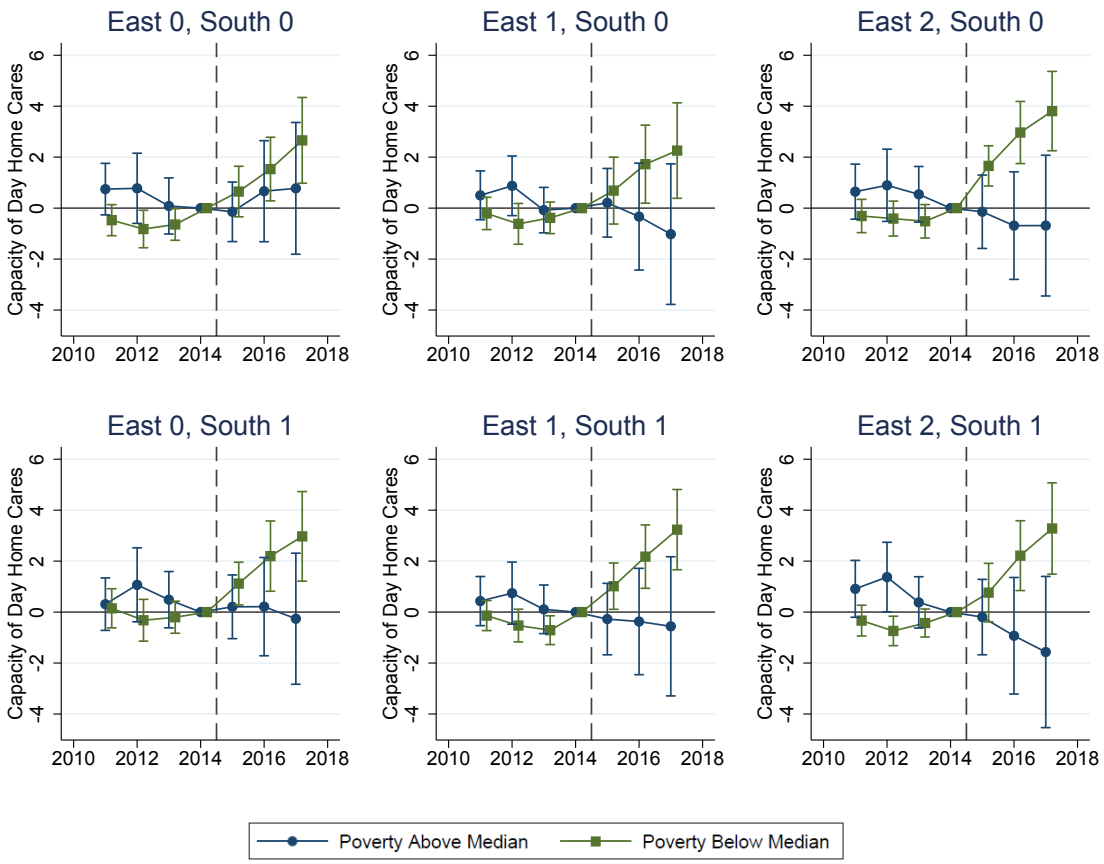
Notes: Figures plot coefficients on interactions between year indicators and the change in the number of Universal Pre-K sites between 2013-14 and 2014-15 in the hexagon ("Own") and the hexagon's immediate neighbors ("Neighbors"). Observations are at the hexagon-year level. Hexagons are approximately twenty city blocks in area and form a grid that covers the city. Each figure represents a different shift of the hexagon grid. "East 0, South 0" is the original grid, while "East 1, South 0" shifts the whole grid one half hexagon to the east, and so on. There are three half hexagons going east-west and two going north-south, so these represent the complete set of half-hexagon shifts. The outcome variable is the total licensed capacity of day care centers serving children between the ages of 0 and 2. Includes hexagon fixed effects, time fixed effects, and hexagon-specific linear time trends. Bands show the 95% confidence interval with standard errors clustered at the hexagon level.

Figure A-9: Effect of New Public Pre-K Site on Capacity of Home Day Cares: Robustness to Shifting the Grid



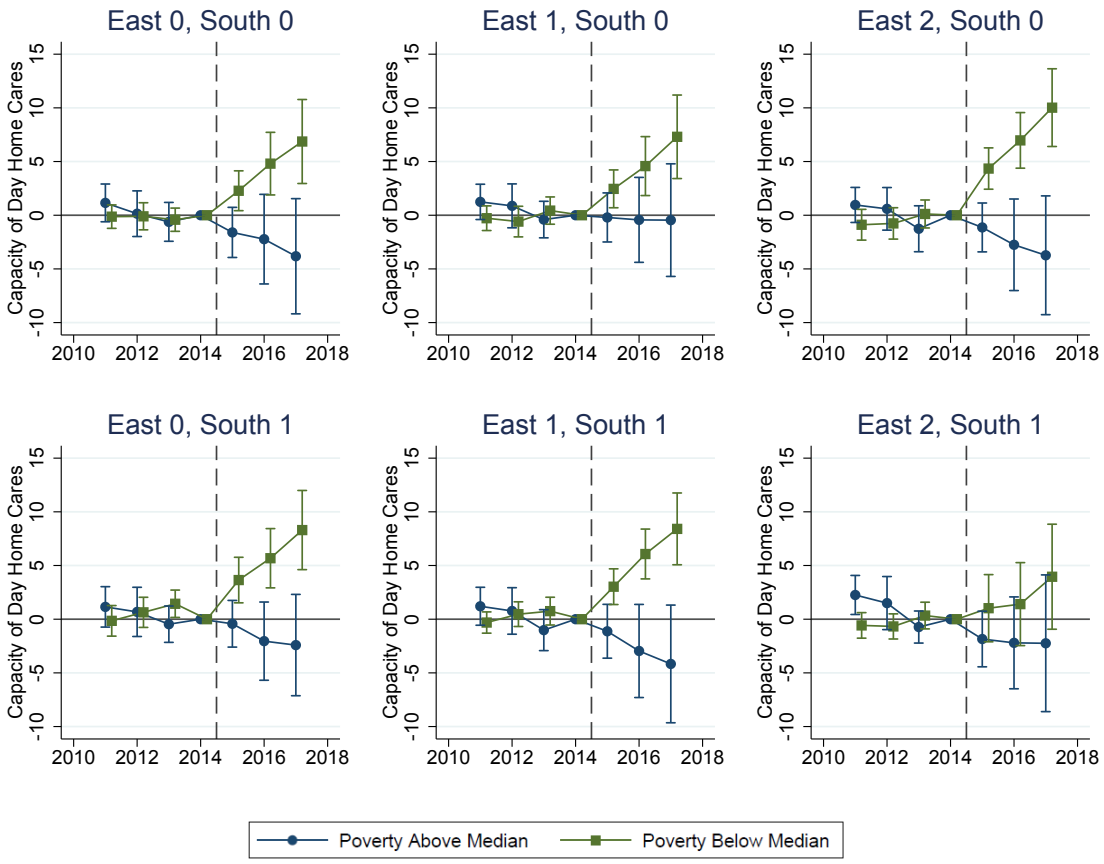
Notes: Figures plot coefficients on interactions between year indicators and the change in the number of Universal Pre-K sites between 2013-14 and 2014-15 in the hexagon ("Own") and the hexagon's immediate neighbors ("Neighbors"). Observations are at the hexagon-year level. Hexagons are approximately ten city blocks in area and form a grid that covers the city. Each figure represents a different shift of the hexagon grid. "East 0, South 0" is the original grid, while "East 1, South 0" shifts the whole grid one half hexagon to the east, and so on. There are three half hexagons going east-west and two going north-south, so these represent the complete set of half-hexagon shifts. The outcome variable is the total licensed capacity of home day cares. Note that the maximum capacity for children under age 2 is approximately one-third of total licensed capacity. Includes hexagon fixed effects, time fixed effects, and hexagon-specific linear time trends. Bands show the 95% confidence interval with standard errors clustered at the hexagon level.

Figure A-10: Effect of a New Pre-K Site on Capacity of Home Day Cares: Heterogeneity by Poverty Level Robustness to Shifting Grid



Notes: Figures compare how a new Universal Pre-K site affects the total capacity of home day cares in areas with above-median levels of people living below the 200% poverty line to areas with below-median levels. In the median area, approximately 30% of people live below 200% of the federal poverty line. Figure plots coefficients on interactions between three things: 1) year indicators, 2) indicators for above-median and below-median fraction of people living below 200% of the federal poverty line as of 2014, and 3) the change in the number of Universal Pre-K sites between 2013-14 and 2014-15 in the hexagon. Observations are at the hexagon-year level. Hexagons are approximately ten city blocks in area. Each figure represents a different shift of the hexagon grid. "East 0, South 0" is the original grid, while "East 1, South 0" shifts the whole grid one half hexagon to the east, and so on. There are three half hexagons going east-west and two going north-south. The outcome variable is the total licensed capacity of home day cares in the given hexagon. Includes hexagon fixed effects, time fixed effects, and hexagon-specific linear time trends. Bands show the 95% confidence interval with standard errors clustered at the hexagon level. Poverty level is determined by applying the American Community Survey 5-year estimates at the block group level to each block within the block group and calculating for each hexagon the average poverty level for the blocks in that hexagon weighting by Census 2010 population at the block level.

Figure A-11: Effect of a New Pre-K Site on Capacity of Home Day Cares: Medium Grid Heterogeneity by Poverty Level Robustness to Shifting Grid



Notes: Figures compare how a new Universal Pre-K site affects the total capacity of home day cares in areas with above-median levels of people living below the 200% poverty line to areas with below-median levels. In the median area, approximately 30% of people live below 200% of the federal poverty line. Figure plots coefficients on interactions between three things: 1) year indicators, 2) indicators for above-median and below-median fraction of people living below 200% of the federal poverty line as of 2014, and 3) the change in the number of Universal Pre-K sites between 2013-14 and 2014-15 in the hexagon. Observations are at the hexagon-year level. Hexagons are approximately ten city blocks in area and form a grid that covers New York City. Each figure represents a different shift of the hexagon grid. "East 0, South 0" is the original grid, while "East 1, South 0" shifts the whole grid one half hexagon to the east, and so on. There are three half hexagons going east-west and two going north-south. The outcome variable is the total licensed capacity of home day cares in the given hexagon. Includes hexagon fixed effects, time fixed effects, and hexagon-specific linear time trends. Bands show the 95% confidence interval with standard errors clustered at the hexagon level. Poverty level is determined by applying the American Community Survey 5-year estimates at the block group level to each block within the block group and calculating for each hexagon the average poverty level for the blocks in that hexagon weighting by Census 2010 population at the block level.

Table A-1: New York City Day Care Center Staffing Requirements

Age Group	Min. Staff to Child Ratio	Max. Group Size
Less than 12 months	1:4	8
12 to 24 months	1:5	10
2 years	1:6	12
3 years	1:10	15
4 years	1:12	20
5 years	1:15	25

Notes: Table provides the minimum staff ratio and maximum group sizes allowed by law for children in day care centers in New York City. A ratio of 1:4 means that there can be at most 4 children for each teacher in the classroom. Group size refers to how many children can be cared for together in one room.

Table A-2: Predictors of Having a Full-Day Universal Pre-K Site in 2014-15

	Number of Full-Day Pre-K Sites in 2014-15			
	(1)	(2)	(3)	(4)
Num. 2013 Pre-K Sites	0.865***	0.862***	0.661***	0.657***
	(0.0137)	(0.0146)	(0.0168)	(0.0170)
Log Population		0.0146	-0.0161	-0.0209*
		(0.0124)	(0.0116)	(0.0121)
Frac. HHs with Children		-0.474***	-0.202**	-0.225**
		(0.0973)	(0.0913)	(0.0933)
% Non-White		0.130***	0.174***	0.165***
		(0.0397)	(0.0369)	(0.0374)
% Owner-occupied		0.0418	0.0797*	0.0903*
		(0.0492)	(0.0456)	(0.0463)
% Land		0.254	0.137	0.136
		(0.174)	(0.161)	(0.161)
Zero Population		-0.00469	-0.207	-0.235
		(0.166)	(0.154)	(0.155)
Num. Elem. Schools			0.263***	0.261***
			(0.0190)	(0.0191)
Num. 2014 DCC 2-5s			0.188***	0.188***
			(0.0105)	(0.0117)
Num. 2014 DCC 0-2s				0.00307
				(0.0257)
Num. 2014 HDCs				0.00358
				(0.00242)
Dep. Var. Mean	0.512	0.512	0.512	0.512
N	2658	2658	2658	2658

Note: Observations are at the hexagon level. These are the hexagons used in the main specification and each is approximately ten city blocks in area. Demographic characteristics are from block-level data from the 2010 Census. Blocks are mapped to hexagons based on which hexagon their centroid falls in. "Zero population" denotes hexagons with a population of zero; this dummy variable is included so that no hexagons are dropped from the specification due to not having a denominator for fraction of households with children, fraction non-white, or percent owner-occupied. The "% Land" variable is the fraction of the hexagon that is land (as opposed to water). * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A-3: Number of Facilities in Non-Expansion v. Expansion Hexagons

Variable	Non-Expansion	Expansion	Diff
Num. 2013 Pre-K Sites	0.36	0.34	-0.026
	(0.02)	(0.03)	(0.037)
Num. 2014 Pre-K Sites	0.32	1.47	1.149***
	(0.01)	(0.03)	(0.035)
Num. 2014 Pre-K Seats	13.96	56.85	42.891***
	(0.67)	(1.99)	(1.734)
Δ Sites per Hexagon	-0.04	1.13	1.176***
	(0.01)	(0.02)	(0.014)
Neighbors' Δ Sites	0.68	0.82	0.137**
	(0.03)	(0.06)	(0.065)
Num. DCC 2-5s as of 1/1/14	0.57	1.31	0.741***
	(0.02)	(0.05)	(0.050)
DCC 2-5 Capacity as of 1/1/14	36.88	92.45	55.566***
	(1.52)	(4.45)	(3.914)
Num. DCC 0-2s as of 1/1/14	0.12	0.27	0.156***
	(0.01)	(0.02)	(0.021)
DCC 0-2 Capacity as of 1/1/14	3.03	6.33	3.302***
	(0.25)	(0.66)	(0.624)
Num. HDCs as of 1/1/14	2.99	2.81	-0.186
	(0.10)	(0.22)	(0.236)
HDC Total Capacity as of 1/1/14	29.54	27.75	-1.785
	(0.92)	(2.14)	(2.252)
HDC Inf/Todd Capacity as of 1/1/14	9.98	9.38	-0.604
	(0.31)	(0.73)	(0.764)
N	2209	449	2658

Significance levels: * $< 10\%$ ** $< 5\%$ *** $< 1\%$
Standard errors in parentheses.
"Expansion" hexagons are ones that had more full-day UPK sites in 2014 than in 2013, and "Non-expansion" hexagons are ones that did not. Statistics are for the hexagon grid in the main specification, which are about 10 city blocks in area.

Table A-4: Robustness: Effect of New Public Pre-K Site on Stock of Day Care Centers Serving Younger Children

	(1)	(2)	(3)	(4)	(5)	(6)
$\Delta Sites * (Year = 2017)$	-1.144*	-1.753***	-1.599**	-1.517**	-0.997	-0.882
	(0.588)	(0.655)	(0.703)	(0.626)	(0.663)	(0.630)
$\Delta Sites * (Year = 2018)$	-1.276*	-2.009***	-2.128***	-1.748**	-1.468**	-0.956
	(0.693)	(0.737)	(0.769)	(0.690)	(0.730)	(0.711)
$\Delta NeighborSites * (Year = 2017)$	-0.0388	-0.00736	0.00793	0.0991	-0.189	-0.171
	(0.159)	(0.213)	(0.190)	(0.172)	(0.216)	(0.203)
$\Delta NeighborSites * (Year = 2018)$	-0.109	0.0157	0.0198	0.0274	-0.133	-0.154
	(0.195)	(0.267)	(0.229)	(0.199)	(0.252)	(0.250)
Dep. Var. Mean	3.408	3.385	3.370	3.413	3.411	3.407
Num. South Shifts	0	0	0	1	1	1
Num. East Shifts	0	1	2	0	1	2
N	23922	24120	24156	23967	23994	23922

Notes: Dependent variable is the capacity in or number of day care centers serving children between the ages of 0 and 2 in the hexagon. Hexagons are approximately ten blocks in area and cover New York City. Each model is estimated on a different shift of the original grid. Each shift is half of a hexagon, so that three shifts east brings the grid back to its original position, as does two shifts south. Includes year and hexagon fixed effects and a hexagon-specific linear time trend. Standard errors are clustered at the hexagon level.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

.2 Appendix: Model of Child Care Pricing and Supply

B-1 Model Introduction

The purpose of this model is to provide intuition for why day cares may price such that they make the greatest profit from the oldest children and to use that model to provide a comparative static for the introduction of UPK.

Assume day cares have local market power and provide products with interrelated demands: care for younger children and care for older children. The demand for these two products is interrelated because the price of care for younger children at a location may affect the demand for care for older children at that same location, and vice versa. For example, a "low" price for infant care may attract more infants to the day care. If parents face switching costs when changing care arrangements, then they are likely to keep their child at the same day care as they age, so a lower price for infant care in one year increases the demand for toddler care in the next year. Also, parents of more than one child may prefer that they be cared for at the same location (for example, they may pay a utility cost for more than one drop off), so the price for the younger sibling could influence their demand for care for the older sibling at that location and vice versa.

Consider a model with day cares that can provide care to two age groups, infants and pre-kindergarteners. For ease of exposition, assume that there are constant marginal costs for infant care (α) and pre-K care (γ), with α significantly higher than γ. Assume that the market structure is monopolistic competition.[49] Demand for infant care is $D_I(p_I, p_K)$ and depends both on the price of infant care, p_I, and the price of pre-K care, p_K. Demand for pre-K care at a given day care depends on the price of infant care, p_I, and the price of pre-K care p_K. For each day care facility, it also depends on whether the child was at that day care when they were an infant in the previous period. Demand for pre-K care is then $D_K(p_I, p_K, D_I(p_I^{-1}, p_K^{-1}))$, where p_I^{-1} and p_K^{-1}) are the prices for infant care and pre-K care in the previous period. For simplicity, and reflecting that day care prices are fairly sticky,

[49] The monopolistic competition assumption can be micro-founded with spatial differentiation.

assume that real prices are the same in each period. Then demand for pre-K care is then $D_K(p_I, p_K, D_I(p_I, p_K))$.

B-2 The Firm's Problem

Using the notation from the previous section, algebraically, the firm's problem is:

$$\max_{p_I, p_K}(p_I - \alpha) * D_I(p_I, p_K) + *(p_K - \gamma) * D_K(p_I, p_K, D_I(p_I, p_K)) \tag{B-1}$$

where p_I is the price of infant care, p_K is the cost of pre-K care, and D_I and D_K are demand for infant care and pre-K care, respectively. Note that sibling effects enter this equation through D_K's dependence on p_I and $D - I$'s dependence on p_I. Switching costs enter the problem through D_K's dependence on the previous period's D_I, which equals the current period's D_I under the assumption that p_I and p_K are the same in each period.

In order to gain some intuition for why day cares cross-subsidize infants with pre-K children, it is simplest to focus only on switching costs. Therefore, consider the case where sibling effects are not relevant. Then $\frac{\partial K}{\partial p_I} = 0$ because p_I only influences the number of pre-K children through its effect on infant enrollment in the previous period and not through attracting younger siblings. To further simplify, assume that parents of infants are not forward-looking and therefore do not take p_K into account when selecting a day care for their infant. Then $\frac{\partial I}{\partial p_K} = 0$. After taking first order conditions and rearranging, the solution to the firm's problem is:

$$p_K = \gamma - \frac{K}{\frac{\partial K}{\partial p_K}} \tag{B-2}$$

$$p_I = \alpha - \frac{I}{\frac{\partial I}{\partial p_I}} + \frac{\partial K}{\partial I} * \frac{K}{\frac{\partial K}{\partial p_K}} \tag{B-3}$$

If there are no switching costs ($\frac{\partial K}{\partial I} = 0$), the first-order conditions yield the standard monopoly solution. With switching costs, $\frac{\partial K}{\partial I} > 0$ because children who come to the day care as infants are more likely to stay for pre-K. Because $\frac{\partial K}{\partial p_K} < 0$, assuming that day cares make positive profit on pre-K children means the whole term is negative added to the monopoly solution is negative, so switching costs lower the price of infant care. Intuitively,

the firm takes into account that the marginal revenue from attracting an infant includes not only the marginal revenue from their infant year but also the revenue they would get from their pre-K year times the increased chance they will stay for pre-K given that they came for infant care. Therefore, in the presence of switching costs, day cares charge lower prices to infants because they take into account the additional profits they will make if the child stays for pre-K.

B-3 Comparative Static: Universal Pre-K Introduction

Now I will consider how introducing free, public pre-K changes the firm's optimal solution. First, the introduction of UPK reduces demand for private pre-K care. It causes a decrease in the retention of infants as future pre-K kids since public pre-K introduces a new substitute for private pre-K care. In the language of the model, a reduction in the retention of infants is a decrease in $\frac{\partial K}{\partial I}$. It also likely increases price sensitivity for pre-K care, increasing $\frac{\partial K}{\partial p_K}$ in absolute value, and decreases K.

A reduction in K and/or an increase in $\frac{\partial K}{\partial p_K}$ reduces the profit from pre-K care. Furthermore, the reduction in $\frac{\partial K}{\partial I}$ means that a child is less likely to stay for pre-K once they come as an infant. So p_I increases because attracting an infant is less likely to lead them to say for the more profitable pre-K age, and even if they do stay, the profit from doing so is lower. The link between starting infant care at a day care and staying for pre-K is weaker, reducing the incentive to charge a "lower" price to attract infants. Note that the amount of change in p_I increases with the degree of crowd out (the amount that $\frac{\partial K}{\partial I}$ decreases). The more crowd out, the larger the decrease in the probability of staying for pre-K, and the larger the price increase for infants.

Figure B-1 presents the UPK introduction comparative static in a simple supply and demand framework. Universal Pre-K shifts the demand curve for private pre-K down, lowering the quanitity of pre-K care at equilibrium and lowering its price. Due to the lower pre-K price and quantity, day cares shift their supply curves for infant care up, requiring a

higher price for a given quantity of infant care supplied. The supply curve shift lowers the quanitity of infant care supplied and increases its price. The degree to which quantity and price change depends on how elastic demand is for infant care. If it is relatively inelastic, there will be more adjustment on the price margin and less on the quantity margin relative to if it is relatively elastic. For parents who can only afford to pay for care with child care subsidies, demand for child care centers would be perfectly elastic and all adjustment would be on the quantity margin.

B-4 Quality Enters as a Change in the Marginal Cost

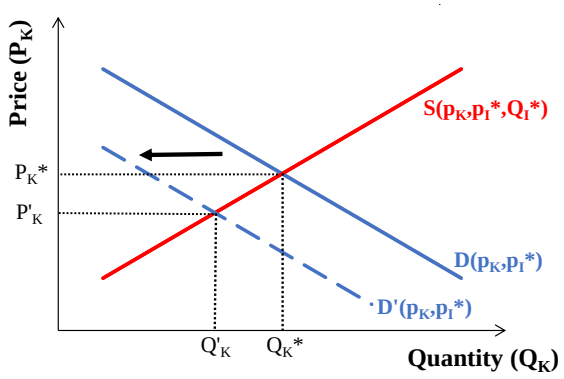
The discussion above of models of day care shutdown and pricing decisions both assume a given constant marginal cost. But another way day cares could react to crowd out of pre-K children would be to reduce their marginal costs by reducing quality of care. For example, they could hire teachers with less experience or education, reduce investments in professional development, lower the quality of provided food, or reduce infrastructure investments or maintenance. These would enter the model as reducing the marginal cost of infants, α, and/or the marginal cost of a pre-K child, γ.

In the model of pricing decisions with the comparitive static of reducing retention of infants for pre-K, $\frac{\partial K}{\partial I}$, in order to maintain the equilibrium condition of equation B-3, day cares could lower the marginal cost of providing pre-K care (γ), which would increase the expected future profits and/or lower the marginal cost of providing infant care (α). Thus, day cares could respond to the crowd out due to UPK by reducing their quality.[50]

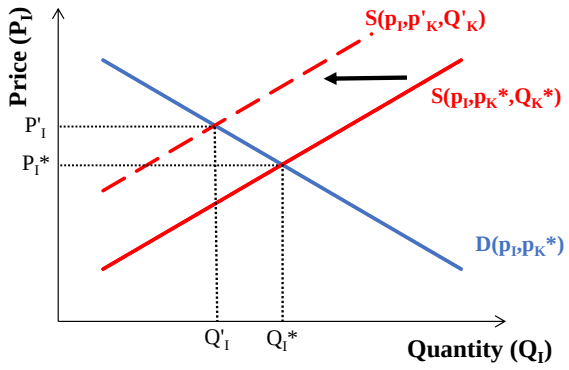
[50]It is also possible that they would try to increase their quality in order to attract more students. Increasing quality would decrease $\frac{\partial K}{\partial p_K}$ because people would be more willing to send their child to the day care for a given price. It could also increase $\frac{\partial K}{\partial I}$ by improving parents' opinion of the day care. However, a quality increase is not observed in the data.

