ESSAYS IN PUBLIC AND LABOR ECONOMICS

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ABSTRACT

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Chapter 1: “State Responses to a Federal Matching Grant and Adoption from Foster Care.” The federal government reimburses states for adoption assistance – monthly cash subsidies – to families of children adopted from foster care through an open-ended matching grant. In this paper, I estimate the effect of states’ responses to the matching grant on foster children’s adoption outcomes. These outcomes are the likelihood of adoption, timing of adoption, and state designation of “special needs” to entitle children to federal grant support. To identify causal state responses, I exploit variation in children’s federal eligibility for grant support from expanded federal criteria introduced in 2010 that continues for children of different ages through 2018. First, I find that federal eligibility for federal grant support increases the probability of adoption only modestly, by about 9 percent. Second, I find the structure of the rollout of new criteria creates short-run distortions in state behavior with delays in adoptions for 1–3 months until children are of an age to qualify. Third, I do not find that state governments specifically designate children special needs to claim federal grant support. Overall, because the federal matching rates vary from 50 to 83 percent, representing large decreases in the state’s cost of adoption assistance, these results imply a small state response.

Chapter 2: “The Effects of State Adoption Incentive Awards for Older Children on Adoptions from U.S. Foster Care.” This paper uses changes in the United States federal Adoption Incentives program in 2003 and 2008 to analyze states’ response to federal incentives to increase adoptions of children in the U.S. foster care system. The 2003 change introduced a

$4,000 incentive paid to states for every adoption of a child aged 9 and older above a state-specific baseline number of adoptions. The 2008 change doubled this incentive to $8,000. I use a semi-parametric hazard model to compare the probability of adoption and timing of adoption among children aged above and below 9 years old in the time periods before and after the 2003 and 2008 changes. I do not find robust evidence that the incentives for older child adoptions resulted in increases in adoptions for older children. The findings illustrate the incentives are unable to help states overcome many of the challenges associated with achieving adoption for older children.

Chapter 3: “Capitalization of Charter Schools into Residential Property Values.” While prior research has found clear impacts of schools and school quality on property values, little is known about whether charter schools have similar effects. Using sale price data for residential properties in Los Angeles County from 2008 to 2011 we estimate the neighborhood level impact of charter schools on housing prices. Using an identification strategy that relies on census block fixed-effects and variation in charter penetration over time, we find little evidence that the availability of a charter school affects housing prices on average. However, we do find that when restricting to districts other than Los Angeles Unified and counting only charter schools located in the same school district as the household, housing prices fall in response to an increase in nearby charter penetration.

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CHAPTER 1

State Responses to a Federal Matching Grant and Adoption from Foster Care

1.1 Introduction

In 1980, the federal government started two programs to promote adoption of children in U.S. foster care: monthly adoption assistance payments to adoptive families and an open-ended federal matching grant to states. Adoption is considered a better placement than long-term foster care for children who cannot be reunified with their parents. Compared to their peers in long-term foster care, adopted children have better educational, psychological, and labor-market outcomes and are less likely to experience negative outcomes, such as delinquency and welfare receipt (Barth 1997; Triseliotis 2002; Wulczyn et al. 2007; Hansen 2008).

The federal matching grant supports adoptive placements by reimbursing states for monthly adoption assistance paid to adoptive families. Existing research provides evidence that adoptive families respond to monthly adoption assistance – payments made directly to families increase adoption rates and reduce time spent in foster care (Hansen 2007b; Argys and Duncan 2013; Buckles 2013). However, little is known about whether the federal matching grant – which supports more than half of state expenditures – increases adoption.

In this paper, I estimate the effect of the federal matching grant on adoption from foster care. I consider the probability of adoption and the timing of adoption to estimate the extent to which the matching grant causes real changes in adoption versus administrative changes to maximize federal grant payments to the state. I also look at state designation of "special needs" that entitle children to federal grant support, as children qualify for matched adoption assistance by meeting state-specific definitions of "special needs" and federal eligibility criteria.

Before 2010, children were federally eligible only if their removal family met their state's income standards of the now defunct Aid to Families with Dependent Children (AFDC) program. Many in the child welfare arena argued that it made no sense to distribute federal funds on the basis of income – especially the income of a child's parents whose legal rights have been severed.[1] While low-income is a strong predictor of child abuse and neglect, children in foster care have been removed from homes that do not provide a safe and nurturing environment and the ultimate goal is to place all children in stable permanent living situations.[2] Also concerning was that the income standards were from 1996 and had not been adjusted for inflation over the last two decades. Sweeping – and, as some would argue, long overdue – changes to the AFDC-based federal eligibility came into place in 2010, and are celebrated for making great strides in ensuring more children in foster care are living with safe, permanent families.[3] The changes enhance the existing federal matching grant by extending federal eligibility for federal grant support to all children in foster care, regardless of their biological family's income.

My research design exploits the 2010 changes in children's federal eligibility to identify causal state responses to the matching grant, which lowers the state's cost of adoption assistance. In 2010, children age 16 and older and all children spending five continuous years in foster care became federally eligible.[4] The age threshold falls by two years each fiscal year (such that children aged 14 and older were eligible in 2011, and so on). The rollout of new federal criteria generates exogenous variation in the state's cost of adoption assistance as children meet the new

[1] See, for example: McDonald et al. (2004); Pew Charitable Trusts (2004); US DHHS (2005); Boo (2008); US CRS (2008b); The Annie E. Casey Foundation (2013).
[2] There is a vast literature on the association between family income and child maltreatment. See for example, Paxson and Waldfogel (2002), Slack et al. (2004), Slack et al. (2007) and Sedlak et al. (2010).
[3] See footnote 1.
[4] A third introduced pathway to federal eligibility is being a sibling of a child who meets the new age or duration criteria. Due to lack of data on sibling relationships, my analysis relies on variation in new federal eligibility based on age and duration.

criteria at different ages, durations, and in different years. I use individual-level administrative data on the universe of children in foster care from 2000 to 2014. To estimate how federal eligibility relates to the probability of adoption, I use a discrete time duration specification to allow for time-varying federal eligibility and account for time spent in foster care.

I find that children's federal eligibility for federal grant support increases the probability of adoption by about 9 percent. The estimated increase is larger among children not already receiving federal grant support for foster care placements (11.5 percent) and for children in states facing larger incentives from the match (sometimes as large as 18 percent). I also find that some of the estimated increase in the probability of adoption can be explained by states delaying adoptions. The rollout creates incentives to delay adoptions for each subsequent age group, starting with children 16 and above, around the start of each fiscal year. By delaying adoptions, states can realize more federal funds opportunistically and without necessarily increasing real adoptions. Using state monthly counts of adoption by age, I find short-run distortions in state behavior with delays in adoptions by 1–3 months so that children are of an age to qualify. However, I find no evidence that states designate children as special needs to generate federal grant support, another margin where states can act opportunistically.

In light of large match rates that vary from 50 to 83 percent, representing a large decrease in the state's cost of adoption assistance, the 9 to 18 percent increase in the probability of adoption suggests a small state response. This is consistent with estimated price elasticities of state spending to federal matching dollars in the AFDC, Medicaid and education literature.[5, 6] The

[5] See, for example Feldstein (1975); Orr (1976); Grannemann and Pauly (1983); Cromwell et al. (1986); Moffitt (1984,1990); Chernick (1998, 2000); Ribar and Wilhelm (1999); Fisher and Papke (2000); Adams and Wade (2001); Baicker (2005); Howard (2010). This research finds a range of estimates of the price elasticity of state spending. The variation in estimates is due to data over different time periods, varying definitions of the price, and differences in econometric specification.

finding that states respond to the matching grant with changes in timing of adoptions is consistent with previous research showing the timing of economic transactions is responsive to financial incentives (Dickert-Conlin and Chandra 1999; Gans and Leigh 2009; Neugart and Ohlsson 2013; Lalumia, Sallee and Turner 2015).

Although the estimated effect of the rollout on adoption is small, the expansion in children's federal criteria appears to be a reasonable policy for increasing adoptions. The magnitudes of the estimates are consistent with those from previous studies estimating the effect of adoption assistance made directly to families on adoption (Hansen 2007b; Buckles 2013). This suggests that the effect of reducing the cost of adoption to the state on adoption is similar to the effect of reducing the cost of adoption for adoptive families. Based on a simple cost-benefit analysis, the policy also seems relatively cost-effective.

The paper is organized as follows. In section 2, I provide an overview of the matching grant and describe the recent changes in children's federal criteria for grant support to states for adoption assistance. I next discuss the financial incentives embedded in the matching grant and predictions about how state responses might affect children's adoption outcomes (section 3). I describe the administrative data I use in section 4. In section 5, I detail the variation in children's federal eligibility that I exploit as a natural experiment in changes to the state's cost of adoption assistance; outline the empirical strategy to estimate effects on adoption; and report results. In sections 6 and 7, I estimate state responses to the matching grant with respect to the timing of adoption and special needs designation. Section 8 concludes.

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[6] My analysis is also related to a strand of literature on the "flypaper effect." Much of this work considers the effect of lump-sum federal grants on state spending, though some work does consider the flypaper effect in the context of a matching grant. See, for example Moffitt (1984), Megdal (1987), and Baker, Payne, and Smart (1999).

1.2 Background

In this section, I provide background information relevant to my analysis. First, I describe the matching grant that supports state expenditures on adoption assistance. Second, I describe the changes in children's federal eligibility criteria that I use to estimate causal state responses to the cost of adoption assistance.

The largest source of federal funds to states for child welfare services is the Title IV-E open-ended matching grant. The matching grant was established in 1980 through the Adoption Assistance and Child Welfare Act (AACWA). In aggregate, states receive federal funds that pay for about half of all costs related to adoption. These costs include training for caseworkers, administrative costs to place children in adoptive homes, and expenditures to provide monthly adoption assistance to adoptive families to meet children's needs (Hansen and Hansen 2006; Hansen 2007b).

The grant matches funds for adoption assistance made on behalf of federally eligible children with special needs. The federal definition of special needs is a factor or condition that makes an adoptive placement difficult without financial assistance to the adoptive family. However, states have discretion to refine this definition. Circumstances qualifying for special needs often include older age, disability, member of a sibling group to be adopted together, or a birth or family history that places the child at increased risk of physical, mental or emotional difficulties in the future.[7] Nationally, over 85 percent of children adopted in 2013 were designated special needs.[8]

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[7] For a broad summary, see US CRS (2012a). Individual state profile pages provided by the North American Council on Adoptable Children describe detailed state special needs definitions. Buckles (2013) provides a table of the minimum age criteria for special needs designation by states.

[8] Author's tabulations from 2013 AFCARS Adoption File. Among children adopted over 2013, 9 percent were designated special needs for racial/original background; 14 percent for age; 27 percent for sibling group membership; 21 percent for medical or other disabilities; and 16 percent for other reasons. While children can be designated special needs for multiple reasons, the AFCARS Adoption File provides the main reason for designation.

The matching grant was created to support states in meeting the AACWA's requirement that states provide adoption assistance on behalf of special needs children (P.L. 96-272).

Adoption assistance is monthly cash payments made directly to adoptive families.[9] Adoptive families receive monthly adoption assistance until a child reaches the age of majority in their state, with age 21 as the maximum age permitted under federal law.[10] States must provide these payments for children with special needs, but many children without special needs also receive adoption assistance. Amounts are typically negotiated individually for each adopted child between adoptive families and state child welfare agencies at the time of adoption finalization and stipulated in a written legal agreement.[11] The only federal limitation is that the amount may not exceed what the child would receive if they remained in foster care (Section 473(a)(3) of the SSA). Generally, rates are based on the needs of the child, the circumstances of the adoptive family, and published rate guidelines in state administrative code or child welfare agency procedure manuals (US CRS 2012a). For example, Maine provides five rates that range from \$502 for children with minimum needs to \$1,996 for children with severe needs.[12] In contrast,

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[9] Other cash and in-kind benefits to adoptive families include lump-sum payments up to \$2,000 at the time of adoption, Medicaid coverage for the adopted child, and post-adoption support in the form of counseling, training, and other support groups.

[10] In 7 states rates are unequivocally not extended past age 18 (Alaska, Florida, Indiana, Louisiana, Nevada, New Hampshire, and North Carolina). The remaining states provide extended subsidies (to 19 or 21) conditional on extenuating circumstances, such as if the child has a medical disability, is pursuing further education, or if the adoption assistance is federally matched. See the "Summary of State Adoption Assistance Programs" page on the North American Council of Adoptable Children's site for an overview: http://www.nacac.org/adoptionsubsidy/summary.html.

[11] For more information on this process and subsidy agreements, see "Eligibility and Benefits for Federal Adoption Assistance" (http://www.nacac.org/adoptionsubsidy/factsheets/title_IV.html) and "Negotiating Title IV-E Adoption Subsidy Agreements". (http://www.nacac.org/adoptionsubsidy/factsheets/negotiating.html), both provided on the website of the North American Council on Adoptable Children.

[12] Payment ceilings in Maine are the foster care rates (Chapter 14. Section 10, 148, Chapter 13, Rules for the Adoption Assistance Program, State of Maine Rule Chapters for the Department of Health and Human Services), published in the State of Maine Rules for Levels of Care for Foster Homes, Section 10 148, Chapter 14. The rules were initially effective in 2003 and amended in 2008.

Kansas provides for a maximum amount of \$500, with an exception for children eligible to receive a higher SSI (Supplemental Social Security) rate.[13]

Figure D.1 shows the gradual increase in total federal and state outlays for matched adoption assistance between 2002 and 2012. Over this time period, total allocations increased to roughly \$4 billion with federal spending of about \$2.3 billion (in 2012 dollars).[14] As noted by Buckles (2013), the majority of the increase in federal spending is from increases in the number of children receiving matched assistance, as opposed to increases in the average amount received. In 2002, there were 285,600 children receiving matched assistance; as of 2012, about 425,000 children received matched adoption assistance.

The level of federal support to states varies based on the state-specific match rate. States are reimbursed at the Federal Medical Assistance Percentage (FMAP) match rate used to determine federal matching funds to states for Medicaid. The FMAP is updated annually depending on the state's per capita income and cannot fall below 50 or exceed 83 percent. For example, with an FMAP of 72 percent, for every dollar a state spends in adoption assistance payments on behalf of an eligible child, the federal government reimburses the state 72 cents. The formula for the FMAP rate is:

$$FMAP_{state} = max\left\{0.5, 1 - 0.45\left(\frac{PCPI_{state}}{PCPI_{US}}\right)^2\right\}$$

[13] PPS Policy and Procedure Manual Printed Documentation for July 1, 2015, Kansas Department for Children and Families, 6202. This rate was increased from \$400 to \$500 in July 2007 (personal communication with the agency). As a comparison, the SSI rate has increased from \$545 in 2002 to \$721 in 2014.

[14] For comparison, the Supplemental Nutrition Program for Women, Infants, and Children (WIC) has received over \$6 billion in federal funds annually since 2008, supporting a caseload of about 8 to 9 million (USDA). State expenditures on the Children's Health Insurance Program (CHIP) (\$3.5 billion in 2010) are comparable to state Title IV-E adoption assistance expenditures, but with much higher federal match rates, federal spending on CHIP is much higher than on the adoption assistance subsidy program (Pew Charitable Trusts 2014).

where *PCPI* is the per-capita personal income.[15] The formula ensures that states with a higher per capita income receive a lower match rate and vice versa. To moderate fluctuations in a state's match rate over time, the PCPI values used in the formula are the average of the three most recent calendar years of data from the Department of Commerce. There is no dollar limit on the amount of matching funds states can claim.

1.2.1 Expansions in Children's Federal Eligibility for Matched Adoption Assistance

Historically, children's federal eligibility for matching funds was linked to the Aid for Families with Dependent Children Program (AFDC). Even with the repeal of AFDC in 1996, federal eligibility remained tied to AFDC such that children were eligible if their removal family qualified as a needy family under AFDC as it existed in their state on July 16, 1996, without adjustment for inflation. Under a second pathway, children could be federally eligible if the child met the medical, disability, income, and resource requirements of the Supplemental Security Income (SSI) program (US CRS 2012a).

This linkage with AFDC generates variation in federal grant support across states and within a state over time for state spending on adoption assistance. Income cutoffs for cash welfare vary across states and in general are quite low. The income limit ranges from \$3,840 annually in Indiana to \$24,408 annually in New Hampshire, with an average of \$8,344 and median of \$7,740 across all states. This cross-state variation in income limits means two children in different states with the same removal family income may have different federal eligibility. Second, the income cutoffs have not been adjusted for inflation, leading to erosion of federal support as fewer children meet the cutoffs. Consider that the median state need standard decreased from 60 percent of the federal poverty guideline for a family of 3 in 1996 to 41 percent of the federal poverty guideline in 2012 (US CRS 2012a).

[15] Section 1905(b) of the Social Security Act. See also US CRS (2013b).

Changes to federal eligibility began in fiscal year 2010 and will replace the income tests over a nine-year period.[16] Table A.1 details the phase-in criteria with respect to a child's age, length of time in care, and membership of a sibling group. For the purposes of the rollout, a child's age is measured at the end of the federal fiscal year. Beginning at the start of fiscal year 2010, all children age 16 and older at the end of fiscal year 2010 were eligible.[17] With each fiscal year, the age drops by two years so that by 2018 every child meets federal criteria. In addition, beginning in 2010, any child in foster care for 60 *continuous* months or more and any sibling of a child meeting the new age or length of stay criteria meets federal criteria.[18]

While there are no official published explanations for the federal criteria expansions, there are several reasons to believe the changes to the federal criteria were intended to increase adoption rates. In the act introducing them, the changes are written up in a section titled "Improvements of incentives for adoption" (P.L. 110-351). Second, the rollout over time provides new eligibility first for older children and children with long duration in care – groups that historically have lower adoption rates than younger children (US CRS 2003; Maza 2009). Prioritizing these groups suggests federal grant support for adoption assistance matters for adoption. Finally, by 2018, the "de-linking" of the grant from the 1996 AFDC income standards is because the Senate Finance Committee saw income as "an inappropriate eligibility factor" (Pew Charitable Trusts 2004; US CRS 2008a). As with the choice of the order of new federal eligibility, this suggests that differences in federal grant support across children with incomes

[16] The Fostering Connections to Success and Increasing Adoptions Act of 2008 – P.L. 110-351.
[17] The federal fiscal year begins on October 1 of the prior calendar year and ends on September 30. The rollout began on October 1, 2009. As an example, if a 15-year-old child is adopted on October 15, 2009, and this child will turn 16 on January 3, 2010, then the age eligibility rules apply. However, this would not be the case if this child's birthday were instead on October 5, 2010. Then this child would not reach age 16 by the end of the fiscal year of adoption, which is September 30, 2010.
[18] For more detail on the rollout, see US CRS (2012a).

above and below the AFDC cutoffs might result in differential spending and adoption rates between these two groups of children.

1.3 Expected State Responses to Federal Spending on Adoption Assistance

In this section, I lay out the changes to the cost of adoption assistance to states created by the matching grant to illustrate why federal spending might induce adoptions. I next discuss how the financial incentives of the matching grant might lead to opportunistic state responses to increase federal funding to states with 1) shifts in the timing of adoption over the multi-year rollout of new federal criteria and 2) labeling adopted children special needs.

1.3.1 Adoption

The federal match affects a child's path to adoption by directly reducing the state's cost of adoption assistance. How much the matching grant affects adoption depends on the elasticity of the state's response to the price of a dollar of adoption assistance. The state budget constraint, where states allocate resources between adoption assistance and other expenditures, is rotated outward when the marginal price of adoption assistance falls. Under the matching grant, an additional \$1 spent on matched adoption assistance now costs \$$(1-\pi)$ to the state where π is the match rate.

For children newly federally eligible beginning in 2010, the state cost of maintaining an adoptive placement compared to foster care placement falls substantially, both in absolute terms and relative to that for children who are not able to receive federal grant support. Table A.2 details examples of how the matching grant reduces the cost of monthly adoption assistance payments to the state. The examples highlight how the amount of the transfer to adoptive families and the applicable federal match rate determine the state cost of adoption assistance. In Massachusetts, without any federal support, the state bears the full cost of the monthly $1,056 adoption assistance. However, with a 56.2 match rate, a federally eligible special needs child is supported by $593 in federal funds. In West Virginia, with a higher match rate of 80.24 and a lower subsidy amount of $623, the cost to the state is $123 with the federal match. In addition to estimating the aggregate effect of children's match eligibility on adoption outcomes, I use this variation across states in match rates and spending to estimate heterogeneous state responses.

Table A.3 illustrates the size of these cost reductions by calculating the difference between the present discounted value of the cost to states for a stream of monthly adoption assistance payments for children with and without the federal match. For example, with a monthly amount of $500 paid until a child turns 18 and a match rate of 50 percent, the present discounted value of the state's cost for an adopted 10 year old is $40,718, compared to $20,359 if the state receives matching funds. The total cost to the state is further reduced for children adopted at younger ages; for an adopted 5 year old receiving $500 monthly and a 50 percent match rate, the savings to the state from the match is $29,590. With the highest match rate of 83 percent, this amount in the same scenario is nearly $50,000. Savings increase proportionally with the monthly amount to families.

These large cost savings from the change in children's federal eligibility could result in new adoptions among newly eligible children that would not have taken place in the absence of the match. One way for states to finalize new adoptions is to increase the supply of adoptive families. To do so, states can persuade long-term foster families to adopt a child already in their care. States may also increase the supply of adoptive families through increased efforts and expenditures to recruit adoptive families for waiting eligible children. Adoptions among newly eligible children may also increase if state caseworkers substitute efforts toward children eligible for federal grant support and away from children for whom the state is unable to claim matching federal funds.

1.3.2 Timing of Adoption

The structure of the rollout and open-ended nature of the matching grant provides incentives to states to delay particular adoptions until after the new federal criteria apply. With or without new eligibility inducing new adoptions that would not have otherwise taken place, timing can increase the amount of federal funds states receive.

The incentive to delay exists within a small window around the start of each fiscal year for each subsequently targeted age group. For example, delaying the adoption of a 16-year-old from September to October in 2009 results in savings to the state, as illustrated in Table A.3. After this window around October 2009, states next face an incentive to delay adoptions of children ages 14 and 15 around October 2010, and so on as the age threshold drops at the start of each fiscal year. Additionally, after the start of fiscal year 2010 there is an incentive to delay around when a child has spent five years in foster care.

Many children live with their adoptive families prior to finalizing an adoption so it is unclear how much of an effect changing the timing of adoption has on the child and adoptive family's welfare. One anonymous source in a large state said the state did delay adoptions that were already going to take place until after the new federal criteria applied in order to capitalize on large savings to the state. The source also noted that doing so does not negatively affect children.

There is a large body of evidence that the timing of economic transactions, more so than real decisions, is responsive to financial incentives, especially when merely a change in the date of an event induces a benefit (Slemrod 1992).[19] In particular, related to the timing of adoption, prior

[19] Examples of intertemporal shifting include capital gains realizations (Auerbach and Porterba 1988), foreign direct investment (Slemrod 1990) and charitable donations (Clotfelter 1990).

research finds that birth timing is responsive to tax incentives and pecuniary bonuses (Dickert-Conlin and Chandra 1999; Gans and Leigh 2009; Neugart and Ohlsson 2013; LaLumia, Sallee and Turner 2015).

1.3.3 Designating Children Special Needs

A second opportunistic state response to the matching grant is for states to generate the federal match by classifying *adopted* children special needs. The marginal cost to states of special needs designation is the mandatory cost of providing adoption assistance to the adoptive family and health insurance coverage via Medicaid or a comparable state plan.[20] The increase in the match rate for an individual child already meeting federal eligibility criteria reduces this cost of special needs designation.

Existing research provides evidence of changes in labeling to induce financial benefits. Hansen (2007a) provides descriptive evidence that the large increase in state Title IV-E claims in the late 1990's and early 2000's were accompanied by a similarly large increase in the share of adoptions designated special needs. This suggests states increased special needs designation rates to increase claims for matching federal funds. However, this is not causal evidence that states respond to the matching grant with special needs designation. Without exogenous changes in Title IV-E claims, it is not clear increases in special needs designation rates are attributable to the matching grant, rather than to changes in the demographic characteristics of adopted children – real changes in special needs – or simultaneous changes in how states designate children special needs – e.g. more expansive definitions, an increase in diagnoses, or improvements in recording

[20] Section 1902(a)(10)(A)(i)(I) and Section 473(b)(3)(A) of the Social Security Act and The Adoption Assistance and Child Welfare Act of 1980, P.L. 96-272; Section 471(a)21 of the Social Security Act. See also US CRS (2012c). Other financial costs to special needs designation may include medical or psychological assessments, as well as other administrative costs to document special needs designation for records and data collection. It is unlikely that adoptive parents object to special needs designation – adoptive parents can claim the federal adoption tax credit for special needs children. This tax credit was $13,190 (non-refundable) in 2014.

needs. Related to special needs designation, prior research finds that school districts respond to special education financial incentives to label children as disabled, increasing special education enrollment rates with more generous funding (Cullen 2003; Mahitivanichcha and Parrish 2005; Kwak 2010).

1.4 Data

I use data from the Adoption and Foster Care Analysis and Reporting System (AFCARS) Foster Care and Adoption files from fiscal years 2000 to 2014. These are annual administrative data on the universe of children who are in foster care and adopted through the state child welfare agency, and are reported by states to the Children's Bureau of the Administration on Children, Youth, and Families.

The Foster Care files provide an annual roster of all children in foster care over the previous fiscal year. As a child has records for each year they spend time in care, I create a panel of spells by matching children's records across years based on the child's state, date of birth, gender, date of first removal, and record number.[21] I identify duplicate observations for each child and use the most recent record, which updates the information from previous years.