Figure B-1: Effect of Universal Pre-K on Private Child Care Market

(a) Demand for Private Pre-K goes down



(b) Supply of Infant Seats goes down due to Lower Private Pre-K at Equilibrium



Notes: Suppose private day cares can provide two goods: infant care and pre-K care. The demand for these goods is interrelated, and empirically, day cares cross-subsidize infant care with pre-K care. When Universal Pre-K opens, parents reduce their demand for private pre-K care as in subfigure (a), lowering the equilibrium quantity of private pre-K. The lower equilibrium quantity of private pre-K care shifts the supply curve in the private infant care market to the left, reducing the equilibrium quantity of infant care and increasing its price.

.3 Pre-existing Reduced Form Identification Methods

This appendix addresses the two current reduced-form approaches that can be used when treatment intensity is based on distance to a point. It describes the methods and how they are implemented and why they are not used in this case.

C-1 Ring Method is biased when rings overlap

There are two ways to implement the ring method, which I will call the "potential sites" design and the "near/far design". With the former, the first step is to locate all potential sites and identify which became UPKs and which did not. Next, one would compare outcomes in a circle surrounding sites that do become UPK sites to ones that do not. In the "near/far design", the circles are instead drawn around all new UPK sites. The changes in the number and capacity of day cares in a smaller circle surrounding any new UPK (say, 0.25 miles) is compared to changes in a larger ring around it, say between 0.25 miles and 0.5 miles. Both approaches are common in the literature. See Dinerstein and Smith (2014) or Davis (2011) for "potential sites" and Currie and Walker (2011) for the "near/far" design.

The more feasible design in this case would most likely be the "near/far" design. Implementing the potential sites design would involve identifying all public schools, private schools, and day care centers since these three groups encompass the vast majority of UPK sites. This list still does not give all potential sites, however, because some sites were in facilities that were not operating in the pre-period. The number of potential sites is also very large as there are 1,848 day care centers alone, so even if the rings were very small, there would still be a lot of overlap. Since the near/far design would involve about 400 observed new sites, it would be more feasible.

However, the ring method is likely to be biased when treatment is spatially dense enough that markets (and therefore rings) overlap. And because observations would not be mutually-exclusive since the same area can be part of more than one ring, the standard errors may also be misestimated. With more than 400 sites in an area of about 300 square miles, in this

application, the rings would overlap substantially even if they were small. In the "near/far" design, if two sites are far enough apart that their treated areas do not overlap but are close enough that their treated area does overlap with the other's "control" ring, the estimate would be biased down due to the contamination of the "control" area (see part a of Figure C-1 for a visual). On the other hand, when two sites are close enough together that there is a large overlap in the areas that are "treated" by them, then estimates would be biased up if there is an intensive as well as extensive effect (that is, if there is a larger effect if there are two UPKs in an area as opposed to just one). The observed area is affected by two UPKs, but the regression treats each area as if it is only affected by one UPK, therefore overestimating the effect by attributing all of it to one UPK instead of two (see part (b) of Figure C-1 for a visual). Similar arguments go through for the alternative "treated/untreated" design. Therefore, when treatment is spatially dense, estimates based on the ring method could be biased and in an unknown direction.

C-2 Testing Robustness to Administrative Boundaries is Challenging

Sometimes data restrictions or the identification strategy require the use administrative boundaries as the geographic units of observation. Sometimes the relevant market is defined as the unit of observation itself without allowing for overlapping markets (Cellini, 2009). For example, I could allow the child care market in a given Census tract to be affected by all UPKs in that Census tract but not any that are outside that Census tract. Other times, the market is defined by a ring around the centroid of the administrative unit (Baum-Snow and Marion, 2009). For example, I could allow the child care market in a given Census tract to be affected by all UPKs in Census tracts whose centroids are within a certain radius of that tract's centroid.[51]

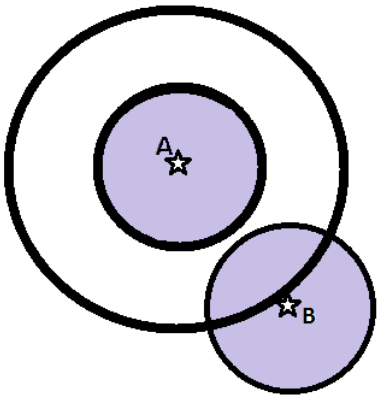
[51] Alternatively, one could allow the child care market in a Census tract to be affected by all UPKs within a given radius of the tract's centroid regardless of which tract the UPK is located in. In practice, it is not usually implemented this way due to the data restrictions that required the analysis to be done at the level of the administrative unit in the first place.

However, administrative boundaries are arbitrary, irregularly-shaped, and there is no natural way to test robustness to them, so they may not be the best option when they are not required by data or other constraints. Foote et al. (2017) demonstrate that uncertainty in the definition of commuting zones is empirically relevant for analysis of local labor markets. Therefore, testing robustness to boundaries can be important. Although using different boundaries could change results, this issue is often brushed under the rug. And for good reason since there is no natural way to shift the boundaries or make them larger or smaller, so there is often little that a researcher can do.

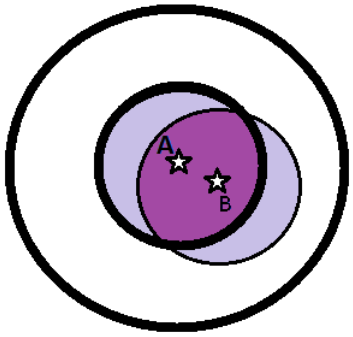
One reason that these boundaries may be preferred even when they are not required by the data is that, although they are arbitrary, they are not defined by the researcher, so they are less subject to manipulation. I will address this concern by demonstrating how results change when boundaries are shifted or enlarged in a systematic way.

Figure C-1: Potential Bias with Ring method

(a) "Treated" area overlaps "untreated" area: biased down



(b) Two "treated" areas overlap: unknown bias



Notes: Assume the ring method model is correctly specified: the effect of the treatment dissipates with distance and the inner ring covers the area affected by the treatment. Then, if in (a), two sites are close enough together that the treated ring overlaps the other's control ring but far enough apart that the treated areas do not overlap, then the estimate of the effect would be biased down in absolute value due to the contamination of the control ring with the other treated ring. If, on the other hand, they are close enough that their treated areas overlap, the effect could be biased up or down. Because all of the effect in the inner ring is attributed to one site in the regression even though in the dark purple shaded area, it is affected by two, it could bias the result away from zero. But if the inner rings overlap then there must also be the issue of the treated ring overlapping the control ring, which biases the effect toward zero. Which effect dominates depends on how close the sites are and how rapidly the effect dissipates with distance.

Chapter 2

The Impact of a Long-Term Care Information Campaign on Insurance Purchase [1]

2.1 Introduction

Spending on long-term care services accounts for approximately two percent of U.S. GDP, totaling $366 billion in 2016. The majority of this spending is done by government programs, predominantly Medicaid. Over half of Americans over 65 will need to use long-term care services at some point in their lives, and the average lifetime cost for someone who needs long-term care is $150,000 (Tell and Cutler, 2011). Despite this risk, only 12% of older Americans hold long-term care insurance policies, potentially leaving many others exposed to large financial risks and therefore possibly underinsured.

In response to potential underinsurance and rising Medicaid costs, federal and state governments have experimented with several different policies, including tax subsidies for long-term care insurance, increasing the Medicaid asset limit for individuals who purchase certain private long-term care insurance policies ("Partnership" policies), and information campaigns. If effective, information campaigns could be a cost-effective way to ameliorate underinsurance and lower Medicaid costs. They are also unique in that, unlike tax subsidies

[1] I am grateful to Alan Krueger for his advice throughout this project. I am also grateful for helpful comments from Leah Boustan, Will Dobbie, Hank Farber, Ilyana Kuziemko, Ethan Lieber, Alex Mas, Steve Mello, and seminar participants at Princeton University. All errors are my own.

and Partnership policies, they target a broad range of potential long-term care planning behaviors in addition to long-term care insurance purchase. Long-term care insurance may not be the best option for everyone, so information campaigns may be particularly useful for their ability to target additional behaviors.

This paper studies the impact of the "Own Your Future" information campaign on long-term care planning behaviors. The campaign was a federal-state partnership that was implemented in four to six states each year for five years from 2005 to 2009, for a total of 24 states and Washington, D.C. The primary stated goal of the campaign was to encourage individuals to plan ahead for long-term care with the hope that doing so would lower Medicaid costs (Iwasaki et al., 2010).

Using data from the Health and Retirement Study, I find that the "Own Your Future" campaign increased long-term care insurance coverage by 1.4 percentage points in participating states, an increase in coverage of 12%. The increase occurred primarily for higher-asset individuals, with individuals in the top 20% of the asset distribution increasing coverage rates by 3.4 percentage points, or 15%. Using a back-of-the-envelope calculation, I estimate that the increase in long-term care insurance coverage will save Medicaid $420 million in present value.

In addition, I find that the campaign increased life insurance coverage, a partial substitute for long-term care insurance, by three percentage points, or 5%, and the increase was spread more evenly through the asset distribution. Long-term care insurane and life insurance can be (imperfect) substitutes because both provide ways to preserve assets for a bequest and both are more valuable in poor health.

I find minimal evidence of an impact on other planning behaviors, such as moving closer to adult children or increasing monetary transfers to children. But consistent with the increase in LTCI, life insurance coverage, and potential unobserved additional planning behaviors, I do find an increase in the self-reported probability of leaving a bequest of over $100,000. Finally, I find some suggestive evidence of a decrease in Medicaid enrollment as a result of

the campaign. If this decline is maintained, it could result in substantial savings in Medicaid outlays for long-term care.

Most closely related to this paper is a literature studying the impact of government interventions intended to increase long-term care insurance take-up. Goda (2011) estimates that state tax subsidies increase coverage by two to three percentage points, while Courtemanche and He (2009) find that allowing LTCI premiums to be deductible on federal taxes increased coverage by 3.3 percentage points. Goda (2011) notes that part of the effect she estimates may be due to information campaigns that occurred with the introduction of the tax subsidies or the perception of government endorsement, and my paper helps shed light on these potential mechanisms. Lin and Prince (2013) find that the Partnership for Long-term Care Insurance Program does not lead to a measurable increase in coverage overall but does increase take-up by high asset individuals. Bergquist et al. (2015) also finds no significant effect of the Partnership Program on LTCI take-up.[2] Compared to these other programs, I find that the OYF campaign was more effective and less costly than the Partnership program and raised coverage by about one-third of an average tax subsidy at a fraction of the cost.

This paper also contributes to research on why take-up of long-term care insurance is so low. Brown and Finkelstein (2007) suggest that there is some role for supply-side market imperfections, but it is not large enough to explain much of the gap, while Brown and Finkelstein (2008) find a significant role for the implicit tax from Medicaid. My work suggests that a small piece of the puzzle could be due to information frictions.

Finally, my work contributes to a large literature on the impact of information interventions. Findings in this literature vary, with some interventions generating no response

[2]In addition, a couple of primarily descriptive studies have considered the effectiveness of the "Own Your Future" campaign itself. The Department of Health and Human Services commissioned an initial report after the first phase of the campaign, which descriptively compares individuals who ordered a Planning Kit to those who did not and analyzes planning behaviors after the campaign. They find that four to six months after the campaign, 1.4% of respondents in the five campaign states report having purchased LTCI after the campaign, which is remarkably similar to my estimate. In pre-post analysis, they find awareness of planning options was higher after the campaign. Iwasaki et al. (2010) use a survey to study dissemination of the campaign in Washington state and find limited evidence that it was effective in that state.

to others generating significant responses. For example, Bettinger et al. (2012) find that an intervention informing families of their eligibility for college aid had no effect on aid application or college enrollment and Chetty and Saez (2013) find that giving EITC-eligible taxpayers information about the EITC structure had no measurable impact on labor force participation on average. On the other hand, sending information on Social Security benefit structure did increase labor force participation (Liebman and Luttmer, 2015) and providing information on the returns to education to a population that had previously significantly underestimated those returns increased years of schooling (Jensen, 2010).

The remainder of the paper proceeds as follows. Section 2.2 provides more background on the "Own Your Future" campaign, while section 2.3 discusses the expected theoretical impact of the campaign. Section 3.4 describes the data used for the analysis. In section 3.5, I outline the empirical strategy, and in section 2.6, I describe the results. Section 3.9 concludes.

2.2 The "Own Your Future" Information Campaign

The "Own Your Future" (OYF) consumer awareness campaign was designed to encourage individuals to plan ahead for their long-term care needs. Following a research and development phase that took place from 2000-2005, the Department of Health and Human Services partnered with 24 states and the District of Columbia to implement the campaign. The campaign roll-out occurred in five phases staggered over five years from 2005 to 2009, with three to six states participating each year. The list of states participating in each phase can be found in Table 2.1 with a map in Figure 2.1. Participating states are spread across the country with all major regions represented and particularly-high participation in the Great Plains.

The federal government spent $3 million each year of the campaign and funded OYF's core campaign activities. In addition to the core campaign activities, states were expected to fund their own complementary activities. These varied from state-to-state and included

funding advertising campaigns or public service announcements, launching websites, and holding town hall meetings.

The core campaign included a campaign launch with a press conference with the governor followed by letters from the governor that were sent to households with a member age 50-70 (for the first two phases) or 45-65 (for the remaining three phases), with a total of 18 million letters sent over the course of the campaign. The letters emphasized the benefits of planning ahead for long-term care needs, and an example letter is shown in Figure 2.2. In a report commissioned by HHS after the first wave of campaigns, nearly half of survey respondents recalled receiving the letter from the governor (Long Term Care Group, Inc. and Lifeplans, Inc., 2006).

Along with the other publicity associated with the campaign, the letters also encouraged individuals to order a Planning Kit. The Planning Kit included more detailed information about ways to plan ahead for long-term care and was tailored to each state with state-specific statistics and information. Planning Kits described several ways to plan ahead, addressed legal issues, options for privately financing LTC, including LTCI, and explained what is and is not covered by public programs, including that Medicare will not cover long-term care expenses. In total, 1.5 million Planning Kits were sent to households who received a letter, for a response rate of 8% (Tell and Cutler, 2011). The HHS report notes that this is a high response rate relative to a general expected social marketing response rate of 5% (Long Term Care Group, Inc. and Lifeplans, Inc., 2006).

The message of the OYF campaign was that planning ahead for LTC needs can lead to more independence, control, and financial security. For example, the letter in Figure 2.2 from Virginia provides some typical language: "most of us will need some type of long term care in our later years. While this is a normal part of aging, the tragedy is that many of us don't plan for it adequately. And when we need long term care, it becomes a crisis for us and for our families."

This paper will focus on the campaign's impact on LTCI coverage, but it is worth noting that purchasing LTCI was just one of several suggested planning options. For example, some state letters and planning kits suggested making home modifications, improving your health, discussing informal caregiving with family and friends, and using reverse mortgages to pay for care. Therefore, any impact found on LTCI coverage should be considered a lower bound on the impact of the campaign as a whole since there are other unmeasured planning actions individuals may have taken as a result of the OYF campaign.

2.3 Theoretical Impact of the Campaign

In a world of complete information, an information campaign would not be expected to change behavior. But in the case of planning ahead for long-term care, there are reasons to believe that an information campaign could influence behavior.

First, there is evidence that individuals do not have full information about long-term care options. For example, an OYF pre-campaign survey found that 80% of individuals believed that Medicare would cover at least some of their long-term care expenses (Long Term Care Group, Inc. and Lifeplans, Inc., 2006). However, Medicare only covers nursing home stays for the purpose of rehabilitation and for 100 days or less, not long-term care.

Second, individuals may have been avoiding planning ahead for long-term care because thinking about a time when one might need care is uncomfortable (Tell and Cutler, 2011). In this case, the campaign may force individuals to think about it and create a plan for the future.

If the campaign had an impact due to lack of full information and/or previous avoidance behaviors, how would we expect individuals to change their behavior? The campaign itself provided a wide variety of suggestions for planning ahead, including doing home renovations to allow individuals to stay in their homes longer, talking to children about expectations for their involvement, and purchasing long-term care insurance.

The focus of this paper is the impact of the information campaign on long-term care insurance coverage because this is one of the more easily-observed outcomes that was directly targeted. LTCI purchase is also a planning behavior that can be observed in the short-term, unlike, for example, moving to live near children, which requires more time and planning. But the impact of OYF on LTCI coverage is theoretically ambiguous. The campaign may induce individuals who were not covered to purchase LTCI or induce individuals who already have coverage not to lapse their policies. On the other hand, OYF also raised awareness of other options for planning ahead for LTC, so it could have caused individuals to substitute away from LTCI. Planning Kits also emphasized that LTCI is not the best option for everyone. The Kit only recommended LTCI for individuals who have significant assets to preserve and can afford the LTCI premiums without financial strain. Individuals who didn't meet those criteria may have decided to drop their insurance.

Either outcome would indicate that the campaign had an impact and reduced welfare loss from suboptimal planning. An increase in LTCI coverage would be expected to reduce future Medicaid expenditures (Brown and Finkelstein, 2008). However, in the case of a reduction in LTCI coverage, the impact of OYF on Medicaid spending is ambiguous. Medicaid spending could increase due to the reduction in LTCI coverage. But depending on what unmeasured alternative planning behaviors were taken up by other individuals, Medicaid spending could decrease. For example, if individuals who otherwise would have gone on Medicaid made modifications to their homes so they could stay in them longer, OYF could still reduce Medicaid spending even in the presence of a reduction in LTCI coverage.

In addition to changes in LTCI coverage, OYF could induce a change in life insurance coverage. The campaign encouraged individuals to plan ahead for the future, and life insurance is an additional lever through which to do so. In particular, life insurance and LTCI can be substitutes (Koijen et al., 2016). Both provide a way to preserve assets and leave a bequest. Additionally, some life insurance policies contain accelerated death benefit coverage, which allows insureds to draw down their future benefits while they are still alive if

they meet certain criteria as specified by the policy, such as a terminal diagnosis or inability to perform activities of daily living. Such policies are even more direct substitutes to LTCI.

Because of the substitutability between LTCI and life insurance, I will study changes in life insurance coverage as an additional outcome that could be affected by OYF. This is in contrast to prior studies of LTCI demand, which are able to use life insurance as a placebo test since it is not directly targeted (Goda 2011; Lin and Prince 2013).[3]

If it was successful, OYF would also be expected to affect other planning behaviors as well. For example, individuals may modify their homes so they can remain in them longer. They may create a plan for their children to care for them under certain circumstances or even move closer to children. Planning Kits also suggested ways to improve health. Any of these behaviors could also be expected to reduce Medicaid claims. But they are either difficult to measure or would also be expected to take more time, so we would not necessarily expect to see an immediate increase in parents living near children, for example. I do consider changes in some of these other measures, like living within ten miles of a child, but do not necessarily expect to see measurable changes.

Additionally, OYF could have affected individuals' expectations for the future either through providing more accurate information that caused them to update their priors or through causing individuals to undertake new planning behaviors that could affect their expectations of their own futures. Here I consider three different outcomes: the self-reported probabilities of leaving any bequest, of leaving a bequest of more than $100,000, and of moving to a nursing home within five years.

For all three outcomes, it is ambiguous whether OYF would increase or decrease the self-reported probabilities of the events. If the campaign provided information that caused

[3]In particular, Goda (2011) studies the impact of tax subsidies for LTCI on LTCI coverage. Lin and Prince (2013) estimates the impact of Partnership policies on LTCI coverage. Partnership policies allow individuals to claim Medicaid at an asset threshold equal to the Medicaid asset threshold plus the maximum benefit from the LTCI insurance. Neither policy is expected to have a very significant effect on life insurance purchase. Lin and Prince (2013) do find a slight negative effect on life insurance, which would be expected if LTCI and life insurance are (imperfect) substitutes.

individuals to update their priors of the events, they may be updating either up or down. In addition, individuals may have undertaken planning behaviors as a result of the campaign. If they improved their future planning, it may increase their probability of leaving a bequest. On the other hand, if they instead decided to shed assets in order to qualify for Medicaid if they need long-term care, it could lower the probability of leaving a bequest. For the probability of moving to a nursing home within five years, improved planning like modifying their home or moving closer to children could decrease the probability of needing a nursing home, but purchasing long-term care insurance may increase the probability.

2.4 Data

2.4.1 Data Sources

The primary data for this analysis comes from the Health and Retirement Study (HRS).[4] The HRS is a longitudinal panel study of older Americans, with surveys conducted every two years and new cohorts added periodically. The sample consists of the randomly-selected respondents and their spouses. The survey includes thorough questions about individuals' demographics, health status, family structure, employment, retirement plans, assets, income, and insurance coverage.

The data that I use for this study comes primarily from the RAND HRS data files. These files provide a cleaned version of the original data and are produced by the RAND Center for the Study of Aging with funding from the National Institute on Aging, the Social Security Administration, and the Health and Retirement Study. I obtained restricted access to the HRS data in order to merge the RAND data with state of residence, which is not available in the public use data.[5] In addition, I merged in some information from the raw HRS data for outcomes that are not available in the cleaned RAND data: whether any of the respondent's

[4]The HRS (Health and Retirement Study) is sponsored by the National Institute on Aging (grant number NIA U01AG009740) and is conducted by the University of Michigan.

[5]I am thankful to the University of Michigan's Institute for Social Research, and particularly to Janet Keller, for assistance in accessing these data.

children live within 10 miles, monetary transfers to children, and whether the respondent has a will.

The HRS data were then merged with OYF campaign states and dates. Information about OYF was acquired primarily through press releases found through the Wayback Machine Internet Archive. An individual was considered exposed to the OYF campaign if he or she was living in a campaign state in the year of or the year after the campaign, depending on the timing of the HRS survey, which is every two years, relative to the information campaign.

2.4.2 Sample Restrictions

I make several restrictions to the data. First, I restrict the data to the survey years occurring from 2000 to 2014. The most recent survey year available is 2014, which is five years after the last phase of OYF in 2009. The restriction to 2000 is symmetrically five years before the first OYF campaigns occurred in 2005. I also restrict to cohorts that entered the survey in 2004 or earlier in order to consider only individuals who would be expected to be in the data both before and after the campaigns.

Furthermore, I restrict to individuals who could be targeted by OYF based on age. The first two phases of the campaign targeted individuals age 50 to 70, while the second phase targeted individuals age 45 to 65. An individual who was 70 in 2005, the first year of the campaign, would be 79 by 2014. Therefore, I restrict the sample to individuals who are younger than 80.[6][7] Note that the lower age limit isn't binding due to the sampled age

[6]Some earlier studies of LTCI, such as Goda (2011) and Lin and Prince (2013), restrict the sample to ages 50 to 69, stating that these are the primary LTCI purchase years. However, OYF could affect lapse behavior as well as purchase behavior, and lapses could occur later in life. Additionally, restricting the data is not necessary except perhaps to increase the precision of the estimates. Therefore, I do not make this restriction.

[7]I could have included all ages and used a regression discontinuity design based on age, but I choose to use difference-in-differences for a few reasons. First, implementing an RD would significantly reduce power since there are very few individuals in the HRS outside of the targeted ages. Second, the OYF campaign was broader than the letter, and individuals of other ages could have been exposed to OYF. And third, the RD would not be able to capture changes in coverage behavior across the whole elderly and near-elderly age spectrum.

range in the HRS. This leaves 115,762 person-year observations. Because all regressions are run using person weights, I next drop 6,609 observations for which the provided weight equals zero. Finally, in order to maintain a consistent sample, I drop 2,488 observations that are missing one or more of the individual control variables, leaving 106,665 person-year observations.[8]

2.4.3 Sample Descriptive Statistics

Descriptive statistics for the sample split by whether the state of residence implements OYF at some point are provided in Table 2.2. The table includes means for all demographic control variables used in the regressions: income, assets, age, female, married, education completed, number of living children, hispanic, black, and an indicator for any ADL limitations. Although many differences between the states are statistically signficant, most are not economically significant. The only difference of greater than 5% is total assets. Individuals in states that implement the OYF campaign have on average $51,650 in assets in 2010 dollars as compared to $57,260 in states that do not implement OYF, a difference of about 10%. Although comparing demographics across treatment and control states is useful, note that random assignment of treatment is not necessary for causal identification. What is necessary is that individuals in states without OYF provide a good counterfactual for how planning behaviors of individuals in OYF states would have evolved in the absence of the information campaign. This assumption is discussed in further detail in Section 3.5.

Table 2.3 presents average coverage rates for LTCI and life insurance split by demographics. Overall, 12.4% of individuals in my sample period have LTCI, and there are not significant gender differences in coverage. Older individuals are more likely to have coverage, reflecting the fact that individuals often wait to purchase LTCI until they are closer to potentially using it. Among individuals with at least some college education, 16.4% have LTCI, as compared to only 8.4% among individuals with a high school education or less,

[8]Note that among these observations there is some variation in non-response to certain outcomes, so the sample size has some variation across regressions with different outcome variables.

likely reflecting that college-educated individuals have more assets to protect with LTCI. LTCI coverage also increases with increasing levels of assets, with only 6.3% of individuals with below-median asset levels carrying LTCI as compared to 22.9% among individuals in the top 20% of the asset distribution. Married individuals are more likely to have LTCI than those who are not married, possibly because married individuals may not want to draw down assets while their spouse may still need them. Finally, coverage rates are higher among individuals who do not have any activity of daily living limitations, perhaps because insurance companies mostly do not sell policies to individuals who already have ADL limitations.

Life insurance is much more prevalent than LTCI, with 67% of individuals in the sample carrying it. Life insurance is more common for individuals who are male, younger than 65, have some college or more, are married, and/or have no ADL limitations.

2.5 Empirical Strategy

I use a difference-in-differences strategy to estimate the impact of OYF on planning behaviors. This design compares the difference between individuals' planning behaviors in areas with OYF (treatment states) and without OYF (control states) before and after the campaign, leveraging the staggered state OYF campaigns. Under the identifying assumption discussed below, differential changes in planning behaviors between treatment and control states before and after the campaign can be causally attributed to the impact of the OYF information campaign.