I use spells in foster care for children whose mother's rights are terminated. This setup assumes the time of mother's rights termination is when a child is first "at risk" for adoption. I measure spell duration in quarters from the date of mother's rights termination until a child's exit from care. Exits can be to a placement of adoption, emancipation, living with a guardian or relative, transferring to a different state agency, runaway, death, or reunification with birth

[21] This procedure is similar to Buckles (2013). Record numbers are insufficient to match children over time because they are not unique to one child. Record numbers are intended to link children in the AFCARS Foster Care records and AFCARS Adoption records. Over 2000-2013, for children who were still in care at the end of the fiscal year, I am able to match to a unique record in the following year (2001-2014) 87.7 percent of the time.

parents.[22] Spells are right-censored if a child remains in care. I measure duration in quarters rather than months for computational reasons; the sample size, described further below, is determined by the number of periods that define each child's spell. To protect privacy in the Foster Care files, the date of birth is recoded to the 15th of birth month and all other dates – including the date of first entry into foster care, dates of parents' rights termination, and date of exit – are similarly adjusted by up to two weeks before or after the actual date. Fortunately, this preserves the spell duration as well as total duration in foster care from the date of first entry and the child's age at the end of the fiscal year across the spell, both of which I use to determine whether children meet the new federal eligibility criteria.

The Foster Care files contain additional demographic information for each child. These include race/ethnicity and disability information and a binary variable indicating whether or not the child receives federally matched foster care payments. One of the qualifying criteria for receiving federally matched foster care payments is removal from an AFDC-eligible home, the same income test used in determining eligibility for matched adoption assistance. I use this variable as a proxy for whether or not children are federally eligible for matched adoption assistance through the low–income pathway. This income criterion for federal grant support for foster care payments was not affected by the 2010 rollout of new federal criteria for matched adoption assistance.

The Adoption files provide an annual record for all children adopted from foster care over the previous fiscal year. The data include special needs designation, which is not in the Foster Care files. The Adoption files also record the exact date of adoption.

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[22] Currently, several states have legislation in place allowing for the reinstatement of parental rights following termination (National Conference of State Legislatures 2012).

1.5 Effect of the Federal Matching Grant on Adoption

In this section, I examine the link between the federal matching grant to states and adoption. I describe the analytic sample of spell data that allows me to take advantage of time-varying covariates – in particular, children's federal eligibility for federal grant support for adoption assistance. As this variation in eligibility is the basis of my identification strategy, I highlight the scale of this variation and explain how the rollout of new federal criteria creates a natural experiment in changes to the cost of adoption assistance. I then describe my econometric methodology to estimate the effect of the state's cost of adoption assistance on the probability of adoption, and present results.

1.5.1 Sample

To relate children's federal eligibility to their adoption outcome, I use a discrete time hazard model. The hazard model allows me to appropriately incorporate censored spells and model duration dependence: the likelihood of exiting to an adoptive home depends on elapsed time already spent in care. To do this, the data containing the start and ending dates of each spell are expanded such that each observation is a child–quarter in care and each child has as many observations as they spend quarters in care from mother's rights termination (until they exit or the spell is censored). Once a child exits care, the child is subsequently no longer in the sample. I restrict the sample to first-time spells in foster care. Additionally, to avoid length-biased sampling, I follow Heckman and Singer (1984) and restrict the sample to only spells that begin after October 1, 1999, or the start of fiscal year 2000.[23]

Table A.4 provides summary information for the 618,150 spells in the sample from 2000 through 2014. In the full sample, children are typically nearly 5 years old at mother's rights

[23] While the first public AFCARS data files are from fiscal year 1995, data are missing from many states until fiscal year 1999 and fiscal year 2000 is the first year with complete data for all states.

termination and an average spell is about 6 quarters from mother's rights termination. About 11 percent of all children become newly federally eligible based on their age in the applicable year of the rollout or spending 60 continuous months in care. Over the sample period, 81 percent of spells end in an exit to an adoptive home. The second column provides summary statistics for the sample of spells ending in adoption. Sample means are similar to those for the full sample, with similar demographic characteristics.

Sample means in columns 3 and 4 are for the 11 percent of children who experience new federal eligibility through the introduction of age and duration criteria over their spell in care ("newly federally eligible"). These children are older at the spell start (10.30 years old) than children who are not newly age or duration eligible (4.23 years old) and spend much longer in foster care overall, five years (19.95 quarters) compared to fewer than three years (11.15 quarters). Newly federally eligible children are also less likely to be adopted: 48 percent of this group are adopted and 16 percent are emancipated, compared to 86 percent and 2 percent, respectively, for children who do not experience new federal age or duration eligibility over their spells.

This stark contrast in the adoption outcomes across the two groups in part reflects the negative correlation between both age and duration and adoption rates. Figure D.2 plots empirical hazards separately by age at mother's rights termination (5–7, 8–9, 10–11, 12–13, and 14–15) to illustrate the importance of both age and duration in care. The empirical hazard provides the probability of adoption in each quarter, calculated as the percentage of children remaining in care from the previous quarters who are adopted in that quarter. The figure on the left uses spells beginning prior to 2009 and the figure on the right uses spells beginning in 2009 and later. Generally, the shape of the hazard comprises an initial sharp increase in the probability of adoption that peaks after roughly 4 to 6 quarters and then drops every quarter thereafter. The entire hazard falls as age increases.

Because children are exposed to the rollout of new age and duration criteria over their spells in care, it is difficult to graphically isolate shifts in the hazard for treated age groups. However, the figures clearly show that the adoption rates of children in all age groups increased significantly around the time of the rollout. In order to determine how much of the shift in the hazard is attributable to new age and duration criteria, I rely on a semi-parametric proportional hazard model that isolates the exogenous variation in federal match eligibility and carefully controls for age and duration.

1.5.2 Identification Strategy: Rollout of Children's Federal Eligibility Criteria

Key to identifying a causal effect of match eligibility is that when children become newly eligible is as good as random after conditioning on other covariates. To highlight the substantial variation across time, age, and duration in new eligibility, Table A.5 provides the percent and number of foster children that receive matched foster care payments or meet the new age or duration federal criteria, separately by year and age. These figures are based on the foster care population at the beginning of each fiscal year who are eligible by income (i.e. receipt of federally matched foster care payments) prior to 2010 and by income, age, or duration in 2010 and later.

By program design, the expansions increased eligibility much more for older children than younger children. This can be seen first through the application of age criteria. As shown in the top panel of the table, 100 percent of children age 16 and older are federally eligible in 2010 (as are children age 14 and older in 2011, age 12 and older in 2012, age 10 and older in 2013, and 8 and older in 2014). The increases in federal eligibility from the rollout are large; 39 percent of 16 year olds are "low-income" eligible in 2009, changing to 100 percent in 2010. Second, the role of the duration criterion can be seen among children younger than each year's age criteria threshold. For example, although 15 year olds were not newly eligible by age in 2010, there is a large increase in the percent of children federally eligible through the duration criterion, jumping from 43 percent in 2009 to 63 percent in 2010. Notably, the duration criterion results in increasing federal eligibility with age, whereas prior to the rollout, there was a slight negative correlation between the low-income proxy and age.

1.5.3 Estimation Strategy

My econometric approach relates the probability of adoption to children's federal eligibility, adjusting for covariates important to explaining adoption, in particular, children's age and duration in care. I model the probability a child i in state s in duration quarter q at time t is adopted as:

$$(1.1)\ Pr\left(\mathrm{Adopt}_{\mathrm{isqt}} = 1 \mid \cdot\right) = f\left(\gamma_0 + \gamma_1 Eligible_{isqt} + C_i\beta_1 + \theta_{st}\beta_2 + S_s\beta_s + \delta_t\beta_t + \alpha_{iqt}\beta_a + q_{iqt}\beta_q + y_{iqt}\beta_y\right)$$

where $Adopt$ is a binary indicator for all quarters a child is still in care that switches to a 1 if the child exits to an adoptive home in that quarter.

The source of identifying variation in the state's cost of adoption assistance is captured by $Eligible$. This equals 1 if children at time t are federally eligible for the match through the new age and duration criteria. The coefficient on $Eligible$, γ_1, captures the average change in the hazard for newly federally eligible children relative to children who are not. Conceptually, this framework compares time until adoption among children in foster care before and after new federal criteria are introduced. I expect γ_1 to be positive such that new federal eligibility increases the hazard of adoption.

To capture time and seasonal trends common to children of all ages across the sample period, I also include a set of quarter-by-year indicators, δ_t. Similarly, I include a set of age (measured in years) indicators, α_{iqt}, to account for the effect of age on the probability of adoption and β_a captures shifts in the hazard by age that are constant across states and time. Because the duration criterion triggers federal eligibility from 2010 forward for 5 or more total years in care, I also include a set of indicators for quarter in foster care from first entry, y_{iqt}. Doing so isolates variation in $Eligible$ among children with similar total durations in care. Last, the baseline hazard, q_{iqt}, is a set of indicators that are equal to 1 if the observation is in quarter q from the start of the spell and 0 otherwise.[24] This allows for a flexible baseline hazard and the vector of coefficients on these indicators, β_q, traces out the hazard of exit to an adoptive home in every quarter of duration from mother's rights termination. The hazard comprises 21 dummies in total; one for each quarter for the first 20 with the remaining grouped over the 21+ interval.[25]

Threats to identification of the coefficient on $Eligible$ include any contemporaneous events, such as other policy changes, that also affect adoption rates. The most notable nationwide change in adoption policy around this time was the change in the federal adoption tax credit to a refundable credit for tax years 2010 and 2011. However, the refundable tax credit benefited

[24] Duration from mother's rights termination is not the same as duration from first entry into foster care. The ASFA (P.L. 105-89) requires states to initiate the process of terminating parental rights once a child has been in foster care for 15 of the most recent 22 months (Child Welfare Information Gateway 2013). There is variation in the time between entry and mother's rights variation varies across individuals. On average, mother's rights termination occurs 21.24 months after entry into foster care, with a median of 17 months. Only in 4,108 out of the 618,150 spells, or in 0.66 percent of all spells, is the mother's termination date the same as the child's entry date.

[25] This grouping is based on results estimating (1) including separate dummies for each quarter. Including the full set of dummies produced similar coefficient estimates on the dummies for quarters 22 and above.

adoptive families of children of all ages, so that this event did not differentially affect adoption rates for age groups treated by the rollout. Further, the refundable tax credit was introduced in January 2010 and did not simultaneously occur with the introduction of the rollout in October 2009. Second, National Adoption Day, which occurs on the Saturday before Thanksgiving, has gathered momentum since its start in 2000, with increasing numbers of adoptions in November of each year.[26] National Adoption Day celebrates adoptions of all children, so trends over time in November adoption rates should not confound a causal estimate of $Eligible$. Even so, the quarter-by-year indicators control for changes in adoption rates over time and seasonal trends in adoption rates that can be attributed to policies and events like the refundable federal adoption tax credit and National Adoption Day.

It is important to emphasize that $Eligible$ captures a change in new federal eligibility and does not reflect whether children are already federally eligible through other federal criteria – removal from AFDC low-income homes, qualifying for Supplemental Security Income (SSI), or a sibling of the newly eligible through age and duration. The vector of child characteristics $\mathrm{C_i}$ includes proxies for the former two pathways: an indicator for whether or not a child receives federally matched foster care payments and whether or not the child has a disability. Including these proxies allows for a more accurate estimate of the effect of new federal eligibility if they are correlated with the outcome, adoption, and $Eligible$. The data do not identify siblings, so I am unable to control for this last pathway.

To account for the ability of states to match prospective adoptive families with waiting children, C_i also includes indicators for gender and race categories (Hansen 2007b). The vector

[26] Although advertised as a one-day event, many communities also sponsor events in the weeks before and after the official National Adoption Day, as well as throughout November, which is National Adoption Awareness Month. For examples of scheduled events and more information, visit http://www.nationaladoptionday.org/events/.

θ_{st} includes demographic attributes of each state – the percent of the black population in the state, percent of the population aged 25 to 64, the state unemployment rate and the log of the state per capita median personal income – that may affect adoptions from foster care (Hansen 2007b).[27] Last, I include a full set of state indicators, S_s, to control for unobservable state characteristics, such as state child welfare policies and practices that influence adoptions rates or parental attitudes toward adoption, that are constant over time.

I estimate a discrete time proportional hazard model, and use the complementary log-log form of the hazard, which is implied by the underlying continuous time proportional hazard specification (Jenkins 1995).[28] The proportional hazards framework results in estimates that are readily interpretable because the exponentiated coefficient provides the hazard ratio. This represents the percent increase or decrease in the probability of adoption with a one-unit increase in a covariate. I also calculate the average treatment effect on *Eligible* – essentially an interaction term between age and year, and duration and year. This is calculated as an average discrete effect on this one term as described in Puhani (2012) and Karaca-Mandic, Norton, and Dowd (2012) instead of as cross difference/derivative so that the magnitude of the interaction effect depends on all the covariates in the model (Ai and Norton 2003).[29] Finally, I adjust the standard errors by clustering at the state level.

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[27] The annual unemployment rates are from the Bureau of Labor Statistics; median income, population, and demographic variables are from the Census Bureau.

[28] A discrete time representation of the continuous time proportional hazards model is given by: $h_i(t) = Pr[t < T_i \leq t+1 \mid T_i \geq t,\ \beta' X_i(t), \gamma(t)] = 1 - exp[-exp\{\beta' X_i(t) + \gamma(t)\}]$ where t denotes time in foster care, $h_i(t)$ is the hazard at time t, $X_i(t)$ is a the vector of covariates that varies across individuals and time, β is a vector of coefficients, T_i is a discrete random variable representing the time at which the spell ends, and $\gamma(t)$ is the log of the integral of the underlying continuous time baseline hazard between t and $t+1$. This assumes that variables and parameters are constant between t and $t+1$ for all t.

[29] These average partial effects allow for comparison of estimates from other functional form choices. Results are comparable using a logit functional form. These results are in Table G.1. Ai and Norton (2003) derive the cross difference in a nonlinear model where the parameter of interest is the treatment effect over three variables – variable 1, variable 2, and the interaction between the two. As Puhani (2012)

1.5.4 Results

The results in Table A.6 show the estimates of new federal eligibility are positive, implying an increase in the hazard of adoption. Column 1 presents estimates of equation (1.1) including sets of indicators for age, duration, quarter-by-year indicators, state indicators and no additional covariates. The estimated coefficient on $Eligible$ indicates new federal eligibility increases the probability of adoption by 7.0 percent relative to children who are not newly federally eligible. This estimate is statistically significant at the 10 percent level. Column 2 includes child covariates, C_i. All the parameter estimates except for that on the proxy indicator for low–income are statistically significant at the 1 percent level. Introducing these covariates slightly increases the estimated effect of new eligibility to 8.8 percent and the estimate is statistically significant at the five percent level. The change in the estimate suggests that the demographic characteristics included in C_i are important in explaining adoption rates and that $Eligible$ is correlated with these characteristics. Finally, column 3 introduces state covariates, θ_{st}, that proxy for the state-by-year adoption market. The estimate is an 8.9 percent increase in the probability of adoption, and remains statistically significant at the five percent level. While not all of the state covariates are individually distinguishable from zero, they are jointly significant at the 5 percent level.

To see whether the changes to the federal eligibility criteria are more important for different age groups, the specification in column 4 of Table A.6 allows the effect of new eligibility on the probability of adoption to vary across age. Ages are grouped into bins by ages 5–7, 8–9, 10–11, 12–13, 14–15, and 16–19, which are interacted with $Eligible$. The interaction terms measure the effect of new federal eligibility relative to children who are not newly federally eligible separately for each age group. The magnitudes of the estimates indicate new federal eligibility

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demonstrates, in a difference-in-difference model, only the interaction between the treatment group and the treatment period indicator variable indicates treatment.

increases the probability of adoption comparably across the 10–11, 12–13, and 14–15 age bins, by about 10.2 to 11.5 percent. These three estimates are statistically significant at conventional levels. This specification also speaks to the relative importance of the age versus duration criteria; variation in new eligibility among children aged 5–7 is through the duration criterion only. The estimated increase of 10.2 percent among this group is similar to that for the other age bins. However, it is not statistically distinguishable from zero. A likelihood ratio test (χ^2 =16.29 vs. a critical value of ${\chi^2}_{0.05}(5) = 11.07$) rejects the hypothesis that these six sub-groups have similar exit behavior. This suggests the changes in federal criteria are relatively more important for children in the age 10 through 15 age bins, though because age and time in foster care are correlated, it is unclear whether the age or duration criterion is playing the larger role for these children.

New federal eligibility may be more salient for waiting children who are not already federally eligible through either the existing low-income or SSI pathways. If meeting the low-income and disability standards is correlated with observable child characteristics, such as race or gender, new federal eligibility may also predict a higher increase in the probability of adoption for certain subgroups. Figure D.3 plots point estimates and confidence intervals from estimating equation (1.1) separately for subsamples of children who are and are not receiving matched foster care payments, have and do not have a disability, and separately by race (white, black, and Hispanic) and gender. Estimating equation (1.1) separately by subsample allows the estimates of the baseline hazard, and differences across time (quarter-by-year fixed effects) and states (state fixed effects) to differ by these child characteristics.

Among the sample of children *not* receiving matched foster care payments, new eligibility increases the probability of adoption by 11.5 percent, compared to 7.6 percent among children receiving matched foster care payments. The former estimate is statistically significant at the 5 percent level; however, the latter is not statistically distinguishable from zero. This is consistent with the relative importance of the new criteria for children not already eligible through the low–income pathway. In contrast, the estimated increase is higher and statistically significant for children with a disability compared to children without. This is somewhat surprising because children with a disability may be able to receive federally matched adoption assistance through the SSI pathway. However, it may reflect that new federal eligibility is more salient for children who are more likely to meet state standards for special needs designation (likely true for children with a disability compared to children without a disability), required for states to actually claim federal matching funds.

New eligibility also disproportionately increases the probability of adoption among whites (11.4 percent) compared to blacks (6.8 percent) and Hispanics (4.5 percent). The estimate among whites is the only estimate by race that is statistically significant. White children already have better adoption outcomes than blacks. Numerous studies show black children are less likely to be adopted and time to adoption is much longer for black children than for children of any other race (e.g. see Barth 1997; Hill 2006). This difference in adoption outcomes by race is also reflected in these data with a higher per-period mean of adoption of 14.5 percent for whites compared to 11 percent for blacks. The new federal criteria do not appear to help equalize adoption outcomes across race and may even amplify existing racial disparities. The effect of new eligibility is similar for boys (10.0 percent) and girls (7.8 percent), and both estimates are statistically significant, at the 5 and 10 percent levels, respectively.

The estimated increases in the probability of adoption upon new federal eligibility could reflect new adoptions that would not have occurred otherwise or shifts in the timing of adoption.

Shifts in timing might be because changes in children's federal eligibility accelerate adoptions that would have occurred anyway or because states wait to finalize adoptions until after children meet the new federal criteria. It is difficult to determine how much of the estimated increase in the probability of adoption is because of shifts in timing using the hazard framework. In section 6 below, I address whether states delay adoptions within the rollout using monthly adoption counts to estimate patterns in adoption rates over smaller time frames.

1.5.5 Interpreting the Magnitude of the Effect: Simulated Adoption Proportions

To assess the economic significance of the estimated increases in the probability of adoption, whether the increase is through new adoptions or changes in the timing of adoption, I compare simulated proportions of children adopted over time in scenarios where all children and no children are newly eligible. Figure D.4 presents estimated proportions based on survivor functions predicted using the estimated hazard model in column 3 of Table A.6. First, the left panel of Figure D.4 plots the actual and estimated proportions of children adopted within a given quarter to show the complementary log-log model fits the data reasonably well. The right panel of Figure D.4 juxtaposes the predicted cumulative hazard in a scenario where all children are newly eligible to one in which no children are. To obtain the estimates, I predict the survivor function after setting new eligibility equal to 1 for all observations and to 0 for all observations, respectively. The right panel figure labels the predicted proportion adopted for quarters 4 and 16.

As shown in the right panel figure, the model predicts the one–year (4 quarter) adoption rate if *all* children are treated as federally eligible is 60 percent; had no children been treated by new eligibility criteria, it would have been about 57 percent. This is the duration quarter in which the difference between the two predicted cumulative hazards is largest, after which the difference closes. After two years, with all children federally eligible, 83 percent would have been adopted compared to 80 percent with no children newly eligible. Considering that the new federal criteria are meaningful for more than half of all children waiting for adoption (who do not already qualify on the basis of low income), this is overall a small increase in the adoption rate.

1.5.6 Falsification Tests

To rule out alternative explanations for these estimates, I re-estimate the treatment effect of new eligibility using placebo changes in the federal criteria. If the estimated increase in the probability of adoption is attributable to changes in children's new eligibility, regardless of whether it reflects new adoptions or changes in the timing of adoption, then the effects using the placebo definition of eligibility in equation (1.1) should be close to zero. I first estimate equation (1.1) where $Eligible$ is defined using placebo years for the rollout. I start the same age and duration rules of the rollout five years earlier in fiscal year 2005 and use a sample of child-quarter observations from 2000 through 2009, prior to actual changes in children's new federal eligibility. The results in column 1 of Table A.7 show the main treatment effect is small and statistically insignificant. Results are similar estimating this placebo test starting the rollout in fiscal year 2004 and using a sample of child-quarter observations from 2000 through 2008. These results show that the structure of the rollout itself is unable to account for the estimated effects of eligibility on adoption. That is, these null results rule out that the estimated treatment effect is picking up trends in increases in eligibility in each year of the rollout, or trends in age and probability of adoption.

This falsification test may not be meaningful if the results are driven by omitted determinants of adoption that are correlated with the fiscal years of the rollout. I address this concern by applying the rollout to ages unaffected by the new federal criteria over the years of the rollout. The third column of results in Table A.7 defines eligibility in 2010 beginning with children aged

4 and older, rolling out eligibility by dropping this age criteria each subsequent year. Again, no child assigned placebo eligibility is ever actually treated by the new age and duration criteria. The results show the positive estimate is not driven by unobserved factors correlated with progression of the rollout over time.

1.5.7 Heterogeneous Effects Across States

In this section, I study differences across states by splitting the sample across six state characteristics and then applying equation (1.1). I find evidence of larger increases in the probability of adoption in states that face larger incentives from the matching grant.

States with higher match rates have more to gain in federal grant funds than states with lower match rates. As Table A.3 illustrates, the cost reduction to the state increases proportionally with the increase in the match rate. For example, increasing the match rate from the lower bound of 50 percent to the highest match rate of 83 percent increases savings to the state by 66 percent. Consistent with larger cost savings for states with higher match rates, I find the estimated increase in the probability of adoption is larger in magnitude and statistically significant for children in states with high match rates compared to children in states with low match rates. Panel A of Table A.8 provides results estimating equation (1.1) across two subsamples of states, where states are divided based on whether their 2000–2009 average of the match rate is above or below the median across all states. In states with above-median match rates, the estimated increase in the probability of adoption is 16.9 percent and is statistically significant at the 1 percent level (column 1). In states with below-median match rates, the estimated increase is 5.0 percent and is not statistically different from zero (column 2). Table G.4 lists states in each of the subsamples of Table A.8.

Second, states with more children experiencing changes in their federal eligibility have more to gain than states with foster populations for whom the changes are less salient. Recall from section 2.1 that the historic linkage of federal eligibility with state AFDC income standards generates variation across states in the share of children eligible for federal grant support. I divide states by above or below median shares of children that are 1) in foster care receiving matched foster care payments; and 2) adopted from foster care receiving matched adoption assistance. These are measures of how applicable the states' 1996 AFDC income standards are to the foster care and adopted populations, where states with low shares have more to gain in federal funds as they are more likely to see a larger increase in newly federally eligible children. Consistent with this, I find larger estimates in both sets of below-median states. As shown in Panel B, for children in states with a below-median share of children in foster care supported by matched foster care payments, the estimated increase in the probability of adoption with new eligibility is 13.5 percent and statistically significant at the 5 percent level. In contrast, the estimated increase for children in above-median share states is 6.6 percent and is statistically insignificant. The results in Panel C, with states divided by above- and below- median shares of adopted children receiving matched adoption assistance, are similar. These findings are consistent with the larger estimated increase in the probability of adoption among children not receiving matched foster care payments in the aggregate sample.

Third, in states choosing to spend less on adoptive placements the new eligibility criteria may be more important for generating additional funding through the matching grant than for states choosing to spend more on adoption. Proxies for low spending states are those with below-median shares of adopted children designated special needs (results in Panel D), below-median shares of adopted children receiving adoption assistance (results in Panel E), and below-median standard deviation of adoption assistance payments made to families (results in Panel F). I find the estimated increase in the probability of adoption is larger in magnitude in states with lower spending on adoptive placements. The estimated increase in the probability of adoption in these lower-spending states ranges from 12 to 18 percent, with estimates statistically significant at least at the 5 percent level. In the higher-spending state subsamples, however, the estimates are smaller in magnitude and not statistically different from zero.

These findings provide evidence that states that face larger financial incentives from the matching grant are more responsive to changes in the cost of adoption assistance. The estimated increases in the probability of adoption, at the highest an increase of 18 percent, are still modest compared to the large decrease in the price of adoption assistance when children become newly federally eligible. Nevertheless, the sample splitting on these measures is interesting for policy purposes as it may be desirable, in future intergovernmental grant reforms, to target federal funds at certain states.

1.6 Temporal Displacement and Dynamics: Effect of the Federal Matching Grant on Adoption Timing

I find the probability of adoption increases as children become newly eligible for federal grant support. However, the structure of changes in federal eligibility criteria provides incentives for states to time adoptions to maximize federal matching funds without necessarily affecting the real number of adoptions. In this section, I estimate the extent to which states time adoptions in response to the rollout of the new age criteria within narrow windows around the start of each fiscal year. I find that states do delay adoptions – an unintended state response to the policy change.

1.6.1 Descriptive Statistics and Estimation Strategy

Over the rollout, the incentive to delay exists for the specified age group of each subsequent age criteria at the start of the fiscal year. To preview whether adoptions of targeted age groups are delayed until after the start of the fiscal year, Figure D.5 plots the monthly number of adoptions for each of the specified age groups in the first five years of the rollout. These figures are from the AFCARS Adoption files as the recoding of dates in the Foster Care files obscures changes in month-to-month counts of adoptions.[30] The time period for each age group covers the re-centered 12-month window around the relevant October. For example, the upper left figure plots the average number of adoptions among children age 16 and older from April 2009 to March 2010, where this group became federally eligible starting October 2009. The bottom panel of Figure D.5 presents monthly adoptions for the same age groups from April 2007 to March 2008 to compare patterns prior to criteria changes.

Among each age group, there is a slight decrease in adoptions leading up to October, with a large increase in the number of adoptions in November and December, both in the rollout years and prior to the 2010 change in federal criteria. However, the strong seasonal cycle in the number of adoptions, especially with National Adoption Day in November, makes it hard to discern whether the patterns in the top panel reflect uncharacteristically low numbers of adoptions leading up to October and uncharacteristically high numbers of adoptions afterwards.

To formally analyze whether states respond to the rollout by timing adoptions, I use a specification similar to an event-study. The estimating equation relates the monthly adoptions of age group a to interactions between a treated age group indicator A_a and indicator variables that measure the months relative to the October of the relevant age criterion implementation,

[30] The shortcoming in using the Adoption files is the lack of foster care history information to determine duration in foster care. As a result, I am able to estimate responses in timing of adoption using variation in new federal eligibility from the age criteria without controls for duration.

$\mathbf{1}(Month_{mt} = k)$. This compares the seasonal patterns of adoption rates across age groups in years with and without the introduction of newly applicable federal criteria for a given age group:

$$(1.2)\quad \text{Adoptions}_{\text{amst}}$$

$$= \gamma_0 + A_{at}\left[\sum_{k=-m}^{k=-1} \pi_k \mathbf{1}(Month_{mt} = k) + \sum_{k=0}^{k=m-1} \gamma_k \mathbf{1}(Month_{mt} = k)\right] + \delta_t \beta_t$$

$$+ \alpha_a \beta_a + m_m \beta_m + S_s \beta_s + \tau_a * trend + \varepsilon_{amt}$$

The dependent variable is the number of adoptions of age (at the end of the fiscal year) a, in state state s, month m, and fiscal year t. Many cells have zero counts. To avoid dropping these cells, I estimate this model using a Poisson regression and use child population by state and year to account for "exposure."

This set-up follows the strategy used in Gans and Leigh (2009) to estimate timing responses of June and July births to a financial bonus offered for babies born on or after July 1, 2004. In this case, equation (1.2) is essentially a differences-in-differences model where treated age groups are those experiencing the introduction of a new age criterion and all other ages are the control group. Treatment age–years, when A_{at} equals 1, are for ages 16+ in 2009, 14–15 in 2010, 12–13 in 2011, 10–11 in 2012, and 8–9 in 2013. The pre/post treatment is split by the October in the year of an age group's criterion implementation. To preserve a differences-in-differences interpretation, the specification also includes calendar year fixed effects, δ_t, age fixed effects, α_a, and calendar month fixed effects, m_m. The calendar year fixed effects group together adoptions around the start of each fiscal year and control for changes over time that are constant across all age groups. The calendar month fixed effects control for the seasonality in adoptions, and in particular, the surge in adoptions in November of each year. Last, $trend$ is a linear time trend and τ_a gives the age-specific coefficient on the time trend. This allows for differential trends in adoption counts for different age groups over time, including differential trends in National Adoption Day. I include state fixed effects, S_s, and cluster standard errors at the month-by-year level.

The coefficients π_k and γ_k capture the differential monthly patterns in adoptions among age groups in years in which they are treated by the introduction of the age criteria. If adoptions are delayed until after an age group is treated, then the π_k coefficients will be negative, with a subsequent increase in the γ_k coefficients following the introduction. To see the effect of the age criteria on the timing of adoptions, I begin with September–October pairs (m=1) and progressively widen the window of analysis to April–September/October–March groups (m=6). Wider windows allow for adoptions to have been moved by more than one month. The total number of age-state-month observations in the widest window is given by 14 years x 12 months x 20 ages x 51 states.

1.6.2 Results

The results in Table A.9 across the widening samples consistently suggest states delayed adoptions by 1 to 3 months in response to the introduction of applicable age criteria. Further, the magnitudes of the Poisson coefficients indicate the size of the timing response is economically meaningful. In all except the +/– 1 month window, the estimate in September is negative and estimates in October through December are positive, with statistical significance at conventional levels. Within the +/– 6 months window, the estimates indicate adoptions fell by 10.1 percent in September and increased by 12.4 percent in October, 18.9 percent in November, and 28.2 percent in December. The larger increase in November rather than October likely reflects that the National Adoption Day infrastructure is particularly suited to accommodating an influx of adoptions delayed until after October.[31] The larger increase in December may be from spillovers at the start of December from National Adoption Day at the end of November.

Following Gans and Leigh (2009), I show that the decrease in adoptions in the months leading up to the start of the fiscal year is approximately offset by the subsequent increases using the average partial effects, presented in Table G.5. In the bottom rows of the table, I provide the sum of the coefficients separately in the pre-period and post-period months. These aggregated figures are statistically different from zero, pointing to a statistically significant timing response in both periods. However, the effect is approximately symmetrical. I calculate the difference in the change from the pre– and post– periods as the post– increase *plus* the pre– decrease in order to obtain the net change in the number of adoptions (Gans and Leigh 2009). The difference is not statistically different from zero, except in the smallest and largest windows. Within the +/– 6 month window, for example, adoptions fell by –0.430 in the pre-period and increased by 2.72 in the post–period, where the net increase of 2.29 adoptions is statistically significant at the 5 percent level. The increasing difference across the windows likely reflects that larger windows allow for new adoptions to occur in response to the criteria in addition to shifting of adoptions into the post–period.

Results from a falsification tests do not rule out these estimated delays in adoptions. The falsification test re-estimates equation (1.2) beginning the rollout in 2005 rather than 2010. The sample includes monthly adoption counts from 2000 through 2009, prior to actual changes in children's new federal eligibility. Poisson coefficients are provided in Table G.6, with the calculated average partial effects in Table G.7. The pattern of September and October estimates

[31] This infrastructure includes participating judges and scheduled events on National Adoption Day itself and in the days and weeks around it. From conversations with child welfare agency workers and information provided on http://www.nationaladoptionday.org/, the day, and even entire month of November, offers opportunities for families, caseworkers, and judges to celebrate and raise awareness about adopting from foster care.

is not present in the falsification tests and none of the estimated coefficients in October are statistically significant. Further, the estimates are less than half the magnitude of the estimates using the real rollout dates. While the estimates in November and December are positive and statistically significant, this suggests there is an existing timing response to National Adoption Day.

This large timing effect is not entirely surprising in light of the large financial incentives to waiting. Further, the infrastructure built around National Adoption Day in November decreases the costs to delaying and mitigates resource constraints in absorbing extra adoptions in November or adjacent months. These results, while estimated using variation in the age criteria alone, suggest that timing of adoptions plays a role in the effect of new federal eligibility increasing the probability of adoption. However, the effect of timing adoptions is present only in a small window around the introduction of age criteria. Considering these results alongside those from the hazard analysis from equation (1.1) suggests that though some of the estimated 9 percent increase in the probability of adoption is driven by delays and subsequent increases in adoption, it does also reflect newly induced adoptions. While the analysis here does not address whether states similarly delay adoptions around the 5-year point in foster care, how children qualify for matched adoption assistance does not matter for cost savings to the state, so it is likely states similarly time adoptions around 5-year durations.

1.7 Effect of the Federal Matching Grant on Special Needs Designation

So far, I have shown that a decrease in the state's cost of adoption assistance is associated with a small increase in the probability of adoption and that some of this increase reflects states opportunistically delaying adoptions to realize federal grant support for particular children. In this section, I estimate whether states respond to the matching grant by designating children special needs. While special needs designation is important for generating federal grant support to states, states may *choose* to designate children special needs specifically to realize federal grant support. This can affect the amount of federal funds states receive without real improvements in adoption rates.

I again use variation in children's federal eligibility to relate special needs designation to the state's cost of adoption assistance. Special needs designation is determined at the time of adoption, so data on special needs is available only in the AFCARS Adoption files. I link the adoption records with records in the AFCARS Foster Care files to obtain duration data, needed to identify new federal eligibility at the time of adoption through the duration criterion. I link records across the two datasets based on the child's state, data fiscal year, and record number, using the child's most recent foster care record. The final sample includes foster care and adoption records on children in 17 states from 2004 through 2014.[32] For the remainder of the states, the record numbers do not uniquely identify children across the two data files. For each of the state-years that are linked, I am able to match at least 85 percent of the adoption records to a corresponding foster care record.

Within this sample, about 96 percent of children who are newly eligible at adoption are designated special needs compared to 91 percent of those who are not newly eligible.[33] This comparison of raw sample means does not take into account trends over time in special needs rates, which may be increasing over time as the new federal criteria are introduced, or that age and duration are both positively correlated with special needs designation. Thus, to determine whether increased new federal eligibility causes higher special needs designation rates, I use a

[32] These states are Arizona, California, Delaware, Florida, Hawaii, Kansas, Louisiana, Maine, Massachusetts, Mississippi, Montana, Ohio, South Carolina, South Dakota, Texas, West Virginia, and Wyoming.
[33] Author's tabulations from the linked analytic data.

specification that compares special needs designation across calendar time, adjusting for children's age and total duration in foster care:

$$(1.3)\ Sn_{ist} = f\left(\gamma_0 + \gamma_1 Eligible_{ist} + C_i\beta_1 + \theta_{st}\beta_2 + S_s\beta_s + \delta_t\beta_t + \alpha_{it}\beta_a + y_{it}\beta_y\right)$$

The dependent variable, Sn_{ist}, is an indicator for whether an adopted child i is designated special needs at the time of adoption, in fiscal year t. $Eligible_{ist}$ is an indicator variable for whether the child is age or duration eligible at the time of adoption. I include state controls (θ_{st}), state indicators (S_s), fiscal year indicators (δ_t), age (measured at the end of the year) indicators (α_{it}), and duration from entry (measured in years), indicators (y_{it}). I also include a vector of child controls, C_i. Unlike the data setup for the hazard model in equation (1.1) with multiple observations for each child, in this case, each child has one observation. This is because the dependent variable, special needs designation, is observed in only one period – at adoption. The coefficient on Eligible measures the average covariate-adjusted difference in the probability of special needs designation of newly eligible adoptees relative to children who are not treated by the new age and duration criteria, before and after changes to the federal criteria. I use a probit for $f()$ and cluster standard errors at the state level.

The results in Table A.10 provide no evidence that states respond to children's federal eligibility with strategic special needs designation. The estimated marginal effect from the probit specification is negative, the opposite sign than expected, but small in magnitude (–0.005) and not statistically different from zero. The next three columns provide results using subsamples of adopted children with lower rates of special needs designation – white children, children with no clinical disability, and children adopted with no siblings.[34] The overall mean of special needs

[34] Following Buckles (2013), I identify siblings using data in the AFCARS Adoption files. I match siblings based on children being adopted in the same state, on the same day, and whose birth parents' years of birth and marital status are the same. These variables are not in the AFCARS Foster Care files,

designation is 91 percent; with slightly lower special needs rates, there is more room for a state response among these groups. Again, the estimated coefficients are negative, though small in magnitude and not statistically significant.

It is possible that among adopted children, there is an unobserved determinant of both $Eligible$ and whether or not a child is designated special needs. If this unobserved determinant is positively correlated with both variables, then estimates of the coefficient γ_1 are biased upward. This may be the case if those adopted *because* they are newly eligible are more likely to meet state guidelines for special needs designation. However, both the small estimated increase in the probability of adoption with new federal eligibility from results above and the null results in Table A.10 suggest this is not a large concern and states are not responding to children's new federal eligibility along the special needs margin.

1.8 Discussion and Conclusion

Since 1980, the federal government has supported state expenditures on adoption assistance through an open-ended matching grant to encourage adoption from foster care. Using variation in children's federal eligibility for federal grant support, I estimate whether a decrease in the state's cost of adoption assistance affects adoption from foster care. I find that children's federal eligibility for grant support increases the probability of adoption by about 9 percent. The effects are much larger – sometimes as large as 18 percent – for children not receiving federal support for foster care placements and in states facing larger financial incentives from the matching grant. I also find evidence of an unintended consequence of the rollout structure of changes to children's federal eligibility, with states delaying adoptions for 1 to 3 months around the start of each fiscal year for the particular age groups for which eligibility is extended that year.

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nor are any other variables that would make it reasonable to try to identify siblings in the Foster Care files. As such, it is not possible to estimate the hazard analysis on a subsample of children with or without siblings.

Given that the large federal match, ranging from 50 to 83 percent, substantially reduces the state's cost of adoption assistance, these findings reflect a small state response. This is not altogether surprising. This result is consistent with small estimated price elasticities in related literature with state AFDC, Medicaid, and education spending (e.g. Moffitt 1984; Ribar and Wilhelm (1999); Fisher and Papke (2000); Baicker 2005). The result is also similar to the findings of two studies that use AFCARs data to examine the effect of monthly adoption assistance made directly to families on adoption. Buckles (2013) estimates that a child's eligibility for subsidy receipt, based on meeting the state's minimum age for special needs, increases the probability of adoption by 3.43 percent. This estimate is not statistically different from zero. Still, the 95 percent confidence interval on the estimate contains the 9 percent estimate in this paper. Hansen (2007b) estimates that a 10 percent increase in adoption assistance payments increases adoptions by 1.6 percent, for an elasticity of 0.16. Taken with the results in this paper, these previous findings suggest the effect of reducing the cost of adoption assistance to the state on adoption is comparable to the effect of reducing the cost of adoption for adoptive families.

In addition to having a small, but reasonable effect on adoptions, the policy change also appears to be relatively cost-effective based on a simple cost-benefit comparison. A 2008 CBO analysis estimates the change to the federal criteria will result in a marginal cost to the federal government of $226 million through 2014, and $1.4 billion through 2018 (US CBO 2008). The marginal benefit for just one adoption is quite high – Hansen (2008) estimates that the combined social and private benefit over a child's lifetime is about $523,000 (2016 dollars).[35] With such a large marginal benefit per adoption, even an increase of 500 adoptions over five years, or less than 1 percent of the 50,000 adoptions that occur annually, would be worth the marginal cost to

[35] The Hansen (2008) estimate is based on $234,518 in government savings, $149,050 in private benefits, and $8,747 in private costs in 2000 dollars for a child entering foster care at age 3.

the federal government from the change in federal criteria. Although these are approximations based on projected costs and benefits, the expansion to the matching grant seems to be a reasonable policy for increasing adoptions.

The primary limitation to drawing conclusions on the effect of the matching grant on adoption exploiting the rollout is that the rollout is still working its way through the foster child population and state systems. The long-run effect of the policy change on adoption may be even larger post-2018. The analysis in this study covers the policy changes for children ages 8 and up, and younger children, with historically higher adoption rates, have yet to be affected by the policy change. States also may need more time to adjust their infrastructure and redistribute resources in response to the changes. Currently, many state resources are allocated to determine children's Title IV-E eligibility according to the complex AFDC-based rules, as well as resources in enforcing compliance and accurate reporting.[36] By 2018, there will no longer be a need for these resources. Once states have been given more time to respond to the changes and implement improvements to their systems, future research will be able to speak to the long-run effect of the changes.

The change in federal criteria replacing the outdated AFDC income standards moves child welfare policy – and, as shown in this paper, adoption rates – in the right direction.[37] An important avenue for future research is to determine why, at least over the first five years of the rollout, the effects are small. Further work is needed to understand the exact mechanisms behind the estimated increase in adoption. It will also be helpful to know what states might be doing differently where the estimated effects are larger than states where there are null effects.

[36] See, for example, California's Adoption Assistance Program Monitoring Manual, which details the review process, located at: http://www.childsworld.ca.gov/res/pdf/AAPManual.pdf.

[37] In terms of the changes making headway in child welfare policy, see, for example: McDonald et al. (2004); Pew Charitable Trusts (2004); US DHHS (2005); US CRS (2008b); The Annie E. Casey Foundation (2013).

Understanding the mechanisms more precisely may inform future reforms to intergovernmental grants to promote adoption, and may even point to a different structure of federal funding to states than the matching grant.

CHAPTER 2

The Effects of Federal Adoption Incentive Awards for Older Children on Adoptions from U.S. Foster Care

2.1 Introduction

Foster care provides a temporary living situation for children who have been legally removed from their homes. At any one time in the United States, there are nearly half a million children in foster care (US DHHS 2014). When children cannot be reunified with their parents, adoption is considered a better option for permanent placement over long-term foster care. Compared to their peers in long-term foster care, children in adoptive placements have better educational, psychological, and labor-market outcomes (Triseliotis 2002; Wulczyn et al. 2007; Hansen 2008). Several studies show there are also substantial long-term cost savings to governments if they move a child out of long-term foster care to an adoptive home (Barth 1993, 1997; Barth et al. 2006; Hansen 2008).[38]

Age is a critical characteristic in the likelihood of adoption – older children are less likely to be adopted and older children are an increasing proportion of children waiting for adoption (US CRS 2003; Maza 2009). To address this, a major theme of the Adoption Promotion Act of 2003 (P.L. 108-145) was to increase the number of adoptions of older children from foster care. The act introduced an award to increase older child adoptions by paying states $4,000 for every qualifying adoption of children aged 9 and older. Adoptions qualify for the award if they exceeded a state-specific baseline, determined by prior numbers of adoptions of older children. In 2008, the amount of the award was doubled to $8,000 for every qualifying adoption of

[38] Barth et al. (2006) estimate an adoption from foster care nets a savings to the government of $143,000 in 2000 dollars. Hansen (2008) estimates each adoption nets between $190,000 and $235,000 in 2000 dollars.

children aged 9 and older. The award was added to the existing Adoption Incentives program, the first performance-based federal child welfare legislation that began providing annual performance bonuses of $4,000 for adoptions of children of all ages above state-specific baselines in 1998 (Adoption and Safe Families Act of 1997 [ASFA], P.L. 105-89; Maza 2000).[39]

Since its introduction in 2003, nearly every state has earned the award for increases in older child adoptions in at least one year (US DHHS 2013).[40] Did the introduced incentive for older child adoptions increase the number of older child adoptions, either absolutely or relative to younger children? Furthermore, do states strategically time adoptions of older children in order to realize awards?

Previous studies on the federal incentive payments to states are descriptive and provide mixed evidence that states respond to federal incentive payments. Maza (2000) studies the 1997 introduction of the incentive payments for overall increases in all adoptions and attributes nearly a doubling of the overall number of adoptions to the program.[41] Cornerstone Consulting Group (2001), also studying the 1998 introduction of the program, in contrast, finds that states did not respond to the awards based on survey responses of representatives of child welfare agencies. With respect to the older child incentive awards introduced in 2003, Maza (2009) presents evidence that the awards have not increased older child adoptions and that adoption outcomes for older children have instead deteriorated.[42]

[39] The Adoption Incentives program precedes a similar performance bonus scheme of The Children's Health Insurance Program Reauthorization Act of 2009 (CHIPRA), which provided awards to states for increased enrollment of children in Medicaid above a state-specific baseline from 2009–2013.
[40] As of the end of fiscal year 2012, only Massachusetts has not earned incentives payments for increases in the number of adoptions of older children.
[41] Maza (2000) uses the same data I use in this paper – the Adoption and Foster Care Analysis and Reporting System (AFCARS).
[42] Maza (2009) shows that the gap between the percentage of children aged 9 and older that are waiting for adoption in each year and the percentage of children aged 9 and older that are adopted has been increasing over time, rather than decreasing. The author's conclusion is that the data presented suggest the

This paper provides a causal estimate of state responses using a strategy that compares children's duration in care and probability of adoption before and after the introduction of the award. I use administrative data from the Adoption and Foster Care Analysis and Reporting System (AFCARS) on all children in foster care since 1998. Using a discrete time hazard model, I estimate the probability of adoption and timing of adoption among children aged above and below 9 years old in the time periods before and after the 2003 and 2008 expansions in the Adoption Incentives program.

I find no evidence that states responded to the offered incentive in ways that resulted in long-term increases in adoptions for older children. There are no robust findings that either the introduction of or the increase in the incentive payments to states for adoptions of children aged 9 and older had the intended effect of increasing the probability of adoption for these children relative to younger children. The award did not have the unintended consequence of shifting the timing of adoption from just before a child's 9th birthday to just after a child's 9th birthday. However, I find weak evidence that once the award amount was doubled to $8,000 per qualifying older child adoption, states with certainty of exceeding the baseline number of adoptions accelerated adoptions of children aged 9 and older to occur at the end of the fiscal year.

These findings show the older child adoption incentive payments to states are inefficient sources of federal funding to states. The award does not accomplish its goal of increasing adoptions of older children and it appears that states that do earn awards for increases in older child adoptions are earning inframarginal awards either for adoptions that would have occurred

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additional incentive category added for older children, in addition to other focused efforts on adoptions for older children through the AFSA, "has not affected the adoption of older children."

in the absence of the award or for strategically accelerating older child adoptions to occur at the end of the fiscal year to maximize the federal award payments.

2.2 Federal Adoption Incentives to States

2.2.1 An Overview

States receive federal funds that pay for about half of all costs related to adoption; these include training for caseworkers, administrative costs to place children in adoptive homes and expenditures to provide support to adoptive families in meeting children's needs (Hansen and Hansen 2006; Hansen 2007b). The largest source of federal funds to states is Title IV-E of the Social Security Act, which includes the federal Adoption Incentives program. (US CRS 2012a).[43] In 2011, states received $2.315 billion under the Title IV-E grant for administrative, training, and adoption assistance expenditures (US CRS 2012a). Other streams of federal funds used to support adoption-related expenditures are the Temporary Assistance for Needy Families (TANF) program and the Social Services Block Grant (SSBG).

Adoption assistance payments to families are the primary form of support offered directly to adoptive families.[44] States choose the amounts of the monthly benefit, and payments are made to families from the time of adoption finalization until the child reaches the age of emancipation. The federal government supports state expenditures on monthly payments with matching funds, also through Title IV-E.[45,46] The majority of children adopted from foster care receive monthly

[43] Title IV-E also provides states federal funds for costs related to foster care.
[44] Other financial and in-kind benefits to adoptive families include payments from the state to support the up-front costs of adoption, Medicaid coverage for the adopted child, and post-adoption support in the form of counseling, training and other support groups for families.
[45] Title IV-E matches states for monthly payments paid to adoptive families of qualifying children. Qualifying children are children with a state-designated special need – a characteristic that makes adoption placement difficult –and who meet federal Title IV-E eligibility criteria. Prior to 2010, a Title IV-E eligible child must be either a child who was removed from a family qualifying as needy under the 1996 AFDC income standards or eligible for Supplemental Security Income. Beginning in 2010, new federal eligibility criteria were rolled out on the basis of children's age, time spent in foster care, and

adoption assistance; from 2000–2006, 87 percent of adopted children received about $572 on average, with states receiving federal funds for 81 percent of adopted children (Buckles 2013).

The existing research on financial incentives and adoption studies the effect of monthly adoption assistance subsidies made directly to adoptive families. These studies find positive effects of subsidy receipt and increased subsidies to adoptive families on a child's probability of adoption from foster care (Hansen 2007a; Argys and Duncan 2012; Buckles 2013). However, there is little evidence on how federal funding to states that supports adoption related activities influences the probability of adoption for children waiting in care.

2.2.2 The Adoption Incentives Program Background

The Adoption Incentives program was established in 1997 and is the first performance-based source of federal funds for child welfare (ASFA, P.L. 105-89; Maza 2000; US CRS 2013a).[47] States earn awards each fiscal year for the number of adoptions above a state-specific baseline number of adoptions.[48] Baselines are established using previous numbers of adoptions in the state. The baselines for awards paid over fiscal years 1998 through 2002 were the average of completed adoptions within each state over the three federal fiscal years 1995 through 1997. Since 1998, states earn $4000 for every adoption above the baseline. ASFA also provided supplemental bonuses each year for increases in the number of adoptions of children with special

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sibling relationships with children meeting the new age and duration criteria to replace the income eligibility rules.

[46] The match rate is the same for which the federal government matches Medicaid expenditures, ranging from 50 to 83 percent and varying inversely with state's median income. For more information on the match rate and the Title IV-E grant, see US CRS (2012a).

[47] Other performance-based federal programs are the previously mentioned CHIPRA awards for increased enrollment of children in Medicaid from 2009-2013 and "high performance" bonuses to states for improvements in Supplemental Nutrition Assistance Program (SNAP). These bonuses were established in the Farm Security and Rural Investment Act of 2002. States receive awards for payment accuracy, error rates, access, and timely application processing.

[48] The federal fiscal year runs from October 1 of the previous calendar year through September 30 of the fiscal year. For example, fiscal year 1998 runs from October 1, 1997 through September 30 1998.

needs.[49,50] Similar to the award structure for increases in the overall number of adoptions, states earned $2,000 for each adoption of a child with special needs over a state-specific baseline – set as the average of completed adoptions of children with special needs over federal fiscal years 1995 through 1997.