Specifically, the regressions take the form:

$$Y_{it} = \beta * OYF_i * Post_t + \gamma * X_{it} + \sigma_s + \eta_t + \sigma_s * t + \epsilon_{ist} \tag{2.1}$$

Y_{it} is the outcome of interest for individual i in year t. The primary outcome of interest is LTC_{it}, an indicator variable that equals one if individual i has long-term care insurance coverage in year t and is zero otherwise. Additional outcomes of interest are discussed in the following paragraph. OYF_i is an indicator that equals one if an individual has been or will be exposed to an OYF campaign. An individual is considered exposed to an OYF campaign

if he or she was living in a state with an OYF campaign the year of the campaign for campaigns in even years or the year after the campaign for campaigns in odd years.[9] $Post_t$ is an indicator that equals one if year t is after campaign exposure and is zero otherwise. All regressions include state fixed effects, σ_s, which account for level difference across states, and year fixed effects, η_t, which account for level differences across years. Some regressions additionally control for state-specific linear time trends, $\sigma_s * t$, to control for any differences in trends. Standard errors are clustered at the state level, and all regressions use the person weights provided by HRS.

In addition to OYF's impact on long-term care insurance coverage, I also estimate its impact on additional long-term care planning behaviors. First, I consider its impact on life insurance coverage because life insurance provides a substitute for LTCI.[10] Next, I estimate the impact of OYF on changes in planning behaviors other than insurance purchase. These include living near children, providing transfers to children, and whether the respondent has a will. Finally, I estimate the impact of OYF on some expectations for the future, which the information campaign could impact: individuals' self-reported probabilities of leaving any bequest, of leaving a bequest of more than $100,000, and of moving to a nursing home within 5 years.

The coefficient of interest from equation (2.1) is β, which estimates the causal impact of the OYF campaign on planning behaviors Y_{it} under the identifying assumption that in the absence of the information campaign, individuals' planning behaviors would have trended similarly across both exposed and unexposed states. One way to provide support for this assumption is to check whether they were trending similarly before the campaign. Given that they were trending similarly in the pre-period, an additional, untestable, assumption is

[9] So for 2005 and 2006 campaigns, an individual is considered exposed if s/he lived in a campaign state as of the 2006 survey. For 2007 and 2008 campaigns, an individual is exponsed if s/he lived in a campaign state as of 2008, and for the 2009 campaigns, an individual is exposed if s/he lived in the state as of the 2010 survey. If an individual is exposed to multiple campaigns, the earliest exposure is used. In practice, only 10% of the sample moves across state lines at any point during the 2000-2014 sample window.

[10] For discussion of how life insurance provides a substitute, see section 2.3.

necessary, which is that there was no other shock to planning behaviors that happened to coincide with the OYF campaigns that differentially affected OYF and non-OYF states.

In regression form, in order to test whether OYF states were differentially deviating from trend in the pre-period, net of the control variables in the model, I run an event-study specification. This model compares outcomes of treated individuals after the campaign to those who are never exposed to the campaign and to individuals who will be exposed to the campaign at a later time.

Concretely, I estimate the following regression:

$$Y_{it} = \sum_{\tau} [\beta_\tau * \mathbb{1}_{\tau_{it}}] + \gamma * X_{it} + \sigma_s + \eta_t + \sigma_s * t + \epsilon_{ist} \tag{2.2}$$

Where τ_{it} is the time period since exposure. Time periods are grouped into two-year bins since the HRS is a biannual survey but treatment occurs in consecutive years. Therefore, one-year bins would use one set of states to identify coefficients for odd years in event time and a different set of states to identify coefficients for even years in event time. Grouping the event-time periods into two-year bins allows for pooling treatment effects across all treated states. The event-time bin containing the year before the treatment and the year of treatment (years -1 and 0 in event time) is the left-out period. Note that the event time for individuals who were not exposed to the campaign is always zero, so they are also part of the left-out group. All observations five or more years before the campaign are pooled together as are all observations six or more years after the campaign, so that $\tau \in [< -5; \{-4, -3\}; \{-2, -1\}; \{1, 2\}; \{3, 4\}; \{5, 6\}; > 6]$. The remainder of the specification is the same as the difference-in-differences specification, equation (2.1). There are state and year fixed effects, individual-level control variables, and state-specific linear time trends. Note that if the assumption that the OYF states were not differentially deviating from trend in the pre-period is satisfied, then we would expect the coefficients on the interactions with the years before the campaign to be zero.

In addition to the main difference-in-differences estimation, I also separately estimate the impact of OYF on individuals' planning behaviors by asset levels. The purpose of this is two-fold. First, LTCI is a more rational choice for individuals with assets to preserve. Individuals with low asset levels can instead rely on Medicaid.[11]. In fact, the OYF Planning Kits emphasized that individuals should purchase LTCI only if they have significant assets to preserve and can afford the LTCI premiums without financial strain. Therefore, estimating the impact of OYF by asset level helps determine whether individuals made rational adjustments in response to OYF. Second, estimated Medicaid savings depend on the asset levels of individuals induced to purchase insurance, with the greatest savings from individuals of the lowest asset levels (Brown and Finkelstein, 2008). Since Medicaid savings were a stated goal of the campaign, estimating responses by asset levels can help determine whether it was successful in achieving this goal.

To estimate responses by asset levels, I use the following specification:

$$Y_{it} = \sum_{\alpha} [\beta_{\alpha} * \mathbb{1}_{\alpha_{it}} * OYF_i + \mu_{\alpha} * \mathbb{1}_{\alpha_{it}}] + \gamma * X_{it} + \sigma_s + \eta_t + \sigma_s * t + \epsilon_{ist} \tag{2.3}$$

Where $\alpha \in$ {High, Medium, Low} are three asset-level bins. Low assets is defined as individuals with asset levels below the 50th percentile in 2010 year dollars, medium assets as between the 50th and 80th percentiles, and high assets as above the 80th percentile. The asset cut offs in 2010 dollars for the 50th and 80th percentiles are $194,092 and $660,479, respectively. This is the same specification as equation (2.1) except that the treatment variable is interacted with indicators for different asset levels. Indicator variables for medium and high assets are also included to account for any non-linearities. In addition, the regression includes the same control variables and fixed effects as equation (2.1).

[11]LTCI may be rational for individuals with even relatively low asset levels if they have a strong preference for non-Medicaid nursing homes, which are on average higher quality (Nyman, 1988).

2.6 Results

2.6.1 LTCI Coverage Increases

The results of the regression described by equation (2.1) with LTCI coverage as the outcome variable are summarized in Table 2.4. Column (1) includes only state and year fixed effects, column (2) adds individual control variables, and column (3) adds state-specific linear time trends. All specifications indicate that OYF increased LTCI coverage by over one percentage point, or about a 10% increase on a mean coverage rate of 12%. Column (3) indicates that OYF increased LTCI coverage by 1.36 percentage points, and this estimate is significant at the 5% level.

Figure 2.3 plots the corresponding event-study coefficients for the main regression in column (3) with error bars representing the 95% confidence interval. As expected, none of the pre-campaign coefficients are significantly different from zero. I performed a joint test on the hypothesis that all three equal zero, and it has an F-statistic of 0.71 with a corresponding p-value of 0.50, validating the assumption that there were no differential deviations from trend in the pre-period.

The increase in LTCI coverage begins in the year immediately following the campaign. There is an initial increase in coverage of about one percentage point in the first two years after OYF, though this coefficient is not statistically significant at the 5% level (p-value=0.15). Beginning in the third year after OYF, there is a sustained increase in LTCI coverage that hovers around two percentage points and is statistically significant at the 5% level. That the coverage increase begins just after the campaign occurred provides additional evidence that OYF was the cause of the increased LTCI coverage from the difference-in-differences estimation.

2.6.2 Life Insurance Coverage Increases

Table 2.5 displays the difference-in-difference results of the impact of OYF on life insurance coverage. As discussed in Section 2.3, life insurance is a potential substitute for long-term

care insurance, so we may expect OYF to increase life insurance coverage rates as well. As before, column (1) contains only state and year fixed effects, column (2) adds individual-level controls, and column (3) adds state-specific linear time trends. Under all three specifications, there is a statistically-significant increase in life insurance coverage of two to three percentage points. The estimate in column (3), which includes all of the control variables, indicates an increase in life insurance coverage rates of 3.16 percentage points as a result of the OYF information campaign. Although larger in absolute terms than the LTCI coverage increase, the life insurance coverage increase in smaller in relative terms, amounting to an increased life insurance coverage rate of 5%.

The corresponding event-study coefficients for the impact of OYF on life insurance coverage are plotted in Figure 2.4. The pre-period point estimates are very close to zero and are not statistically different from zero at the 5% level, validating the assumption that there were no differential deviations from trend in the pre-period. After OYF, the event study coefficient immediately turns positive, and all post-period coefficients are positive and statistically different from zero at the 5% level. On average, in the first two years post-treatment, there is a 2.4 percentage point increase in life insurance coverage relative to the predicted coverage for that period due to the OYF campaign. For years three and beyond, there is a four to six percentage point increase in coverage attributable to OYF.

2.6.3 Other Planning Behaviors

In addition to affecting insurance purchase, OYF could impact other planning behaviors. I estimate the impact of OYF on whether any children live within 10 miles, the number of children living within 10 miles, total transfers to children, and whether an individual has a will. Living closer to children could allow the surveyed individuals to receive assistance from their children at their own home, allowing them to stay in their home longer. There is evidence that individuals give transfers to children as a form of "pre-payment" for offering assistance in the future, so the campaign may alter gifting behavior (Lopez-Anuarbe, 2013).

Additionally, having a will is a planning behavior that was not directly targeted, but if OYF caused individuals to plan ahead in some areas, it may have had spillovers in leading them to plan ahead in other, less related, areas.

Results from difference-in-differences regressions with these other planning behaviors as the outcome variables are available in Table 2.6. Odd columns include state and year fixed effects and individual demographic controls and even columns add state-specific linear time trends.

Overall, there is little impact on these other observed planning behaviors. The point estimate for the probability of having a child within ten miles is positive, but it is not significant at the 10% level. The only statistically-significant result is an increase in the number of children within 10 miles of 0.06 after OYF, but it is not robust to leaving out the state-specific linear time trends. The estimates for changes in total transfers to children and in the fraction who have a will are both not statistically different from zero at the 10% level, and the point estimate for having a will is very small. Given that these behaviors were not directly targeted by the information campaign, it is perhaps not surprising that little effect is observed.

The corresponding event study plots for these other outcomes can be found in Figure 2.5. In the pre-period, all four outcomes have estimated coefficients that are not different from zero, validating the assumption that planning behaviors of individuals in treated states would have trended similarly to individuals in untreated states in the absence of the information campaign. With the outcomes of any children within 10 miles and having a will in panels (a) and (d), respectively, the event studies show the same zero effect as the difference-in-differences regressions. Looking at panel (b), there is a statistically significant impact of OYF on number of kids for the first six years after OYF. Finally, although the change in total transfers to children in panel (c) appears to be growing over time, this increase is not statistically different from zero at the 10% level.

2.6.4 Expectations for Future

Finally, an information campaign may alter individuals' views about the future. I address this question by estimating the impact of OYF on self-reported probabilities of leaving any bequest, leaving a bequest greater than $100,000, and whether the individuals will enter a nursing home within the next five years.

The results from these regressions are available in Table 2.7. As before, the odd columns have state and year fixed effects and individual-level control variables while even columns add state-specific linear time trends. The estimates show no change in the self-reported probability of leaving *any* bequest but a robust two to three percentage point increase in the probability of leaving a bequest greater than $100,000. This increase is statistically significant at the 1% level and represents an increase of 6% on the average self-reported probability of 46%. The increase in the self-reported probability of leaving a large bequest is consistent with individuals improving their planning for the future in response to the OYF campaign, and in particular with the increase in long-term care insurance and life insurance coverage.

Turning to the third outcome, individuals' self-reported probability of entering a nursing home within the next five years may have slightly increased as a result of the OYF campaign, but this finding is not robust across specifications. In column (6), the regression with the state-specific linear time trends, there is a 0.9 percentage point increase in the self-reported probability of entering a nursing home within five years, representing a 7% increase on a base of 13.3%, and the estimate is statistically significant at the 10% level. However, the point estimate is somewhat attenuated when excluding the state-specific linear time trends and is not statistically significant at the 10% level in this specification.

As discussed in Section 2.3, the impact of OYF on the expectation of entering a nursing home is ambiguous, so the imprecise estimate may reflect either no influence of OYF on the self-reported probability or individuals changing expectations in opposite directions. Additionally, the survey question provides the relatively narrow time window of entering a

nursing home within five years. Expectations over other time windows could have still been affected.

Figure 2.6 plots the corresponding event-study coefficients for the future expectation outcomes. Subfigure (a) confirms the zero result for changes in the probability of leaving any bequest. All coefficients, both before and after OYF, are not statistically different from zero at the 10% level, and the point estimates themselves are very close to zero.

Subfigure (b) of Figure 2.6 provides convincing evidence that the increase in the self-reported probability of leaving a bequest from the difference-in-difference estimation was due to OYF. All coefficients in the pre-period are statistically indistinguishable from zero, validating the assumption that net of the control variables, all units would have trended similarly in the absence of the campaign. Beginning in the year after the OYF campaign, there is a statistically significant increase in the self-reported probability of leaving a bequest greater than $100,000. Point estimates in the post-period range increase from two percentage points in the first two years to five percentage points more than six years after the campaign, and all coefficients are statistically significant at the 5% level.

Event-study coefficients for changes in the self-reported probability of entering a nursing home are presented in subfigure (c) of Figure 2.6. These results tell a less consistent story than the other two outcomes with a strongly positive and statistically-significant increase of over two percentage points in the two latest periods, 5-6 years and 6 or more years after OYF, and no change in earlier time periods. The zero pre-trend assumption is also not satisfied with the last pre-treatment period statistically less than zero, making the results more difficult to interpret since the identification assumption that treatment and control states would have trended similarly in the absence of the campaign may not be satisfied.

2.6.5 Medicaid

Finally, I directly consider the probability of being enrolled in Medicaid as an outcome since reducing the number of Medicaid enrollees is one of the campaign's stated goals. It is likely

a bit early to see an effect on Medicaid since many individuals will enter the Medicaid rolls after spending down assets to pay for long-term care. But if individuals are better managing their assets as a result of OYF, an effect could show up sooner. Survey year 2014, the last survey year included in the sample period, is excluded from all Medicaid regressions since this was the first year of the Medicaid expansion.

Table 2.8 displays the results from this regression. In addition to the interaction between the treatment variable and an indicator for post-treatment, column (1) includes only state and year fixed effects. Column (2) adds individual-alevel control variables, and column (3) adds state-specific linear time trends. The point estimate in column (2) suggests a statistically-significant decline of 0.8 percentage points, or 10%, in the fraction of individuals enrolled in Medicaid after the OYF campaign. However, adding the state-specific linear time trends in column (3) reduces this point estimate to 0.5 percentage points, and it is no longer significant at the 10% level.

The corresponding event-study coefficients are plotted in Figure 2.7. Subfigure (a) plots the coefficients for the regression with state and year fixed effects and individual control variables, while subfigure (b) adds the state-specific linear time trend. These figures provide some insight into why adding the linear time trends attenuated the coefficient. It is important to note that the time trends are estimated using both the pre- and post-periods, and in this case, the trends seem to be capturing that the treatment effect is growing slightly over time instead of capturing true baseline trend differences among the states. The model without trends therefore appears to fit the data better in this case.

In subfigure (a), the model without state trends, the post-period point estimates indicate a decline in Medicaid coverage of about one percentage point, and the linear combination is statistically significant at the 1% level. Although there does appear to be a decline in Medicaid coverage as a result of OYF, the result is noisy. It is also too early to tell whether there will be an impact on long-term care spending from this reduction. If the 10% reduction

in Medicaid coverage is maintained and translates into a 10% reduction in spending on long-term care, it could potentially reduce government spending by several billion dollars annually.

2.6.6 Heterogeneity by Asset Levels

Table 2.9 summarizes the results from the insurance coverage regressions allowing for heterogeneity by asset levels. The odd columns contain state and year fixed effects as well as individual-level control variables, while the even columns add state-specific linear time trends. These regressions show that the majority of the increase in LTCI coverage comes from individuals with high assets (over the 80th percentile), who increase coverage rates by 3.3 percentage points after the information campaign. There is a smaller response among those with medium levels of assets (50th to 80th percentile) of 1.3 percentage points, though the point estimate is not statistically significant at the 10% level. Individuals with below-median levels of assets do not change their LTCI coverage as a result of OYF.

Figure 2.8 plots the estimated event-study coefficients for the event-study regression corresponding to the difference-in-differences regression in the table. Each subfigure plots the coefficients on the interaction term between the given level of assets, the OYF treatment variable, and an indicator for years since treatment. Note that the confidence intervals are largest for the high-asset group, followed by medium because these coefficients are estimated on significantly smaller segments of respondents. Across all three plots, the pre-period coefficients are not statistically different from zero, providing support for the identifying assumption that all units would have trended similarly in the absence of the information campaign.

For individuals with a medium level of assets, there appears to be a small increase in coverage, though the individual point estimates are not statistically significant. The average of the post-period coefficients amounts a two percentage point increase in coverage, and the linear combination is statistically significant at the 10% level. The high-asset plot in subfigure (a) shows a much larger jump in coverage in response to OYF. Again, most of the

individual point estimates are not significant at the 5% level, but jointly, they have a mean of four percentage points, and the linear combination is statistically different from zero at the 10% level. In the post-period, subfigure (c) clearly shows zero impact of OYF on LTCI coverage for individuals with below-median levels of assets, with all point estimates very close to zero and not statistically different from zero.

On the other hand, individuals with below median levels of assets do significantly increase their life insurance coverage, as seen in columns (3) and (4) of Table 2.9. In the specification with the state-specific linear time trends, individuals in this lowest asset category increased life insurance coverage by 4.7 percentage points, a 7% increase, and this increase is significant at the 1% level. Individuals with high asset levels also increase life insurance purchase, perhaps reflecting its ability to provide a substitute for long-term care insurance. In the specification with state-specific linear time trends in column (4), they increase coverage by 3.6 percentage points. No response is estimated for individuals with medium levels of assets.

2.6.7 Back-of-the-envelope Medicaid Savings Calculation

One of the stated goals of the OYF campaign was to reduce Medicaid expenditures. Therefore, in this section, I estimate the expected Medicaid savings from the increase in LTCI coverage induced by OYF. The calculation uses the estimates from this paper on the response of LTCI coverage to OYF and estimates from the literature on the effect of LTCI on Medicaid spending. Note that the analysis here only considers reductions in Medicaid expenditures due to the increase in LTCI coverage. Purchasing LTCI is just one of many possible planning behaviors that could result from OYF. Therefore, this estimate should be seen as a lower bound.[12]

The prior literature has estimated the share of the expected present discounted value (EPDV) of long-term care expenditures by gender, wealth decile, and LTCI coverage that

[12]The exception to the estimated Medicaid savings being a lower bound would be if, in response to the campaign, individuals made plans to shed assets in order to qualify for Medicaid. This particular planning behavior was not one suggested by the campaign, but it is a possible, though perhaps unlikely, response.

can be expected to fall to Medicaid (Brown and Finkelstein, 2008). I will use the EPDV estimates for men and women from Goda (2011) of $17,510 and $43,750, respectively, in order to turn these shares into dollars saved.

Since the literature estimates these Medicaid costs by wealth deciles, I begin by rerunning the difference-in-differences asset heterogeneity models replacing the "Low, Medium, High" asset designations with asset decile groups. The results are shown in Table 2.10. Point estimates of the impact of OYF on LTCI coverage are very close to zero through the 60th percentile of the wealth. For individuals in the 60th to 70th percentiles, it is estimated that OYF increased LTCI coverage by 1.45 percentage points, though the estimate is not statistically significant at the 10% level. Similarly, the estimates for 70th to 80th and 80th to 90th are 2.46pp and 1.48pp, respectively, though again these are not statistically significant at the 10% level. Finally, for individuals in the top 10% of the wealth distribution, it is estimated that OYF increased LTCI take up by 5.08pp, and this is statistically significant at the 5% level.

Now these estimates are the *average* causal impact of OYF in each wealth decile. The prior work estimated share of EPDV borne by Medicaid *at* wealth deciles. Because these shares are fairly linear through the deciles, I will average adjacent deciles in order to estimate the *average* Medicaid share within each decile. I assume that at the 100th percentile, zero costs would be expected to be paid by Medicaid regardless of whether the individual has LTCI. For the 0th percentile, I extend the linear trend. The resulting shares are shown in Table [TBD]. Multiplying these shares by the EPDV and subtracting the amount expected to be paid by Medicaid when an individual has LTCI from the amount expected to be paid by Medicaid when they do not have LTCI yields the total dollars saved per new enrollee.

These rates are estimated separately for men and women and are also for individuals at age 65. Therefore, I estimate models for heterogeneity in the impact of OYF by gender and by age greater than or less than 65. Table 2.12 presents the results of these regressions. Odd columns include state and year fixed effects and individual level control variables. Even

columns add state-specific linear time trends. The point estimates for women in columns (1) and (2) are larger than for men and are statistically significant at the 10% and 5% levels, respectively, while the male coefficients are not. However, there is not enough power to reject that the coefficients are equal, as seen by the F-statistic on the test for equality of 0.46 (p=0.50). Therefore, for the back-of-the-envelope calculation, I assume that men and women have the same response to OYF. Note that this is a conservative assumption for calculating Medicaid savings because LTCI coverage for a woman saves Medicaid more on average than LTCI coverage for a man.

Looking at columns (3) and (4), which estimate the impact of OYF on LTCI coverage for individuals younger versus older than 65, here there is again no statistically-significant difference in OYF response between the two groups, with an F-statistic of 0.069 (p-value=0.79). Since responses to OYF were similar before and after 65, I assume that the impact at age 65 is the same as the average impact from the difference-in-differences regression.

Next, I use these coefficients to estimate the total number of new enrollees. Governors across OYF states sent out 18 million letters total as part of the campaign. In theory, these letters covered all citizens in the targeted 20-year age band. Assuming these letters were split evenly across deciles, there were 1.8 million letters sent per decile. Multiplying 1.8 million letters by the treatment effects yields the total number of new enrollees. With the additional assumption that 54% of these are women, which is the share in my sample, allows me to split the number of new enrollees by gender.

Specifically, in order to estimate the Medicaid savings from the increase in LTCI coverage, I calculate:

$$PV(Savings) = \sum_{F} \sum_{\alpha} [1,800,000 * \beta_{\alpha} * \xi_{F} * (M_{\alpha,F,LTCI=1} - M_{\alpha,F,LTCI=0}) * EPDV_{F}] \tag{2.4}$$

Where F is an indicator that equals one for female and zero for male and α is the set of asset deciles. The treatment coefficients are β_{α} and ξ_{F} is the fraction of individuals age 65 of

gender F (=0.54 for women). The proportion of EPDV of long-term care expenses expected to be paid by Medicaid for an individual with gender F in asset group α is $M_{\alpha,F,LTCI=1}$ if they have private LTCI coverage and $M_{\alpha,F,LTCI=0}$ otherwise. Finally, $EPDV_F$ is the expected present discounted value of long-term care expenditures for gender F at age 65.

So then $1,800,000 * \beta_\alpha$ is the number of letters sent to individuals in a given decile times the increased LTCI take-up by individuals in asset decile α, so it is the total number of new enrollees in decile α. Multiplying by ξ_F yields the number of new enrollees of gender F in asset decile α. This is multiplied by the predicted savings per enrollee of gender F from asset category α, which is the expected present discounted value of care paid for by all sources ($EPDV_F$) times the reduction in the fraction paid by Medicaid ($M_{\alpha,F,LTCI=1} - M_{\alpha,F,LTCI=0}$).

Because the coefficients for the first six asset deciles are essentially zero, I do not include them in the calculation. Summing the savings across the remaining four deciles yields an estimated present value savings of $417 million, which is a savings of $23 per letter sent. Including only savings from increased coverage at the top decile of the asset distribution since it is the only one that is significant at the 10% level would yield a savings of $72 million, which is still a savings of $4 per letter.

At a total cost of $15 million from the federal government with some additional state spending, I estimate significant savings to the government from the campaign just from the increase in LTCI coverage without considering changes in other planning behaviors. These estimated savings from OYF stand in sharp contrast to findings from the work on long-term care insurance tax subsidies and the Partnership program, which are estimated to be a net cost to the government (Goda 2011; Lin and Prince 2013).

2.7 Discussion and conclusion

This research builds on a literature working to understand why the take-up of long-term care insurance is so low, given the large risk long-term care presents for the elderly. I find that the "Own Your Future" information campaign increased take-up of long-term care insurance

by 1.4 percentage points, with the strongest response among those with assets above the 80th percentile, who increased take up by three to four percentage points. Using the price elasticity of demand for LTCI estimated by Goda (2011), the information campaign had the same impact as a reduction in LTCI premiums of 4.4%.

The identification for this result comes from the staggered roll out of the OYF campaign, where four to six states participating in the campaign each year for five years and about half the states remaining untreated. I use both the variation in whether a state participates in the campaign at all and in the year of participation among the treated states.

I also find that individuals are using other means of insuring or partially insuring themselves against the long-term care risk, particularly by purchasing life insurance as a partial substitute. I estimate that the OYF campaign increased life insurance coverage by three percentage points. I also find evidence that individuals are finding ways to increase their level of assets, as seen by an increase in the self-reported probability of leaving a bequest of more than $100,000 and a reduction in the fraction of individuals covered by Medicaid. There is also some tentative evidence that individuals exposed to the campaign prepared for long-term care by increasing their number of adult children living nearby, either by the respondent or the children relocating.

This paper shows that information campaigns may be a cost-effective way to encourage individuals to plan ahead for long-term care. I estimate that the increase in long-term care insurance coverage alone would be expected to provide $417 million in present value Medicaid savings. Although this effect is modest, it significantly exceeds the cost of the campaign and should be seen as a lower bound since it only takes into account one specific planning behavior. The information campaign may also have corrected suboptimal levels of formal and informal insurance against the long-term care risk, a benefit that is not quantified here.

Table 2.1: "Own Your Future" Campaign States

State	Phase	Year
Arkansas	1	2005
Colorado	5	2009
District of Columbia	5	2009
Georgia	3	2007
Idaho	1	2005
Iowa	5	2009
Kansas	2	2006
Kentucky	5	2009
Maryland	2	2006
Michigan	3	2006
Missouri	3	2007
Nebraska	3	2006
Nevada	1	2005
New Jersey	1	2005
North Dakota	4	2008
Ohio	4	2008
Oklahoma	5	2009
Pennsylvania	4	2008
Rhode Island	2	2006
South Dakota	3	2006
Tennessee	3	2007
Texas	3	2006
Virginia	1	2005
Washington	2	2006

Notes: Table lists states participating in the "Own Your Future" campaign by phase and year of campaign launch. Information was gathered from press releases and state campaign websites using the Wayback Machine. States not listed did not participate in the "Own Your Future" campaign.

Table 2.2: Summary Statistics for OYF v. non-OYF states

Variable	Non-OYF States	OYF States	Difference
Income (thousands)	8.06	8.21	0.150
	(0.14)	(0.21)	(0.252)
Assets (thousands)	57.26	51.65	-5.610***
	(0.74)	(0.60)	(0.955)
Age	64.42	64.27	-0.150***
	(0.04)	(0.04)	(0.053)
Female	0.54	0.54	-0.002
	(0.00)	(0.00)	(0.004)
Married	0.66	0.68	0.018***
	(0.00)	(0.00)	(0.004)
Less than high school	0.18	0.19	0.009***
	(0.00)	(0.00)	(0.003)
High School	0.32	0.33	0.008**
	(0.00)	(0.00)	(0.003)
Some college	0.24	0.23	-0.010***
	(0.00)	(0.00)	(0.003)
College or more	0.27	0.26	-0.008**
	(0.00)	(0.00)	(0.003)
Num. Living Chldren	3.02	2.99	-0.027*
	(0.01)	(0.01)	(0.014)
Hispanic	0.08	0.07	-0.013***
	(0.00)	(0.00)	(0.002)
Black	0.10	0.10	-0.003
	(0.00)	(0.00)	(0.002)
Any ADLs	0.13	0.12	-0.009***
	(0.00)	(0.00)	(0.002)
N	60979	45686	106665

Notes: "OYF" states are states the implement an OYF campaign at some point from 2005 to 2009. "Non-OYF" states do not implement a campaign during this period. Income and assets are reported in 2010 dollars. Observations are at the person-year level, and means are weighted using the HRS-provided person weights. Standard errors are in parentheses.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 2.3: LTCI and Life Insurance Coverage by Demographic Groups

	LTCI Coverage	N	Life Insurance Coverage	N
All	12.4%	104,931	66.9%	105,546
Gender				
Male	12.1%	44,907	71.4%	45,155
Female	12.6%	60,024	63.2%	60,391
Age				
Younger than 65	10.3%	45,357	70.9%	45,770
65 or older	14.7%	59,574	62.4%	59,776
Education				
High school or less	8.4%	58,977	63.3%	59,365
Some College or more	16.4%	45,954	70.6%	46,181
Asset Level				
Low Assets	6.3%	52,467	63.2%	52,796
Medium Assets	14.0%	31,476	71.7%	31,676
High Assets	22.9%	20,988	68.3%	21,074
Marital status				
Married	13.6%	69,249	71.4%	69,688
Not married	9.9%	35,682	58.0%	35,858
ADL limitations				
None	13.0%	90,230	68.3%	90,700
One or more	7.9%	14,701	57.5%	14,846

Notes: Table provides mean coverage rates for LTCI and life insurance by demographic groups weighted using the HRS-provided person weights. The number of observations varies across these two outcomes because response rates differ slightly. Observations are at the person-year level.

Table 2.4: Difference-in-Differences Estimates on LTCI Coverage

	(1)	(2)	(3)
OYF*Post	0.0185**	0.0120*	0.0136**
	(0.00731)	(0.00628)	(0.00635)
Mean	.121	.121	.121
Control variables		X	X
State linear trends			X
Observations	104931	104931	104931

Notes: Table displays coefficient β from the difference-in-differences regression equation (2.1) in the text: $Y_{it} = \beta * OYF_i * Post_t + \gamma * X_{it} + \sigma_s + \eta_t + \sigma_s * t + \epsilon_{ist}$. Dependent variable is an indicator for whether an individual has long-term care insurance coverage in period t. Columns with control variables include individual control variables, X_{it}. Column with state-specific linear time trends include the $\sigma_s * t$ term. All regressions use individual-level weights. Standard errors are clustered at the state level.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 2.5: Difference-in-Differences Estimates on Life Insurance Coverage

	(1)	(2)	(3)
OYF*Post	0.0224**	0.0187**	0.0316***
	(0.00920)	(0.00818)	(0.0107)
Mean	.653	.653	.653
Control variables		X	X
State linear trends			X
Observations	104068	104068	104068

Notes: Table displays coefficient β from the difference-in-difference regression equation (2.1) in the text: $Y_{it} = \beta * OYF_i * Post_t + \gamma * X_{it} + \sigma_s + \eta_t + \sigma_s * t + \epsilon_{ist}$. Dependent variable is an indicator for whether an individual has life insurance coverage in period t. Columns with control variables include individual control variables, X_{it}. Column with state-specific linear time trends include the $\sigma_s * t$ term. All regressions use individual-level weights. Standard errors are clustered at the state level.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 2.6: Difference-in-Differences Estimates on Other Planning Behaviors

	Any Kids w/in 10 miles		Num. Kids w/in 10 miles		Transfers to Kids ($s)		Have a will	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
OYF*Post	0.00534	0.0154	0.0303	0.0595**	107.6	269.5	-0.0000127	0.00245
	(0.00992)	(0.0141)	(0.0219)	(0.0276)	(356.7)	(372.7)	(0.00749)	(0.00974)
Mean	.621	.621	.982	.982	4093.567	4093.567	.526	.526
Control variables	X	X	X	X	X	X	X	X
State linear trends		X		X		X		X
Observations	104931	104931	92007	92007	95887	95887	89386	89386

Notes: Table displays coefficient β from the difference-in-difference regression equation (2.1) in the text: $Y_{it} = \beta * OYF_i * Post_t + \gamma * X_{it} + \sigma_s + \eta_t + \sigma_s * t + \epsilon_{ist}$. Columns with control variables include individual control variables, X_{it}. Columns with state-specific linear time trends include the $\sigma_s * t$ term. Outcome of interest in columns (1) and (2) is a binary variable that equals one if the respondent has any children living within 10 miles, and in columns (3) and (4), the outcome is the number of children living within 10 miles. Outcome in columns (5) and (6) is the total dollar amount of transfers to all children. In columns (7) and (8), the outcome is a binary variable that equals one if the respondent has a will. All regressions use individual-level weights. Standard errors are clustered at the state level.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 2.7: Difference-in-Differences Estimates on Expectations for Future

	Any Bequest		Bequest $> \$100k$		Nursing Home in 5 years	
	(1)	(2)	(3)	(4)	(5)	(6)
OYF*Post	0.00461	0.00448	0.0222**	0.0292***	0.00730	0.00948*
	(0.0112)	(0.0186)	(0.00910)	(0.0104)	(0.00447)	(0.00486)
Mean	.771	.771	.457	.457	.133	.133
Control variables	X	X	X	X	X	X
State linear trends		X		X		X
Observations	55075	55075	94484	94484	52052	52052

Notes: All outcomes are expressed as self-reported probabilities of the event occuring on a scale from 0 to 1. Table displays coefficient β from the difference-in-difference regression equation (2.1) in the text: $Y_{it} = \beta * OYF_i * Post_t + \gamma * X_{it} + \sigma_s + \eta_t + \sigma_s * t + \epsilon_{ist}$. Columns with control variables include individual control variables, X_{it}. Columns with state-specific linear time trends include the $\sigma_s * t$ term. All regressions use individual-level weights. Standard errors are clustered at the state level.

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

Table 2.8: Difference-in-Differences Estimates on Medicaid Coverage

	(1)	(2)	(3)
OYF*Post	-0.0163***	-0.00797**	-0.00513
	(0.00345)	(0.00359)	(0.00448)
Mean	.078	.078	.078
Control variables		X	X
State linear trends			X
Observations	94531	94531	94531

Notes: Table displays coefficient β from the difference-in-difference regression equation (2.1) in the text: $Y_{it} = \beta * OYF_i * Post_t + \gamma * X_{it} + \sigma_s + \eta_t + \sigma_s * t + \epsilon_{ist}$. Dependent variable is an indicator for whether an individual is covered by Medicaid in period t. Columns with control variables include individual control variables, X_{it}. Column with state-specific linear time trends include the $\sigma_s * t$ term. All regressions use individual-level weights. Standard errors are clustered at the state level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 2.9: Heterogeneity in Effect on Insurance Coverage

	Long-term Care Insurance		Life Insurance	
	(1)	(2)	(3)	(4)
OYF*Post*Low_Assets	-0.000354	-0.000123	0.0348***	0.0468***
	(0.00602)	(0.00844)	(0.00847)	(0.0115)
OYF*Post*Medium_Assets	0.0125	0.0131	-0.0103	0.00216
	(0.00934)	(0.00871)	(0.0147)	(0.0169)
OYF*Post*High_Assets	0.0334	0.0337*	0.0234	0.0359**
	(0.0205)	(0.0197)	(0.0155)	(0.0152)
Mean	.121	.121	.653	.653
Control variables	X	X	X	X
State linear trends		X		X
Observations	104931	104931	104068	104068

Notes: Table displays coefficients β from the difference-in-difference regression equation (2.3) in the text. Columns with control variables include individual control variables, X_{it}. Columns with state-specific linear time trends include the $\sigma_s * t$ term. Outcome of interest is a binary variable that equals one if the respondent is covered by a long-term care insurance policy for columns (1) and (2) and life insurance for columns (3) and (4). All regressions use individual-level weights. Standard errors are clustered at the state level.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 2.10: Heterogeneity in Effect on Insurance Coverage (Deciles)

	Long-term Care Insurance		Life Insurance	
	(1)	(2)	(3)	(4)
OYF*Post*0th-10th	0.000160	-0.000900	0.0561**	0.0666**
	(0.0155)	(0.0188)	(0.0253)	(0.0274)
OYF*Post*10th-20th	-0.000868	-0.00127	0.0141	0.0246
	(0.00899)	(0.0104)	(0.0167)	(0.0167)
OYF*Post*20th-30th	-0.00161	-0.00115	0.0329*	0.0440**
	(0.00983)	(0.0116)	(0.0168)	(0.0175)
OYF*Post*30th-40th	-0.00249	-0.00197	0.0301	0.0419**
	(0.00753)	(0.00877)	(0.0180)	(0.0194)
OYF*Post*40th-50th	0.00162	0.000684	0.0288*	0.0408**
	(0.0126)	(0.0121)	(0.0160)	(0.0177)
OYF*Post*50th-60th	-0.00289	-0.00237	-0.0126	-0.00143
	(0.0126)	(0.0116)	(0.0225)	(0.0254)
OYF*Post*60th-70th	0.0146	0.0145	-0.0102	0.00147
	(0.0145)	(0.0157)	(0.0168)	(0.0157)
OYF*Post*70th-80th	0.0247	0.0246	-0.0167	-0.00562
	(0.0172)	(0.0159)	(0.0227)	(0.0243)
OYF*Post*80th-90th	0.0158	0.0148	0.00566	0.0175
	(0.0287)	(0.0281)	(0.0181)	(0.0183)
OYF*Post*90th-100th	0.0502**	0.0508**	0.0386*	0.0497***
	(0.0244)	(0.0241)	(0.0195)	(0.0185)
Mean	.121	.121	.653	.653
Control variables	X	X	X	X
State linear trends		X		X
Observations	104931	104931	104068	104068

Notes: Table displays coefficients β from the difference-in-difference regression equation (2.3) in the text with the "high, medium, low" asset categories replaced by asset deciles. Columns with control variables include individual control variables, X_{it}. Columns with state-specific linear time trends include the $\sigma_s * t$ term. Outcome of interest is a binary variable that equals one if the respondent is covered by a long-term care insurance policy for columns (1) and (2) and life insurance for columns (3) and (4). All regressions use individual-level weights. Standard errors are clustered at the state level.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 2.11: Medicaid Share of Expected Present Discounted Value of LTC Expenditures

Wealth decile	Men		Women	
	No LTCI	With LTCI	No LTCI	With LTCI
10th	0.99	0.55	1.00	0.30
20th	0.94	0.48	0.96	0.53
30th	0.85	0.43	0.91	0.48
40th	0.76	0.39	0.84	0.45
50th	0.66	0.35	0.76	0.41
60th	0.53	0.29	0.66	0.36
70th	0.39	0.23	0.53	0.29
80th	0.25	0.16	0.35	0.20
90th	0.12	0.09	0.16	0.11
100th	0.04	0.03	0.04	0.03

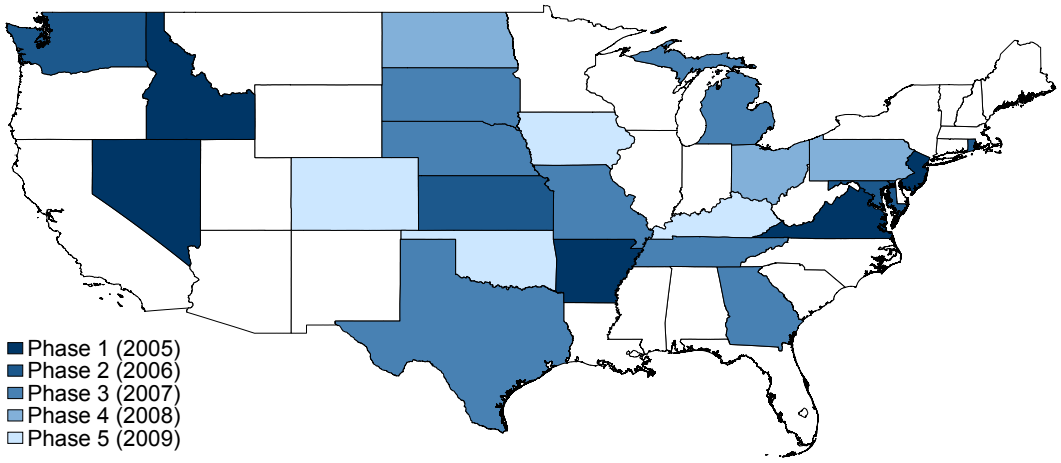
Notes: Table displays share of total future long-term care expenditures that would be expected to be paid by Medicaid by gender, wealth decile, and whether the individual has LTCI at age 65. Shares come from calculations using shares from Brown and Finkelstein (2008). Brown and Finkelstein (2008) calculate expected shares *at* given deciles. I linearly interpolate to provide the average expected shares *within* deciles. For more detail, see Section 2.6.7 of the text.

Table 2.12: Effect on LTCI Coverage: Heterogeneity by Gender and Age

	(1)	(2)	(3)	(4)
OYF*Male*Post	0.00936	0.0110		
	(0.00686)	(0.00660)		
OYF*Female*Post	0.0143*	0.0158**		
	(0.00752)	(0.00780)		
OYF*Age <65*Post			0.0105	0.0119
			(0.00868)	(0.00812)
OYF*Age >=65*Post			0.0133	0.0150
			(0.00845)	(0.00927)
Mean	.121	.121	.121	.121
Control variables	X	X	X	X
State linear trends		X		X
F-stat for equality	.46	.46	.059	.069
p-value	.5	.5	.81	.79
Observations	104931	104931	104931	104931

Notes: Table displays coefficients β from the difference-in-difference regression equation (2.3) in the text with the "high, medium, low" asset categories replaced by gender for columns (1) and (2) and age bins for columns (3) and (4). Columns with control variables include individual control variables, X_{it}. Columns with state-specific linear time trends include the $\sigma_s * t$ term. Outcome of interest is a binary variable that equals one if the respondent is covered by a long-term care insurance policy. All regressions use individual-level weights. Standard errors are clustered at the state level.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Figure 2.1: "Own Your Future" Campaign States



Notes: Map displays states participating in the "Own Your Future" campaign by phase and year of campaign launch. Information was gathered from press releases and state campaign websites using the Wayback Machine.

Figure 2.2: Example "Own Your Future" Campaign letter



COMMONWEALTH of VIRGINIA

Office of the Governor

Mark R. Warner
Governor

Dear Fellow Virginian:

I am writing you about a very important issue: planning for your future long term care needs.

Many people, myself included, do not like to think about getting older, or the possibility that at some time in the future we may not be able to perform such routine tasks as bathing and dressing.

But the hard truth is that most of us will need some type of long term care in our later years. While this is a normal part of aging, the tragedy is that many of us don't plan for it adequately. And when we need long term care, it becomes a crisis for us and for our families.

That is why I want to tell you about a new program that we here in Virginia and the federal government are working on together called: "*Own Your Future.*" The goal of the program is to encourage persons between the ages of 50 and 70 to start planning ahead for their future long term care needs now.

As a first step in the planning process, I encourage you to order the "Long Term Care Planning Tool Kit" created by the U.S. Department of Health and Human Services. You can do so either by calling 1-866-PLAN-LTC (1-866-752-6582) or returning the postage paid reply card attached to the enclosed brochure.

Long term care needs are best met when they are planned for ahead of time. So please "*Own Your Future*" and order your free "Long Term Care Planning Tool Kit" today!

Sincerely,

Mark R. Warner

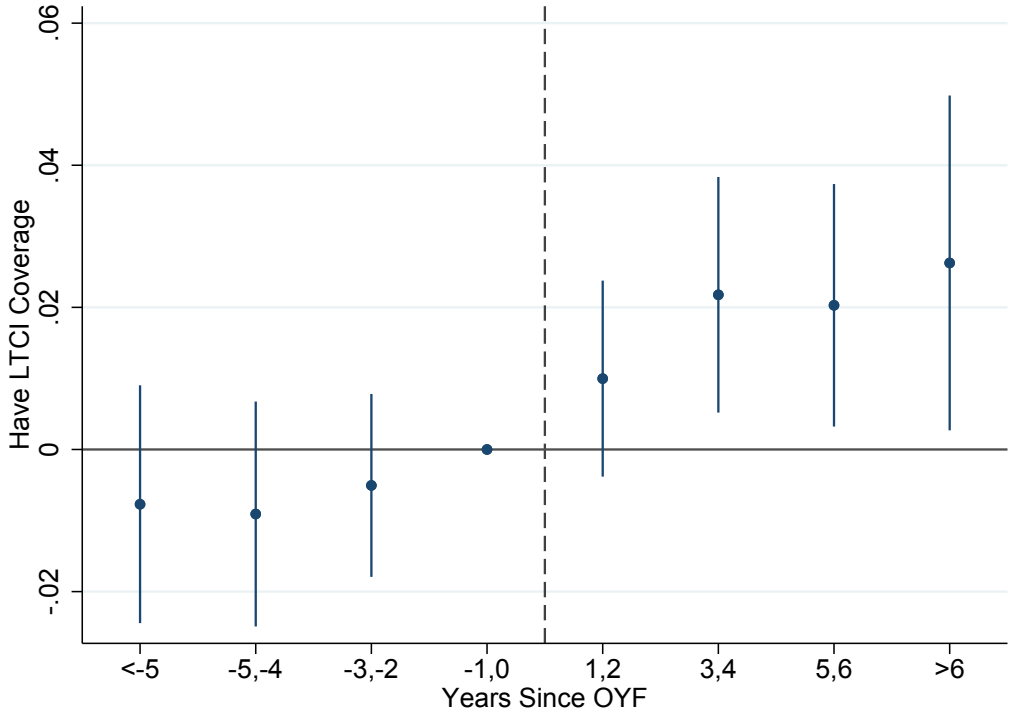
Mark R. Warner

VA/LG

State Capitol • Richmond, Virginia 23219 • (804) 786-2211 • TTY (804) 371-8015 • www.governor.virginia.gov

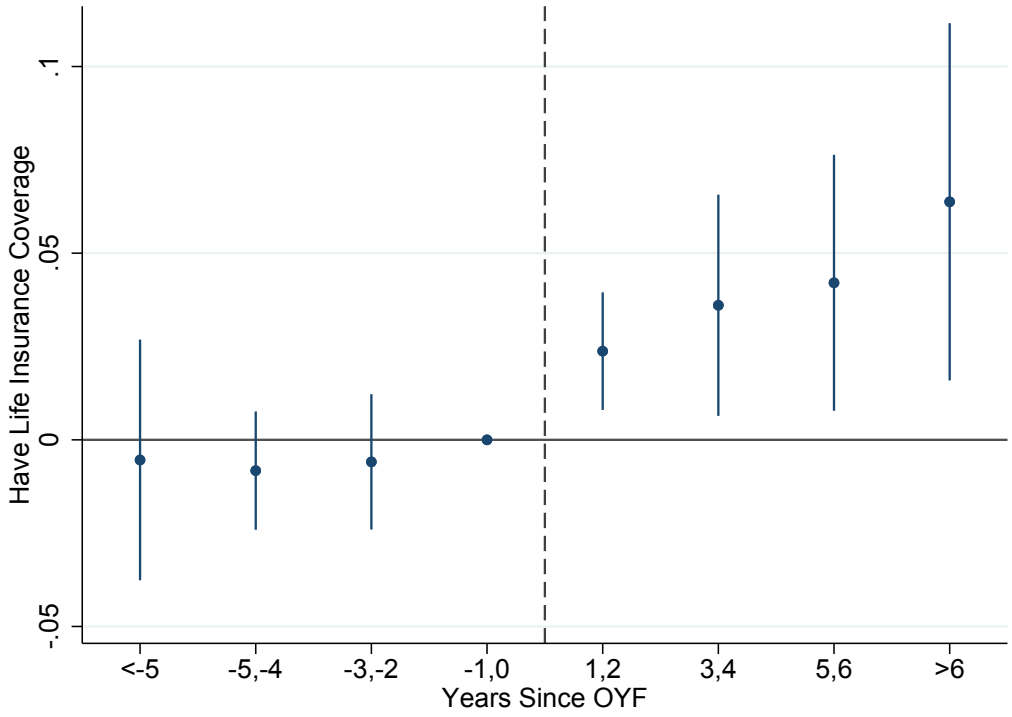
Notes: Figure provides an example "Own Your Future" campaign letter from the Commonwealth of Virginia. Letter was accessed with the Wayback Machine.

Figure 2.3: Event-study coefficients for LTCI Coverage



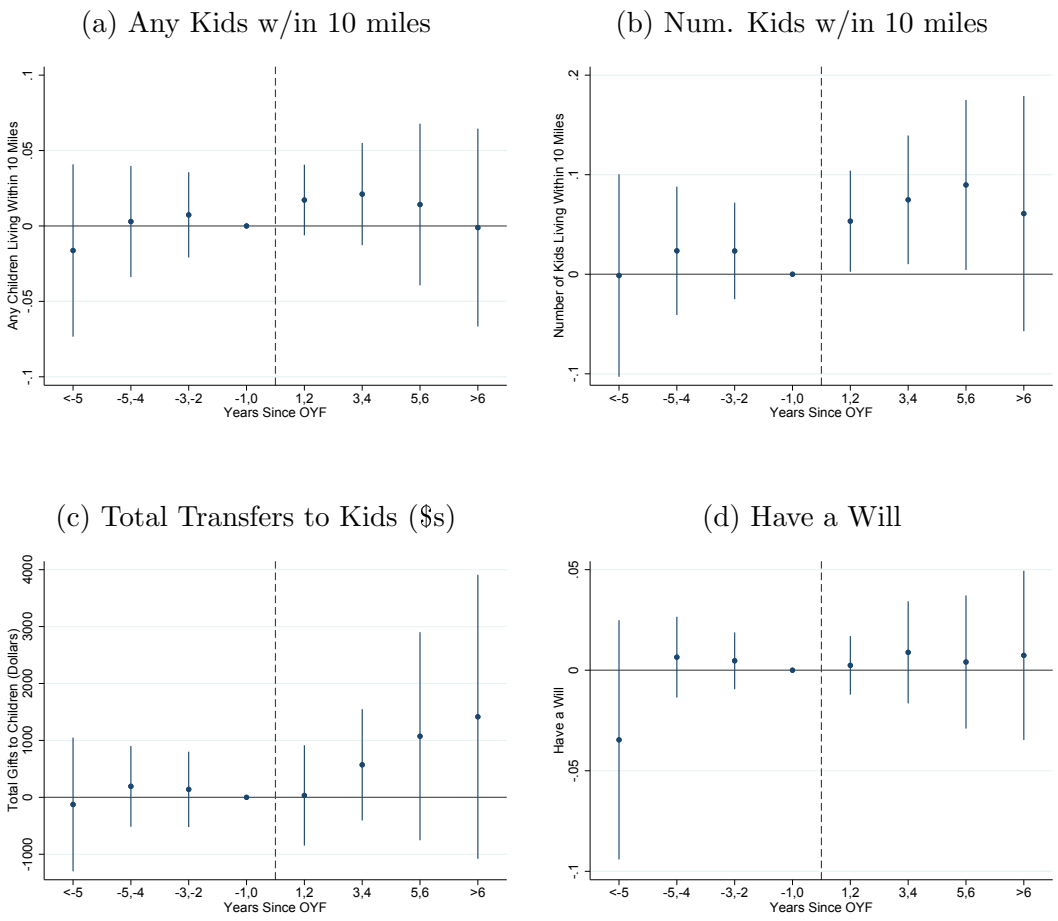
Notes: Figure plots event-study coefficients from equation (2.2), a regression of LTCI coverage on dummies for pooled years since OYF listed on the x-axis. Outcome is an indicator variable that equals one if the individual has LTCI coverage in that period and is zero otherwise. Years are pooled into two-year bands because the HRS is biannual but the five OYF campaign phases occur in five consecutive years. Regression also controls for state fixed effects, year fixed effects, and state-specific linear time trends and uses indvidual-level weights. Error bars represent the 95% confidence intervals, and standard errors are clustered at the state level.