Congress reauthorized the Adoption Incentives program within the Adoption Promotion Act of 2003 (Act of 2003). The Act of 2003 updated the state-specific baselines for states to exceed as the number of adoptions finalized in fiscal year 2002. The Act of 2003 was signed after the end of fiscal year 2003, in December 2003, but states first earned awards retroactively for increases in adoptions finalized beginning October 2002.[51]

The Act of 2003 included a new bonus specific to older children. States earned $4,000 for each adoption of a child aged 9 or older, regardless of whether the child has special needs, above state-specific baselines for adoptions of older children. The older child baseline was set as the number of completed adoptions of children 9 years and older in fiscal year 2002. The supplemental bonus for children with special needs was revised to apply to children aged 9 and younger. With these changes, states could earn up to $8,000 for an adoption of a child aged 9 and

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[49] Each state sets its own definition of "special needs." A child with special needs has a need or characteristic that may make an adoptive placement without financial assistance to the adoptive family more difficult. Such circumstances include older age, disability, member of a sibling group to be adopted together, or a birth or family history that places the child at increased risk of physical, mental or emotional difficulties in the future. See the state profile pages provided by the North American Council on Adoptable Children for more detailed state information:
http://www.nacac.org/adoptionsubsidy/stateprofiles.html.

[50] The marginal adoptions above and beyond the baselines for both the bonus for overall increases in adoptions and the supplemental bonus might have included adoptions of children aged 9 and older. However, because age is only one of several characteristics that determine special needs, older children are not the majority of children designated special needs – among all special needs adoptees in 2000 – 2002, about 32 percent were aged 9 and older (author's tabulations from AFCARS data). For a table of the minimum age cutoffs for special needs designation by state see Table 3 of Buckles (2013).

[51] I am not aware of any evidence that states were knowledgeable of this policy change prior to the signing date. However, it is possible states could have responded to the awards over fiscal year 2003 with advance knowledge of the change. I address this ambiguity in the start date of the incentives in the analyses below.

older (regardless of special needs): $4,000 if the adoption exceeded the overall baseline of adoptions and $4,000 if the adoption exceeded the baseline of older child adoptions.

The program was authorized again within the Fostering Connections and Increasing Adoptions Act of 2008 (Act of 2008) through fiscal year 2012.[52] The Act of 2008 doubled the incentives states could earn for adoptions of children aged 9 and older from $4,000 to $8,000 (and for adoptions of children aged less than 9 with special needs from $2,000 to $4,000). The Act of 2008 was signed at the start of fiscal year 2009 and states first retroactively earned awards at the new levels beginning in fiscal year 2008, starting October 1, 2007.[53] With the 2008 update to the awards, states could earn up to $12,000 for an older child adoption – $4,000 provided the stated exceeded the baselines for overall adoptions and $8,000 for exceeding the older child adoptions baseline. Table B.1 summarizes the changes in the incentive structure specific to awards for adoptions of children aged above and below the 9-year-old threshold, with and without special needs.

Table B.2 provides the number of older child adoptions by state from 2000 through 2010. Cells in the table are shaded to indicate states exceeded the older child baseline in that year. Over this period, every state except Massachusetts and Montana earned the award (for increases in adoptions of older children) in at least one year.[54] In fiscal years 2003 through 2007, the amount each state receives is $4000 per the number of children adopted above the figure in 2002; in 2008 and later, it is $8,000 per the number of children adopted above the figure in 2007. For

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[52] Most recently, the program has been reauthorized through the Preventing Sex Trafficking and Strengthening Families Act, signed into law in September 2014.
[53] President Bush signed the Act of 2008 into law on October 7, 2008. Again, it is unclear when states were first knowledgeable of the policy change and whether advance knowledge could have resulted in states responding to the awards over fiscal year 2008.
[54] Montana earned the award in 2011, but as of 2012, the most recent year for which awards are published, Massachusetts had not earned the award.

example, Alabama finalized 136 adoptions of older children in 2008, 21 more than the 2007 baseline of 115, resulting in an award of $168,000.

2.3 Changed Incentives for Older Child Adoptions

2.3.1 Increase in the Marginal Benefit for Adoptions of Older Children

The incentive creates a discontinuous increase in the marginal benefit around the age 9 threshold while the marginal cost of adoption to states is likely smooth through age. As such, states face a relative increase in the marginal benefit around age 9, which can encourages states and caseworkers to increase efforts to finalize adoptive placements for older children. State-run child welfare agencies are responsible for children in foster care and state caseworkers work on behalf of children whose parents' rights have been terminated to arrange for long-term permanent placements, whether in foster care until emancipation, with a guardian, or an adoptive placement.

To illustrate how states might improve the likelihood of adoption for older children relative to younger children, Figure E.1 provides a simple model of the market for adoptive placements. The horizontal axis measures the number of adoptions and the vertical axis measures the benefit to adoptive families. One can think of this benefit as monthly adoption assistance payments paid by state governments to adoptive families. The supply curve of adoptive homes is upward sloping, with more families willing to adopt when monthly payments are higher.[55] The demand in the market is the population of children in foster care whose parental rights have been terminated. The model assumes a vertical demand curve: the number of children in foster care

[55] Doyle and Peters (2007) uses foster care monthly subsidy rates to trace out the foster care supply curve and estimate the foster care supply elasticity. Doyle (2007) also finds that reducing subsidy rates to relatives in Illinois by 30 percent led to a 20 percent decrease in the likelihood of providing care.

seeking adoptive homes is constant irrespective of the benefits offered to adoptive families.[56] Holding benefits offered to families and demand fixed, states can increase the number of finalized adoptions by shifting outward the supply curve of adoptive homes. Thus, the award for older child adoptions encourages states to improve the likelihood of adoption for older children by providing a larger marginal benefit explicitly for these adoptions.

It is not clear states chose to, or were able to, respond to the awards for adoptions of older children by changing their infrastructure to specifically focus efforts on adoptive placements for older children. Previous anecdotal reports suggest states did respond to the introduction of the incentive payments for overall increases in adoptions in 1998, and advocates of the program point to a doubling of the overall numbers of adoptions (Maza 2000; McDonald et al. 2003). However, increases in adoptions, either overall or for older children, are not necessarily attributable to states responding to the incentives and may instead be due to trends in older child adoptions or state activities for older child adoptions that would have occurred in the absence of the award.

Further, the design of the award around fixed baselines is not a perfect mechanism to reward states for encouraging adoption – the structure means awards for increases in older child adoptions are not guaranteed, and second, states can earn awards without even responding to the award. Consider that the within-state number of older child adoptions is not very stable from year to year, suggesting there is noise in the baselines as a measure of state "ability" to finalize older child adoptions. This is highlighted in Figure E.2, which plots the annual within-state percentage change in the number of older child adoptions. The change from year to year

[56] Doyle and Peters (2007) also make this assumption, allowing for demand to be a function of risk factors and state preferences, but not financial incentives to adoptive families, which do enter the supply function.

indicates a shortcoming of the baselines is that states can be advantaged by relatively low baselines, or alternatively, disadvantaged by relatively high baselines.

States with a low baseline can earn the award without any change in effort or infrastructure. As an example, New York exceeded its 2002 baseline of 1,374 by over 25 percent in 2003 and 2004. Consider however, that New York would have received much lower awards with these numbers had the baseline been instead set to the 2001 figure of 1,682 adoptions. On the other hand, a high baseline increases the cost of changes in effort or infrastructure to near the baseline. For example, California's 2002 baseline (of 2,248) is the highest number of older child adoptions in the state in any year over the decade. A high baseline such as this could be set in a particularly good year for older child adoptions, or in subsequent years the baseline is no longer a reasonable number of adoptions to reach with respect to the composition of the waiting child population. Additionally, low baselines as a noisy measure of state ability to finalize older child adoptions can generate awards without any state response to the award.[57]

However, even if states cross their baselines so that award payments are guaranteed, the value of the award is small relative to the inherent savings states realize from moving a child from a foster care to an adoptive placement. Previous research estimates a marginal benefit of between $179,000 and $294,000 for an adoption from foster care in 2008 dollars over a child's lifetime (Barth et. al. 2006; Hansen 2008). In real terms, even the higher award of $8,000 represents less than a 5 percent increase in the marginal benefit of adoption to states.[58]

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[57] This shortcoming of the baselines using absolute numbers of adoptions is addressed in the most recent iteration of the Adoption Incentives program within the Preventing Sex Trafficking and Strengthening Families Act (P.L. 113-183). The new incentive structure provides states awards for increases in *rates* of adoptions, which takes into account the fluctuation in the number of children in foster care.

[58] This approximately 5 percent figure is calculated as $8,000 over the lower figure of $179,000. The $179,000 estimate is Barth et. al. (2006) estimate of $126,825 in 1995 dollars adjusted to 2008 dollars.

The small size of the awards is also apparent when comparing the Adoption Incentives program to other sources of federal support for adoptive placement activities. Since the implementation of the incentives in 1997, the total amount of awards paid to states through fiscal year 2014 (in 2014 dollars) is just over $567 million (US DHHS 2015).[59] The total awards also substantially vary from year to year, ranging from a nominal low of just over $7 million in fiscal year 2006 to a nominal high of nearly $44 million in fiscal year 2012 (US DHHS 2015). Further, in the most recent phase of the incentives program, from fiscal years 2008 through 2012, of the $219 million total awards, $61 million was for older child adoptions (2014 dollars) (US DHHS 2013; US DHHS 2015). In comparison, since 2008 the federal government has consistently provided over $2 billion (2014 dollars) annually in Title IV-E payments to states for adoption assistance payments (US CRS 2012a).

2.3.2 Incentive to Strategically Time Adoptions

Despite the small size of the awards, the award creates an incentive to shift an adoption finalization until after a child turns 9. By delaying adoptions until after a child's 9th birthday, states can maximize awards for older child adoptions from the federal government without a real change in the supply of adoptive families for older children.

The structure of the award around a state's annual success in exceeding a baseline creates an additional timing incentive – for states to opportunistically time adoptions of children aged 9 and older around the end of the fiscal year. In this case, there is an incentive for states to increase the annual number of older child adoptions by accelerating adoptions of children aged 9 and older at the end of the year. This incentive is particularly strong when states already neared or exceeded

[59] The Adoption Incentives program is comparable in size to the "high-performance" SNAP bonuses and much smaller than the CHIPRA awards for increases in children's Medicaid enrollment. In 2012, states earned $48 million in SNAP bonuses (USDA 2012). Under the CHIPRA program, however, in 2013, 23 states earned $307 million, compared to the nearly $44 million states earned in 2012 under the Adoption Incentives program (Medicaid.gov, US DHHS 2015).

their baselines and there is no longer uncertainty that the state will realize the bonus for each additional older child adoption.

Economic theory suggests states will behave in this way if the opportunity to realize the available award dominates the cost of shifting adoption finalization (Slemrod 1992). The costs of manipulating the timing of adoption involve setting a court date and coordinating agreement and schedules of involved parties, including the child, adoptive family, caseworker, and judge. Many children live with their adoptive families prior to finalizing an adoption so it is unclear how much of an effect changing the timing of adoption finalization has on the child and family's welfare. However, provided it does not incur a large welfare cost, shifting the timing of adoption finalization for older children can allow states to realize greater award payments for older child adoptions.

There is a large body of evidence that the timing of economic transactions, more so than real decisions, is responsive to financial incentives, especially when merely a change in the date of an event induces a benefit (Slemrod 1992).[60] In particular, related to the timing of adoption, prior research finds that birth timing is responsive to tax incentives and pecuniary bonuses (Dickert-Conlin and Chandra 1999; Gans and Leigh 2009; Neugart and Ohlsson 2013; Lalumia, Sallee and Turner 2015).

2.4 Data

I use administrative data on children in and adopted from foster care from the Adoption and Foster Care Analysis and Reporting System (AFCARS) Adoption and Foster Care files in federal fiscal years 2001 through 2010. The data are distributed by the National Data Archive on Child Abuse and Neglect and were originally collected by the Children's Bureau.

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[60] Examples of intertemporal shifting include capital gains realizations (Auerbach and Porterba 1988), foreign direct investment (Slemrod 1990) and charitable donations (Clotfelter 1990).

The Foster Care files provide an annual roster of all children in foster care over the previous fiscal year. Children who are in care over multiple years will have an annual record for each year they spend time in care. I identify children over time by matching records based on the child's state, date of birth, gender, date of first removal.[61] I identify duplicate observations for each child and use the most recent record, which updates the information from previous years. I construct spells in foster care for children whose mother's rights have been terminated, measuring duration in months from the date of mother's rights termination until a child's exit from care. While a child is not legally available for adoption until both parents' rights have been terminated, I assume state agencies, acting in the best interest of the child begin to seek a permanent placement (which may be an adoptive home) once one parent's rights have been terminated. An exit from care occurs when a child is legally no longer the responsibility of the state and is discharged to a permanent living situation. Exits can be to a placement of adoption, emancipation, living with a guardian or relative, transferring to a different state agency, runaway, death, or reunification with birth parents.[62] Spells are right-censored if a child remains in care.

To protect privacy in the administrative data, dates in the Foster Care files have been recoded by up to two weeks before or after the actual date. The date of birth is recoded to the 15th of birth month and all other dates are similarly adjusted to the recoded date of birth. This preserves spell durations and children's ages across the spell. The recoding of dates results in error for when an exit occurs relative to a calendar date. As children's birthdays are moved a maximum of two weeks, the recorded exit date is up to two weeks before or after the real exit date. I address this empirically by excluding from analytic samples spells for children who exit in the two weeks

[61] This procedure is similar to Buckles (2013). Record numbers are insufficient to match children over time because they are not unique to one child. Record numbers are intended to link children in the AFCARS Foster Care records and AFCARS Adoption records.
[62] Currently, several states have legislation in place allowing for the reinstatement of parental rights following termination (National Conference of State Legislatures 2012).

before and after a date of interest. For example, to estimate whether a child is adopted before or after December 2, 2003, the signing of the introduction of the incentive payments, I use a sample of spells excluding children exiting from November 18, 2003 through December 18, 2003. In analyses estimating the effect of the incentives on the timing of adoption, I exclude spells of children exiting in the +/– two weeks around the end of each fiscal year to eliminate measurement error in whether or not an adoption occurs in an award-earning year.

The Adoption files provide annual records of all children adopted from foster care over the previous fiscal year. These data provide the actual adoption finalization date and age at adoption.

2.5 Estimates of the Effect of the Introduction of Incentives on the Probability of Adoption

2.5.1 Descriptive Statistics

I estimate the effect of the introduction of, and update to, awards for adoptions for children aged 9 and older using spells of children in foster care whose mother's rights are terminated. I construct spells from the AFCARS Foster Care files, as opposed to the Adoption files, in order to include right-censored spells and spells for children "at risk" for adoption but end in alternative exits from foster care. My empirical strategy is to use a discrete-time duration model to compare the duration in foster care of children aged 9 and older before and after the signing dates of the incentive award for this treated group, compared to duration data on children aged less than 9. To do this, I expand the data such that each observation is a child-month in care and each child has as many observations as they spend months in care (until they exit or, if the spell is censored, their last observed month in care). This allows age to vary over time in small intervals, rather than for example, full years at the annual level.

I use a sample of child-month observations in which children's ages are near the age 9 threshold for an older child. This allows me to compare adoption outcomes among populations that are similar except for eligibility for the incentive payment bonus if adopted. I use a sample of child-month observations in which children are aged 8 up to age 10 and aged 7 up to age 11. It is likely that children of ages closer to the age 9 cutoff are more similar in terms of their likelihood of adoption than children of ages further away from the age 9 cutoff. It is possible that the award for older child adoptions also influences adoption outcomes for children aged less than 9. For example, this is true if states substitute their efforts of finalizing adoptive placements toward children aged 9 and older and away from younger children. In this case, the control group of younger children is also treated by the award, and the incentives will negatively affect the probability of adoption for younger children as it increases the probability of adoption for older children.

Table B.3 provides summary statistics separately by age to illustrate observable characteristics are similar for children on either side of the age 9 cutoff. I provide sample means for child-month observations in the two time frames around the introduction – fiscal years 2003 and 2004 – and around the update – fiscal years 2008 and 2009.[63] Means of demographic variables, such as the percentage black, are similar across age (38 percent among 7 year olds and 42 percent among 10 year olds). While the mean months in care increases with age, this is expected because age and duration are positively correlated. For example, mean duration from the spell start increases from 15.82 months among 7 year olds to 19.57 months among 10 year olds. These patterns in sample means across age are similar using child-month observations from

[63] As described above, each sample excludes spells of children who have recorded adoptions occurring in the two weeks before and after the calendar date of interest, December 2, 2003 and October 1, 2008. For example, this drops 4,412 child-month observations of 553 children from the 2003–2004 ages 8 through 9 sample; and drops 3,076 child-month observations of 426 children from the 2008–2009 ages 8 through 9 sample.

2008 through 2009, with means and standard deviations for ages 7 through 10 in columns [5]-[8].

Figure E.3 plots empirical hazards of adoption separately by age to graphically compare adoption probabilities for older and younger children before and after the policy changes. The empirical hazard provides the probability of adoption in each month of duration, calculated as the percentage of children remaining in care in from the previous month who exit to an adoptive home in that month. I plot hazards for the samples of children aged 7 up to 9 and aged 9 up to 11 at the start of their spell in care. The two figures in the upper panel compare the pattern of adoption over duration among each age group using spells from time periods before and after the introduction of the award.[64] I use spells of children whose mother's rights were terminated in fiscal years 2001–2002 and 2004–2005, leaving out spells beginning in fiscal year 2003. It is difficult to isolate spells that entirely occur in either the pre or post periods around the introduction of the incentives, but leaving out spells beginning in fiscal year 2003, the first year for which states can retroactively earn the awards, is an attempt at a cleaner before/after comparison. Similarly, the lower panel of the figure provides the empirical hazards for children aged 7 through 8 and aged 9 through 10 before and after the reauthorization of the incentive in 2008, leaving out spells beginning in fiscal year 2008.

The hazards for each age group shift up in the periods following both the introduction and the reauthorization. However, the figures provide little evidence that the introduction or reauthorization of the awards for adoptions of children aged 9 and older shifted the hazard of exit from care to an adoptive home for children above the age 9 cutoff more so relative to younger children. In order to determine whether there is a shift in the hazard for older children relative to

[64] These empirical hazards are statistically and significantly different from each other at standard levels.

younger children after the incentives are introduced, I use a discrete-time duration model to carefully control for age and duration in care.

2.5.2 Estimation Strategy

I model the probability a child i in state s at time t exits from care as:

$$(2.1)\ Pr(y_{ist} = 1 \mid \cdot) = f(\alpha + \gamma(Post_t * Older_{it}) + \delta_t \beta_t + a_{it}\beta_a + X_i\beta_1 + \theta_{st}\beta_2 + S_s\beta_s + m_{it}\beta_m)$$

where y_{ist} is a binary variable equal to one when a child is adopted. Using the reshaped spells data, the dependent variable is a 0 in all months the child is in care and switches to a 1 if the child is adopted in that month.

I include the interaction variable $Post_t * Older_{it}$ to estimate whether the award differentially affects an older child's probability of adoption relative to a younger child. The latter term is an indicator variable equal to one if the child is aged 9 or older at time t. To estimate the effect of the introduction of the award, I estimate equation (2.1) defining the former term in this interaction as equal to one if time t is after December 2003. In a second regression, I re-estimate equation (2.1) defining $Post_t$ as equal to one if time t is after October 2008 to estimate the effect of the award reauthorization. In each regression, I use a different sample of child-month observations in time periods around the signing date, as explained further below.

The parameter γ captures the magnitude by which the award affects the hazard of exit to an adoptive placement for children aged 9 and older relative to younger children in the "post" period. If the marginal benefit to states for finalizing adoptions of older children exceeds the marginal cost, then γ is positive. For γ to capture a causal shift in the hazard, I also include a set of month-by-year indicators, represented by δ_t. The vector of coefficients on these indicators,

β_t, captures time and seasonal trends common to children of all ages across the sample period. Similarly, I include a set of age (measured in months) indicators, a_{ist}. These indicators account for age effects on the probability of adoption and β_a captures age trends that are constant across states and time. As mentioned above, estimates of γ may be biased upward if the control group of younger children is also treated by the award, with states substituting efforts away from younger children and toward older children.

The vector X_i includes time-invariant child characteristics that may explain exit rates and include gender, race, and an indicator for whether or not the child receives federally funded Title IV-E foster care payments. This last variable serves as a proxy for the child's "removal" family's income (the family the child left when they were taken into foster care); Title IV-E receipt requires the removal family income meet the state's 1996 AFDC income standards for a "needy" family.[65] I also include state covariates to account for differences in the supply of potential adoptive families across states and time. θ_{st} includes demographic characteristics of state populations each year and is a vector of the unemployment rate, the log of median income, the log of the population aged 0 to 17, aged 18 to 29, 30 to 49, and log of the black population.[66] I include a full set of state indicators, S_s, to control for unobservable state characteristics that are constant over time.

To control for duration in care, m_{ist} is a set of indicators for the number of months from the start of the spell as of time t. The vector of coefficients on these indicators, β_m, traces out the

[65] For more information on the Title IV-E foster care program, see US CRS (2012a).

[66] The annual unemployment rates are from the Bureau of Labor Statistics; median income, population, and demographic variables are also measured annually and are from the Census Bureau.

hazard of exit to an adoptive home in every month of duration.[67] I specify $f(\cdot)$ as the logistic function. Finally, I adjust the standard errors by clustering at the state level.

2.5.3 Results

Table B.4 provides the results estimating equation (2.1) where $Post_t$ is defined as December 2003 and later. The logit coefficient on $Post_t * Older_{it}$ is –0.0430 (s.e. 0.0515) using the sample of child-month observations of children aged 8 through 9 in care over fiscal years 2003 through 2004. While negative, the estimate is not statistically different from zero. I calculate the average partial effect (presented in brackets) to provide a measure of the magnitude of the estimated effect. The average partial effect is –0.0018, representing a decrease in the per-period probability of adoption of 0.18 percentage points. With about 4.3 percent of children exiting to an adoptive placement in any month, this represents a small decrease in the average probability of about 4 percent.

This negative estimate remains but decreases in magnitude if I expand the sample along the age or time dimension. Expanding the sample along either of these dimensions increases the sample size – improving efficiency – but compares children either further away from the age 9 cutoff or in care further from the signing date – introducing bias from unobserved differences in these children at the extremes of the sample. Expanding the sample to include to child-month observations in which children are aged 7 up to age 11, the estimated coefficient is –0.0048 (s.e. 0.0435), remaining statistically indistinguishable from zero. Expanding the sample using child-month observations over fiscal years 2001 through 2005 is –0.0291 (s.e. 0.0363) and imprecisely estimated. The results suggest that the introduction of incentive payments for adoptions of

[67] The results are not sensitive to the specification of the month-of-duration indicators or age indicators as polynomials in the month of duration or in age, whether measured in months or years. Additionally, the results are not sensitive to replacing the month-by-year fixed effects with a time trend or year fixed effects or including age-specific or state-specific time trends.

children aged 9 and older did not materially affect the probability of adoption for older children relative to younger children. I provide the estimated coefficients for the child and state covariates for each of the three samples in Appendix Table H.1.

The conclusion that the introduction of the award did not increase the hazard of exit for older children is robust to an alternate specification of equation (2.1) similar to an event study that shows there is no pattern in state response over time. Although the award was signed into place in December 2003, the award retroactively applied beginning in October 2002. Thus, for the event study, I use child-month observations from fiscal year 2001 through 2005. Using the expanded time period allows me to see whether there is a pattern in adoption probabilities among older children around either the signing date of the award or when states could first earn the award for older child adoptions. The event study specification replaces $Post_t * Older_{it}$ in equation (2.1) with 19 interaction terms comprised of an indicator for whether a child is aged 9 and indicators for a given quarter for each quarter over fiscal year 2001 through fiscal year 2005. I exclude the interaction term indicating a child is aged 9 in the quarter prior to the start of federal fiscal year 2003, the third calendar quarter of 2002. Appendix Figure H.1 plots the coefficients on these interaction terms. The figure shows there is no pattern in the relative probability of adoption for older children either leading up to the quarter of October 1, 2002 or December 2, 2003 and except for one estimate, none of the estimates are statistically different from zero.[68] These results illustrate the specification in equation (2.1), which estimates the

[68] If there were a shift in the hazard of exit to adoption for older children relative to younger children, I would expect to find a positive coefficient in the quarters following either the start of fiscal year 2003, represented as quarter 0 in Appendix Figure H.1, or in the quarter in which the Act of 2003 was signed, represented as quarter 4 in Appendix Figure H.1. The only estimated term that is statistically different from zero is that for the last quarter of federal fiscal year 2003, or the quarter prior to the signing of the Act of 2003.

average relative shift in the probability of adoption in the post-period, does not mask a pattern in changes in the probability of adoption for older children over time.

Table B.5 provides the results estimating equation (2.1) redefining $Post_t$ as equal to one if time t is after October 2008. The estimated logit coefficient on the interaction term is 0.0927 (s.e. 0.0418) and is statistically different from zero at the five percent level using the narrowed sample of 8 through 9 year olds in federal fiscal years 2008 and 2009. The average partial effect is an increase in the per-period probability of adoption of 0.47 percentage points. This represents a 9 percent increase in the hazard of exit to an adoptive home above the mean per-period probability of adoption of 5.4 percent. While this result provides evidence of a statistical and economically significant increase in the probability of adoption for older children relative to younger children, it is not robust to the choice of sample. The positive estimate drops in magnitude to 0.0377 (s.e. 0.0353) and loses statistical significance when the sample is expanded to include children aged 7 up to age 10. The average partial effect represents a per-period increase in the probability of adoption of 0.19 percentage points. Using child-month observations of 8 through 9 year olds from fiscal years 2006 through 2010, the estimated coefficient falls to 0.0222 (s.e. 0.0227) with an average partial effect of 0.0011. I provide the estimated coefficients for the child and state covariates for each of the three samples in Appendix Table H.2.