Figure 2.4: Event-study coefficients for Life Insurance Coverage



Notes: Figure plots event-study coefficients from equation (2.2), a regression of life insurance coverage on dummies for pooled years since OYF listed on the x-axis. Outcome is an indicator variable that equals one if the individual has life insurance coverage in that period and is zero otherwise. Years are pooled into two-year bands because the HRS is biannual but the five OYF campaign phases occur in five consecutive years. Regression also controls for state fixed effects, year fixed effects, and state-specific linear time trends and uses indvidual-level weights. Error bars represent the 95% confidence intervals, and standard errors are clustered at the state level.

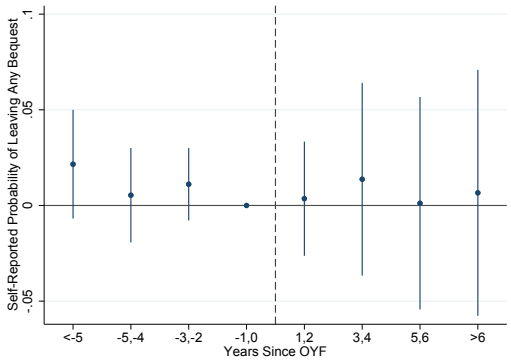
Figure 2.5: Event-study coefficients for Other Outcomes



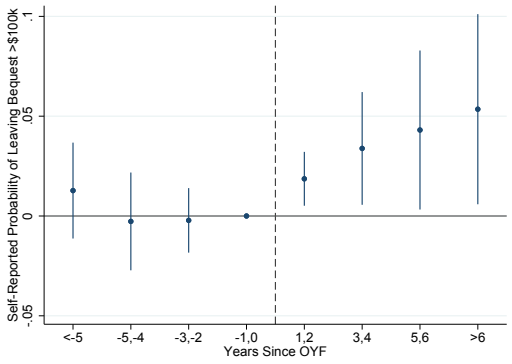
Notes: Figures plot event-study coefficients from equation (2.2), a regression of the outcome variable on dummies for pooled years since OYF listed on the x-axis. Years are pooled into two-year bands because the HRS is biannual but the five OYF campaign phases occur in five consecutive years. Regression also controls for state fixed effects, year fixed effects, and state-specific linear time trends and uses indvidual-level weights. Error bars represent the 95% confidence intervals, and standard errors are clustered at the state level.

Figure 2.6: Event-study coefficients for Expectations

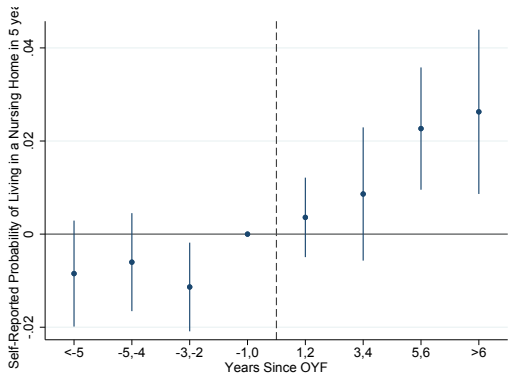
(a) Leave Any Bequest



(b) Leave Bequest >$100k

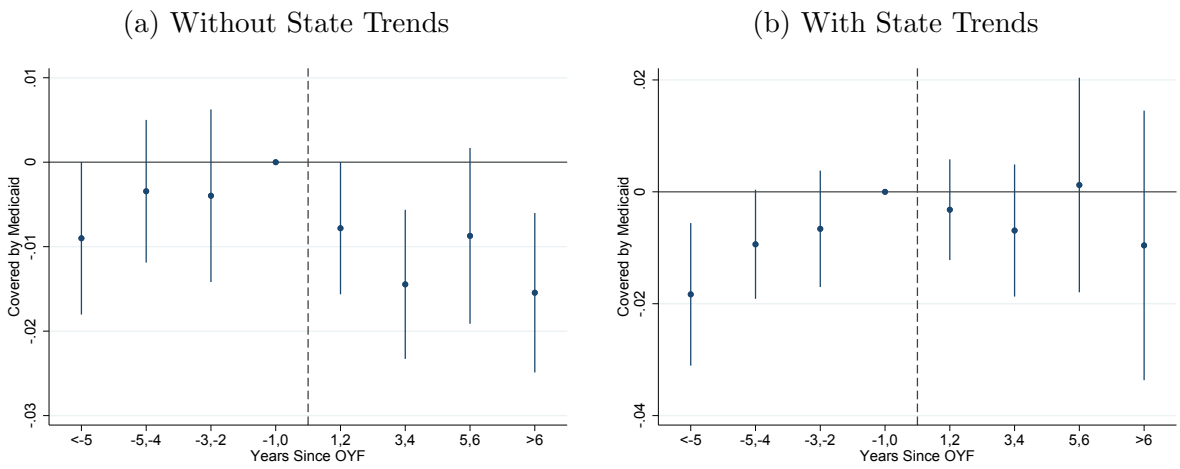


(c) Move to Nursing Home within 5 years



Notes: Figures plot event-study coefficients from a regression equation (2.2), of the outcome variable on dummies for pooled years since OYF listed on the x-axis. Outcome variables are self-reported probabilities measured on a scale from 0 to 1. Years are pooled into two-year bands because the HRS is biannual but the five OYF campaign phases occur in five consecutive years. Regression also controls for state fixed effects, year fixed effects, and state-specific linear time trends and uses indvidual-level weights. Error bars represent the 95% confidence intervals, and standard errors are clustered at the state level.

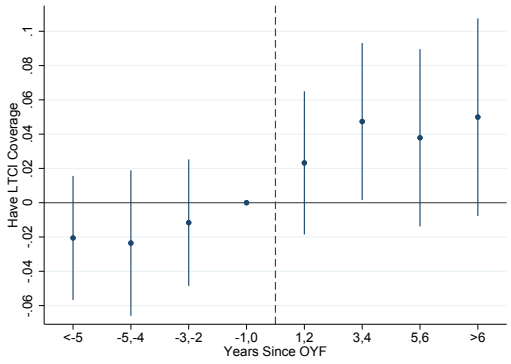
Figure 2.7: Event-study coefficients for Medicaid



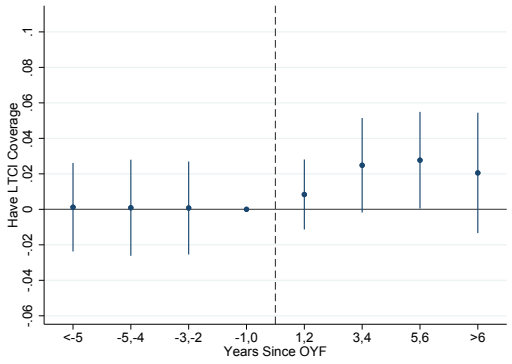
Notes: Figures plot event-study coefficients from equation (2.2), a regression of an indicator for whether the individual is covered by Medicaid on dummies for pooled years since OYF listed on the x-axis. Subfigure (a) includes state and year fixed effects and individual control variables, and subfigure (b) adds state-specific linear time trends. Years are pooled into two-year bands because the HRS is biannual but the five OYF campaign phases occur in five consecutive years. All regressions include individual-level weights. Error bars represent the 95% confidence intervals, and standard errors are clustered at the state level.

Figure 2.8: Event-study coefficients for LTCI Coverage: Heterogeneity by Assets

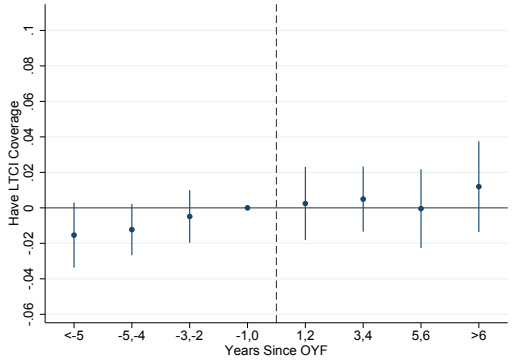
(a) High (>80th percentile)



(b) Medium (50th-80th percentiles)



(c) Low (<50th percentile)



Notes: Figures plot event-study coefficients from equation (2.2), a regression of of LTCI coverage on an interaction between the designated asset level and indicator variables for the pooled years since OYF listed on the x-axis. All coefficients are from the same regression. Years are pooled in two-year bands because the HRS is a biannual survey but the five OYF campaign phases occur in five consecutive years. Regression also controls for state fixed effects, year fixed effects, and state-specific linear time trends and uses indvidual-level weights Error bars represent the 95% confidence intervals, and standard errors are clustered at the state level.

Chapter 3

Mandated Benefits, Imperfect Information, and Moral Hazard: A Model and Application to Temporary Disability Insurance [1]

3.1 Introduction

In the United States, Social Security Disability Insurance (SSDI) provides protection for workers in the event that a long-term disability prevents them from working. However, SSDI is only for workers expected to be disabled for at least a year, and payments do not begin until the worker has been out of the labor force for five months. Workers are therefore still exposed to the significant risk of five months to a year of lost wages. To mitigate this risk, five states (California, New Jersey, New York, Rhode Island, and Hawaii) ensure workers are covered by Temporary Disability Insurance (TDI) to replace lost wages during the first five (or more) months of disability either through an employer mandate or public provision with a dedicated benefit tax.

If employees value these benefits, then deadweight loss will be lower than for an equivalent increase in the payroll tax, as first laid out in Summers (1989). This is because when employees value a new benefit, their labor supply curve shifts, decreasing the employment

[1] I am grateful to Alan Krueger for his advice throughout this project. I am also grateful for helpful comments from Orley Ashenfelter, Steve Levitt, Alex Mas, Steve Mello, and seminar participants at Princeton University. All errors are my own.

loss from the mandate or tax. If workers fully value the benefit, wages are reduced by the cost of the benefit and there is no change in employment, and thus the benefit is provided with no deadweight loss. However, in order for workers to value a benefit, they must know about it. Without knowledge of the benefit, the mandated benefit is equivalent to a payroll tax.

In the case of TDI, the program will still provide effective insurance coverage and consumption smoothing to workers who do not know about it ex ante because they will learn about it when they suffer a disability preventing them from working. Three of the five states (California, New Jersey, and New York) legally require such a notification on the part of the employer. In the other states, this may happen less formally without the legal requirement. For example, the Rhode Island TDI Progress Report from 2007 tells the story of a man who was diagnosed with lung cancer, saying, "I checked with my employer to see if they had any type of short-term disability, and they reminded me that I have been paying into the TDI program; therefore, I should apply." If employees like this one do not know about the benefit until they need to use it, there is less opportunity for moral hazard in the application benefits, which would reduce program cost, potentially lowering deadweight loss.

Thus, when an individual does not know about the TDI benefit, it causes two effects moving in opposite directions: his labor supply curve does not shift, increasing the deadweight loss, and there is less moral hazard in the application of benefits, decreasing program costs and decreasing deadweight loss. It is this tradeoff that I will study in more detail in this paper, with the goal of answering the question of whether an information campaign informing individuals of their benefits would increase employment.

This paper contributes to and combines two main strands of the literature. The first is that on mandated benefits, a literature begun by Summers (1989). I begin with his model of mandated benefits and expand it to allow for workers to be unaware of their benefits, tying the literature on salience and perception of taxes and benefits into that on mandated benefits. In his paper on female labor supply, Rosen (1976) was the first to write about perception

of taxes. In the paper, the husband's marginal tax on his last dollar of earnings is treated as the wife's marginal tax on her first dollar of earnings, but he introduces a "coefficient of tax perception", allowing the couple to imperfectly account for the actual marginal tax rate. Solon (1985) applied this idea to the perception of the tax rate on unemployment insurance benefits. By using a coefficient of tax perception, the authors allow for less than perfect information about taxes. Benefits can be thought of as negative taxes, and I contribute to the literature by applying this concept of imperfect information about taxes and benefits to the mandated benefits model. I take it a step further by asking whether it would increase or decrease deadweight loss to try to correct this imperfect information.

Taking the imperfect information into account in a model in order to calculate the government's optimal response is similar to the idea in Goldin (2015) where he models how the government can maximize welfare using a combination of more and less salient taxes (for example, taxes included in the sticker prices versus taxes that are added at the register). Using more salient taxes somewhat corrects consumers' internalization of tax rates into their purchase decisions, and Goldin is trying to balance the tradeoffs of correcting consumer budget misallocation and increasing distortionary substitution. In this paper, I will consider the tradeoffs when knowledge of the benefit enters the labor supply decision. Informing workers about the benefit allows them to take it into account in their decision of whether to accept a job at a given wage but also increases the moral hazard cost of providing the benefit.

In the next section, I propose a modification to the mandated benefits model that takes into account moral hazard and lack of knowledge of benefits. Section 3.3 provides background information on the Temporary Disability Insurance programs. Section 3.4 describes the data used for the analysis, and Section 3.5 describes the empirical strategy. Section 3.6 presents empirical evidence on knowledge of TDI and its impact on labor force outcomes. Section 3.7 discusses the values used for the model's parameters, and Section 3.8 uses those parameters to generate predictions from the model. Section 3.9 concludes.

3.2 The Mandated Benefit Model

In order to understand the tradeoffs of a public information campaign informing citizens about their mandated benefits, I develop a model including 1) the labor supply response of individuals who can now value the benefit and 2) the increase in program cost due to moral hazard from workers who now know about the program. This model builds on the classic mandated benefits model by making adjustments to account for less than full awareness of the benefit.[2]

3.2.1 Updated Mandated Benefit Model

In the mandated benefit model, the government mandates that employers provide a benefit and the employers pay the cost of providing the benefit. The proposed model builds on the standard model from Summers (1989). It adds parameters for the fraction of workers who know about the benefit (β) and for the increase in program cost due to moral hazard from those who do know about the benefit (κ). I will begin by presenting the original model and then add in these new parameters.

In the original model with full information, the government mandates that employers provide employees with a benefit. The benefit costs employers c per employee, and the employees value the benefit at αc. Then the labor market is in equilibrium when:

$$L^D[w + c] = L^S[w + \alpha c]$$

Totally differentiating and rearranging yields:

$$\frac{dw}{dc} = \frac{\epsilon^D - \alpha\epsilon^S}{\epsilon^S - \epsilon^D} \tag{3.1}$$

[2]In the case of TDI, only the New York and Hawaii laws function primarily as mandated benefits with the employer paying the cost to provide the employee with the benefit (and even in those states, a small amount of cost sharing is allowed), but the incidence, and therefore the efficiency, is in theory the same regardless of how the benefit is financed. So even though California and Rhode Island primarily (or exclusively) have state plans financed solely through a tax on employees and New Jersey has a hybrid plan where the employer can choose whether to purchase a private plan or participate in the state plan, the change in total compensation and the change in employment should be the same as it is for the mandated benefit model.

$$\frac{dL}{L} = \frac{\epsilon^D(dw + dc)}{w} \tag{3.2}$$

Equation (3.1) shows how much the wage changes with the cost of the mandated benefit. Equation (3.2) similarly describes how labor force participation changes in response to the mandated benefit. If employees fully value the benefit ($\alpha = 1$), the wage is decreased dollar-for-dollar with the cost of the program and employment does not change, so there is no deadweight loss. On the other hand, if employees do not value the benefit at all ($\alpha = 0$), the mandated benefit is equivalent to a tax on the employer and there is no efficiency gain from having a mandated benefit.

The original model assumes that workers value the benefit at αc and adjust their labor supply accordingly. But if they do not know about the benefit, then they cannot value it. If, in the extreme, no workers know about the benefit, this is equivalent to the $\alpha = 0$ case, and the mandated benefit is equivalent to a payroll tax. Informing workers of the benefit could then result in an efficiency gain if their valuation is greater than zero. On the other hand, knowledge of the benefit could increase moral hazard. For example, in the case of TDI, individuals with a less serious disability who may have stayed on the job when they did not know about the program may choose to apply for TDI if they do know about the program. Autor et al. (2014) find evidence of such moral hazard in the private long-term disability market. Moral hazard increases program cost, increasing the deadweight loss from providing the benefit.

In order to model these tradeoffs, define β as the fraction of individuals who know about the program, κ as the percent increase in cost due to moral hazard when an individual knows about the program, and c_0 as the per-employee cost of the program in the absence of this moral hazard. Note that κ encompasses the increased costs of insuring someone who knows ex ante about the program versus someone who does not know about the program ex ante. It is moral hazard in the application for benefits and does not include, for example, moral hazard in the duration of work absence (when a worker is absent from work for longer than

he would have been without the benefit). The cost of insuring an individual who knows about the program is then $c_1 = (1+\kappa)c_0$, and the average cost of insurance is $c = (1+\beta\kappa)c_0$.

Finally, I need to define individuals' valuation of the benefit. In the original model, workers value the benefit at αc. In the updated model, when a worker knows about the benefit, replace c with c_1, the cost to provide the benefit to an individual who knows about the benefit, so that $\alpha c_1 = \alpha(1+\kappa)c_0$ is their valuation when they know about the benefit. If they do not know about the benefit, their valuation is zero.

With these parameters and assumptions, the labor market is in equilibrium when:

$$L^D[w + (1+\beta\kappa)c_0] = (1-\beta) * L^S[w] + \beta * L^S[w + \beta\alpha(1+\kappa)c_0]$$

Labor demand is a function of the expected cost of employing a worker, which is the wage plus the cost of the mandated benefit. Labor supply is a weighted average of the labor supply curves of the $(1-\beta)$ fraction of individuals who are not aware of the benefit and the β fraction of individuals who are.[3]

Totally differentiating and rearranging yields:

$$\frac{dw}{dc_0} = \frac{(1+\beta\kappa)\epsilon^D - \beta\alpha(1+\kappa)\epsilon^S}{\epsilon^S - \epsilon^D} \tag{3.3}$$

$$\frac{dL}{L} = \frac{\epsilon^D[dw + (1+\beta\kappa)dc_0]}{w} \tag{3.4}$$

The differentiation assumes that $\frac{d\beta}{dc_0}$=0. That is, the fraction of people who know about the benefit does not change with the program cost. This should be a good approximation

[3]For the purposes of this model, I will assume that the mandated benefit does not have an effect on the labor market independent of its cost to employers and valuation by employees. The application in this paper is to TDI, so this assumption would be violated if, for example, individuals used TDI as a pathway out of the labor force when they otherwise would have remained in the labor force. Given that Autor et al. (2014) finds that a reduction in the waiting period for receiving private long-term disability benefits does not increase the number of claims that eventually qualify for SSDI, I think this is a reasonable approximation. An individual in a TDI state on the margin of leaving the labor force due to disability but who is not eventually going to qualify for SSDI can only look forward to a few more months of partial income replacement than an individual in a non-TDI state, and a few months of benefits should not be enough to induce many (if any) individuals to permanently leave the labor force.

for small values of c_0. Note that when $\beta = 1$ (as is assumed in the original model), $dc = (1 + \kappa)dc_0$, and these equations reduce to equations 3.1 and 3.2 from the original model.

3.2.2 Updated Model Graphical Representation

This section presents the model graphically and discusses its predictions on the effect of increasing awareness of the mandated benefit (increasing β). As depicted graphically in Figure 3.1, in the standard model, introducing a mandated benefit has two effects. First, the mandated benefit makes it more costly for employers to hire workers at a given wage, so they are willing to pay less for a given amount of labor, causing the labor demand curve to shift. Second, when individuals value a benefit, they factor it into their decision of whether to accept a job at a given wage. With the benefit, they are willing to accept a lower wage, so their labor supply curve shifts out, lowering the impact of the mandated benefit on labor supply.

Figure 3.2 depicts the updated model graphically. The demand curve shift remains the same as in the original model, although the cost per employee is now denoted by $(1 + \beta\kappa)c_0$. On the supply side, individuals who do not know about the benefit stay on the original supply curve, which is marked as S1 in the diagram. Individuals who do know about the benefit shift their supply curve in response to the benefit, moving to the line denoted S2a. The aggregate supply curve is then the weighted average of these two curves, where the weighting is done by the parameter β, the fraction of individuals who know about the benefit.

Increasing the parameter β will have two effects. Because of moral hazard, the increased awareness raises program costs as individuals who would have stayed at work decide to apply for benefits once they know about the program. Higher program costs increase the cost to the employers of providing the benefit, so their labor demand curve shifts out again, decreasing employment. Second, the labor supply curves of individuals who now know about the benefit shift out, which shifts the aggregate labor supply curve, increasing employment. It

is ambiguous which effect will dominate and whether increasing β would increase or decrease employment on net.

Whether increasing awareness increases or decreases employment on net is ambiguous and depends on the relative sizes of α and κ. Figures (3.3) and (3.4) depict two extreme cases for the value of α (either zero valuation of the benefit or full valuation) and demonstrate the impact on the deadweight loss of increasing β (the fraction of individuals who know about the benefit) in each case.

Figure 3.3 shows the case where workers fully value the benefit ($\alpha = 1$). In this case,with full awareness of the mandated benefit, all of the deadweight loss disappears. Because workers fully value benefit, the benefit is equivalent to receiving the additional wage in the amount of the per-employee cost of the benefit, so the supply curve shift offsets the demand curve shift.

On the other end of the spectrum, in Figure 3.4, workers do not value the benefit at all ($\alpha = 0$), so increasing β results in an increase in deadweight loss since there is no offsetting labor supply effect, but there is an increase in benefit cost per employee due to moral hazard.

These extreme values are used to demonstrate how the value of α affects the change in employment from an increase in β and are for expositional purposes only as it would be strange to think about workers not valuing a benefit at all but then responding to knowledge of the benefit by using it more, increasing the program cost. But these examples do demonstrate that the value of α is important for determining whether a change in β results in an increase or decrease in deadweight loss. The true value of α likely lies between these two extremes, but without further analysis, it is unclear whether at the true α increasing β would increase or decrease employment.

3.2.3 Calculating Deadweight Loss

The previous section graphically demonstrated that the change in deadweight loss induced by an information campaign could be positive or negative. This section analytically derives the "break-even" condition.

The first step is to calculate the deadweight loss associated with the mandated benefit. Then I can determine whether an information campaign would increase or decrease this amount. The deadweight loss from the TDI program is assessed relative to having no TDI program and measures the reduction in the sum of the consumer surplus and producer surplus from having TDI as a mandated benefit.

The deadweight loss from the TDI program is the same as for a tax of the amount of the difference between the cost to the employer of providing the benefit and the employee's valuation of the benefit. In the model, this is: $(1+\beta\kappa)c_0 - \beta\alpha(1+\kappa)c_0$. Assuming locally-linear supply and demand curves, the deadweight loss from such a tax is:

$$\begin{aligned} DWL &= \frac{1}{2}\cdot[1+\beta\kappa-\beta\alpha(1+\kappa)]\cdot c_0\cdot dL \\ &= \frac{1}{2}\cdot[1+\beta\kappa-\beta\alpha(1+\kappa)]\cdot\frac{dc_0}{w}\cdot\frac{dL}{L}\cdot L\cdot w \\ &= \frac{1}{2}\cdot[1+\beta\kappa-\beta\alpha(1+\kappa)]^2\cdot(\frac{dc_0}{w})^2\cdot\frac{\epsilon^D\epsilon^S}{\epsilon^S-\epsilon^D}\cdot L\cdot w \end{aligned}$$

where the last line comes from substituting equation 3.4 for $\frac{dL}{L}$ and then after some algebra, substituting in equation 3.3 for $\frac{dw}{dc_0}$. Note that the equation includes $L\cdot w$, the total wage bill for the state. This value would be necessary to calculate the dollar value of the loss of surplus. Without it, we are calculating the deadweight loss as a percentage of the wage bill. Also, since it is a constant, it will not be necessary for calculating the percent change in deadweight loss when going from one parameter scenario to another.

For an increase in β from β_1 to β_2, the percent change in deadweight loss (assuming the elasticties are nonzero) is:

$$\frac{DWL_2 - DWL_1}{DWL_1} = \frac{[1+\beta_2\kappa-\beta_2\alpha(1+\kappa)]^2}{[1+\beta_1\kappa-\beta_1\alpha(1+\kappa)]^2} - 1 \tag{3.5}$$

When this value is positive, the deadweight loss is increasing, and when it is negative, it is decreasing. Setting it equal to zero allows me to calculate the "break-even" point, which yields:

$$\kappa = \frac{\alpha}{1-\alpha}. \tag{3.6}$$

For values of κ larger than this, deadweight loss will be lower if the mandated benefit remains unknown until a worker needs to use it, and for smaller values, deadweight loss will be lower if all workers know about the benefit, making it the key relationship.

Note that the percent change in employment is:

$$\frac{dL_2 - dL_1}{dL_1} = \frac{1 + \beta_2\kappa - \beta_2\alpha(1+\kappa)}{1 + \beta_1\kappa - \beta_1\alpha(1+\kappa)} - 1.$$

This is the same as equation 3.5 but without the squares in the numerator and denominator. Therefore, the same relationship between κ and α holds at the "break-even" point for change in employment, and for larger values of κ, increasing awareness decreases employment, while for smaller values, increasing awareness increases employment. Although the break-even point is the same, other percent changes will be different.

3.3 The Case of Temporary Disability Insurance

This paper applies the updated mandated benefits model to Temporary Disability Insurance programs. This section provides some background on those programs.

TDI programs affect workers in five states and, with the exception of Hawaii (1969), have been in place since the 1940s with Rhode Island in 1942, California in 1946, New Jersey in 1948, and New York in 1949. TDI replaces a percentage, called the replacement rate, of income lost up to a cap, called the maximum weekly benefit, when an individual cannot work due to an off-the-job disability. (Work-related disabilities are covered by Workers' Compensation.) All provide payments beginning on the eighth day of disability and lasting for up to at least five months, at which point a disabled worker may be eligible for SSDI payments depending on the severity of the disability. In addition to the length of disability

required for benefits, the state TDI programs differ from SSDI in that they only require the claimant to be unable to do her customary or regular job, whereas SSDI requires her to be unable to engage in *any* substantial gainful activity.

In the states for which data is available, in the most recent years, the TDI programs have been almost as large as UI in terms of the dollar value of total claims paid. The Rhode Island TDI fund paid out $158 million during 2014 compared to $184 million from the UI fund.[4] In California, the state fund paid $4.8 billion in TDI claims in the calendar year 2015 compared to $5.3 billion in UI claims.[5] The average weekly benefit amount is higher for TDI claims than for UI claims, so overall this represents fewer claimants for TDI compared to UI. Rhode Island paid about 36,000 TDI claims with an average weekly payment of $446 and about 47,000 UI claims with an average weekly payment of $329. For California, there were about 636,000 TDI claims filed in 2015 and an average weekly benefit amount of $551 for those accepted compared to 2.4 million UI claims filed and an average weekly benefit payment of $307. Based on claim volume and total claims paid, the TDI programs are a significant budget item, even compared to a large program like UI. Despite this, little research has been done about them to date.

The states vary in whether TDI is a textbook mandated benefit or whether a state-run plan is an option or requirement. Note that a state plan has the same theoretical predictions as a mandated benefit as long as the premiums are equal to the cost c. In Hawaii and New York, employers are required to secure a private disability insurance policy satisfying minimum requirements with limited employee cost sharing allowed (in New York, the state insurance fund operates like a private insurer and is one of the options). Rhode Island has a monopoly state plan financed solely through employee contributions. In California and New Jersey, employers are allowed to choose whether to purchase a private plan or participate in

[4]Rhode Island Department of Labor and Training 2015, *Rhode Island Income Support Programs: UI & TDI/TCI*. Available from: http://www.dlt.ri.gov/lmi/uiadmin.htm. [15 February 2016].

[5]State of California Employment Development Department 2016, *Quick Statistics*. Available from: http://www.edd.ca.gov/about_edd/quick_statistics.htm. [15 February 2016].

the state plan, which in California is financed with employee contributions and in New Jersey with contributions from employees and experience-rated contributions from employers.

The programs vary widely in their minimum coverage requirements – from a fifty percent replacement rate in New York to a two-thirds replacement rate in New Jersey, and a maximum weekly benefit of $170 in New York to $1104 in California. Additional program details can be found in Table 3.1.

In all states, there are few excluded employers in the private sector, though most exclude the self-employed and at least some government workers. In four of the five states, the TDI program is administered by the same department as Unemployment Insurance (UI) and generally covers the same workers. The exception is New York, where it is administered by the Workers' Compensation Board. Table 3.2 contains the details of the coverage and exclusions.

All states place some requirements on the minimum number of weeks of covered employment and/or the minimum amount of covered wages a worker must have before becoming eligible for TDI, from just $300 in covered earnings in the previous year in California to either $8,400 in covered earnings or 20 covered weeks in New Jersey. These details are also in Table 3.2 along with a description of the coverage during unemployment.

3.4 Data

In the empirical part of the paper, I provide evidence on the impact of TDI on employment and also on overall awareness of the benefit using data from the Census and from the Current Population Survey (CPS).

The TDI programs were established primarily in the 1940s, so in order to compare employment before and after program establishment, I use 1% samples from the 1930, 1940, 1950, and 1960 Censuses. My sample includes working-age individuals age 23 to 60 and excludes individuals living in Hawaii since it did not become a state until 1959.

In addition to the Census data, I also do cross-section analysis comparing absence from work in states with and without TDI in present day. This analysis uses data from the March CPS each year from 1980 to 2015. The sample is again restricted to working-age individuals age 23 to 60. This sample also excludes workers not working in the private sector since some of the mandates exclude government workers and the self-employed.[6]

Finally, to estimate the fraction of workers who are aware of their TDI benefits, I use the CPS Employee Benefits Supplements from April 1993 and May 1988. The supplement questions were administered to 26,000 workers in 1993 and 25,000 in 1988. I use these two survey years because they are the only ones that ask the question, "On your job, do you get any paid sick leave or sickness insurance?" I use this question as a proxy for whether individuals have short-term disability coverage. Nearly all private-sector employees in TDI states are covered by the TDI laws, so they have sickness insurance and should answer "Yes" to this question.

Although this is the best survey question from the supplement, it does have limitations. First, it encompasses more than just temporary disability insurance. If a worker in a TDI state is unaware of the TDI benefit but does know that her company provides her with paid sick days, she would answer "Yes." There is no way to distinguish someone who knows about TDI from someone who does not know about TDI but does have paid sick days. I will be looking at cross-state differences, so the hope is that the prevalence and awareness of sick days does not vary by state (at least when controlling for demographics), but it is unclear whether this holds. Second, the question uses the phrase "sickness insurance" as opposed to "disability insurance". Although disability insurance is sickness insurance, without the keyword "disability" in the question, even someone who knows about TDI may mistakenly answer that he or she does not have "sickness insurance."

This sample again includes working-age individuals ages 23 to 60 who are employed in the private sector. I include only private-sector employees since the TDI mandates usually

[6]See Table 3.2 for coverage details.)

exclude government workers as well as the self-employed. More details on coverage exclusions can be found in Table 3.2. Additionally, only respondents who answered the question with "Yes" or "No" are included, excluding individuals who said "Don't Know" (about 2%) or left the question blank.

3.5 Empirical Strategy

The empirical work is divided into three parts. First, I do the standard mandated benefit analysis of the impact of the mandated benefit on employment using a difference-in-differences design.[7] Second, due to the data limitations around the introduction of TDI, I also look at a present-day cross-section of TDI vs. non-TDI states to observe whether individuals are more likely to be absent from work in these states. Finally, I analyze evidence of whether individuals are aware of their TDI benefits.

For the difference-in-differences analysis of the impact of TDI on employment, data is aggregated into state-by-year cells (and for the gender-specific regressions, state-by-year-by-gender cells) using the Census population weights. I use difference-in-difference regressions to estimate the impact of the TDI laws on the fraction of people employed in the state. Since the laws were all passed in the 1940s, the 1940 and 1930 Censuses are used for the pre-period and the 1950 and 1960 Censuses are used for the post-period. Cells are weighted by the number of observations in the cell and all regressions include state fixed effects. Robust standard errors are presented because in most cases standard errors clustered at the state level were actually lower, so not using clustered standard errors is more conservative. This does not impact the statistical significance of the results.

Specifically, I run the following regression:

$$Y_{st} = \beta * TDI_s * Post_t + \sigma_s + \eta_t + \epsilon_{st} \tag{3.7}$$

[7]Typically, an evaluation of the mandated benefit on the labor market would also include a difference-in-differences analysis on the impact on wages. However, there is not a good measure of wage in the available Census data. Annual wage and salary income and weeks worked last year are available, but usual hours worked per week is not available (only hours worked last week), so I cannot compute the hourly wage for the past year.

The outcome of interest is Y_{st}, the fraction of individuals ages 23 to 60 in state s at time t who report being in the labor force. TDI_s is an indicator variable that equals one if the state is one of the four with a TDI law. Note that Hawaii is excluded since it did not become a state until 1959. $Post_t$ is an indicator variable that equals one in 1950 and 1960, the years after the TDI laws were implemented in the 1940s. The regression also includes state fixed effects (σ_s) and year fixed effects (η_t). The coefficient of interest is β, which can be interpreted as the causal impact of the TDI mandated benefit on labor force participation under the identifying assumption that in the absence of TDI, labor force participation would have trended similarly in TDI and non-TDI states.

Because the TDI programs were established so long ago, it is difficult to find data to analyze their effect on wages and employment. Even with a good data set, it is unclear what labor market analysis from 70 years ago would be able to tell us about the valuation of TDI benefits by workers today since the composition of workers has changed and elasticities may have changed over time as well. More importantly for application to this model, there may have been changes in the knowledge of benefits over time. In particular, when a new law is passed, it is more likely to be in the public consciousness, so workers are more likely to know about the benefit. This knowledge may fade over time if the benefit does not continue to garner publicity.

Since there is limited data dating back to when the TDI programs were established, and even if there were data, it is unclear whether it would be applicable to today's labor market, I also performed cross-sectional analysis considering whether workers are more likely to be absent from work due to illness in the states with TDI programs. If workers in TDI states are more likely to be absent, then it could indicate moral hazard because a worker with a less serious disability may choose to continue working in a state that does not have TDI benefits but in a TDI state, with the enticement of partial income replacement, decide to apply for TDI benefits or take a longer absence from work.

In particular, I run the following regression:

$$Y_{ist} = \beta * TDI_s + X_{it} + \eta_t + \epsilon_{ist} \tag{3.8}$$

The outcome of interest Y_{ist} is an indicator variable that equals one if individual i living in state s at time t was absent from work at the time of the survey. Additional outcomes include whether the individual is absent from work due to sickness, vacation, or maternity leave. When maternity leave is the outcome, observations are restricted to women age 23-44 and in some cases additionally restricted to women ages 23-44 with a child under the age of one year. X_{it} is a set of individual-level demographic control variables consisting of age, age squared, gender, race, ethnicity, marital status, and education. η_t are year fixed effects. The coefficient of interest is β, which indicates how much more likely individuals in states with TDI laws are to be absent from work in a given period.

Finally, in the third set of analysis, I consider evidence of whether individuals in TDI states know that they are covered by TDI using the May 1988 and April 1993 CPS Employee Benefits Supplements. First, I look just at the fraction of individuals in TDI states who answer "Yes" to the question, "On your job, do you get any paid sick leave or sickness insurance?" In TDI states, this fraction should be very close to one since nearly all private sector workers are covered by TDI.

In addition, to test whether the difference in means between TDI and non-TDI states is due to observable factors, I also run the same cross-section regression specified in equation 3.8. The outcome of interest Y_{ist} is an indicator variable that equals one if individual i residing in state s at time t answered "Yes" to the question, "On your job, do you get any paid sick leave or sickness insurance?" The coefficient of interest is β, which is interpreted as the increase in probability that a worker in a TDI state would say that they have sick leave or sickness leave on their job net of differences due to the X_{it} observable characteristics.

The "on your job" language lead-in may cause individuals who are insured under a state plan to answer "No" because the benefit is being provided by the state as opposed to by their employer. For this reason, I break the results down by state since employees in New York

and Hawaii are entirely insured through their employers by private disability companies, so these states are not impacted by this concern. The means and regressions for individual TDI states include workers in the named TDI state along with workers in non-TDI states and exclude workers in the unnamed TDI states.

3.6 Empirical Evidence

3.6.1 Difference-in-Difference Estimation

Evaluation of the incidence and worker valuation of mandated benefits using the standard model begins with estimating the change in employment and wage as a result of the policy. The standard model predicts that the more the workers value the benefit, the smaller the decrease in employment and the larger the change in the wage. Full valuation of benefits would mean that the wage is decreased by the cost of the benefit and there is no change in employment. If this is not the case, the model says that workers do not fully value the benefit. However, in the model described in Section 3.2, a decline in employment could also mean that not all workers are aware of their benefit.

Results for the difference-in-differences model are presented in Table 3.3. Odd columns include only the 1940 and 1950 Censuses, and even columns add the 1930 and 1960 Censuses. The regressions suggest that female employment in TDI states fell relative to non-TDI states by one percentage point after the implementation of the TDI laws, which could suggest less than full valuation by women (note that at the outset, Rhode Island was the only one of the four states to cover maternity).

However, the sign on male employment is in the other direction; male employment rose by about two percentage points. Interpreting this result within the mandated benefits framework would imply that workers value the benefit at *more* than it costs to provide. In that case, it is not clear why employers would not offer the benefit willingly in the first place. This large, unexpected result may indicate that this identification strategy is not valid here. This is a long time period over which to try to find a change in employment from these laws,

particularly with World War II in the middle of the period and the large effects it had on the labor force. Any labor market impacts from TDI may just not be identifiable from this data with all of the other concurrent changes.

3.6.2 Cross-section Analysis

The results of the cross-section linear probability models on the probability of being absent from work are presented in Table 3.4. The first regression shows that overall, workers are actually less likely to be absent from work in TDI states, which was not the expected result. The following three columns break down this result into different reasons individuals in the CPS can give for being absent from work. It can then be seen that the overall result is driven by the fact that workers in TDI states are less likely to be on vacation, which is not targeted by TDI. The primary result of interest is whether they are more likely to be absent from work due to sickness, and the second column shows that there is no statistically significant difference in the probability of being absent due to sickness in the TDI versus non-TDI states.

Without a model allowing for workers to be unaware of their benefits, the conclusion from this result might be that it is circumstantial evidence that there is not moral hazard in the TDI programs. However, given the literature finding significant moral hazard in long-term disability insurance (Autor et al. (2014); Gruber (2000); Autor and Duggan (2003)), it would be surprising if there were not moral hazard in short-term disability insurance. This literature, coupled with the results in Subsection 3.6.3 regarding the lack of awareness of TDI benefits, leads me to conclude that this is additional circumstantial evidence that workers are not aware of their TDI benefits. They are no more likely to be absent than their peers in non-TDI states not because there is no moral hazard but because they are not aware of their benefits.

Notably, women in TDI states are more likely to be absent due to maternity leave (column 4), and TDI does cover maternity. In general, a woman would get six weeks of disability

payments from TDI if she has a vaginal delivery and eight weeks if she has a Cesarean Section. Women of childbearing age (defined as ages 23-44) in TDI states are 0.13 percentage points more likely to be absent from work due to maternity than women in non-TDI states, which is over 25% of the mean of 0.47% of women absent due to maternity leave.

Table 3.5 explores this result further. These regressions use the same CPS data as before but restrict the sample to working women of childbearing age. The first column repeats the maternity regression from the previous table. The second column restricts the sample to women whose youngest child is less than one year old in order to remove any effects from cross-state differences in fertility. Women with a child less than one year old in TDI states are 1.6 percentage points more likely to be absent from work due to maternity leave than their counterparts in non-TDI states, which is almost 20% of the mean in non-TDI states of 8.2%.

Three of the TDI states have also passed paid family leave laws that allow workers to replace a fraction of their wages while caring for a newborn child for four (Rhode Island) or six (New Jersey and California) weeks, and women can use these programs to extend their paid leave after their disability claim has ended. These are the only three states with such laws, and the first was California, where parents were eligible for benefits beginning July 1, 2004. In order to only capture the effect of the TDI laws and not the paid family leave laws, the third and fourth columns repeat the analysis restricting the sample to the years 1980-2003. The results are not markedly different. Women in TDI states were 0.086 percentage points more likely to be on maternity leave, over 25% of the mean of 0.3%, and women in TDI states with children less than one year were 1.4 percentage points more likely to be absent due to maternity leave, over 25% of the mean of 5.4%.

These cross-section results suggest that the TDI laws increase the probability that a woman will be on maternity leave. Note, however, that this likely operates on the intensive margin, and is therefore not the type of moral hazard that is prevented when individuals do not know about their benefits. Even if a woman does not know about TDI benefits, she

would need to take some time off of work after the birth of her child, so her employer would be required to notify her of her benefits. Then any difference in the probability of being on maternity leave would be driven by length of leave. Given that women in TDI states are more likely to be on maternity leave even conditioning on having a child under one year old, it seems that TDI induces women to take longer leaves than they would have otherwise. Lack of knowledge of TDI benefits prevents moral hazard in the application for benefits, not in the length of benefits once they have been approved. The only way that maternity could fit in to the former case would be if women were more likely to have babies if they knew about TDI. Since the results are similar before and after conditioning on having a young child, increased fertility does not seem to be playing a major role here.

Overall, the results of the cross-section analysis suggest that there has not been moral hazard induced by the TDI benefits since workers in TDI states are no more likely to be absent from work due to sickness. However, this may be because they do not know about the program. Women in TDI states are more likely to be on maternity leave, which is most likely driven by an increase in the length of the leave as opposed to an increase in the number of women taking any leave for childbirth, which is not the kind of differential absence from work captured by the moral hazard parameter κ.

3.6.3 Awareness of TDI

The mean fractions of workers in the different states who answered "Yes" to the sickness insurance question are available in Table 3.6. Overall, about 64% of workers in TDI states said that they have sick leave or sickness insurance compared to 60% in the non-TDI states, for a difference of four percentage points, which is statistically significant at the 1% level. There was no difference between California and the non-TDI states, possibly because most employees are insured through the state plan, so they answer that they do not have coverage "on their job." The remaining TDI states have means that are 10.5 (New Jersey), 8.0 (New York), 4.5 (Rhode Island), and 13.6 (Hawaii) percentage points above the mean of the

non-TDI states. These differences suggest that there may be some knowledge of the TDI programs, though with 74% of Hawaiians (the state with the highest mean) answering that they have sick leave or sickness insurance, there are still many workers who are unaware of their benefits. The 74% statistic can be seen as a very conservative upper bound on the fraction of workers who know about their benefits since they may be answering "Yes" knowing that they have sick leave but unaware that they have sickness insurance.

Regressions of TDI state on having paid sick leave or sickness insurance are presented in Table 3.7. They include demographic controls for age, race, marital status, education, and gender as well as a dummy variable for the survey year. The results are very similar to the means analysis with the effect slightly dampened in a couple of states. Overall, the regressions indicate that workers in TDI states are slightly more likely (3.5 percentage points) to say that they have paid sick leave or sickness insurance. Consistent with the concern that the "on the job" qualifier in the question could lower the response for workers insured by state plans, the effect is larger than the mean and significant in New York and Hawaii, where employers purchase private insurance for their employees. Also with a strong and significant positive coefficient is New Jersey, perhaps because employers in this state also have the option of purchasing private plans. Still, there is far from universal awareness of the insurance coverage.

Overall, these regressions support the idea that the awareness of TDI is low. Certainly no more than 75% of workers seem to know about their benefits since the highest affirmative response to the question was 74% in Hawaii. However, given that the question also asks about sick leave, a benefit for which there is probably more awareness, the awareness of TDI is likely substantially lower than 75% of the population.

3.7 Parameter Estimation

In order to evaluate the expected impact of a public awareness campaign, I need estimates for the parameters in the model. Only estimates for the fraction of individuals aware of the

benefit (β), the moral hazard (κ), and the valuation of the benefit (α) will be needed to calculate the percent change in deadweight loss and employment when increasing awareness of the benefit (see section 3.2.3). The other parameters will be necessary to calculate the change in employment in response to the mandated benefit for a given value of β. In this section, I describe the parameter estimates I will be using, which are primarily based on reasonable ranges from the literature.

Moral Hazard (κ)

For the purposes of the model, the moral hazard parameter is assumed to be a constant that has the same value for all workers. The value of interest is the percent increase in the cost of providing the benefit when a worker becomes aware that they have the benefit. In the absence of results from such an experiment, a similar measure would be to estimate the change in days absent from work due to sickness when a new disability insurance program is introduced. I am not aware of any literature analyzing either of these ideal experiments, so I will have to use estimates from the literature that use other sources of variation.

In the United States and Canada, there have been a number of studies on the presence of moral hazard in long-term disability insurance, particularly studying government-provided disability insurance. Gruber (2000) uses a 36% increase in the benefit payments in the Canadian disability insurance (DI) system in January 1987 to compare the labor force participation of men ages 45 to 59 in Quebec, which was covered by a different program that remained unchanged, to the rest of Canada. He finds an elasticity of labor force nonparticipation with respect to DI benefits of 0.25 to 0.32. Since this is long-term disability insurance, this is a measure of moral hazard because workers are choosing to leave the labor force when benefits increase (though workers need to be off the job for several months before they can apply for benefits, so this figure includes individuals who leave the labor force to apply for benefits, are denied, and remain out of the labor force). In the United States, Autor and Duggan (2003) use Bartik instruments to study how the number of SSDI applications

varies with state labor market conditions, estimating that a one percent projected log state employment contraction yielded 1.3 additional DI applications per 1000 nonelderly adults over 5 years. Both papers indicate significant moral hazard in public long-term disability insurance, though it is unclear what they say about short-term disability insurance or how these could be translated into estimates of moral hazard from becoming aware of a benefit.

There are two papers that may be able to speak a little more directly to this parameter. The first, Johansson and Palme (2005), is important because it finds evidence of moral hazard in short-term disability insurance. The authors study a reduction in benefits in the Swedish national sickness insurance program. Prior to March 1, 1991, the program paid claimants 90% of earnings below the social security ceiling regardless of claim length. After March 1, 1991, it paid 65% for the first 3 days, 80% for days 4-90, and continued to pay 90% after that. Johannsson and Palme estimate that there was a 20-30% decrease in incidence in response to the reduction in benefits, a large decrease for a benefit reduction in the first three days of 28%. The response to the Swedish program may be larger than for an equivalent TDI change since there is no waiting period in Sweden and a doctor's certification is not required for the first seven days, making it easier for individuals to file initial claims (the TDI programs have seven-day waiting periods and require a doctor's certification).

The second paper, Autor et al. (2014), uses variation that is the closest I am aware of to the effect of establishing a new short-term disability program. Using data from a private disability insurance company, they study the effect of reducing the waiting period for long-term disability insurance benefits from 180 days to 90 days and find that it induces a non-mechanical 40% increase in the number of claims. The distinction between studying private long-term disability insurance and public long-term disability insurance is important because for the first two years, the definition of disability for private long-term disability insurance is the same as for TDI - the claimant cannot do his customary job - while for the public long-term disability programs, workers are only disabled if they cannot engage is substantial gainful activity. The change in waiting period is similar in theory to establishing

a new program covering days of disability 90-179, so it may have more bearing on the correct value of κ than the other papers previously discussed. TDI provides coverage starting on day 8 and continues until at least day 150 depending on the state, so because it starts sooner and provides benefits for more days, the increase in claims when workers become aware of the benefit could be even more significant for TDI. Importantly, however, the definition of disability is broader for TDI than for SSDI, so not all disabled workers on TDI can expect to continue receiving disability payments when they have exhausted TDI benefits, which would make it relatively less attractive than the long-term disability program, where benefits would last for at least two years. Autor, Duggan, and Gruber find that the 40% increase in incidence comes entirely from claims where the recipients ultimately either return to work or don't return to work but also do not collect SSDI - that is, there is not an increase in incidence for claims that ultimately qualify for SSDI. These are the less costly claims for insurance since they have a shorter duration than ones that do eventually qualify for SSDI, so the 40% increase in incidence translates into a less than 40% increase in cost, and here the parameter of interest is the increase in costs.

Still, it seems reasonable that if a three-month increase in benefits starting 91 days after disability induced a 40% increase in incidence that an increase of five months of benefits starting eight days after disability would induce at least a 40% increase in costs. Therefore, I will focus on values of κ of 0.4-0.8, though I will present results for values of κ ranging from 0 to 1. Note that there is no reason to assume that κ is bounded by one, but I think it is a reasonable upper bound here.

Workers' Valuation (α)

This model makes two simplications in its consideration of α. The first is that α is the same for all workers, and in particular, it does not vary by current knowledge of the program. In reality, workers almost certainly have different valuations of TDI. It would also be reasonable to think that current knowledge of the benefit is not random and that individuals who value

TDI more are the ones who currently know about it. Those who care more about having disability insurance are probably more likely to be proactive in finding out about their benefits. Thus, a public awareness campaign would reach individuals with a lower α on average compared to individuals who already know about the coverage. The current model extrapolates away from this complication, but it may be a good extension to add in the future.

The second simplification made with respect to α is that it is unchanged by a public awareness campaign. The analysis assumes that β would be the only parameter affected by such a campaign. The assumption is that once a worker knows about the TDI program, she perfectly evaluates its value to her, so a public information campaign will not change that valuation. But given that this model is premised on a lack of perfect information about the existence of the benefit, it does not take a large leap to imagine that even among workers who know about the benefit, there could be a lack of perfect information about the provisions of the benefit or its implications for their expected utility. Depending on the details of the public awareness campaign, it is possible that it could increase (or decrease) α as individuals become more aware of the details and necessity of the benefit. For the purposes of this paper, I will extrapolate away from such effects.