The results from the event-study version of equation (2.1) over this later time period also show the positive estimate using the 2008–2009/ages 8–9 sample is not robust to this alternative specification. Because the reauthorization of the award retroactively applied to adoptions beginning in fiscal year 2007, the event-study uses child-month observations over the expanded time frame from fiscal years 2006 through 2010. Appendix Figure H.2 plots the coefficients on interaction terms between whether a child is aged 9 and an indicator for each calendar quarter.

The coefficients do not reveal any consistent pattern in the relative probability of adoption for older children around October 2007 or October 2008.

2.6 Estimates of the Effect of the Introduction of the Incentives on the Timing of Adoption

2.6.1 Timing Around the 9th birthday

Although I do not find robust evidence the award led to increases in adoptions of older children, it is possible states responded to the award with delays in adoptions for older children until after a child's 9th birthday. The introduction of the award for increases in adoptions of older children may give rise to bunching of adoptions on the award-favored side of children's 9th birthdays without any real changes in the level of adoption. Using a sample of spells of children in foster care over fiscal years 2001 through 2005 from the AFCARS Foster Care files, Figure E.4 provides the raw number of adoptions by age, measured in months, from 8 through age 9 at the time of adoption.[69] The counts of adoptions in each age month are noisy. There is not an obvious drop off in adoptions of children of ages approaching aged 9 or a jump up in the counts of adoptions of children aged 9 and just above in the award years.

It is possible timing occurs only in states that actually earned an award for older child adoptions. Figure E.5 plots the average number of adoptions by age, measured in months from age 8 through age 9, for all states in fiscal years 2001 and 2002, when an incentive to time adoptions opportunistically around the 9th birthday does not exist, and then state-year observations separated by whether the state earned an ward in that year or not (shaded vs. non-shaded cells in Table B.2) from fiscal years 2003 through 2005. There does appear to be a differential pattern in the number of adoptions across the age 9 cutoff in state-years in which the

[69] As noted in the data section, these figures exclude counts of adoptions that occur in the +/– two week window around the start of each fiscal year.

number of older child adoptions exceeded the baseline, with a steady decrease in the number of adoptions approaching the age 9 cutoff, and a parallel shift up in the number of adoptions beyond the age 9 cutoff.

I again use a hazard model to estimate the probability of exiting to an adoptive home varies before and after the period in which a child turns 9. Using data from spells expanded by months, I use a specification that includes interactions with the $Post_t$ indicator and indicators for whether the period is in one of the four quarters prior to the quarter in which a child turns 9 and the four quarters following. I choose to aggregate the indicators for the quarter relative to the period containing the child's 9th birthday to improve precision. I include seven interactions between $Post_t$ and indicators for whether time t is k quarters away from quarter in which a child turns 9:

$$(2.2)\ P(y_{ist} = 1 \mid \cdot)$$

$$= f\left(\alpha + \sum_{k=-4}^{-2} \gamma_k (Post_t * 1[Quarter_{it} = k]) + \sum_{k=0}^{3} \rho_k (Post_t * 1[Quarter_{it} = k]) + \delta_t \beta_t + a_{it}\beta_a + X_i\beta_1 + \theta_{st}\beta_2 + S_s\beta_s + m_{it}\beta_m\right)$$

The interaction for the quarter immediately before the quarter of the child's 9th birthday is omitted, which normalizes the estimates of the corresponding coefficient to zero in that quarter and makes this the period of reference for the other coefficient estimates, γ_k and ρ_k. The coefficients on the interactions for quarters leading up to the quarter of a child's 9th birthday, γ_k, and on the interactions for quarters following the quarter prior the quarter of a child's 9th birthday, ρ_k, measure the covariate-adjusted difference in probability of adoption relative to the quarter prior to that in which a child turns 9. If the introduction of the incentives for adoptions of children aged 9 and older alters the timing of adoption finalization around the 9th birthday, then the γ_k coefficients will decline in the period leading up the 9th birthday and then the ρ_k coefficients will show an increase in the probability of adoption as children age past their 9th birthday, when adoption finalization may qualify the state for the incentive payments for increasing adoptions of children aged 9 and older. As in equation (2.1), I estimate equation (2.2) using $Post_t$ as an indicator for whether the period is October 2003 and later.

Figure E.6 shows the estimated logit coefficients and standard errors using a sample of child-month observations where children are age 8 through 9 year over 2003 through 2004, in the left figure, and over 2001 through 2005, in the right figure.[70, 71] I use this age sample because children age just above and just below 9 are similar except that immediately after a child's 9th birthday, their adoption counts toward an incentive payment. Over both time periods, none of the estimated coefficients on these interaction terms are statistically significant. There is also not a consistent pattern that suggests a dip in the probability of adoption in the quarter prior to that in which a child turns 9 or an increase in the probability of adoption in which a child turns 9. I also check for robustness defining $Post_t$ as October 2002 and later using a sample of child-month observations over fiscal years 2001 – 2005. The results, presented in Appendix Figure H.3 confirm there is also not a timing response around the 9th birthday when states could first earn

[70] This results in a sample size of 140,057 child-month observations of 17,724 children in the 2003 through 2004 sample and 363,301 child-month observations of 35,375 children in the 2001 through 2005 sample. I expect these samples to be large enough to precisely estimate timing responses if they are occurring. In the 2003 through 2004 sample, I drop 13,296 child-month observations of 1,480 children who were adopted in the +/– two week window around the end of the fiscal year to eliminate measurement error in whether or not an adoption occurs in a given fiscal year. This method drops 35,037 child-month observations of 3,202 children in the 2001 through 2005 sample.

[71] Results are similar excluding from the sample children whose birthdays occur in calendar quarter 4, the same quarter as National Adoption Day. National Adoption Day, started in 2000, occurs every year on the Saturday before Thanksgiving in November to finalize adoptions of children in foster care. It is possible that adoptions on National Adoption Day may overwhelm any timing effects induced by the payment incentives. For example, 20 percent of the 6,209 adoptions occurring in November 2004 took place on National Adoption Day, November 20, 2004 (author's tabulations from AFCARS).

the award for older child adoptions prior to the signing date. The results are also robust to using interactions for the month relative to a child's 9^{th} birthday month, rather than quarter; I provide results in Appendix Figure H.4 measuring age in months, using the sample of 8 through 9 year olds from 2003 through 2004 and 2001 through 2005.

I also do not find differential trends in the probability of adoption around children's 9^{th} birthday around the reauthorization of the award. Figure H.5 provides results using samples of children aged 8 through 9 in fiscal years 2008 through 2009 and the expanded sample from 2006 through 2010. Again, these results are robust to re-defining $Post_t$ to the retroactive date of October 2007, using the expanded sample from 2006 through 2010 (results shown in Figure H.6).

Finally, I do not find differential patterns in timing across states based on award receipt. I estimate a specification similar to equation (2.2) replacing $Post_t$ in the interaction terms with a binary indicator for whether that child's state exceeded the baseline in that year and separately including this indicator. This specification compares the pattern of the probability of adoption across a child's 9^{th} birthday in states that did and did not exceed their baseline to earn the award for older child adoptions. Once the award for older child adoptions was introduced, all states face the incentive to time adoptions around the 9^{th} birthday. However, separating states by this metric estimates whether states that did earn the award did so by differentially timing around the 9^{th} birthday. I use the sample of 8 and 9 year olds from 2001 through 2005 because states first earned awards for exceeding the 2002 baseline in fiscal year 2003. Figure E.7 plots the logit coefficients on the interaction terms and the standard errors. The plotted coefficients represent the shift in the hazard of adoption in each quarter relative to the quarter prior to that in which they turn 9 in award-earning states and years. The pattern of the coefficients does not suggest there is a differential trend in the probability of adoption across a child's 9th birthday in award-earning state-years. Relative to the quarter prior to a child's 9th birthday in these state-years, the coefficients are stable around zero, with no estimated drop in the probability of adoption in the quarter prior to that in which a child turns 9. While there is then a slight increase in the probability of adoption in subsequent age quarters, none of the coefficients on the interactions are statistically different from zero.

This pattern provides further evidence that states are not timing adoptions around a child's 9th birthday. Even states that do earn the award for older child adoptions are not doing so by strategically timing adoptions around a child's birthday.

2.6.2 Timing Around the End of the Fiscal Year

While I find no evidence of temporal shifting around a child's 9th birthday, there is still the possibility states shift older child adoptions to the award-earning side of the fiscal year. With states needing to finalize older child adoptions above and beyond the baseline, there is uncertainty throughout the fiscal year over whether older child adoptions will count towards any award payment. All states face incentives to reach their baselines by the end of the fiscal year for children of all ages, by exceeding both their overall baseline and older child baseline. However, states that have already neared or exceeded their baseline for older children may be more inclined to bunch adoptions of older children in September than states that have not.

In this section, I test for acceleration of adoptions of 9 year olds prior to the end of the fiscal year using a sample of adoptions that take place close to September 30. Adoptions of children aged 9 and older finalized in September could have been shifted forward from October for award reasons. I use data on September and October adoptions from the AFCARS Adoption data files, which preserve the exact adoption date by day, from fiscal years 2003 through 2010. I use data on adoptions in years for which the incentives exist in order to compare the timing of adoptions around the end of the fiscal year between states that have and have not exceeded their baselines by the start of September. The data also include a variable with the child's age in years at the time of adoption, calculated using the child's actual birthday. I again narrow the sample of adoptions around the age 9 cutoff to children adopted at age 8 through 9 and expanded to include children aged 7 through 10 in order to compare the timing of adoption among similar populations that differ only in whether their adoption is eligible for the incentive payment for older children.

Figure E.8 provides descriptive evidence of shifting of adoptions to occur on or prior to September 30, which may reflect award-motivated acceleration of adoptions of children aged 9 and older. The figure plots the mean number of adoptions of children aged 8 and 9 at the time of adoption by day of adoption finalization in each calendar year September-October pair from 2003 through 2010. There is an increase in the average number of adoptions of 9 year olds leading up to the end of the fiscal year on September 30, with a drop on October 1 that then remains stable and slightly increases over the rest of October. The mean number of adoptions of 8 year olds displays a similar pattern, which is not surprising because states can earn incentives for overall increases in the number of adoptions above a baseline.

I estimate whether the probability of a September adoption is related to states being able to earn the award for older child adoptions. I compare the relative timing of children aged above and below the age 9 cutoff between states that have and have not exceeded their older child baseline by the beginning of September. I estimate regressions of the form:

$$(2.3)\ SepAdoption_{it} = \alpha + \gamma_1(ExceededBaseline_{it} * Older_{it}) + \gamma_2 ExceededBaseline_{it} + \gamma_3 Older_{it} + \delta_t \beta_t + X_i\beta_1 + \theta_{st}\beta_2 + S_s\beta_s$$

where $SepAdoption_{it}$ is an indicator variable for whether an adoption in calendar year t takes place in September versus October. $ExceededBaseline_{it}$ is an indicator for whether the number of older child adoptions finalized by the beginning of September already exceeded the applicable baseline for that fiscal year. The coefficient γ_2 is the increase in the likelihood of a child's adoption occurring in September if the state has exceeded the older child baseline. $Older_{it}$ is a binary indicator for whether the child is aged 9 and older at the time of adoption. The coefficient on γ_3 is the increase in the likelihood of a September adoption if a child is aged older than 9. The coefficient on the interaction of these two terms, γ_1, is a measure of the extent to which states that are already earning an award for older child adoptions by September "pull-forward" adoptions of older children to the end of the fiscal year. I do not have a prior on the sign of the estimates of $ExceededBaseline$ (γ_2) or $Older$ (γ_3) and I expect the estimate on their interaction (γ_1) to be positive.

I include a set of calendar year indicators, represented by δ_t , to compare the probability of a September adoption across the age 9 cutoff in same-year September-October pairs. I again include child characteristics (X_i), demographic characteristics of state populations (θ_{st}), and a full set of state indicators (S_s).

This set-up follows the strategy used in Dickert-Conlin and Chandra (1999) and LaLumia, Sallee, and Turner (2015) to estimate whether December births (as compared to January births) are related to tax values. In this case, I estimate a difference-in-differences specification, comparing the probability of a September adoption between younger and older children, in states with and without certainty of earning the award for older child adoptions. Because the sample is limited to adoptions that occur in the presence of the award for increases in older child adoptions, this specification deviates from that in equations (2.1) and (2.2) by not using a variable $Post_t$.[72] I estimate equation (2.3) separately using data on adoptions in 2003–2007 and 2008–2010 in order to determine whether states were more responsive on this timing margin when the award amount increased from \$4,000 to \$8,000. I also estimate equation (2.3) over the combined period 2003–2010.

The results in Table B.6 provide weak evidence that once the per-qualifying adoption amount was increased to \$8,000, states with certainty in earning the award for older child adoptions accelerated adoptions of older child adoptions at the end of the fiscal year. First, estimates of γ_1, using September–October adoptions from the first five years following the introduction of the award for older child adoptions, when the award was \$4,000, are negative and not statistically different from zero. This is robust to using either a sample of 8–9 year olds (column [1]) or 7–10 year olds (column [2]). Notably, the estimates of the coefficient on $ExceededBaseline_{it}$ are positive and statistically significant at the ten percent level. This estimate indicates that exceeding the older child baseline is associated with an increase in the probability of a September adoption for both younger and older children. These findings could reflect positive correlation between a state exceeding their older child baseline and exceeding the all adoptions baseline, so that there is bunching of adoptions of children of all ages at the end of the fiscal year. Nevertheless, the estimates of γ_1 do not provide evidence of "pull-forward" of older children in the first five years of the incentive for older child adoptions.

The results in the next two columns, however, provide weak evidence of differential likelihood of September adoptions for older children in states with certainty of award receipt after the 2008 reauthorization. In column [3], using adoptions of 8–9 year olds from 2008

[72] Doing so would attempt to estimate a triple-difference regression. However, this requires comparing the probability of a September adoption for 8 and 9 year olds (first difference) between states that did and did not exceed their baselines (second difference) in the before and after periods (third difference). There is not a counterfactual measure for $ExceededBaseline_{it}$ in the pre-years, however, so I choose to conduct a differences-in-differences using only the post-years.

through 2010, the estimate of γ_1 is 0.057 (s.e. 0.0326) and is statistically significant at the 10 percent level. The magnitude is also economically meaningful; compared to 8 year olds, 9 year olds are 5.7 percentage points more likely to be adopted in September in states that have exceeded that year's baseline for older children compared to states that have not. This is nearly an 11 percent increase in the base probability of a September adoption of 0.523.

However, this result is not robust to expanding the sample to include children aged 7 through 10. The estimate among this expanded age sample, in column [6], is 0.0161 (s.e. 0.0261), which although positive, is not statistically different from zero and much smaller in magnitude. Finally, and not surprisingly, the result is also not robust to including September–October adoptions beginning in 2003; columns [5] and [6] provide results using the full 2003 through 2010 samples of 8 and 9 year olds and 7 through 10 year olds, respectively.

2.7 Discussion and Conclusion

The Adoption Incentives program provides annual performance bonuses to states for increases in adoptions from U.S. foster care. States earn awards for the number of adoptions relative to state-specific baselines, determined by past numbers of adoptions. The program was established in 1997, and in 2003, included a new $4,000 award to states for every adoption of children aged 9 and older above a state-specific baseline. The new award was introduced to address the increasing proportion of older children waiting for adoption in foster care. In 2008, the reauthorization of the program doubled the award for increases in adoptions of older children to $8,000.

I use a hazard model to estimate whether there was an increase in the probability of exit to an adoptive placement for children aged 9 and older relative to children aged less than 9 following the introduction and reauthorization of the incentive payments in 2003 and 2008. To my knowledge, this is the first study to provide a causal analysis of the effect of the financial incentives to states encouraging adoptions. Following the 2003 introduction and 2008 policy update, I do not find robust evidence that the awards for increasing adoptions of children aged 9 and older increased adoptions of older children relative to younger children.

Despite the distortion in the marginal benefit to states for adoptions after a child turns 9, I do not find that the program has the unintended consequence of delays in adoptions of older children until after their 9th birthday. I do find weak evidence that in 2008 and later, states with certainty of earning the award accelerated older child adoptions to occur at the end of the fiscal year. This slight evidence of "pull-forward" at the end of the fiscal year after the 2008 reauthorization of the award is still consistent with the finding that neither the introduction nor update resulted in an increase in the probability of adoption. This is because intertemporal shifts in the timing of adoptions of older children to occur on the award-earning side of the fiscal year can result in award payments to states without increases in the total number of older child adoptions.

Perhaps these results are not altogether surprising. The awards are made to states and not adoptive parents, and state responses that would result in a real supply shift of adoptive families for older children require infrastructure changes such that states focus efforts specifically on adoptions of older children. In contrast, states face a lower cost to shift the timing of adoption in order to realize the award. The finding that states time adoptions at the end of an award-earning fiscal year is consistent with a large literature that timing – and in particular, the timing of demographic events, such as births – is a responsive margin in the face of financial bonuses (e.g. Slemrod 1992; Dickert-Conlin and Chandra 1999; Neugart and Ohlsson 2013; Gans and Leigh 2009; LaLumia, Sallee and Turner 2015).

This study provides evidence that the current design of the well-intentioned award to encourage exit to an adoptive placement among older children incentives is not sufficient to meet this goal. Instead of increasing adoptions of older children, states receive windfalls through this award for older child adoptions that either may have occurred in the absence of the award, or that reflect strategic acceleration of older child adoptions in the presence of realizing an award at the end of the fiscal year. This suggests there are efficiency costs to the program, including deadweight loss from states allocating resources towards the timing of adoption. Without directly addressing the challenges states face to achieve adoption for older children, states will continue to earn awards with no real increases in older child adoptions.

CHAPTER 3

Capitalization of Charter Schools into Residential Property Values

3.1 Introduction

The charter school movement began about twenty years ago and was driven by the belief that privately run and publicly financed schools could be superior to traditional public schools. Proponents argue that charters can adapt more smoothly in times of financial hardship than traditional schools (e.g. by reducing non-unionized labor force or changing administrative policies). They also argue that charters are leaders in methodological innovations in education. On the other hand, opponents argue that charters are able to restrict admission to make them look better than they are and that they divert necessary resources from public schools. While existing research has generally shown charter effectiveness to be mixed (e.g. Angrist, Pathak and Walters, 2013; Angrist et al., 2012; Abdulkadiroglu et al., 2011; Dobbie and Fryer, 2011; Imberman, 2011b; Hoxby and Murarka, 2009; Bifulco and Ladd, 2006; Sass, 2006; Bettinger, 2005), the impacts of these schools on the wider economy is not well known. In this paper we attempt to establish the extent to which charter schools impact residential property markets by examining how charter penetration rates in a community are capitalized into surrounding home prices using data in Los Angeles County (LA County), California. Understanding whether housing markets are responsive to charter availability is important given the increasing prevalence of charter schools across the country. Indeed, California has seen significant growth in the number of charter schools since they were authorized in 1992; the overall number of charters has increased from 299 in 2000 to 912 in 2010, with 242 of those in LA County alone. This is the highest number of charter schools in any county in the U.S.[73]

[73] California Charter Schools Association, accessed via www.calcharters.org.

While there is also a substantial literature relating housing values and school characteristics (e.g. Imberman and Lovenheim, forthcoming; Gibbons, Machin and Silva, 2011; Bayer, Ferreira and McMillan, 2007; Kane, Riegg and Staiger, 2006; Figlio and Lucas, 2004; Gibbons and Machin, 2003; Black, 1999), only Buerger (2014) in an unpublished working paper specifically considers home owners' valuation of charter schools. To identify the impact of charters on housing prices, we use data on single-family home sales from 2008-2011, obtained from Los Angeles County Assessor's Office. We estimate the impacts of both the number of charters and the share of public enrollment in charters within various distances of a property up to two miles. To account for endogenous charter locations and changes in the geographic distribution of sales we include census block fixed effects along with a set of housing and school characteristics to account for the non-random location of charter schools. Month-by-year fixed-effects account for any general changes to the education and housing markets over time in LA County.[74] Thus, our identification comes from houses sold in the same census block at different times as charters open, close, expand and shrink. As a result, we note that our study does not identify how existing charter enrollment affects housing prices but rather how contemporaneous changes in charter enrollment and the number of charters affect housing prices in localized areas, specifically within census blocks.

The charter school movement began about twenty years ago and was driven by the belief that privately run and publicly financed schools could be superior to traditional public schools. Proponents argue that charters can adapt more smoothly in times of financial hardship than traditional schools (e.g. by reducing non-unionized labor force or changing administrative policies). They also argue that charters are leaders in methodological innovations in education.

[74] We acknowledge, nonetheless, that since we do not have neighborhood controls that vary over time, our model does not account for changes in neighborhoods independent of changes in local schools that may affect charter penetration. We discuss this issue in more detail in the empirical strategy section below.

On the other hand, opponents argue that charters are able to restrict admission to make them look better than they are and that they divert necessary resources from public schools. While existing research has generally shown charter effectiveness to be mixed (e.g. Angrist, Pathak and Walters, 2013; Angrist et al., 2012; Abdulkadiroglu et al., 2011; Dobbie and Fryer, 2011; Imberman, 2011b; Hoxby and Murarka, 2009; Bifulco and Ladd, 2006; Sass, 2006; Bettinger, 2005), the impacts of these schools on the wider economy is not well known. In this paper we attempt to establish the extent to which charter schools impact residential property markets by examining how charter penetration rates in a community are capitalized into surrounding home prices using data in Los Angeles County (LA County), California. Understanding whether housing markets are responsive to charter availability is important given the increasing prevalence of charter schools across the country. Indeed, California has seen significant growth in the number of charter schools since they were authorized in 1992; the overall number of charters has increased from 299 in 2000 to 912 in 2010, with 242 of those in LA County alone. This is the highest number of charter schools in any county in the U.S.[75]

Overall, our results suggest that neither the increase in the number of charter schools nor the expansion in charter enrollment relative to public school enrollment – our proxy for the availability of charter school slots to local residents – is capitalized into housing prices on average. This holds both for Los Angeles Unified School District (LAUSD) and other parts of Los Angeles County. It also holds for both startup charters – new schools that begin as charters – and conversion charters – public schools that convert to charter status, though we caution that very few schools convert during our sample period. Further, we find no evidence that capitalization varies with income level, minority population, or achievement levels of the local public elementary school.

[75] California Charter Schools Association, accessed via www.calcharters.org.

However, we do find that when we count charters located only within the household's school district's boundaries and exclude LAUSD there is a significant negative effect of additional nearby charter schools on housing prices. This restriction is reasonable as students who reside within the charter's authorizing school district (which is almost always the district they are located in) have admissions priority, thus generating a link between these schooling options and local district boundaries. A potential explanation for this finding is that opening a nearby charter school reduces the value of a local community school, thus weakening the link between the availability of local schooling as a public good and house prices.

3.2 Charter Schools Background

Charter schools are public schools that are tuition-free and managed by an independent operator. Typically they are open to any student wishing to attend, regardless of where they live, though some schools give preference to students who reside nearby. Many schools require an application, and those that are in high demand will often have a waitlist. Charters are typically governed by parents, teachers, members of the local community, or a private company and are reviewed for renewal every few years by an authorizer, usually the state or a local school district. In California, charters are funded through a mix of block grants and a state-based funding formula that provides funding at the same per-pupil rate to all charters of a given grade level across the state.[76] There is substantial heterogeneity across schools in the way they are managed, their goals, their targeted student population, and level of autonomy from the local school system.

An important distinction to recognize among charter schools is that they are either brand new schools – startup charters – or were previously a traditional public school that switches to a

[76] "Charter Schools FAQ Section 3," California Department of Education, accessed http://www.cde.ca.gov/sp/cs/re/qandasec3mar04.asp.

charter model – conversion charter. According to the California Charter Schools Association, there are many reasons why traditional schools decide to convert to charter status, but above all is the appeal of increased flexibility and autonomy. Conversion charters must satisfy the same legal requirements and processes as startup charter schools. This involves submitting a charter petition establishing features such as the school's goals, finances, and governance plan, as well as obtaining signatures of at least fifty percent of the permanent teachers currently employed at the school.[77] However, California law does require that conversions give priority to students in the school's district and many districts, including Los Angeles Unified, give priority to students in a local catchment area. Typically startup charters do not have catchment areas, but if they are over-subscribed they are also required to give priority to students who reside in the authorizing school district and may choose to give priority to those in the local school zone if the neighborhood school has high rates of economic disadvantage.

As of the 2010-2011 school year, conversion charters represented 16 percent of California's charter schools, enrolling about 25 percent of all charter school students.[78] Charter school facilities vary with type of charter, with some building brand new structures, renting available spaces in churches, community centers, or commercial buildings, or occupying a previously traditionally run public school campus.[79] When a school converts to charter status, it usually remains in the same building and retains teachers, staff, and students. In contrast, startup charters need to recruit a student body because parents have the option to enroll their child in the charter or in the assigned public school.

[77] "School Conversion," California Charter Schools Association, accessed via www.calcharters.org/starting/conversion/.
[78] "Conversion Charter Schools: A Closer Look," California Charter Schools Association, accessed via www.calcharters.org/2012/04/conversion-charter-schools-a-closer-look.html.
[79] California Charter Schools Association, accessed via www.calcharters.org.

Another important distinction between types of charter schools that has drawn interest recently is the role of larger charter management organizations (CMOs). CMOs are non-profits that operate multiple charter schools and charters within an organization are able to pool management and resources in order to gain economies of scale, a benefit often shared by schools within a traditional public school district. Evidence of the impacts of these types of charters on student outcomes suggest that effectiveness varies substantially across CMOs and students (Furgeson, et al., 2012; Angrist, et al., 2012). Another heterogeneous distinction between charter schools is whether a charter has a waiting list. Recent work using oversubscription lotteries has indicated that waitlist charters perform better than local public schools but are unable to assess the impacts of non-waitlist charters (Angrist, Pathak and Walters, 2013; Angrist et al., 2012; Abdulkadiroglu et al., 2011; Dobbie and Fryer, 2011; Hoxby and Murarka, 2009). Unfortunately, while it would be interesting to see whether housing prices respond differently to these two ways charters vary, we do not have data on whether charters are operated by CMOs or have waitlists.

3.3 Theory of Charter Impacts on Housing Prices

The theory behind the relationship between housing prices and local school quality predicts that, due to the close link between residential location and the school attended via attendance zones, higher quality schooling will generally lead to an increase in housing prices, though the extent of this increase depends on a number of factors (Black and Machin, 2011; Rosen, 1974). This relationship has been well established through empirical analyses (Gibbons, Machin and Silva, 2013; Bayer, Ferreira and McMillan, 2007; Kane, Reigg and Staiger, 2006; Figlio and Lucas, 2004; Downes and Zabel, 2002; Black, 1999). However, since charter schools do not typically have attendance zones and typically students may attend a charter regardless of their location of residence, the theoretical link between charter schools and housing prices is ambiguous.

Despite a less obvious link between charter schools and housing prices, economic theory suggests homeowners may respond to charters in a neighborhood for a few reasons. First, charters provide an option value. Even if a child does not attend a charter school, the availability of charters nearby may make a location more attractive for parents. Since charters rarely offer busing, travel distance is especially important if transport costs are expensive as is the case in Los Angeles County where there is limited public transportation, heavy traffic congestion and high gas prices. Further, as previously mentioned, in California oversubscribed charters much give priority to students who reside in the school district containing the charter which could increase the option value to living in the district.

Second, charters may have an indirect effect on housing prices if they affect the performance of local public schools. Evidence on how charters affect local public schools is mixed. While Booker, Gilpatric, Gronberg and Jansen (2008), Bifulco and Ladd (2006), and Sass (2006) find positive effects of charters on nearby public schools, Imberman (2011a) finds negative effects. Thus it is unclear how this mechanism might influence housing prices.

Third, the public may value the direct infrastructure and community improvements charters sometimes provide. Indeed, Cellini, Ferreira and Rothstein (2010) show that housing prices respond to non-charter public school facility investments. While many charters rent or use donated space, some build their own facilities or convert abandoned properties for use as schools. Even those that rent will often fill up vacant properties in locations like strip malls (Imberman, 2011a). Thus the additional economic activity generated by the charters may influence local housing prices.

Another theory is that charter schools may serve to break the connection between local public schools and housing prices. In so doing we might expect additional charters (and more school choice options more broadly) to lead to increased housing prices where existing schools are low performing as these locations would have artificially low housing values due to the poor school quality. Alternatively, in high performing areas, additional charters may actually reduce housing prices as the availability of nearby charters weakens a key benefit of being zoned to a high-performing school if, through attending charters, high school quality becomes available to households outside the attendance zone (Nechyba, 2003). Another possibility, however, is that by severing this link, the availability of having a public school option at all, irrespective of school quality, is less valuable. The public good of a local school provides less utility and thus, without a commensurate reduction in property taxes, lowers the value of living near that school.

The theories outlined above indicate that it is unclear how charter schools may affect housing prices as some economic effects may be positive and some may be negative. As such, understanding the overall effect on local property markets is necessarily an empirical question. We should also note that while it may be tempting to interpret housing price responses as measures of how much people value charters, the complexity of the underlying processes makes it difficult to do this. In fact the theories described above of how charter schools may sever the link between local public schools and property values highlight that the effects could be showing something entirely different from valuation.

3.4 Previous Literature

Most of the existing literature on charter schools focuses on the effect of charters on student achievement. Early research that relies on panel data methods have found mixed results, with some researchers finding insignificant or significant negative impacts of attending a charter school on student test scores (Imberman, 2011b; Hanushek, Kain, Rivkin and Branch, 2007; Bifulco and Ladd, 2006; Sass 2006; Zimmer and Buddin, 2006), and others finding positive impacts (Booker, Gilpatric, Gronberg and Jansen, 2008; Hoxby and Rockoff, 2004). More recent research employing random lotteries (Angrist, Pathak and Walters, 2013; Angrist et al., 2012; Abdulkadiroglu et al., 2011; Dobbie and Fryer, 2011; Hoxby and Murarka, 2009) and natural experiments (Abulkadiroglu et al., 2014) have found large positive effects. Some research has also recognized the distinction between conversion and startup charters and suggests there is a differential impact on performance across the two types (Sass, 2006; Buddin and Zimmer, 2005; Zimmer and Buddin, 2009).

There are two studies in particular that are similar to ours. First, Chakrabarti and Roy (2010) try to use the impact of charter schools on enrollment in private schools as a proxy for how much parents prefer charters to other schooling options. They find modest declines in private school enrollment when charters locate nearby. Second, in an unpublished working paper Buerger (2014) looks at differences in housing prices across school districts in New York due to charter penetration and finds positive effects. His identification relies on differences in charter penetration across school districts and census-tract fixed effects.

Nonetheless, our paper is distinct from Buerger (2014) in a few key ways. First, the focus on differences across districts, while useful in areas with many school districts, is less relevant to areas like Los Angeles that are dominated by a large central core district. Indeed, most charter schools tend to locate in urban core areas dominated by large urban districts. Thus, our analysis allows for identification of charter impacts within these urbanized areas. Second, Buerger looks at the impacts on housing prices from the entry of the first charter school into the district. In our analysis, we look at capitalization of marginal changes in charter penetration using multiple charter penetration measures. Third, our inclusion of census-block fixed-effects instead of the geographically larger census-tract fixed effects allows us to account for more potential sources of time-invariant unobserved characteristics.

A separate branch of literature focuses on the relationship between housing prices and school characteristics. There is ample evidence from previous work that housing prices are responsive to test score differences across schools.[80] Both Black (1999) and Bayer, Ferreira and McMillan (2007) estimate regression discontinuity models across school zone boundaries to identify how school-average test scores are capitalized into housing prices. Figlio and Lucas (2004) examine the effect of the release of "school report card" data in Florida on property values. These report cards rated schools from A to F based on average performance on statewide exams. All three studies find sizable, positive impacts of higher school test scores on home values, suggesting that parents place significant value on this school quality measure. Gibbons, Machin and Silva (2013) find similar results in England using boundary discontinuities using test score gains. On the other hand, Imberman and Lovenheim (forthcoming) find little impact of the release of teacher and school value-added information on housing prices in Los Angeles.

Several studies have considered the effects of other school characteristics such as student demographics, per-pupil spending, and pupil-teacher ratio, on housing prices. In the footsteps of Oates' (1969) seminal paper, which uses per pupil spending and pupil-teacher ratio as measures of school quality, much of this research has found positive relationships between similar measures and housing prices (Bradbury, Mayer and Case, 2001; Bogart and Cromwell, 1997; Weimer and Wolkoff, 2001). Clapp, Nanda and Ross (2008), using panel data from Connecticut, find that an increase in the percentage of Hispanic students has a negative effect on housing

[80] For a comprehensive review see Black and Machin (2011).

prices. Using data from Chicago, Downes and Zabel (2002) find that households do not capitalize per-pupil expenditures.

Bogart and Cromwell (2000) exploit school redistricting in Ohio and find that disruption of neighborhood schools - in terms of student demographics, changes in transportation services, and geographic location within the neighborhood - reduces house values by nearly 10 percent. Reback (2005) analyzes the effect of adoption of a public school choice program in Minnesota to estimate the capitalization effects related to changes in school district revenues, as districts' state revenues depend on enrollment. He finds that a one percentage point increase in outgoing transfer rates is associated with an increase in house prices of about 1.7 percent.

Our analysis builds off the approaches of these studies, by estimating the impact of charter schools on local housing prices while carefully accounting for selection of charters into neighborhoods. In particular, our baseline specification includes census block fixed effects to account for unobserved heterogeneity across local neighborhoods in the propensity for charters to open or close nearby.

3.5 Data

Our home price data come from the Los Angeles County Assessor's Office (LACAO). The data contain the most recent sale price of every home in Los Angeles County as of October 2011. In addition to Los Angeles Unified School District (LAUSD), the second largest district in the country, the data encompasses 75 other school districts. Since our data is based on most recent sales, to avoid endogenous selection into the sample and small sample sizes in early years, we restrict our data to include only residential sales that occurred between September 1, 2008 and September 30, 2011. From LACAO, we also obtained parcel-specific property maps, which we overlay with school zone maps from 2002, which is the most recent year such data is available for the whole county.[81] The data also include home and property characteristics, such as the number of bedrooms, the number of bathrooms, units on the property, square footage, and the year the structure was built.

We drop all properties with sale prices above $1.5 million in order to avoid results being driven by home price outliers. Further, about 25 percent of the residential properties in the dataset do not have a sale price listed. Usually, these are property transfers between relatives or inheritances. Hence, we limit our sample to those sales that have "document reason codes" of "A," which denotes that it is a "good transfer" of property. We also drop all properties with more than either eight bedrooms or eight bathrooms.

The charter school data is from the California Department of Education. We rely on two measures of charter school penetration: the counts of the number of charter schools within a specified distance from a home and the percentage of total enrollment in the public sector attributable to charter schools within a specified distance from a home. For the former measure, we calculate the distance between each charter and the home, and count the number of charters falling within a specified distance. For the latter measure, we use enrollment figures for all public schools in Los Angeles County from the Common Core of Data, managed by the Institute of Education Sciences at the U.S. Department of Education. An explanation for why we choose these variables and our specified distances is provided in the empirical strategy section below.

We combine these data with school-by-academic year data on Academic Performance Index (API) scores, API rank, school average racial composition, percent on free and reduced price lunch, percent disabled, percent gifted and talented, average parental education levels and enrollment. The API score is California's summary index of school test score performance.

[81] The 2002 LA County maps come from the Los Angeles County eGIS portal at http://egis3.lacounty.gov/dataportal/. The maps were created using a variety of sources and thus may not match precisely to actual school zones.

These covariates, which are available through the California Department of Education, control for the differences in charter school penetration that are correlated with underlying demographic trends in each school.

Our main analytic sample consists of 158,211 house sales occurring from September, 2008 through September, 2011. Of these, 65,170 are sales of homes zoned to an elementary school in LAUSD and 93,041 are sales of homes zoned to an elementary school in another school district in LA County. Table C.1 provides information on the types of charter and public schools that operate in LA County over our sample period. Panel A provides schools by grade level. Charters are more common for middle and high schools but still account for a substantial portion of elementary schools at 9 percent. Conversion charters in particular are common for elementary schools but not middle and high schools. Panel B shows that over the time period of our study, the percent of schools that are charters grows from 7.7 percent in 2008 to 11.7 percent in 2011. Table C.2 and C.3 provide sample means and standard deviations at the property level for several of the variables we include in our regressions. In Table C.2 we see that properties in Los Angeles County have an average sale price of $383,546 and tend to be of modest size, averaging around 3 bedrooms, 2 bathrooms and 1600 square feet. We also have a ranking of the quality of the structures on the property which will be useful for conducting validity tests. The property is given a rating on a scale of 1 to 12.5 by LACAO assessors, where a rating of 12.5 is the highest assessed quality. Not surprisingly, the average quality of a property in LA County is close to the midway point on this scale at 6.45. For charter penetration, the number of charters in each distance ring increases as we go further out, primarily due to the larger amount of land area in larger distance rings. When we look at charters as a percentage of total public school enrollments, the rates are relatively constant across distance rings at 5 - 6 percent.

We note that our data covers some periods of abnormal rigidity in the Los Angeles housing market due to the housing collapse of 2008 and the Great Recession. Figure F.1 shows the Case-Shiller House Price Index for the Greater Los Angeles area from 2008 through 2011.[82] Even though housing prices in Los Angeles fell dramatically until May 2009, afterwards they had begun to rebound, increasing by 11 percent through July 2010. The prices fell slightly thereafter until the end of our data in September 2011. Thus, the housing market had been in recovery for most of our sample period. Even so, we may be worried that market rigidities would continue to limit capitalization. To address this we provide results in the appendix that vary by year of sale and show that our estimates are similar to baseline in later years of the sample when the market had more fully recovered.

In panel A of Table C.3 we provide information on the characteristics for the elementary, middle, and high schools to which each property is zoned. Panel B provides a comparison with charters at each grade level within 1 mile of the property. For elementary and middle schools, the characteristics of charters are pretty similar to those of the zoned school in terms of enrollment, API score and demographics. For high schools, however, there are some differences. Charter high schools tend to be substantially smaller (1,140 students versus 2,002) but lower performing as measured by API score. Zoned and charter high schools are demographically similar, though high school charters tend to have fewer gifted students.

3.6 Empirical Strategy

Our identification strategy relies on variation across households and over time within a census block in the number of charters within various distance radii. To achieve this, in addition to controls for characteristics of the local elementary school and property characteristics, we

[82] Acquired from http://us.spindices.com/indices/real-estate/sp-case-shiller-ca-los-angeles-home-price-index.

include census block fixed-effects along with month-of-sale fixed-effects. Including census block fixed-effects allows us to compare the sale prices of properties that are geographically very close by; the mean land area for census blocks in LA County is 108,322 square feet with a median of 19,283 square feet. While it may be preferable to use repeated sales on the same property, this is not possible with our data as we only have sale price information for the most recent sale. Even if we did have repeated sales, given the short time frame, restricting to those types of households would create a selected sample as a disproportionate number of those properties may be distressed, in fast changing neighborhoods, or houses that are often "flipped."

We believe that multiple sales within census blocks provide a reasonably small enough geographic area to closely mimic repeated sales for specific properties while avoiding the potential selection issues generated by using repeated sales. For example, in our final estimation sample the median census block in LA County has three sales during the study period with a mean of 3.9. Figure F.2 provides a histogram of the distribution of sales within census blocks, conditional on having any sales, over the study period. While our econometric strategy identifies the effect of charter penetration only from blocks with more than one sale, a substantial number of census blocks provide this identification. There are 29,512 blocks with at least two sales and of those, 14,494 blocks have at least four sales and 7,387 blocks have at least six sales. Further, of all blocks with at least one sale, 73 percent have multiple sales, providing wide geographic variation in blocks that contribute to identification. Finally, we conduct an ANOVA analysis of property characteristics to assess the within and between census block variance. In our estimation sample only 39 percent of the variance in house size and 20 percent of the variance in housing quality is within census block, along with less than half of the variation in bedrooms and bathrooms.[83] These results suggest that different houses within a block have largely similar characteristics.

By including census block fixed effects, our identification strategy assumes that there are no changes in neighborhood conditions over time that are correlated both with housing prices and charter penetration. Of course, housing prices are increasing in general in Los Angeles during our analysis period as is the number of charter schools. Hence, to account for general changes in house prices related to overall market conditions, we include year-by-month indicators in all of our regression models.

Even with census-block fixed effect and year-by-month fixed effects, it is possible there are factors changing locally that could bias our estimates. Of primary concern is the possibility that charters select into neighborhoods where the local public school is under-performing and the poor quality of the school is reflected in lower housing prices. Ideally, we would be able to at least control for changes in neighborhood characteristics as we do for school characteristics and housing supply. Unfortunately, the data available to us for this is very limited. To our knowledge, only the American Communities Survey (ACS) provides neighborhood data at a small enough geographic level (e.g. census tract) to be relevant for this analysis. However, the ACS only provides five-year estimates at the census tract level as estimates based on smaller periods of time are too imprecise. As a result, the ACS data does not provide temporal variation in neighborhood characteristics over our three-year time period and any data on neighborhood characteristics would be absorbed by the census-block fixed-effects. Thus, we assume that selection of charter location is unrelated to time-varying neighborhood characteristics that are themselves not captured in our housing and school characteristics controls. While we cannot test

[83] An ANOVA using the residuals from regressions of the characteristics on month-by-year indicators provides similar results.

this assumption directly, we do attempt to address it indirectly by testing whether our observable measures of housing characteristics change when more charters move in and by testing whether charter penetration can be explained by prior changes in house prices. If time varying neighborhood characteristics are correlated with prior house prices and the types of houses put on the market then we should expect to see some impact on these observables, and indeed we do not find evidence for this. Nonetheless, while we do not have temporal variation in neighborhood variables, we do have such variation for local elementary school characteristics. Thus in Table I.1 of the appendix, we look at how charter entry relates to public school characteristics when we condition on school fixed- Without school fixed-effects the estimates show that charters tend to locate in the zones of elementary schools with fewer minorities, more gifted students, more English language learners and more disabled students. When school fixed-effects are added some characteristics are statistically significant, but importantly they are all economically small. The largest statistically significant coefficient is on percent of black residents in the public school zone, but this coefficient is still rather small. For a one charter increase in the school zone, there would need to be an increase in percent black by of 84 percentage points. Given this pattern and the general shift in the coefficients towards zero as the school fixed-effects are added, these results suggest that lower levels of geographic fixed-effects, specifically census-block effects, should reduce these correlations further to the point where they are negligible.

Another difficulty in this analysis is deciding how to measure charter penetration. There are two key factors here. First, there is the question as to whether the important factor is the existence of a charter school as a whole or the relative size of a charter school. Arguably, while the former is the most visible aspect of the school to the wider public (people in the neighborhood know that a school exists but may be uncertain as to how large it is), the latter is a potentially better indicator of the supply constraints on a family that wishes to send a child to the charter. The second issue is that it is unclear how far from the charter a household must be before we can be confident that the household should not care about the charter's existence. To deal with both of these issues we follow the prior literature on the effects of charter schools on public schools (Imberman, 2011a, Booker et al., 2008; Bifulco and Ladd, 2006; Sass, 2006). The analyses in these studies estimate the effects of charter schools on traditional public schools within concentric rings of various distances. Since it is not obvious whether what matters is relative enrollment in charters or the number of charters they estimate the effects of both charter counts and enrollment in the charters as a share of total enrollment.

We use measures of charter penetration equal to (a) the number of charters and (b) the share of all public school enrollments in charters in concentric rings between 0 and 0.5 miles, 0.5 and 1 miles, 1 and 1.5 miles and 1.5 and 2 miles from a property. We focus our attention on charters within relatively short distances of properties due to the urbanicity and size of school zones in LA County. The mean elementary school zone in LA County has an area of 3.2 square miles. With this area, if school zones were circular, the radius of the average zone would be 1.0 miles. The median school zone has an area of 0.8 square miles translating into a radius of 0.5 miles. Hence, given the size of school zones in LA County, these are reasonable distances within which to measure the effect of charters. Indeed, in a large Southwestern city that is less densely populated than Los Angeles, Imberman (2011a) shows that charters only impact enrollment of public schools within 2 miles of the charter. Further, in an analysis of charter applicants in Boston, Walters (2014) finds that 40 percent of applicants apply to the closest charter school while a further 22 percent apply to the second closest. While we do not have data on who actually applies to or attends charters, we note that in LA County the median property is 1.35

miles from the nearest charter while the second closest charter 2.18 miles away. Since these measures include all properties, it is likely that the average distances for charter attendees are substantially smaller. Based on these factors, we believe that 2 miles is a reasonable maximum distance, though we also check distances between 2 and 5 miles in the appendix.

Our baseline model estimates the impact of charter penetration on the log of the sales price of property *i* in census block *s* at time *t* as

(3.1) $Ln(SalePrice_{ist}) = \alpha + \boldsymbol{Charter_{it}\beta} + \boldsymbol{X_{it}\Gamma} + \boldsymbol{H_i\Phi} + \lambda_t + \gamma_s + \varepsilon_{it}$

where Charter is a vector of charter penetration variables calculated as the number of charters or the share of public school enrollment in charters between 0 and 0.5 miles, 0.5 and 1 mile, 1 and 1.5 miles, and 1.5 and 2 miles from the property. The $\boldsymbol{\beta}$ coefficients can be interpreted as jointly identifying a house price gradient that captures the differential valuation of charter penetration by homeowners over distance. $\boldsymbol{X}$ is a vector of school-by-year observables, where the school is the elementary school to which the property is zoned. $\boldsymbol{H}$ is a vector of house-specific characteristics, such as the number of bedrooms, the number of bathrooms, age, quality and square footage. The model also includes month-by-year fixed effects (λ_t) to control for common time trends and census block fixed effects (γ_s) to control for time-invariant neighborhood quality and quality of the locally zoned school.[84] We cluster standard errors at the school zone level to account for correlation between prices of properties in the same census block. An adjustment to this model also restricts to charter schools within school-district boundaries. This is relevant since, as previously mentioned, California requires oversubscribed charters to give admissions priority to within-district students.

[84] The baseline model excludes school-zone fixed effects since most census blocks do not straddle school zones. Nonetheless, inclusion of school-zone fixed effects has a negligible impact on the results.

We expand the baseline model to account for heterogeneous effects on housing price by disaggregating our charter penetration variables by type of charter: conversion or startup. In this model, the charter penetration vector is split into two:

(3.2) $Ln(P_{ist}) = \alpha + \boldsymbol{StartupCharter}_{\mathbf{it}}\boldsymbol{\beta}_{\mathbf{1}} + \mathbf{ConversionCharter}_{\mathbf{it}}\boldsymbol{\beta}_{\mathbf{2}} +$

$$\mathbf{X}_{\mathbf{it}}\boldsymbol{\Gamma} + \mathbf{H}_{\mathbf{i}}\boldsymbol{\Phi} + \lambda_{\mathrm{t}} + \gamma_{\mathrm{s}} + \varepsilon_{\mathrm{it}}$$

In this set-up, the $\boldsymbol{\beta}_{\mathbf{1}}$ coefficients will provide a gradient for startup charters and the $\boldsymbol{\beta}_{\mathbf{2}}$ coefficients will provide a gradient for conversion charters. We include the same controls as in equation (3.1). As mentioned above, we would expect to find differing valuation of these two types of charters if homeowners place different weights on the inputs of each type; conversion charters often remain in the same building, with the same student body and staff, and adopting new operating styles while startup charters are often in rental spaces, tend to be smaller than conversions and traditional public schools, and need to recruit students and staff in addition to operating under a new management style.[85]

3.7 Results

3.7.1 Effect of Charter Penetration on Housing Prices

Table C.4 provides the baseline results of our analysis using variations of equation (3.1) and the sample of homes sold across all of LA County. The table includes two panels, one for each charter measure, overall numbers of charters and percentage of total enrollment attributed to charters. Each specification in the table includes month-by-year time dummies, housing controls – square footage, number of bedrooms, number of bathrooms, and quality – and controls for the locally zoned elementary school – enrollment, API score, school demographics, percentage

[85] The fact that conversions usually maintain the same attendance zone after converting suggests the potential for using a difference-in-differences approach to assessing the impacts of these schools on housing prices. Unfortunately, only five schools in LA County convert to charter status during our study period making the estimates from this type of analysis too imprecise.

disabled, gifted, free or reduced price lunch eligible, and English language learners. All standard errors are clustered at the school-zone level, where the school is the elementary school to which a property was zoned in 2002.

In columns (i) and (iv) of Table C.4, we regress the log of the house price on charter counts and the share of public school enrollment in charters within half mile diameter rings, respectively, without geographic fixed effects. The estimates suggest that there is a positive relationship that strengthens as the distance from the property increases. However, in columns (ii) and (v), we include elementary school-zone fixed effects to account for characteristics of the locally zoned school. In these models the patterns differ depending on how we measure charter penetration. When using charter counts, the results indicate that charters negatively impact housing prices, becoming more negative the closer charters are to the property. The coefficient on the zero to half mile radius charter measure indicates that an additional charter is associated with a statistically significant 3.5 log point decrease in the sale price. When using enrollment share, however, only 1 – 1.5 miles is significant.

However, we may still be concerned that there are endogenous differences within school zones, but across neighborhoods, that affect both housing prices and charter penetration. Thus in columns (iii) and (vi) we provide our preferred estimates that replace school-zone fixed effects with census-block fixed-effects. In this model, estimates are all statistically insignificant and small. The largest estimate in column (iii) suggests, when taken at face value, that an additional charter school increases housing prices between 1 and 1.5 miles away by 0.2 percent, with smaller values for other distances. For the enrollment share measure, all of the values are negative, insignificant, and economically small with a 10 percentage point (pp) increase in charter share reducing housing prices by less than 0.2 percent at all distance levels. To provide additional context, if we focus on charter penetration within 0.5 miles of the property, the 95% confidence interval for the impact of an additional charter is [-2%, 1%] while for a 10 pp increase in charter enrollment share it is [0.4%, -0.4%].

One potentially important issue in interpreting the estimated effect of charter penetration is that as the distance increases, the area in which the charter could locate increases. This is not a substantial concern when focusing on share of enrollment, but it does indicate that there may be more variation in the number of charters in farther rings making comparing the estimated effects of charter penetration at different distances difficult. To address this we also provide estimates using charter penetration within the full 2 mile radius around the property in Panel B. The results are similar to those in Panel A and show no impact of charters on housing prices when we include census block fixed-effects. It is also interesting to note that the standard errors decrease when we add census block (or school) fixed-effects. This is another indicator that there is substantial identifying power within blocks and that including between-block variation adds uninformative noise to the analysis.

Table C.5 provides results for our preferred model that includes census-block fixed-effects when we split the sample by whether the properties are within the boundaries of LAUSD, which is the largest district in LA County, or all other school districts in the county. We may suspect there are different property effects for the two samples because LAUSD covers the main urban core of the county, and recent evidence suggests that urban charters are more effective than suburban charters (Angrist, Pathak and Walters, 2011). Our results, however, provide little evidence that house price effects vary via this location difference. Only one estimate – for charter counts in LAUSD from 1 – 1.5 miles – is statistically significant.