There are several papers in the literature studying worker valuation of mandated benefits, and they generally find that there is significant variation in their value across individuals. (Although as noted earlier, this could also indicate variation in the knowledge of the benefit across individuals.) Gruber and Krueger (1991) studies Workers' Compensation Insurance and finds that there is full or almost full valuation by truck drivers but lower valuation in some other industries. Looking across ten industries with high rates of workers' compensation claims, they find that 56% of the costs were shifted to wages, indicating less than full valuation by workers. Given the prominence of the Workers' Compensation program, particularly in high-claim industries, I do think that this differential shifting is a result of differential valuation as opposed to differential awareness. Gruber (1994) studies the incidence

of mandating that health insurance cover maternity claims in the same way as claims for other health conditions and finds that even after adjusting for the group-specific additional cost of coverage, married women of child-bearing age fully value the benefit while finding no evidnece that married men of the same ages (many of whom would elect coverage for their wives on their plan) value it at all.

Given these wide ranges previous studies have found for worker valuation of mandated benefits, including for the disability insurance provided through Workers' Compensation, I will consider a range of values for α from 0 to 1. Note that there is no reason that α needs to be bounded above by 1. If $\alpha > 1$, that would imply workers value the benefit at more than the cost of providing it. However, if this is the case, it would seem to indicate some kind of market failure if the employer was not providing it prior to the mandate since providing the benefit would be cheaper than paying additional wage. Keeping in mind the note that workers with lower valuations of the benefit may be ones who do not know about the benefit, I will also pay particular attention to some lower values of α less than 0.5 since workers with lower valuations may be the ones reached by an information campaign.

Knowledge of TDI (β)

I would like to evaluate whether a public awareness campaign would decrease the deadweight loss of the TDI programs. Since β represents the fraction of people who know about the program, it is the key parameter that would be influenced by such a campaign. Therefore, I would like to estimate the decrease (or increase) in deadweight loss when β increases from its current value. I will focus on increasing β to 1, which would mean that all workers know about the TDI program. This perfect awareness is what is assumed in the original model.

As a baseline for the current value of β, I would like to use the results from the CPS Employee Benefits Supplements discussed in section 3.6.3. However, the survey does not ask the exact questions I would like in order to get a sense for whether workers are aware of their TDI benefits. Further discussion of the details of the question and its shortcomings are in

section 3.6.3. Since this measure is far from ideal, it can only be used to get a general sense of what the starting value of β might be. Since the largest fraction of workers who said they have sick leave or sickness insurance was just under 75% (from Hawaii), I think this is a very conservative upper bound for the value of β. Overall, the mean affirmative response in TDI states was only 4 percentage points higher than in non-TDI states (and 14 percentage points in Hawaii) when it should have been 40 percentage points, indicating significant lack of knowledge of the program. A very low lower bound for β is then 4%, which leaves a large range of possible values.

Because I do not have a precise measure for the current value of β, I will consider the implications of a range of starting values for β, starting with models going from no awareness to full awareness - that is, from $\beta = 0$ to $\beta = 1$. This first model will speak to the importance of the β parameter by giving a sense for how large (or small) of an effect it can have. I will also consider three other starting values for β - 0.25, 0.5, and 0.75.

It is worth noting that although the different starting values of β change the magnitude of the percent change in deadweight loss, employment, or wage, they do not change their signs. Therefore, if the deadweight loss increases (decreases) when going to full information ($\beta = 1$) from one starting on value of β, then it increases (decreases) when going to perfect information from all other starting values of β.

Elasticity of Labor Supply (ϵ^S)

The Congressional Budget Office prepared a report in 2012 [McClelland and Mok (2012)] synthesizing the recent literature on labor supply elasticities. They report that estimates for the general population generally fall between 0 and 0.2. For men and single women, estimates of the elasticity of participation usually fall between 0 and 0.1, and for married women between 0 and 0.3. These ranges will be used as a guide for the appropriate value of this parameter. Note that if the true elasticity were identically zero, by definition there

would be no labor supply response to a change in taxes or mandated benefit, so the awareness of the benefit would be irrelevant.

Elasticity of Labor Demand (ϵ^D)

In his book *Labor Demand*, Hamermesh (1993) estimates that the elasticity of labor demand is between -0.15 and -0.75, with his best guess being that it is -0.3. More recently, Lichter et al. (2015) performed a meta-regression analysis to try to account for the heterogeneity in the estimates across papers estimating the elasticity of labor demand. Based on that work, they estimate that the elasticity lies between -0.072 and -0.446, with their best guess that it is -0.246. Given the proximity of the two best guesses, I will focus on an elasticity of labor demand of -0.3.

Benefit Cost ($\frac{dc_0}{dw}$)

The tax rates in the three states with public funds can be used as a guide for the benefit cost as a percent of the wage. Under the assumption that β is fairly low, the current tax rates should be close to this parameter, which is the benefit cost as a percent of the wage when no one knows about the benefit. The current tax rates are 1.2% in Rhode Island, 0.9% in California, and vary from 0.3-0.95% in New Jersey depending on the employer's experience rating. Since these values hover around 1%, that is what will be used for the analysis.

3.8 Model Predictions

Percent Change in Employment Loss and Deadweight Loss

As shown in section 3.2.3, the percent change in deadweight loss and employment loss when increasing awareness of the mandated benefit depends only on α, κ, and the starting and ending values of β. Figures 3.5 and 3.6 explore this relationship for values of α and κ between 0 and 1 and for four different starting values of β - 0, 0.25, 0.5, and 0.75 by displaying the percent change in employment loss and deadweight loss, respectively, when increasing awareness of the benefit to 100%.

By "employment loss" I mean the employment level after the introduction of the mandated benefit minus the employment level before the introduction of the mandated benefit. I compare the employment loss for the initial value of β to the employment loss when $\beta = 1$ and look at the percent change. For example, a 50% change in employment loss means that the employment loss from the mandate is 50% higher under perfect information. A -50% change means that employment loss from the mandate is 50% lower under perfect information compared to the initial β.

One important takeaway is that if the moral hazard parameter κ is less than one, then in order for a public information campaign to lower employment (increase employment loss), the worker valuation α must be less than 0.5. This can also be seen from equation 3.6. As shown in section 3.2.3, this cut-off value does not depend on any other parameters and is the same for both employment and deadweight loss. Another immediate takeaway from the figures is that if workers would fully value the benefit ($\alpha = 1$), then informing them of the benefit eliminates deadweight loss and employment loss, as previously discussed in section 3.2. The figures also make it clear that the higher the initial awareness, the smaller the percent changes in employment and deadweight loss when awareness is increased. Intuitively, with a higher initial awareness, there are fewer additional people who are being informed of the program, so the shifts in the aggregate supply and demand curves are smaller.

In comparing the change in the employment loss to the change in deadweight loss, it can be seen that the deadweight loss percent changes are much more extreme. The deadweight loss is given by the formula:$DWL = \frac{1}{2} \cdot (Equivalent\ Tax) \cdot (Employment\ Change)$. The equivalent tax is $(1 + \beta\kappa)c_0 - \beta\alpha(1 + \kappa)c_0$, which moves in the same direction as the employment change, compounding its effects.

Now I will take a look at a few values in particular. As discussed in section 3.7, I think 0.4 is a reasonable lower bound for the moral hazard parameter. At this value for κ, in order for an increase in awareness to decrease employment, α would need to be less than 0.3. If $\kappa = 0.4$, $\alpha = 0.5$, and the initial $\beta = 0.25$, then increasing awareness to 100% would

reduce the employment loss from the mandated benefit by 24% and decrease the deadweight loss by 43%. On the other hand, if the moral hazard parameter is 0.7 and employees value the benefit at only 0.3 (and the initial $\beta = 0.25$), then the employment loss is predicted to increase by 14% and the deadweight loss is predicted to increase by 29%. At $\alpha = 0.3$, with an initial value of $\beta = 0.25$, the moral hazard parameter would have to be larger than 0.8 in order for the employment loss to increase by more than 20%.

Based on this analysis, over a broad swath of reasonable values, the model predicts that informing workers about their benefits would either increase employment (decreasing deadweight loss) or not decrease employment by a large amount.

Absolute Change in Employment Loss and Deadweight Loss

The previous section discussed the percent change in the employment loss and deadweight loss when awareness of the mandated benefit was increased. This section discusses the values of the change in employment and deadweight loss under different assumptions. Overall, under most assumptions, the absolute employment loss is moderate, and the deadweight loss is very small.

The first table in Figure 3.7 shows the percent change in employment due to the introduction of a mandated benefit using the parameter values from Section 3.7. If the labor supply elasticity is truly zero, then there is no effect on employment from a mandated benefit. Results using smaller labor supply elasticities are more representative of men and single women, while larger ones are more representative of married women. For a larger supply elasticity of 0.2, a demand elasticity of -0.3, and a smaller employee valuation ($\alpha = 0.3$) coupled with a larger moral hazard ($\kappa = 0.8$), increasing awareness from 25% to 100% would decrease overall employment by about 0.02%. Smaller supply elasticities would have a smaller effect.

On the other end of the spectrum, using the same elasticities, for a larger employee valuation ($\alpha = 0.7$) coupled with a smaller moral hazard ($\kappa = 0.4$), increasing awareness from 25% to 100% would increase employment by about 0.05%. This result may be relevant

for married women of childbearing age who are likely to have larger labor supply elasticities than the general population, to value the maternity benefits that TDI provides, and to be less likely than older workers to be on the margin of taking time off of work due to a disability.

The first table in Figure 3.8 presents the total deadweight loss due to TDI per $1 million of payroll under the same parameter assumptions as discussed for employment in the previous paragraph. Even for the most extreme values, the total deadweight loss from the mandated benefit is $15 or less per $1 million, and the change in deadweight loss when going from 25% awareness to 100% is always less than $5 per million.

Because the employment changes using the assumptions from the TDI model are fairly small (and in the case of the deadweight loss results, very small), I also asked the question of how much the results would change for a larger program. In order to keep awareness low, the program cannot be very large. I chose to look at a mandated benefit that costs 5% of wage for employees who do not know about it. I think 5% is near the outside limit of what a program could cost while still having awareness be low. The results from this exercise are in the second tables in Figures 3.7 and 3.8. The deadweight loss values in the latter figure are still mostly under a couple hundred dollars per $1 million of wage bill, which is a fairly small loss of surplus.

On the employment side, even for a small elasticity of supply of 0.05, with $\alpha = 0.3$ and $\kappa = 0.8$, increasing awareness from 25% to 100% is estimated to decrease employment by about 0.04%. At the larger elasticity of 0.2, it would decrease employment by about 0.11%. With a higher employee valuation of 0.7 and the same values for the remaining parameters, employment rises 0.07% with an elasticity of 0.05 and 0.2% with an elasticity of 0.2. Note that the program cost enters the change in employment formula linearly, so having a program that is five times as large as the original multiplies the employment change by five times as well.

3.9 Conclusion

Standard mandated benefit theory ignores the fact that workers may be unaware of the benefits the government has mandated their employer to provide. This paper examines the implications of accounting for less-than-perfect awareness. There are two main tradeoffs from informing workers of their benefits: (1) their supply curve will shift out, working to increase employment and (2) they will file more claims, increasing program cost, working to decrease employment. In order to evaluate this tradeoff, I developed a model that allows workers to be unaware of their benefit.

I presented empirical evidence that workers in the five states with Temporary Disability Insurance laws (California, New Jersey, New York, Rhode Island, and Hawaii) are largely unaware of their benefits, so the standard mandated benefit model does not apply to them. With the literature as a guide for reasonable parameter estimates, I used the model I developed to calculate the change in employment and deadweight loss from informing workers about their TDI benefits. I found that across a wide range of reasonable parameter values, informing workers about their benefits would likely not have significant negative effects on overall employment and in many cases would increase employment and reduce deadweight loss.

Table 3.1: TDI Program Overview by State

	California	New Jersey	New York	Rhode Island	Hawaii
Year Established	1946	1948	1949	1942	1969
Private or State Fund?	Choice	Choice	Private	State	Private
Funding	Employee tax 0.9% on first $106,742; or employer provision	Employee tax 0.2% and employer tax 0.1%-0.75% experience rated, both on first $32,600; or employer provision	Employer provision; small cost sharing allowed	Employee tax 1.2% on first $64,200	Employer provision; some cost sharing allowed
Current Replacement Rate[a]	55%	2/3	50%	60% (more if have dependents)	58%
Current Max. Weekly Benefit [b]	$1104	$604	$170	$795 (more if have dependents)	$552
Required Employer Notification of Benefits	Brochure at hire; Posted notice; Upon disability	Posted notice; Upon disability	Upon disability	Posted notice	Posted notice

[a]Minimum in states where private insurance is required or allowed.
[b]Minimum in states where private insurance is required or allowed.

Table 3.2: TDI Coverage by State

	Employment/Wage Requirements	Covered Employment	Unemployment Provisions
California	Have $300 in covered wages during year-long base period	Employers with quarterly payroll of $100; Excludes some domestic workers, government workers, railroad workers, nonprofits, and religious exemptions	Covered if were actively looking for work and had $300 wages in base period
New Jersey	In previous year have $8,400 in covered earnings OR 20 weeks with covered earnings of at least $168 each	Covers all employees covered by Unemployment Compensation except certain government workers	TDI if within 14 days; otherwise, a different program
New York	Working for 4 consecutive weeks; Immediate coverage if switch from one covered employer to another	Employ at least one person on each of 30 days in a year; Excl. govt, railroad, maritime, farm workers, some nonprofits, family employees, secondary students, golf caddies	Covered for first 26 weeks of unemployment if claiming UI
Rhode Island	Wages of $11,520 in last year OR Wages of $3,840 in last year with one quarter at least $1,920 and total wages at least 1.5 times highest quarter	Same as Unemployment Compensation except minors age 14 or 15, state and local government employees (unless their employer has opted in), and religious exemptions	Covered if become disabled while collecting UI
Hawaii	During 52 weeks before claim: 14 weeks of HI employment in which paid for at least 20 hours of work and in total earned at least $400	Excl. fed. govt workers, domestic workers with quarterly wages <$225, some family employees, fishing industry, students working at their school, interns, and a few other minor categories	Covered if disability occurs within 14 days of job separation

Table 3.3: Impact of TDI on Fraction Employed

	All		Men		Women	
	(1)	(2)	(3)	(4)	(5)	(6)
Post*TDI	0.00153	0.00378	0.0175***	0.0225***	-0.0113*	-0.0122**
	(0.00495)	(0.00460)	(0.00602)	(0.00614)	(0.00581)	(0.00491)
Dep. Var. Mean	0.583	0.595	0.878	0.884	0.292	0.309
Incl. 1930, 1960		X		X		X
N	98	196	98	196	98	196

Notes: Data in odd columns is from 1940-1950 1% Census samples, and data in even columns is from 1930-1960 1% Census samples. Data in the first two columns is collapsed into state-by-year and data in the remaining colums is collapsed into state-by-gender-by-year cells, both using person weights. Observations in the regression are weighted by the number of observations in the cell. Individuals age 23-60 are included. Regressions include state-level fixed effects and year fixed effects.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 3.4: Linear Probability Models: Relationship between Absence from Work and TDI States

	Absent from Work	Sick	Vacation	Maternity
TDI State	-0.0012***	0.00023	-0.0015***	0.0013***
	(0.00038)	(0.00022)	(0.00024)	(0.00030)
Male	-0.0095***	-0.0025***	-0.0022***	
	(0.00031)	(0.00018)	(0.00020)	
Age	-0.0016***	-0.000072	-0.00058***	0.0014***
	(0.00013)	(0.000077)	(0.000086)	(0.00021)
Age Squared	0.000023***	0.0000037***	0.0000097***	-0.000028***
	(0.0000016)	(0.00000096)	(0.0000011)	(0.0000030)
White	0.00039	0.0010***	-0.00079^{+}	0.00015
	(0.00068)	(0.00040)	(0.00044)	(0.00056)
Black	0.0027***	0.0045***	-0.0039***	0.0015**
	(0.00083)	(0.00051)	(0.00051)	(0.00065)
Hispanic	-0.0036***	-0.0028***	0.00016	-0.00086**
	(0.00046)	(0.00027)	(0.00028)	(0.00036)
Married	0.0047***	0.00012	-0.00014	0.0073***
	(0.00041)	(0.00023)	(0.00026)	(0.00031)
Sep/Div/Wid	0.0033***	0.0031***	-0.0021***	0.0033***
	(0.00057)	(0.00037)	(0.00034)	(0.00030)
High School	-0.0056***	-0.0040***	0.0019***	-0.0000089
	(0.00055)	(0.00038)	(0.00029)	(0.00034)
Some College	-0.0049***	-0.0064***	0.0051***	0.0010***
	(0.00057)	(0.00038)	(0.00031)	(0.00037)
Bachelor's or more	-0.0050***	-0.010***	0.0084***	0.0034***
	(0.00061)	(0.00038)	(0.00036)	(0.00045)
Dep. Var. Mean	0.029	0.0096	0.012	0.0049
N	1737784	1737784	1737784	548078

Notes: Outcome variables are indicators that equal one if the individual is absent from work or absent from work for the given reason (sick, vacation, or maternity). Includes year fixed effects. Data are from the March 1980-2015 CPS and include individuals age 23-60 employed in the private sector, except for the regression where the outcome is an indicator for whether the individual is on maternity leave, in which case the regression is restricted to women ages 23-44.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 3.5: Linear Probability Models: Relationship between Absence from Work due to Maternity and TDI States

	Years 1980-2015		Years 1980-2003	
	All Women	Have Child <1yr	All Women	Have Child <1yr
TDI State	0.0013***	0.016***	0.00086***	0.014**
	(0.00030)	(0.0059)	(0.00030)	(0.0060)
Age	0.0014***	0.0075	0.00052**	-0.0039
	(0.00021)	(0.0060)	(0.00021)	(0.0077)
Age Squared	-0.000028***	-0.00012	-0.000013***	0.000058
	(0.0000030)	(0.000098)	(0.0000031)	(0.00013)
White	0.00015	0.00027	-0.00016	-0.0022
	(0.00056)	(0.0100)	(0.00069)	(0.013)
Black	0.0015**	0.0035	0.00099	-0.0018
	(0.00065)	(0.012)	(0.00078)	(0.014)
Hispanic	-0.00086**	-0.020***	-0.00068+	-0.021***
	(0.00036)	(0.0065)	(0.00035)	(0.0063)
Married	0.0073***	0.0063	0.0055***	0.024***
	(0.00031)	(0.0083)	(0.00032)	(0.0088)
Sep/Div/Wid	0.0033***	-0.0012	0.0027***	0.019
	(0.00030)	(0.012)	(0.00030)	(0.012)
High School	-0.0000089	-0.0031	0.00035	0.0041
	(0.00034)	(0.0081)	(0.00031)	(0.0071)
Some College	0.0010***	0.010	0.00099***	0.012
	(0.00037)	(0.0085)	(0.00033)	(0.0074)
Bachelor's or more	0.0034***	0.034***	0.0032***	0.040***
	(0.00045)	(0.0097)	(0.00051)	(0.010)
Dep. Var. Mean	0.0049	0.084	0.0032	0.056
N	548078	24078	342489	14340

Notes: Includes year fixed effects. Data are from the March CPS for the years indicated (either 1980-2015 or 1980-2003) and include women age 23-44 employed in the private sector. Three of the TDI states also have recently added paid family leave that can be used after the period of disability due to childbirth. California was the first in 2004, so the regressions that go through 2003 exclude any effects from the paid family leave programs.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 3.6: Have Sick Leave or Sickness Insurance (Means)

State	TDI State Mean	Non-TDI States Mean	Difference	Std. Error	N
All	0.639	0.599	0.041***	0.0085	56,368
CA	0.598	0.599	-0.001	0.0127	49,536
NJ	0.704	0.599	0.105***	0.0138	48,083
NY	0.679	0.599	0.080***	0.0129	49,003
RI	0.644	0.599	0.045	0.0299	46,176
HI	0.735	0.599	0.136***	0.0287	46,178

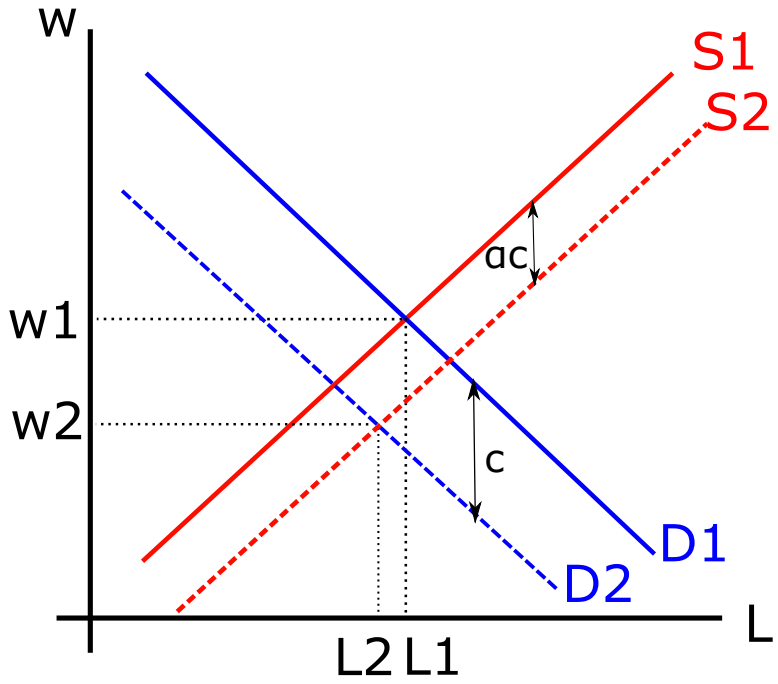
Notes: Table gives mean number of individuals age 23 to 60 in the May 1988 and April 1993 CPS Employee Benefits Supplements who are working in private industry and respond "Yes" to the question: "On your job, do you get any paid sick leave or sickness insurance?" Excludes missing values and those who answered "Don't Know." The "TDI State Mean" column gives the mean for the listed TDI state. CPS supplement weights are used.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 3.7: Have Sick Leave or Sickness Insurance (Regressions)

	All	CA	NJ	NY	RI	HI
TDI State	0.0350***	-0.00147	0.0898***	0.0700***	0.0535*	0.140***
	(0.00827)	(0.0123)	(0.0135)	(0.0125)	(0.0288)	(0.0329)
Age	0.0164***	0.0174***	0.0190***	0.0172***	0.0185***	0.0185***
	(0.00270)	(0.00288)	(0.00288)	(0.00283)	(0.00294)	(0.00294)
Age Squared	-0.000162***	-0.000174***	-0.000193***	-0.000170***	-0.000186***	-0.000185***
	(0.0000331)	(0.0000354)	(0.0000354)	(0.0000348)	(0.0000362)	(0.0000362)
White	0.0524***	0.0620***	0.0692***	0.0441*	0.0646***	0.0544**
	(0.0184)	(0.0215)	(0.0234)	(0.0227)	(0.0250)	(0.0239)
Black	0.0725***	0.0770***	0.0897***	0.0611**	0.0824***	0.0721***
	(0.0214)	(0.0245)	(0.0261)	(0.0253)	(0.0276)	(0.0267)
Married	0.0310***	0.0293***	0.0251**	0.0304***	0.0258**	0.0256**
	(0.00905)	(0.00972)	(0.00978)	(0.00964)	(0.0100)	(0.0100)
Sep/Div/Wid	0.00609	0.00612	-0.00100	0.00353	-0.000153	-0.00149
	(0.0118)	(0.0126)	(0.0127)	(0.0125)	(0.0129)	(0.0129)
High School	0.199***	0.204***	0.191***	0.190***	0.194***	0.192***
	(0.0104)	(0.0109)	(0.0110)	(0.0110)	(0.0113)	(0.0113)
Some College	0.283***	0.286***	0.273***	0.278***	0.276***	0.275***
	(0.0111)	(0.0117)	(0.0119)	(0.0117)	(0.0121)	(0.0121)
Bachelor's or more	0.402***	0.409***	0.393***	0.391***	0.396***	0.395***
	(0.0109)	(0.0115)	(0.0116)	(0.0116)	(0.0119)	(0.0119)
Year=1993	-0.00733	-0.00624	-0.00277	-0.00214	-0.00215	-0.00228
	(0.00624)	(0.00666)	(0.00665)	(0.00657)	(0.00680)	(0.00680)
Male	0.00277	-0.000198	-0.000929	0.00192	-0.00147	-0.00177
	(0.00636)	(0.00677)	(0.00677)	(0.00669)	(0.00692)	(0.00693)
Dep. Var. Mean	0.607	0.599	0.602	0.605	0.599	0.599
N	30832	27558	26918	27294	25995	25975

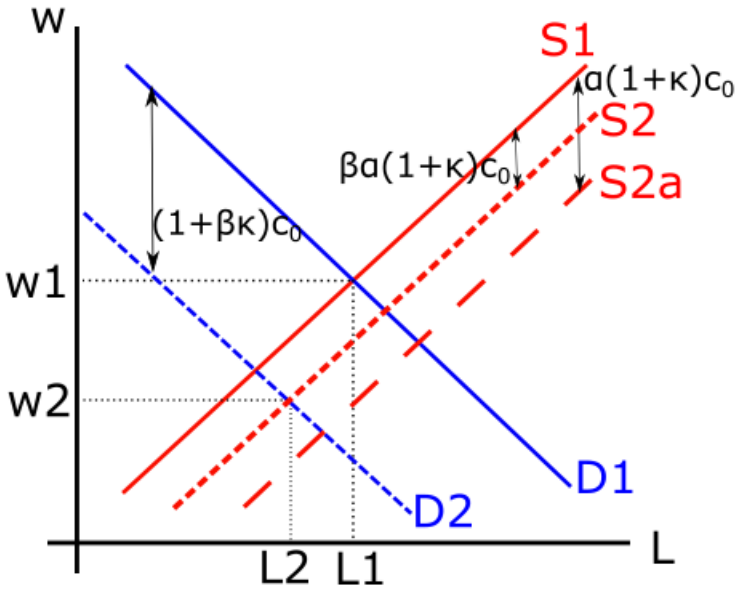
Notes: Includes individuals age 23 to 60 in the May 1988 and April 1993 CPS Employee Benefits Supplements who are working in private industry. CPS supplement weights are used. Dependent variable in all regressions is a dummy variable indicating whether an individual answered "Yes" to the question: "On your job, do you get any paid sick leave or sickness insurance?" Excludes missing values and those who answered "Don't Know." First column includes all states, remaining columns include the listed TDI state and the non-TDI states, excluding non-listed TDI states.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Figure 3.1: Standard Mandated Benefit Model



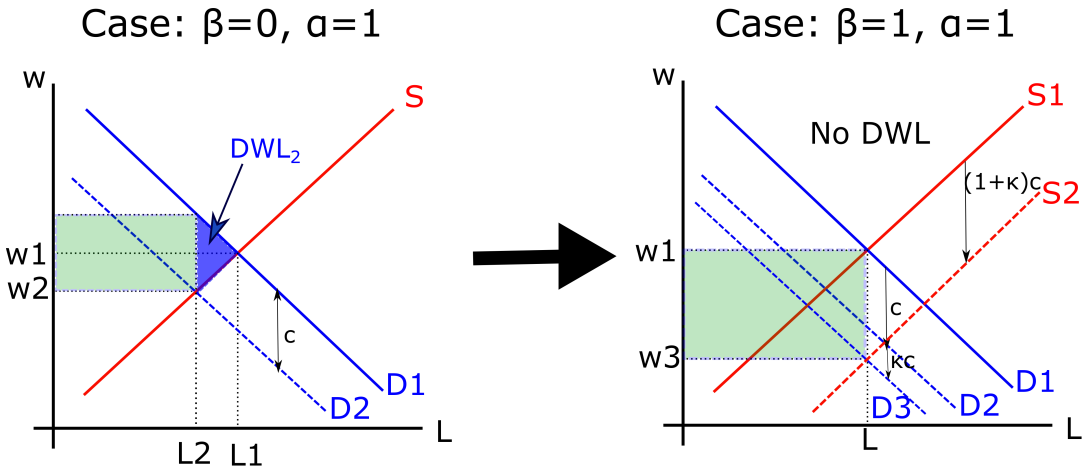
Notes: Figure graphically depicts the standard mandated benefit model where the government mandates employers provide employees with a benefit that costs c per employee, and employees value that benefit at αc.