Table C.6 provides the results for equation (3.2), splitting the charter penetration variable by charter type – conversion and startup – for homes in all of LA County. As in our regression split by school district, we focus on our preferred model with census block fixed-effects, zoned elementary school controls, and housing controls. As in the pooled model, none of the coefficients are statistically significant and the magnitudes and signs of the estimates do not reveal a consistent relationship between charter counts or charter enrollment rates and sale price for either charter type.

In Table C.7 we provide estimates that look at how charters affect house prices when we restrict the charters included in the count and enrollment share variables to those that are located in the same school district as the household. In California, within-district students get priority for charter enrollment and so there may be a stronger link with housing prices for these charters than those outside the district. Since LAUSD is especially large with most properties located far from district boundaries, when we estimate this model for LAUSD the estimates are little changed from baseline. Hence in Table C.7 we only provide estimates using the districts in LA County outside LAUSD. These estimates are the only ones in this paper that provide a consistent indicator of a charter impact on housing prices. Intriguingly, this estimated effect is negative. An additional charter school within 2 miles reduces house prices by 1.9 percent while a 10 percentage point increase in charter share of enrollment within 0.5 miles reduces prices by 1.2 percent. This analysis provides some evidence that charters weaken the link between public schools and housing prices.

We build on this analysis further by testing whether we see larger effects in areas with higher quality schools. Tables I.2 and I.3 in the appendix provide estimates that are allowed to differ by terciles of income and school API score counting all charters, and counting only within-district charters, respectively. While there are some marginally significant estimates in the low income schools in Table I.3, overall the evidence for a pattern across school types is weak. However, one possibility is that the district quality is what matters. In Table I.4 we investigate this by extending the model in Table C.7 to allow for different estimates by tercile of district API score. Here a clearer pattern emerges. In fact, the estimates suggest that there is a small increase in property values for high performing districts but a reduction for low performing districts. However, the relationship remains weak with only one estimate for bottom tercile schools significant at the 5% level. It is unclear why such a pattern emerges, but one possibility is that the low achieving districts that are competing with LAUSD, which is also low performing (12^{th} percentile API), benefit from a premium over LAUSD that is weakened by charters. Or it could be that the relationship between housing prices and school quality are more sensitive to charters in low performing districts. Nonetheless, it is unclear to what extent this restriction to within district charters should matter. While district students get priority, this is only relevant if charters are over-subscribed. Hence, given the null results when we do not make this restriction we think it is best to consider these estimates to be a bound on the potential negative effect of charters.

3.7.2 Testing for Endogenous Charter Location

A consistent estimate of the relationship between charter penetration and housing prices rests on the assumption that the variation in charter penetration is exogenous conditional on the included controls and, most important, the census block fixed-effects. As a test of this, we regress our limited set of housing characteristics on charter penetration variables and census block fixed effects. Ideally we would like to test the relationship between charter penetration and local neighborhood characteristics. However, including census block fixed-effects precludes such an analysis as we do not have access to time-varying neighborhood characteristics. Thus, we must rely on characteristics of the specific households that can be acquired from the property sales data.

Table C.8 presents results that estimate whether charter penetration is related to square footage, the number of bedrooms, the number of bathrooms, and the quality of the structures on the property as measured by the county assessor. We find no statistically significant relationship between the numbers of charters in any radius ring and square footage, the number of bedrooms, or the number of bathrooms. For quality, only the estimate for charters between 1.5 and 2 miles is statistically significant and only at the 10% level. For charter seats as a percentage of all public education seats, no estimate is statistically significant.

In a second analysis, we regress the log of house price on charter penetration within a half mile of the home in twelve month lag and lead intervals up to three years before the home was sold and three years after the home was sold. For example, the 12 month lag measure corresponds to charters within a half mile of the property that were in operation 12 months prior to the home's sale. The purpose of this analysis is to test for pre-existing trends and to see if there are any anticipatory or delayed impacts of charter openings. Thus, a clear pattern of higher prices from charters in operation after the house sale would be evidence of either anticipatory effects or preexisting trends in housing prices, the latter of which would invalidate the identification strategy. A pattern of higher prices from charters in operation prior to the home sale would indicate that housing prices are affected by charters but with a delay, potentially due to short-term price stickiness.

Table C.9 provides the impacts of lags and leads, which show little evidence of responses to charter penetration. Of the two significant coefficients, one is for the 12 month lead in charter enrollment percentage that suggests an increase in enrollment rates of 10 percentage points within a half-mile of the property 12 months following the sale of the home increases the sale price by 0.3%. While this could be indicative of a pre-existing trend, the other estimates indicate this is not likely to be the case. First, estimates for charter penetration 24 and 36 months after the sale show no impact. Second, there is no similar impact when measuring penetration using the number of charters. The other significant coefficient is for the 36 month lead in number of charters, suggesting an additional charter school within a half-mile of the property 36 months following the sale of the home increases the sale price by 1.9 percent. However, if this were indicative of an anticipatory response or pre-existing trend, we would expect to find significant impacts from charter penetration 24 months and 12 months after the sale, as well. Thus, while there are a couple estimates that indicate anticipatory responses or pre-trends, the bulk of the evidence in Table C.7 argues against such a pattern. Further, we note that the results in the table also provide little indication of a delayed response since there is no significant impact from the number of charters open or the charter enrollment rates 12, 24 or 36 months prior to the sale.

Finally, in Table C.10, we test the concern that the addition (or closure) of charter schools may generate sample selection by inducing some people to enter or stay out of the housing market. To do this we regress the number of annual sales in a census block on charter penetration near the block centroid. Further, even though we only have price data for the most recent sale of a property, we can see the dates for the three most recent sales. Thus in the second column we repeat the analysis using the three most recent sales of properties in the sales counts. The results show little impact of charter share of enrollment on housing sales. There is also no significant relationship between charter counts and sale counts within 1 mile of the centroid. Nonetheless, there is a statistically significant but economically small relationship between sales counts and the number of charters one to two miles from the centroid. The estimates suggest that, after conditioning on census block fixed-effects, a new charter opening one to two miles from the block centroid is related to an increase of 0.1 to 0.2 sales in a year. To put this in perspective it would take 5 to 10 new charter openings in a year to generate an additional sale. Given that the average number of charters in that distance range from properties is 1.9, we believe this impact is too small to substantially affect our estimates.

3.7.3 Effect of Charter Penetration on Housing Prices: Heterogeneity and Specification Checks

In the appendix we provide a series of analyses to look at impacts of charters when we allow the characteristics of the charters, local neighborhoods, and local public schools to vary. First, in Table I.5 we provide different estimates by the grade level of the charter. Thus we split charter penetration measures into four categories – elementary, middle, high and multi-level schools. We see little evidence of differential impacts on housing prices by the level of the charter school at any distance up to two miles from the property. Only one estimate out of 32 is statistically significant at the 5% level. Three more are significant at the 10% level, but show no clear pattern and differ in sign.

In Table I.6 we interact the charter penetration measures with the year of the property sale. Since the housing market in Los Angeles had undergone substantial declines just prior to our study, we may be concerned that the lack of capitalization is due to abnormal rigidities in the market, though we note that the significant effects when we restrict to within-district charters suggests this is not the case. Nonetheless, to address this, we focus on the estimates for 2010 and 2011, well after the market had started its recovery. As with our main results, we find no statistically significant impacts of charter penetration at any distance within 2 miles of a property in 2010 or 2011. In fact only one estimate out of the 32 shown is statistically significant - 1 to 1.5 miles in 2008.

In Table I.7 we provide evidence on whether the overall mean charter impacts may be hiding heterogeneous effects between neighborhoods with high performing and low performing schools by interacting the charter penetration variables with both the distance from the property and quartiles of household income (across all properties in the data) in the Census tract, the zoned elementary school's API score, percent minority enrollment in the zoned elementary school, and minority enrollment in the census tract. Only five estimates out of 128 are statistically significant at the 10% level (1 estimate at the 1% level) and do not show a clear pattern. Thus we see little indication our pooled estimates hiding heterogeneous impacts amongst these characteristics. Thus further indicates that the overall null results are not due to differential impacts from weak and strong schools canceling each other out.

Finally, in Table I.8 we provide estimates under different specifications and sample restrictions. Through all of these specification and sample checks, no estimates are statistically significant. These checks include using sale price levels rather than log sale prices, splitting the sample by the number of bedrooms, keeping properties with more than 8 bedrooms in the regression, dropping large (5000 square feet or larger) properties, dropping multi-unit properties, and limiting to the summer months of June, July and August as families with children are more likely to move during this period between school-years. Further we show that adding a fifth distance ring of 2 to 5 miles does not change the estimates, nor is the estimate on the added ring significant and adding in school fixed-effects (in addition to census block fixed effects) has little impact on the baseline estimates.

3.8 Conclusion

Research has previously shown close links between school quality and property prices. This has been explained as a capitalization of both the quality and capital stock of schooling into local property values given that typically students are required to attend a specific local public school. Hence, properties zoned to schools and districts with higher performance and more resources have seen higher values, all else equal. Charter schools have the potential to weaken this relationship. Students can typically attend a charter regardless of where they reside, thus making the local school potentially less important to residency decisions. Given that enrollment in charter schools has been increasing across the country over the past twenty years and, if present trends continue, is likely to increase further, the breaking of the link between housing prices and school quality can have implications for local public finance as well as socio-economic diversity across schools. .

In this study we directly estimate how charter schools affect local property values in Los Angeles County. We also expand our analysis to separate our measures of charter penetration by urbanicity, charter type, and grade level of the school along with wealth of the local neighborhood and the achievement levels of the local elementary school. Our approach follows the work other researchers have done relating school characteristics to housing prices, and carefully accounts for the correlation between neighborhood characteristics and housing prices by including census block fixed effects. This method allows us to estimate the impacts of charters net of any time-invariant differences between local neighborhoods and, by extension, local public schools. Using data from the Los Angeles County Assessor's Office on property sale prices from 2008 through 2011, our estimates show that there is very little impact of charters on home prices on average. The results are not sensitive to sample selection or model specification, nor do we find differential impacts by whether a charter is a startup or conversion, whether the property is in the primary urban school district in the area, Los Angeles Unified School District, by the grade level of the charter, by the income level of the neighborhood, or by test scores in the zoned elementary school. However, given that in California over-subscribed charters must provide priority enrollment to students within the local school district, we also estimate a model that restricts to charters located in the same school district as the property. In this case, which we consider a negative lower-bound impact as it is not clear whether such a restriction is appropriate, we find some evidence that housing prices actually fall by 2 percent for each additional charter within two miles. Since evidence of differential impacts by school quality is weak and, at best, negatively related to income and performance, this suggests that perhaps charter schools weaken the capitalization of schooling as a public good into property values rather than the capitalization of school quality in particular.

APPENDICES

APPENDIX A Tables for "State Responses to a Federal Matching Grant and Adoption from Foster Care"

Table A.1: Federal Title IV-E Eligibility Criteria

Aid to Families with Dependent Children (AFDC) Income Guidelines	Child's removal family meets state 1996 AFDC income standard
	OR
Supplemental Security Income (SSI)	Child meets income and disability requirements for Title XVI SSI[a]
	OR
Age at the end of the fiscal year	16 as of end of FY2010 14 as of the end of FY2011 12 as of the end of FY2012 10 as of the end of FY2013 8 as of the end of FY2014 6 as of the end of FY2015 4 as of the end of FY2016 2 as of the end of FY2017 birth as of FY2018
	OR
Length of time in care	A child of any age who has been in foster care for 60 continuous months
	OR
Sibling	A sibling of a child who meets the above age or length of stay requirements

For a more detailed description, See CRS Report, 2012. Section 473(e) of the Social Security Act. Additionally, infants born to foster youth are IV-E eligible if the minor parent is in foster care and receiving Title IV-E foster care maintenance payments.

a. There are income requirements for SSI benefits, but the child is considered to have no income if the child is in the custody of an agency.

Table A.2: Examples of State and Federal Shares of Adoption Assistance

	Meets federal critera	Special needs	Federal match?	Adoption assistance amount	X	Applicable federal share	=	Federal cost of adoption assistance	State cost of adoption assistance
	[1]	[2]	[3]	[4]		[5]		[6]	[7]
Massachusetts									
[a]	Yes	Yes	Yes	$1,056		56.2		$593	$462
[b]	No	Yes	No	$1,056		0		$0	$1,056
West Virginia									
[c]	Yes	Yes	Yes	$623		80.24		$500	$123
[d]	No	Yes	No	$623		0		$0	$623

[1] - [3]: [3] is a "No" if either either [1] or [2] is "No".
[4]: Represents total transfer to the adoptive family. Examples taken from AFCARS adoption data in fiscal year 2010 with 2012 real dollar figures for a female child aged 16 with special needs.
[5]: Federal Medical Assistance Percentages from U.S. Department of Health and Human Services.
[6]: [4] * [5].
[7]: (100 - [5]) * [4].

Table A.3: Present Discounted Value of State Cost of Adoption Assistance Payments

Monthly amount: $500, paid from age of adoption:	PDV, ($), Without federal match	PDV, ($), With federal match	Difference
	[1]	[2]	[3]
Match Rate = 50%			
15	17,156	8,578	8,578
10	40,718	20,359	20,359
5	59,180	29,590	29,590
Match Rate = 65%			
15	17,156	6,005	11,152
10	40,718	14,251	26,467
5	59,180	20,713	38,467
Match Rate =83%			
15	17,156	2,917	14,240
10	40,718	6,922	33,796
5	59,180	10,061	49,119

Notes: These calculations assume adoption assistance payments are paid 12 months a year for each year until a child turns 18, with a discount rate of 5%.

Table A.4: Summary Statistics: Characteristics of Children in Foster Care, 2000-2014

	Full sample	Discharge Reason: Adoption	Ever newly age or duration eligible	Never newly age or duration-eligible
Federal Title IV-E Eligibility Criteria				
Ever age- or duration- eligible	0.11	0.07	1.00	0.00
	(0.31)	(0.24)		
Duration				
Quarters in foster care from spell start	6.08	5.28	12.14	5.30
	(5.80)	(3.85)	(10.95)	(4.14)
Quarters in foster care from entry	12.16	11.28	19.95	11.15
	(8.36)	(6.66)	(13.03)	(6.94)
Child characteristics				
Age at entry (years)	3.27	2.64	8.11	2.65
	(4.01)	(3.45)	(4.13)	(3.54)
Age at spell start (years)	4.92	4.26	10.30	4.23
	(4.38)	(3.89)	(3.66)	(3.97)
Male	0.51	0.51	0.51	0.51
	(0.50)	(0.50)	(0.50)	(0.50)
White	0.48	0.49	0.45	0.48
	(0.50)	(0.50)	(0.50)	(0.50)
Black	0.27	0.27	0.28	0.27
	(0.45)	(0.44)	(0.45)	(0.44)
Hispanic	0.18	0.19	0.18	0.18
	(0.39)	(0.39)	(0.39)	(0.38)
Receiving matched foster care payments	0.39	0.38	0.40	0.39
	(0.49)	(0.49)	(0.49)	(0.49)
Disability	0.30	0.30	0.45	0.28
	(0.46)	(0.46)	(0.50)	(0.45)
Percent of spells ending in:				
Adoption	0.81	1.00	0.48	0.86
Emancipation	0.04	0.00	0.16	0.02
Relative/Guardian	0.02	0.00	0.03	0.02
Censored	0.11	0.00	0.30	0.08
Number of children	618,150	502,483	70,611	547,539

Source: Adoption and Foster Care Analysis and Reporting System (AFCARS) Foster Care Files, 2000–2014. Figures represent means with standard deviations in parentheses. Sample restricted to childrenwith mothers' rights terminated in fiscal years 2000 and later. A spell is completed if the child is observed to exit prior to October 1, 2014; spells are censored if the child remains in care after this date. Other discharge reasons are reunification, runaway, death, or transfer to another agency. Less than 2 percent of the spells in this sample exit for these reasons.

Table A.5: Increases in Federal Match Eligibility through Age and Duration

Percent receiving matched foster care payments or meeting age or duration federal criteria for adoption assistance[a]

Age at end of fiscal year	2006	2007	2008	2009	2010	2011	2012	2013	2014
0	0.40	0.42	0.40	0.36	0.38	0.42	0.41	0.44	0.50
1	0.54	0.52	0.51	0.49	0.50	0.51	0.50	0.52	0.54
2	0.49	0.50	0.50	0.46	0.49	0.49	0.49	0.51	0.53
3	0.49	0.49	0.47	0.46	0.47	0.48	0.47	0.50	0.53
4	0.49	0.49	0.49	0.45	0.47	0.48	0.48	0.50	0.54
5	0.49	0.50	0.48	0.46	0.48	0.50	0.49	0.51	0.53
6	0.48	0.48	0.48	0.44	0.49	0.50	0.51	0.52	0.54
7	0.46	0.48	0.47	0.45	0.51	0.53	0.50	0.53	0.54
8	0.48	0.48	0.48	0.44	0.51	0.52	0.53	0.52	1.00
9	0.48	0.47	0.46	0.45	0.52	0.53	0.53	0.52	1.00
10	0.48	0.49	0.47	0.43	0.52	0.53	0.53	1.00	1.00
11	0.47	0.48	0.46	0.43	0.54	0.55	0.53	1.00	1.00
12	0.47	0.48	0.47	0.45	0.55	0.57	1.00	1.00	1.00
13	0.47	0.47	0.47	0.43	0.59	0.60	1.00	1.00	1.00
14	0.46	0.46	0.46	0.43	0.60	1.00	1.00	1.00	1.00
15	0.45	0.46	0.45	0.43	0.63	1.00	1.00	1.00	1.00
16	0.42	0.43	0.44	0.39	1.00	1.00	1.00	1.00	1.00
17	0.41	0.41	0.41	0.38	1.00	1.00	1.00	1.00	1.00
18	0.29	0.32	0.30	0.27	1.00	1.00	1.00	1.00	1.00

Number receiving matched foster care payments or meeting age or duration federal critiera for adoption

Age at end of fiscal year	2006	2007	2008	2009	2010	2011	2012	2013	2014
0	331	377	356	336	333	346	358	406	430
1	7,261	6,915	6,841	6,017	5,622	5,484	5,258	5,651	6,208
2	7,016	7,569	7,755	6,828	6,662	6,214	6,005	6,207	6,637
3	6,108	6,267	6,058	5,650	5,432	5,403	5,083	5,296	5,496
4	5,160	5,191	5,229	4,594	4,561	4,564	4,461	4,583	4,926
5	4,603	4,687	4,541	4,255	4,069	3,954	3,998	4,168	4,377
6	4,179	4,258	4,233	3,589	3,763	3,558	3,557	3,779	4,044
7	3,746	3,846	3,682	3,492	3,527	3,352	3,164	3,407	3,629
8	3,531	3,603	3,561	3,120	3,377	3,098	3,067	3,001	6,051
9	3,413	3,341	3,180	2,988	3,205	3,032	2,812	2,800	5,554
10	3,245	3,228	3,072	2,779	3,073	2,869	2,701	4,904	4,999
11	3,192	3,057	2,941	2,612	3,096	2,831	2,621	4,759	4,746
12	3,183	3,068	2,830	2,577	3,027	2,907	4,763	4,507	4,428
13	3,326	3,031	2,829	2,459	3,146	2,985	4,702	4,541	4,495
14	3,371	3,175	2,899	2,541	3,177	5,032	4,726	4,587	4,533
15	3,454	3,321	3,019	2,624	3,544	5,127	4,911	4,741	4,693
16	3,148	3,362	3,176	2,688	6,182	5,579	5,293	5,099	4,970
17	2,915	3,072	3,110	2,810	6,826	6,067	5,610	5,336	5,147
18	1,717	2,031	1,967	1,805	6,250	5,825	5,030	4,617	4,362

Author's tabulations from AFCARS Foster Care files.

a. Figures calculated using data on the stock of children in foster care at the start of each fiscal year whose mother's rights have been terminated. Income-eligiblity is proxied by receipt of Title IV-E matched foster care payments. In fiscal years 2010 and later, age eligibility based on the child's age on the last day of the fiscal year and duration eligibility based on duration in care from the most recent removal date to the first day of the fiscal year.

Table A.6: Hazard Model Estimates of Effect of New Federal Match Eligibility on the Probability of Adoption

	Fiscal years 2000–2014			
	[1]	[2]	[3]	[4]
Age- or duration- eligible	1.070*	1.088**	1.089**	
	(0.044)	(0.045)	(0.043)	
	[0.008]	[0.010]	[0.010]	
Age- or duration- eligible x 16-19				1.044
				(0.072)
				[0.005]
Age- or duration- eligible x 14-15				1.115**
				(0.057)
				[0.013]
Age- or duration- eligible x 12-13				1.102**
				(0.049)
				[0.012]
Age- or duration- eligible x 10-11				1.111***
				(0.042)
				[0.013]
Age- or duration- eligible x 8-9				1.055
				(0.041)
				[0.006]
Age- or duration- eligible x 5-7				1.102
				(0.067)
				[0.012]
Age, duration from spell start, duration from entry, and quarter x year indicators	Y	Y	Y	Y
State indicators	Y	Y	Y	Y
Child covariates		Y	Y	Y
State covariates			Y	Y
Child-quarter observations	3,751,686	3,751,686	3,751,686	3,751,686
Number of children	618,150	618,150	618,150	618,150
Log pseudolikelihood	-1,314,119	-1,305,255	-1,304,920	-1,304,912
Mean of dep. var.	0.13	0.13	0.13	0.13

Source and notes: Adoption and Foster Care Analysis and Reporting System (AFCARS) Foster Care Files, 2000–2014. Results are from a complementary-log-log specification; exponentiated coefficients shown with marginal effects in brackets. Baseline hazard comprises 21 parameters and there are 72 duration from entry indicators. Child covariates are: whether or not the child receives matched foster care payments, has a disability, is male, and 6 race/ethnicity categories. State covariates are percent of the population aged 25 to 64, percent of the population black, unemployment rate, and log of per capita median income. Standard errors are clustered at the state level and shown in parentheses. Marginal effects are computed as the average derivative of the probability of adoption with respect to age or duration eligibility. *** p< 0.01, ** p<0.05, * p<0.10.

Table A.7: Falsification Tests of Hazard Model Estimates

	2005 rollout, 2000–2009	2004 rollout, 2000–2008	2010 rollout for younger ages, 2000–2014
	[1]	[2]	[3]
Age- or duration- eligible	1.042	1.022	1.017
	(0.035)	(0.042)	(0.025)
	[0.005]	[0.002]	[0.002]
Child-quarter observations	2,518,926	2,221,844	3,751,686
Number of children	418,648	374,978	618,150
Log pseduolikelihood	-837,507	-731,691	-1,308,869
Mean of dep. var.	0.12	0.12	0.13

Source and notes: Adoption and Foster Care Analysis and Reporting System (AFCARS) Foster Care Files, 2000–2014. Results are from the complementary-log-log specification in column 3 of Table A.6; exponentiated coefficients shown with marginal effects in brackets. Eligibility in [1]–[2] defined by starting rollout based on age and duration rules in the named year; eligibility in [3] defined by starting rollout in 2010 where eligibility defined by age at end of the fiscal year as 4+ in 2010 and later; 3+ in 2011 and later; 2+ in 2012 and later; age 1+ in 2013 and later; and ages 0+ in 2014. Standard errors are clustered at the state level and shown in parentheses. Marginal effects are computed as the average derivative of the probability of adoption with respect to age or duration eligibility. *** p< 0.01, ** p<0.05, * p<0.10.

Table A.8: Hazard Model Estimates: Heterogeneity Across States

	High	Low
	[1]	[2]
A. Match rate		
Age- or duration- eligible	1.169***	1.050
	(0.043)	(0.054)
	[0.019]	[0.006]
Mean of dep. var.	0.130	0.136
B. Share of matched foster care payments		
Age- or duration- eligible	1.066	1.135**
	(0.049)	(0.073)
	[0.008]	[0.015]
Mean of dep. var.	0.138	0.129
C. Share of matched adoption assistance		
Age- or duration- eligible	1.051	1.172***
	(0.046)	(0.051)
	[0.005]	[0.019]
Mean of dep. var.	0.138	0.129
D. Special needs		
Age- or duration- eligible	1.057	1.120**
	(0.051)	(0.055)
	[0.007]	[0.014]
Mean of dep. var.	0.134	0.133
E. Adoption assistance generosity		
Age- or duration- eligible	1.052	1.157***
	(0.044)	(0.053)
	[0.006]	[0.018]
Mean of dep. var.	0.135	0.133
F. Standard deviation of adoption assistance amounts		
Age- or duration- eligible	1.010	1.180***
	(0.040)	(0.053)
	[0.001]	[0.019]
Mean of dep. var.	0.151	0.120

Source and notes: Adoption and Foster Care Analysis and Reporting System (AFCARS) Foster Care Files, 2000–2014. Results are from the complementary-log-log specification in column 3 of Table A.6 (equation 1.1) using state subsamples in each panel; exponentiated coefficients shown with marginal effects in brackets. "High" and "Low" indicate whether the state subsample is above or below the median of the named panel characteristic. State subsamples are listed in Table G.4. Standard errors are clustered at the state level and shown in parentheses. *** p< 0.01, ** p<0.05, * p<0.10.