Figure 3.2: Updated Mandated Benefit Model



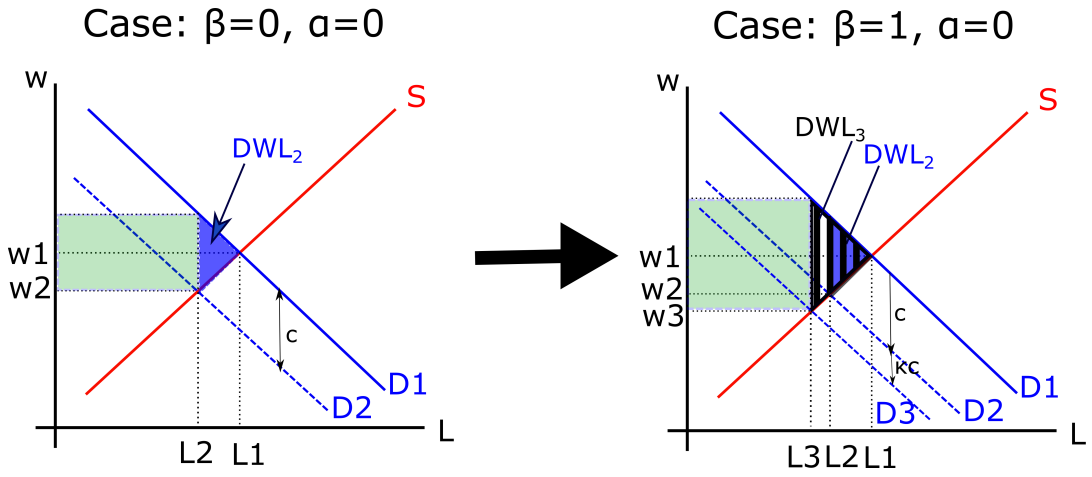
Notes: Figure graphically depicts the updated mandated benefit model where the government mandates employers provide employees with a benefit that costs c_0 per employee who does not know about the program and, due to moral hazard, costs $(1+\kappa)c_0$ per employee who does know about the program. A fraction β of workers know about the benefit, and they value it at $\alpha(1+\kappa)c_0$ each.

Figure 3.3: Comparative Static: Increasing Awareness when Workers Fully Value Benefit



Notes: Figure graphically demonstates the updated model's prediction for the change in deadweight loss when moving from no knowledge ($\beta = 0$) to full knowledge ($\beta = 1$) of the mandated benefit when workers fully value the benefit ($\alpha = 1$). The green shaded region represents the total cost of the program. Increasing awareness eliminates deadweight loss because the supply curve shift offsets the demand curve shift.

Figure 3.4: Comparative Static: Increasing Awareness when Workers Do Not Value Benefit



Notes: Figure graphically demonstates the updated model's prediction for the change in deadweight loss when moving from no knowledge ($\beta = 0$) to full knowledge ($\beta = 1$) of the mandated benefit when workers do not value the benefit ($\alpha = 0$). The green shaded region represents the total cost of the program. Increasing awareness increases deadweight loss because there is no supply curve shift when workers do not value the benefit, but there is an additional demand curve shift from increased costs due to moral hazard.

Figure 3.5: Percent Change in Employment Loss When Move to Full Knowledge of Mandated Benefit

Case 1: Initial β = 0

Worker Valuation (α) \ Moral Hazard (κ)	0	0.1	0.2	0.3	0.4	0.5	0.6	0.7	0.8	0.9	1
0	0%	10%	20%	30%	40%	50%	60%	70%	80%	90%	100%
0.1	-10%	-1%	8%	17%	26%	35%	44%	53%	62%	71%	80%
0.2	-20%	-12%	-4%	4%	12%	20%	28%	36%	44%	52%	60%
0.3	-30%	-23%	-16%	-9%	-2%	5%	12%	19%	26%	33%	40%
0.4	-40%	-34%	-28%	-22%	-16%	-10%	-4%	2%	8%	14%	20%
0.5	-50%	-45%	-40%	-35%	-30%	-25%	-20%	-15%	-10%	-5%	0%
0.6	-60%	-56%	-52%	-48%	-44%	-40%	-36%	-32%	-28%	-24%	-20%
0.7	-70%	-67%	-64%	-61%	-58%	-55%	-52%	-49%	-46%	-43%	-40%
0.8	-80%	-78%	-76%	-74%	-72%	-70%	-68%	-66%	-64%	-62%	-60%
0.9	-90%	-89%	-88%	-87%	-86%	-85%	-84%	-83%	-82%	-81%	-80%
1	-100%	-100%	-100%	-100%	-100%	-100%	-100%	-100%	-100%	-100%	-100%

Case 2: Initial β = 0.25

Worker Valuation (α) \ Moral Hazard (κ)	0	0.1	0.2	0.3	0.4	0.5	0.6	0.7	0.8	0.9	1
0	0%	7%	14%	21%	27%	33%	39%	45%	50%	55%	60%
0.1	-8%	-1%	6%	12%	18%	24%	30%	35%	40%	45%	50%
0.2	-16%	-9%	-3%	3%	9%	14%	20%	25%	30%	35%	39%
0.3	-24%	-18%	-13%	-7%	-2%	4%	9%	14%	18%	23%	27%
0.4	-33%	-28%	-23%	-17%	-12%	-8%	-3%	1%	6%	10%	14%
0.5	-43%	-38%	-33%	-29%	-24%	-20%	-16%	-12%	-8%	-4%	0%
0.6	-53%	-49%	-45%	-41%	-37%	-33%	-30%	-26%	-23%	-19%	-16%
0.7	-64%	-60%	-57%	-54%	-51%	-48%	-45%	-42%	-39%	-36%	-33%
0.8	-75%	-73%	-70%	-68%	-66%	-64%	-61%	-59%	-57%	-55%	-53%
0.9	-87%	-86%	-85%	-83%	-82%	-81%	-80%	-79%	-77%	-76%	-75%
1	-100%	-100%	-100%	-100%	-100%	-100%	-100%	-100%	-100%	-100%	-100%

Case 3: Initial β = 0.5

Worker Valuation (α) \ Moral Hazard (κ)	0	0.1	0.2	0.3	0.4	0.5	0.6	0.7	0.8	0.9	1
0	0%	5%	9%	13%	17%	20%	23%	26%	29%	31%	33%
0.1	-5%	-1%	4%	8%	12%	15%	18%	21%	24%	26%	29%
0.2	-11%	-6%	-2%	2%	6%	9%	12%	15%	18%	21%	23%
0.3	-18%	-13%	-9%	-5%	-1%	2%	6%	9%	12%	14%	17%
0.4	-25%	-20%	-16%	-12%	-9%	-5%	-2%	1%	4%	7%	9%
0.5	-33%	-29%	-25%	-21%	-18%	-14%	-11%	-8%	-5%	-3%	0%
0.6	-43%	-39%	-35%	-32%	-28%	-25%	-22%	-19%	-16%	-14%	-11%
0.7	-54%	-50%	-47%	-44%	-41%	-38%	-35%	-32%	-30%	-27%	-25%
0.8	-67%	-64%	-61%	-59%	-56%	-54%	-52%	-49%	-47%	-45%	-43%
0.9	-82%	-80%	-79%	-77%	-75%	-74%	-72%	-71%	-69%	-68%	-67%
1	-100%	-100%	-100%	-100%	-100%	-100%	-100%	-100%	-100%	-100%	-100%

Case 4: Initial β = 0.75

Worker Valuation (α) \ Moral Hazard (κ)	0	0.1	0.2	0.3	0.4	0.5	0.6	0.7	0.8	0.9	1
0	0%	2%	4%	6%	8%	9%	10%	11%	13%	13%	14%
0.1	-3%	0%	2%	4%	5%	7%	8%	9%	11%	12%	12%
0.2	-6%	-3%	-1%	1%	3%	4%	6%	7%	8%	9%	10%
0.3	-10%	-7%	-5%	-2%	-1%	1%	3%	4%	5%	7%	8%
0.4	-14%	-11%	-9%	-7%	-5%	-3%	-1%	0%	2%	3%	4%
0.5	-20%	-17%	-14%	-12%	-10%	-8%	-6%	-4%	-3%	-1%	0%
0.6	-27%	-24%	-21%	-19%	-16%	-14%	-12%	-11%	-9%	-7%	-6%
0.7	-37%	-34%	-31%	-28%	-26%	-23%	-21%	-19%	-18%	-16%	-14%
0.8	-50%	-47%	-44%	-42%	-39%	-37%	-35%	-33%	-31%	-29%	-27%
0.9	-69%	-67%	-65%	-63%	-61%	-59%	-57%	-55%	-53%	-52%	-50%
1	-100%	-100%	-100%	-100%	-100%	-100%	-100%	-100%	-100%	-100%	-100%

Notes: Figures show the percent change in employment loss when awareness of the mandated benefit (β) increases from the given value to 1. For example, a value of -50% indicates that moving to full knowledge cuts employment loss from the mandated benefit in half.

Figure 3.6: Percent Change in Deadweight Loss When Move to Full Knowledge of Mandated Benefit

Case 1: Initial β = 0

Worker Valuation (α) \ Moral Hazard (κ)	0	0.1	0.2	0.3	0.4	0.5	0.6	0.7	0.8	0.9	1
0	0%	21%	44%	69%	96%	125%	156%	189%	224%	261%	300%
0.1	-19%	-2%	17%	37%	59%	82%	107%	134%	162%	192%	224%
0.2	-36%	-23%	-8%	8%	25%	44%	64%	85%	107%	131%	156%
0.3	-51%	-41%	-29%	-17%	-4%	10%	25%	42%	59%	77%	96%
0.4	-64%	-56%	-48%	-39%	-29%	-19%	-8%	4%	17%	30%	44%
0.5	-75%	-70%	-64%	-58%	-51%	-44%	-36%	-28%	-19%	-10%	0%
0.6	-84%	-81%	-77%	-73%	-69%	-64%	-59%	-54%	-48%	-42%	-36%
0.7	-91%	-89%	-87%	-85%	-82%	-80%	-77%	-74%	-71%	-68%	-64%
0.8	-96%	-95%	-94%	-93%	-92%	-91%	-90%	-88%	-87%	-86%	-84%
0.9	-99%	-99%	-99%	-98%	-98%	-98%	-97%	-97%	-97%	-96%	-96%
1	-100%	-100%	-100%	-100%	-100%	-100%	-100%	-100%	-100%	-100%	-100%

Case 2: Initial β = 0.25

Worker Valuation (α) \ Moral Hazard (κ)	0	0.1	0.2	0.3	0.4	0.5	0.6	0.7	0.8	0.9	1
0	0%	15%	31%	46%	62%	78%	94%	109%	125%	141%	156%
0.1	-15%	-1%	12%	26%	40%	54%	68%	83%	97%	111%	125%
0.2	-29%	-18%	-6%	6%	18%	31%	43%	56%	68%	81%	94%
0.3	-43%	-33%	-23%	-13%	-3%	8%	18%	29%	40%	51%	62%
0.4	-56%	-48%	-40%	-32%	-23%	-15%	-6%	3%	12%	21%	31%
0.5	-67%	-62%	-56%	-49%	-43%	-36%	-29%	-22%	-15%	-7%	0%
0.6	-78%	-74%	-70%	-65%	-60%	-56%	-51%	-45%	-40%	-35%	-29%
0.7	-87%	-84%	-82%	-79%	-76%	-73%	-70%	-66%	-63%	-59%	-56%
0.8	-94%	-93%	-91%	-90%	-88%	-87%	-85%	-83%	-82%	-80%	-78%
0.9	-98%	-98%	-98%	-97%	-97%	-96%	-96%	-95%	-95%	-94%	-94%
1	-100%	-100%	-100%	-100%	-100%	-100%	-100%	-100%	-100%	-100%	-100%

Case 3: Initial β = 0.5

Worker Valuation (α) \ Moral Hazard (κ)	0	0.1	0.2	0.3	0.4	0.5	0.6	0.7	0.8	0.9	1
0	0%	10%	19%	28%	36%	44%	51%	59%	65%	72%	78%
0.1	-10%	-1%	8%	16%	24%	32%	39%	46%	53%	59%	65%
0.2	-21%	-12%	-4%	4%	12%	19%	26%	33%	39%	46%	51%
0.3	-32%	-24%	-17%	-9%	-2%	5%	12%	18%	24%	30%	36%
0.4	-44%	-37%	-30%	-23%	-17%	-10%	-4%	2%	8%	14%	19%
0.5	-56%	-50%	-44%	-38%	-32%	-27%	-21%	-16%	-10%	-5%	0%
0.6	-67%	-63%	-58%	-53%	-48%	-44%	-39%	-34%	-30%	-25%	-21%
0.7	-79%	-75%	-72%	-69%	-65%	-61%	-58%	-54%	-51%	-47%	-44%
0.8	-89%	-87%	-85%	-83%	-81%	-79%	-76%	-74%	-72%	-70%	-67%
0.9	-97%	-96%	-95%	-95%	-94%	-93%	-92%	-92%	-91%	-90%	-89%
1	-100%	-100%	-100%	-100%	-100%	-100%	-100%	-100%	-100%	-100%	-100%

Case 4: Initial β = 0.75

Worker Valuation (α) \ Moral Hazard (κ)	0	0.1	0.2	0.3	0.4	0.5	0.6	0.7	0.8	0.9	1
0	0%	5%	9%	13%	16%	19%	22%	24%	27%	29%	31%
0.1	-5%	-1%	4%	8%	11%	14%	17%	20%	22%	25%	27%
0.2	-11%	-6%	-2%	2%	6%	9%	12%	15%	17%	20%	22%
0.3	-18%	-13%	-9%	-5%	-1%	2%	6%	8%	11%	14%	16%
0.4	-27%	-22%	-17%	-13%	-9%	-5%	-2%	1%	4%	6%	9%
0.5	-36%	-31%	-27%	-22%	-18%	-15%	-11%	-8%	-5%	-3%	0%
0.6	-47%	-42%	-38%	-34%	-30%	-27%	-23%	-20%	-17%	-14%	-11%
0.7	-60%	-56%	-52%	-48%	-45%	-41%	-38%	-35%	-32%	-29%	-27%
0.8	-75%	-72%	-69%	-66%	-63%	-60%	-57%	-55%	-52%	-50%	-47%
0.9	-91%	-89%	-88%	-86%	-84%	-83%	-81%	-80%	-78%	-77%	-75%
1	-100%	-100%	-100%	-100%	-100%	-100%	-100%	-100%	-100%	-100%	-100%

Notes: Figures show the percent change in deadweight loss when the fraction of workers who know about the mandated benefit (β) increases from the given value to 1.

Figure 3.7: Percent Change in Total Employment due to Mandated Benefit under Different Parameter Assumptions

Change in Employment due to Mandated Benefit Costing 1% of Wages

		α = 0.3				α = 0.7			
		κ = 0.4		κ = 0.8		κ = 0.4		κ = 0.8	
ε^D	ε^S	β = 0.25	β = 1	β = 0.25	β = 1	β = 0.25	β = 1	β = 0.25	β = 1
-0.3	0.00	0.000%	0.000%	0.000%	0.000%	0.000%	0.000%	0.000%	0.000%
-0.3	0.05	-0.043%	-0.042%	-0.046%	-0.054%	-0.037%	-0.018%	-0.038%	-0.023%
-0.3	0.10	-0.075%	-0.074%	-0.080%	-0.095%	-0.064%	-0.032%	-0.066%	-0.041%
-0.3	0.20	-0.119%	-0.118%	-0.128%	-0.151%	-0.103%	-0.050%	-0.106%	-0.065%
-0.3	0.30	-0.149%	-0.147%	-0.160%	-0.189%	-0.128%	-0.063%	-0.133%	-0.081%
-0.1	0.00	0.000%	0.000%	0.000%	0.000%	0.000%	0.000%	0.000%	0.000%
-0.1	0.05	-0.033%	-0.033%	-0.036%	-0.042%	-0.029%	-0.014%	-0.030%	-0.018%
-0.1	0.10	-0.050%	-0.049%	-0.053%	-0.063%	-0.043%	-0.021%	-0.044%	-0.027%
-0.1	0.20	-0.066%	-0.065%	-0.071%	-0.084%	-0.057%	-0.028%	-0.059%	-0.036%
-0.1	0.30	-0.075%	-0.074%	-0.080%	-0.095%	-0.064%	-0.032%	-0.066%	-0.041%
-0.5	0.00	0.000%	0.000%	0.000%	0.000%	0.000%	0.000%	0.000%	0.000%
-0.5	0.05	-0.045%	-0.045%	-0.048%	-0.057%	-0.039%	-0.019%	-0.040%	-0.025%
-0.5	0.10	-0.083%	-0.082%	-0.089%	-0.105%	-0.071%	-0.035%	-0.074%	-0.045%
-0.5	0.20	-0.142%	-0.140%	-0.152%	-0.180%	-0.122%	-0.060%	-0.126%	-0.077%
-0.5	0.30	-0.187%	-0.184%	-0.200%	-0.236%	-0.160%	-0.079%	-0.166%	-0.101%

Change in Employment due to Mandated Benefit Costing 5% of Wages

		α = 0.3				α = 0.7			
		κ = 0.4		κ = 0.8		κ = 0.4		κ = 0.8	
ε^D	ε^S	β = 0.25	β = 1	β = 0.25	β = 1	β = 0.25	β = 1	β = 0.25	β = 1
-0.3	0.00	0.000%	0.000%	0.000%	0.000%	0.000%	0.000%	0.000%	0.000%
-0.3	0.05	-0.213%	-0.210%	-0.228%	-0.270%	-0.183%	-0.090%	-0.190%	-0.116%
-0.3	0.10	-0.373%	-0.368%	-0.399%	-0.473%	-0.321%	-0.158%	-0.332%	-0.203%
-0.3	0.20	-0.597%	-0.588%	-0.639%	-0.756%	-0.513%	-0.252%	-0.531%	-0.324%
-0.3	0.30	-0.746%	-0.735%	-0.799%	-0.945%	-0.641%	-0.315%	-0.664%	-0.405%
-0.1	0.00	0.000%	0.000%	0.000%	0.000%	0.000%	0.000%	0.000%	0.000%
-0.1	0.05	-0.166%	-0.163%	-0.178%	-0.210%	-0.143%	-0.070%	-0.148%	-0.090%
-0.1	0.10	-0.249%	-0.245%	-0.266%	-0.315%	-0.214%	-0.105%	-0.221%	-0.135%
-0.1	0.20	-0.332%	-0.327%	-0.355%	-0.420%	-0.285%	-0.140%	-0.295%	-0.180%
-0.1	0.30	-0.373%	-0.368%	-0.399%	-0.473%	-0.321%	-0.158%	-0.332%	-0.203%
-0.5	0.00	0.000%	0.000%	0.000%	0.000%	0.000%	0.000%	0.000%	0.000%
-0.5	0.05	-0.226%	-0.223%	-0.242%	-0.286%	-0.194%	-0.095%	-0.201%	-0.123%
-0.5	0.10	-0.415%	-0.408%	-0.444%	-0.525%	-0.356%	-0.175%	-0.369%	-0.225%
-0.5	0.20	-0.711%	-0.700%	-0.761%	-0.900%	-0.611%	-0.300%	-0.632%	-0.386%
-0.5	0.30	-0.933%	-0.919%	-0.998%	-1.181%	-0.802%	-0.394%	-0.830%	-0.506%

Notes: Figures show the percent change in employment due to a mandated benefit (relative to no mandated benefit) given different assumptions about the parameters α (employee valuation of benefit as a percent of cost to provide), κ (percent increase in cost of providing benefit when workers find out about benefit), β (fraction of workers who know about benefit), ϵ^S (labor supply elasticity), ϵ^D (labor demand elasticity), and $\frac{dc_0}{w}$ (as a percent of wages, the cost of providing workers with benefit when they do not know about it).

Figure 3.8: Deadweight Loss due to Mandated Benefit under Different Parameter Assumptions

Deadweight Loss (per $1 million) due to Mandated Benefit Costing 1% of Wages

		α = 0.3				α = 0.7			
		κ = 0.4		κ = 0.8		κ = 0.4		κ = 0.8	
ε^D	ε^S	β = 0.25	β = 1	β = 0.25	β = 1	β = 0.25	β = 1	β = 0.25	β = 1
-0.3	0.00	0	0	0	0	0	0	0	0
-0.3	0.05	2	2	2	3	2	0	2	1
-0.3	0.10	4	4	4	6	3	1	3	1
-0.3	0.20	6	6	7	10	4	1	5	2
-0.3	0.30	7	7	9	12	5	1	6	2
-0.1	0.00	0	0	0	0	0	0	0	0
-0.1	0.05	2	2	2	3	1	0	1	0
-0.1	0.10	2	2	3	4	2	0	2	1
-0.1	0.20	3	3	4	5	2	1	3	1
-0.1	0.30	4	4	4	6	3	1	3	1
-0.5	0.00	0	0	0	0	0	0	0	0
-0.5	0.05	2	2	3	4	2	0	2	1
-0.5	0.10	4	4	5	7	3	1	3	1
-0.5	0.20	7	7	8	11	5	1	6	2
-0.5	0.30	9	9	11	15	7	2	7	3

Deadweight Loss (per $1 million) due to Mandated Benefit Costing 5% of Wages

		α = 0.3				α = 0.7			
		κ = 0.4		κ = 0.8		κ = 0.4		κ = 0.8	
ε^D	ε^S	β = 0.25	β = 1	β = 0.25	β = 1	β = 0.25	β = 1	β = 0.25	β = 1
-0.3	0.00	0	0	0	0	0	0	0	0
-0.3	0.05	53	51	61	85	39	9	42	16
-0.3	0.10	93	90	106	149	69	17	73	27
-0.3	0.20	149	144	170	238	110	26	117	44
-0.3	0.30	186	180	213	298	137	33	147	55
-0.1	0.00	0	0	0	0	0	0	0	0
-0.1	0.05	41	40	47	66	30	7	33	12
-0.1	0.10	62	60	71	99	46	11	49	18
-0.1	0.20	83	80	95	132	61	15	65	24
-0.1	0.30	93	90	106	149	69	17	73	27
-0.5	0.00	0	0	0	0	0	0	0	0
-0.5	0.05	56	55	64	90	42	10	45	17
-0.5	0.10	103	100	118	165	76	18	82	30
-0.5	0.20	177	172	203	284	131	32	140	52
-0.5	0.30	232	225	266	372	171	41	184	68

Notes: Figures show the deadweight loss (per $1 million of payroll) due to a mandated benefit (relative to no mandated benefit) given different assumptions about the parameters α (employee valuation of benefit as a percent of cost to provide), κ (percent increase in cost of providing benefit when workers find out about benefit), β (fraction of workers who know about benefit), ϵ^S (labor supply elasticity), ϵ^D (labor demand elasticity), and $\frac{dc_0}{w}$ (as a percent of wages, the cost of providing workers with benefit when they do not know about it).

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