Table A.9: Short–Run Effects of the Introduction of Age Criteria on Monthly Adoptions

	+/- 1 months	+/- 2 months	+/- 3 months	+/- 4 months	+/- 6 months
	[1]	[2]	[3]	[4]	[5]
Dependent variable is number of adoptions					
Not age-eligible					
April					0.087***
					(0.029)
May					-0.054
					(0.083)
June				-0.061	-0.039
				(0.041)	(0.038)
July			-0.067	-0.033	-0.011
			(0.056)	(0.057)	(0.055)
August		-0.026	-0.050	-0.016	0.006
		(0.068)	(0.075)	(0.077)	(0.074)
September	-0.055	-0.133**	-0.156***	-0.123**	-0.101**
	(0.054)	(0.058)	(0.053)	(0.050)	(0.048)
Age-eligible					
October	0.169***	0.092**	0.068*	0.102***	0.124***
	(0.033)	(0.037)	(0.039)	(0.037)	(0.032)
November		0.157***	0.133***	0.167***	0.189***
		(0.044)	(0.046)	(0.051)	(0.050)
December			0.227***	0.260***	0.282***
			(0.069)	(0.072)	(0.076)
January				-0.039	-0.036
				(0.085)	(0.076)
February					0.008
					(0.059)
March					0.004
					(0.059)
N	28,560	57,120	85,680	114,240	171,360
Log pseduolikelihood	-53,673	-117,127	-174,973	-230,390	-332,611
Mean of dep. var.	3.85	4.59	4.57	4.40	4.22

Source and notes: Adoption and Foster Care Analysis and Reporting System (AFCARS) Adoption Files, 2000–2014. Dependent variable is state monthly adoption counts by age, as measured at the end of the fiscal year at the time of adoption. Sample is monthly adoptions within the relevant window. Window denotes the number of months before and after the start of the fiscal year (October). Estimates are based on a Poisson regression, with standard errors shown in parentheses. All specifications include age, calendar month, year, and state fixed effects, and an age-specific linear time trend. *** p< 0.01, ** p<0.05, * p<0.10.

Table A.10: Special Needs Designation

	All	White	No clinical disability	No siblings
	[1]	[2]	[3]	[4]
Age- or duration- eligible	-0.041	-0.064	-0.004	-0.012
	(0.117)	(0.118)	(0.096)	(0.153)
	[-0.005]	[-0.010]	[-0.001]	[-0.002]
	(0.015)	(0.019)	(0.016)	(0.019)
Observations	220,840	85,750	135,119	102,376
Mean of dep. var.	0.91	0.87	0.87	0.90

Source and notes: Adoption and Foster Care Analysis and Reporting System (AFCARS) Adoption and Foster Care Files, 2004-2014. Sample is adopted children in the linked Adoption files and Foster Care files, consisting of children adopted over federal fiscal years 2004 through 2014 in: Arizona, California, Delaware, Florida, Hawaii, Kansas, Louisiana, Maine, Massachusetts, Mississippi, Montana, Ohio, South Carolina, South Dakota, Texas, West Virginia, and Wyoming. Regressions are probit specifications; probit coefficients shown with marginal effects in brackets. Standard errors for each shown in parentheses below the etimate. All specifications include state, fiscal year, age, and duration indicators. Additional controls are log of per capita median incom and child covariates: whether or not the child receives matched foster care payments, has a disability, is male, and 6 race/ethnicity categories (except in column 2). Standard errors are clustered at the state level and shown in parentheses. *** p< 0.01, ** p<0.05, * p<0.10.

APPENDIX B Tables for “The Effects of Federal Adoption Incentive Awards for Older Children on Adoptions from U.S. Foster Care”

Table B.1: Changes in Incentive Structure for Adoptions of Children Aged 9 and Older

Fiscal years of payments:	1998-2002	2003-2007	2008-2012
Qualifying adoptions of children *with* special needs			
Aged less than 9 years	\$2,000	\$2,000	\$4,000
Aged 9 years and older	\$2,000	\$4,000	\$8,000
Qualfiying adoptions of children *without* special needs			
Aged less than 9 years	N/A	N/A	N/A
Aged 9 years and older	N/A	\$4,000	\$8,000

Source: 42 USC 673b: Adoption incentive payments.

Note: Adoptions qualify for awards if they exceed the state-specific baseline number of adoptions in the appropriate category. Award amounts are those states can earn in the specified award categories; these award categories are in addition to amounts states can earn for an overall increase in the total annual number of *all* adoptions (also over a state-specific baseline).

Table B.2: Number of Adoptions of Children Aged 9 and Older, 2000-2010

State	2000	2001	**2002 baseline**	2003	2004	2005	2006	**2007 baseline**	2008	2009	2010
	[1]	[2]	[3]	[4]	[5]	[6]	[7]	[8]	[9]	[10]	[11]
Alabama	58	69	**80**	97	135	108	118	**115**	136	186	220
Alaska	67	66	**54**	68	58	78	60	**72**	87	99	114
Arizona	258	282	**228**	217	182	233	288	**345**	388	392	536
Arkansas	131	134	**115**	125	100	119	121	**102**	116	147	135
California	1,745	2,009	**2,248**	1,879	1,743	1,812	1,664	**1,646**	1,734	1,555	1,293
Colorado	196	168	**251**	245	269	212	202	**236**	207	204	210
Connecticut	115	92	**179**	119	137	149	147	**140**	157	156	142
Delaware	23	30	**52**	27	17	18	17	**24**	18	31	14
D.C.	115	80	**114**	119	199	137	72	**63**	38	36	49
Florida	444	376	**652**	798	1,011	816	824	**703**	951	919	843
Georgia	360	273	**343**	345	346	289	342	**356**	356	405	370
Hawaii	51	57	**78**	76	67	88	79	**48**	66	63	53
Idaho	34	35	**28**	44	63	62	55	**56**	60	92	83
Illinois	1,934	1,441	**1,289**	924	704	529	432	**336**	358	358	302
Indiana	413	299	**345**	296	319	264	307	**383**	458	433	367
Iowa	230	234	**280**	317	279	230	241	**240**	213	217	179
Kansas	178	146	**166**	170	207	217	152	**205**	214	208	168
Kentucky	115	192	**220**	254	285	319	272	**209**	247	290	293
Louisiana	181	168	**154**	156	125	126	115	**96**	117	103	140
Maine	108	107	**109**	99	95	109	107	**113**	93	83	62
Maryland[a]	151	274	**303**	277	262	175	109	**150**	61	170	167
Massachusetts	217	207	**233**	198	220	171	185	**189**	125	137	141
Michigan	931	1,002	**991**	911	965	1,001	891	**828**	843	963	758
Minnesota	205	163	**185**	160	164	176	169	**153**	158	158	162
Mississippi	105	102	**68**	44	112	93	73	**95**	84	86	91
Missouri	415	340	**427**	504	461	430	342	**286**	317	292	291
Montana	65	103	**85**	63	46	57	70	**70**	61	49	46
Nebraska	93	107	**83**	79	110	100	133	**141**	150	139	104
Nevada	44	55	**70**	82	69	77	116	**122**	122	111	153
New Hampshire	21	24	**48**	44	44	30	51	**43**	55	50	59
New Jersey	178	218	**330**	181	316	317	307	**375**	311	361	366
New Mexico	96	133	**94**	89	81	125	117	**118**	127	156	119
New York	1,891	1,682	**1,374**	1,712	1,772	1,472	1,207	**1,053**	976	952	798
North Carolina	389	408	**440**	444	359	338	305	**376**	438	455	460
North Dakota	9	40	**23**	46	32	31	30	**27**	26	24	37
Ohio	527	645	**810**	868	700	667	627	**541**	454	396	325
Oklahoma	381	332	**315**	350	351	298	347	**343**	376	350	381
Oregon	220	301	**294**	226	221	215	215	**234**	227	250	154
Pennsylvania	587	606	**721**	721	687	705	589	**538**	516	501	554
Rhode Island	68	73	**70**	72	65	61	59	**57**	64	63	44
South Carolina	110	124	**110**	78	98	111	119	**113**	135	125	126
South Dakota	23	18	**32**	37	40	37	45	**51**	38	42	36
Tennessee	169	264	**377**	433	394	475	420	**524**	435	342	379
Texas	498	516	**512**	572	508	662	725	**805**	1,007	1,122	1,172
Utah	68	72	**68**	57	58	69	99	**80**	93	83	105
Vermont	33	39	**50**	67	54	48	55	**67**	50	50	54
Virginia	159	177	**159**	181	226	182	196	**215**	164	217	224
Washington	252	257	**206**	264	221	231	201	**246**	240	307	392
West Virginia	126	134	**136**	108	121	118	111	**105**	107	153	183
Wisconsin	224	204	**312**	414	443	319	261	**219**	175	187	178
Wyoming	18	18	**16**	18	25	21	15	**12**	23	19	18

Sources
a. Maryland's 2007 baseline was 43 for fiscal year 2008. It was corrected to 150 for 2009 and beyond.
[1] - [2]: Author's tabulations from AFCARS Adoption data files, fiscal years 2000 and 2001.
[3]: Older child baseline: US DHHS (2004). Program Instruction, Procedures for the Implementation of the Adoption Promotion Act of 2003 (P.L. 108-145). Administration on Children, Youth and Families.
[4] - [7]: Author's tabulations from AFCARS Adoption data files, fiscal years 2003 through 2006.
[8]-[11]: U.S. Department of Health and Human Services (2013). Adoption Incentives Awards by Category for Earning Years 2008-2012.
[9]-][11]: Author's tabulations from AFCARS Adoption data files, fiscal years 2008 through 2010.

Table B.3: Descriptive Statistics for Children in Foster Care

Time period (fiscal years)	2003-2004				2008-2009			
Ages	7	8	9	10	7	8	9	10
	[1]	[2]	[3]	[4]	[5]	[6]	[7]	[8]
Number of child-month observations	70,410	72,702	76,155	81,738	72,785	66,423	60,752	54,905
Percentage ending in adoption[a]	0.05	0.05	0.04	0.03	0.06	0.06	0.05	0.05
	(0.22)	(0.21)	(0.20)	(0.18)	(0.23)	(0.23)	(0.22)	(0.22)
Mean months in foster care from TPR:								
Among all child-month observations	15.82	16.52	18.36	19.57	14.25	14.85	15.49	16.57
	(11.47)	(11.77)	(12.14)	(12.54)	(10.82)	(11.12)	(11.47)	(11.77)
Among child-month observations ending in adoption[a]	17.68	18.35	18.60	19.57	16.70	16.78	16.72	18.08
	(10.45)	(10.65)	(10.76)	(12.58)	(9.82)	(9.96)	(10.22)	(10.56)
Among child-month observations not ending in adoption[b]	15.72	16.44	18.35	19.57	14.10	14.73	15.42	16.49
	(11.52)	(11.82)	(12.20)	(12.58)	(10.86)	(11.18)	(11.53)	(11.83)
Child characteristics								
Age at entry	3.45	4.26	4.93	5.65	3.87	4.75	5.64	6.44
	(1.84)	(2.05)	(2.26)	(2.51)	(1.58)	(1.73)	(1.90)	(2.04)
Age at mother's rights termination	5.62	6.50	7.27	8.07	5.78	6.71	7.62	8.44
	(1.36)	(1.49)	(1.63)	(1.81)	(1.25)	(1.33)	(1.45)	(1.60)
Percentage male	0.53	0.53	0.54	0.54	0.52	0.50	0.51	0.52
	(0.50)	(0.50)	(0.50)	(0.50)	(0.50)	(0.50)	(0.50)	(0.50)
Percentage white	0.41	0.40	0.40	0.41	0.46	0.46	0.47	0.48
	(0.49)	(0.49)	(0.49)	(0.49)	(0.50)	(0.50)	(0.50)	(0.50)
Percentage black	0.38	0.41	0.42	0.42	0.29	0.29	0.29	0.29
	(0.49)	(0.49)	(0.49)	(0.49)	(0.45)	(0.45)	(0.45)	(0.45)
Percentage hispanic	0.16	0.15	0.14	0.14	0.21	0.21	0.21	0.19
	(0.37)	(0.36)	(0.35)	(0.34)	(0.40)	(0.40)	(0.40)	(0.39)
Percentage receiving Title IV-E foster care payments	0.29	0.29	0.30	0.31	0.35	0.34	0.34	0.33
	(0.45)	(0.45)	(0.46)	(0.46)	(0.48)	(0.47)	(0.47)	(0.47)

Source: Adoption and Foster Care Analysis and Reporting System (AFCARS). Baseline sample restricted to children whose mother's parent rights are terminated during the first spell in foster care (with rights terminated prior to the child's first discharge from care). Data means and standard deviations in columns [1]-[4] are for children in care over federal fiscal years 2003 through 2004. The 2003-2004 sample excludes spells with adoptions occurring in the +/– two week window around December 2, 2003. The 301,005 child-month observations in columns [1]-[4] represent spells of 31,202 children, for whom 82 percent of spells end in adoption. Data means and standard deviations in [5]-[8] are for children in care over federal fiscal years 2008 through 2009. The 2008-2009 sample excludes spells with adoptions occurring in the +/– two week window around October 1, 2008. The 254,865 total child-month observations in [5]-[8] represent 28,363 spells, 94.5 percent of which end in adoption. Ages reference whether the child's age, measured in months, during the child-month observation is 84-95 months (age 7), 96-107 months (age 8), 108-119 months (age 9), or 120-131 months

a. Percentage of child-month observations in which the child exits to an adoptive home in that month.

b. References child-month observations in which the child is still in care or is discharged via another discharge reason. Discharge reasons include adoption, reunification, guardianship, living with a relative, emancipation, runaway, death of a child, or transfer to another agency.

Table B.4: Effect of the Introduction of Incentives for Adoptions of Older Children

Time period (fiscal years)	2003-2004		2001-2005
Ages	8-9	7-10	8-9
	[1]	[2]	[3]
Post x older (γ)	-0.0430	-0.0048	-0.0291
	(0.0515)	(0.0435)	(0.0363)
	[-0.0018]	[-0.0002]	[-0.0011]
Mean of dependent variable	0.043	0.043	0.042
Child-month observations	148,857	301,005	390,920
Number of children	18,146	31,202	37,895
Log likelihood	-25,542	-51,195	-65,281

Source: Adoption and Foster Care Analysis and Reporting System (AFCARS). See text for description of sample. *Post* is an indicator for December, 2003 and later and *older* is an indicator for aged 9 and older. Fiscal year restrictions in columns [1] and [2] are for child-month observations in FY03 through FY04. The fiscal year restriction in column [3] is for child-month observations in FY01 through FY05. Age restrictions in columns [1] and [3] are for child-month observations with children aged 8 up to, but not including, age 10, and in columnn [2], for child-month observations with children aged 7 up to, but not including, age 11. All specifications include child controls, state controls, indicators for duration month from mother's date of rights termination, indicators for age as measured in months, state indicators, and month-by-year indicators. Logit coefficients with average partial effects in brackets. Standard errors are clustered at the state level and shown in parentheses. *** p< 0.01, ** p<0.05, * p<0.10.

Table B.5: Effect of the 2008 Update to Incentives for Adoptions of Older Children

Time period (fiscal years)	2008-2009		2006-2010
Ages	8-9	7-10	8-9
	[1]	[2]	[3]
Post x older (γ)	0.0927**	0.0377	0.0222
	(0.0418)	(0.0353)	(0.0227)
	[0.0047]	[0.0019]	[0.0011]
Mean of dependent variable	0.054	0.055	0.055
Child-month observations	127,175	254,865	309,685
Number of children	16,601	28,363	33,165
Log likelihood	-25,645	-51,620	-63,451

Source: Adoption and Foster Care Analysis and Reporting System (AFCARS). See text for description of baseline sample. Fiscal year restrictions in columns [1] and [2] are for child-month observations in FY08 through FY09. Fiscal year restriction in column [3] is for child-month observations in FY06 through FY10. Age restrictions in columns [1] and [3] are for child-month observations with children aged 8 and up to, but not including, age 10, and in columnn [2], for child-month observations with children aged 7 up to, but not including, age 11. All specifications include child controls, state controls, indicators for duration month from mother's date of rights termination, indicators for age as measured in months, state indicators, and month-by-year indicators. Logit coefficients, average partial effects in brackets. Standard errors are clustered at the state level and shown in parentheses. *** $p< 0.01$, ** $p<0.05$, * $p<0.10$.

Table B.6: Probability of September Adoption Among Older Children

Time period (calendar years)	2003-2007		2008-2010		2003-2010	
Ages	8-9	7-10	8-9	7-10	8-9	7-10
	[1]	[2]	[3]	[4]	[5]	[6]
Older x Exceeded baseline (γ_1)	-0.0206	-0.0238	0.0570*	0.0161	0.0136	-0.0119
	(0.0317)	(0.0230)	(0.0326)	(0.0261)	(0.0192)	(0.0161)
Exceeded baseline (γ_2)	0.181**	0.148**	-0.0005	0.0142	0.0629	0.0597*
	(0.0685)	(0.0580)	(0.0629)	(0.0542)	(0.0397)	(0.0346)
Older (γ_3)	0.0115	0.0120	-0.0238	-0.0180	-0.0006	0.0038
	(0.0196)	(0.0087)	(0.0275)	(0.0196)	(0.0118)	(0.0078)
Mean of dependent variable	0.520	0.521	0.523	0.520	0.521	0.521
Observations	4,076	8,091	2,506	5,072	6,582	13,163

Source: Adoption and Foster Care Analysis and Reporting System (AFCARS). Age restrictions in column [1], [3], and [5] are for children aged 8 or 9 at the time of adoption. Age restrictions in column [2], [4], and [6] are for children aged 7 through 10 at the time of adoption. Each column shows the result of a linear probability model predicting September adoption. All specifications include child controls, state controls, and calendar year indicators (for September-October pairs). Standard errors are clustered at the state level and shown in parentheses. *** p< 0.01, ** p<0.05, * p<0.10.

APPENDIX C Tables for "Capitalization of Charter Schools into Residential Property Values"

Table C.1: Schools in LA County

A. Schools by Grade Level

	Elementary	Middle	High	Multiple Levels
Non-charter public schools	1,196	243	390	68
Total charter schools	113	48	88	35
% Charter Schools	8.6%	16.5%	18.4%	34.0%
Conversion charters	21	1	10	3
Start-up charters	92	47	78	32

B. Schools by Years of Operation

	Non-Charter Public	Conversion	Start-up	% Charter Schools
2008	1,743	19	127	7.7%
2009	1,758	23	147	8.8%
2010	1,777	24	181	10.3%
2011	1,809	26	213	11.7%

Note: Schools included in panel A are those open and active at any point September 2008 through September 2011. Data obtained from California Department of Education.

Table C.2: Summary Statistics of Properties with Sale Prices

Property Characteristics	
Sale price	383,546
	(247,685)
# of Beds	2.98
	(1.05)
# of Baths	2.11
	(0.92)
Square Footage	1,573
	(718)
Quality	6.45
	(1.25)
Number of Charters	
0 - 0.5 miles	0.16
	(0.54)
0.5 - 1 mile	0.47
	(1.11)
1 - 1.5 miles	0.78
	(1.59)
1.5 - 2 miles	1.06
	(2.04)
Charters as percentage of enrollment	
0 - 0.5 miles	0.05
	(0.18)
0.5 - 1 mile	0.06
	(0.15)
1 - 1.5 miles	0.06
	(0.13)
1.5 - 2 miles	0.06
	(0.12)
Observations	158,211

Notes: Summary statistics are means for sales from September 2008 through September 2011. Property sample excludes homes with a sale price exceeding $1.5 million, and a bedroom or bathroom count in excess of eight. Homes are divided into the "LAUSD" or "Rest of LA County" samples via the location of the elementary school to which the property is zoned. Standard deviations in parentheses.

Table C.3: Summary Statistics – School Near Properties with Sale Prices

	A: Characteristics of zoned school			*B: Characteristics of charters within 1 mile (enrollment weighted)*		
	Elementary	Middle	High	Elementary	Middle	High
Enrollment	440.5	1,197.4	2,002.6	443.0	1,121.3	1,140.5
	(165.6)	(488.0)	(680.6)	(138.0)	(435.7)	(814.3)
API Score	805.6	746.1	707.0	800.1	744.7	663.7
	(73.6)	(90.3)	(88.2)	(64.5)	(93.0)	(112.4)
% Black	10.9	10.1	11.2	10.6	10.3	11.1
	(14.4)	(11.9)	(13.3)	(13.2)	(11.6)	(12.6)
% Hispanic	58.2	62.9	60.1	62.0	63.6	65.2
	(28.5)	(24.7)	(24.8)	(25.7)	(25.2)	(23.9)
% Asian	7.1	7.0	7.6	7.2	7.7	5.9
	(12.6)	(12.0)	(12.3)	(12.0)	(13.2)	(11.5)
% Disabled	11.4	11.4	10.3	11.8	11.1	10.6
	(4.5)	(2.8)	(3.1)	(4.1)	(2.7)	(12.2)
% Gifted	8.4	13.8	11.4	7.5	12.6	7.3
	(7.3)	(9.7)	(8.8)	(5.4)	(8.6)	(7.0)
% Free or Reduced Price Lunch	64.7	66.9	55.9	68.7	67.5	61.2
	(30.0)	(25.7)	(26.9)	(26.6)	(26.6)	(24.1)
% English Language Learner	28.1	19.8	18.0	30.3	20.2	20.8
	(17.2)	(11.5)	(10.5)	(15.2)	(11.7)	(12.2)
Observations	158,211	127,558	141,212	136,546	81,204	83,079
for api score:	158,211	127,174	140,866	136,536	80,686	80,979

Notes: Summary statistics are means for sales from September 2008 through September 2011. Sample excludes homes with a sale price exceeding $1.5 million, and a bedroom or bathroom count in excess of eight. School zones are based on 2002 zoning. See text for details on how to access school zone maps. Standard deviations in parentheses.

Table C.4: Effect of Charters on Log Sale Prices for Los Angeles County

	LA County					
	Number of charters			Charter seats as percentage of enrollment		
	(i)	(ii)	(iii)	(iv)	(v)	(vi)
A. Distance gradient						
0 - 0.5 miles	-0.00725	-0.0353***	-0.00543	0.0741*	0.00648	-0.00134
	(0.0131)	(0.00800)	(0.00827)	(0.0438)	(0.0249)	(0.0194)
0.5 - 1 mile	0.00858	-0.0253***	0.000950	0.117**	-0.0166	-0.0128
	(0.00748)	(0.00609)	(0.00476)	(0.0564)	(0.0270)	(0.0195)
1 - 1.5 miles	0.0252***	-0.0149***	0.00223	0.140**	-0.0442*	-0.0123
	(0.00578)	(0.00387)	(0.00313)	(0.0616)	(0.0268)	(0.0239)
1.5 - 2 miles	0.0239***	-0.00460	-0.00110	0.120	-0.0217	-0.00470
	(0.00494)	(0.00309)	(0.00279)	(0.0770)	(0.0340)	(0.0255)
B. Condensed 0-2 miles						
0 - 2 miles	0.0193***	-0.0101***	-9.80e-05	0.328***	-0.0301	-0.00750
	(0.00321)	(0.00255)	(0.00207)	(0.112)	(0.0609)	(0.0544)
Observations	158,211	158,211	158,211	158,211	158,211	158,211
Housing Characteristics	Y	Y	Y	Y	Y	Y
School Characteristics	Y	Y	Y	Y	Y	Y
School Fixed-Effects	N	Y	N	N	Y	N
Census Block Fixed-Effects	N	N	Y	N	N	Y

Sample includes property sales from April 2009 through September, 2011. The independent variable denotes either the number of charters in operation or the share of enrollment in operating charters as of the sale date in various distance rings from the property. Housing chracteristics include number of bedrooms, bathrooms, square footage, and quality. School chracteristics include API levels overall, lags and second lags of overall API scores, % of students of each race, % free lunch, % gifted, % English language learners, % disabled, and parent education levels for elementary school zoned to the property in 2002. All regressions include month-by-year fixed-effects. Robust standard errors clustered by elementary school zone in 2002 in parentheses. *, **, and *** denote significance at the 10%, 5% and 1% levels, respectively.

Table C.5: Effect of Charters on Log Sale Prices for Los Angeles County by School District

	LAUSD		Rest of LA County	
	Number of charters	Charter seats as percentage of enrollment	Number of charters	Charter seats as percentage of enrollment
0 - 0.5 miles	0.000504	0.00923	-0.0143	-0.0220
	(0.00970)	(0.0236)	(0.0169)	(0.0373)
0.5 - 1 mile	0.00591	-0.00620	-0.00447	-0.0114
	(0.00559)	(0.0278)	(0.00869)	(0.0241)
1 - 1.5 miles	0.00712**	-0.00607	-0.00225	0.000579
	(0.00349)	(0.0314)	(0.00632)	(0.0351)
1.5 - 2 miles	0.00233	-0.0130	-0.00375	0.0259
	(0.00342)	(0.0408)	(0.00470)	(0.0206)
Observations	65,170	65,170	93,041	93,041
R-squared	0.83	0.83	0.91	0.91
Housing Characteristics	Y	Y	Y	Y
School Characteristics	Y	Y	Y	Y
School Fixed-Effects	N	N	N	N
Census Block Fixed-Effects	Y	Y	Y	Y

Notes: See Table C.4 for a description of baseline sample and controls. Robust standard errors clustered by elementary school zone in 2002 in parentheses. *, **, and *** denote significance at the 10%, 5% and 1% levels, respectively.

Table C.6: Effect of Charters on Log Sale Prices by Charter Type

	LA County	
	Number of charters	Charter seats as percentage of enrollment
Start-up charters		
0 - 0.5 miles	-0.00450	0.00389
	(0.00952)	(0.0226)
0.5 - 1 mile	0.00253	0.000917
	(0.00537)	(0.0241)
1 - 1.5 miles	0.00341	0.00269
	(0.00357)	(0.0240)
1.5 - 2 miles	-0.00110	0.0273
	(0.00299)	(0.0234)
Conversion charters		
0 - 0.5 miles	-0.0137	-0.0226
	(0.0133)	(0.0362)
0.5 - 1 mile	-0.0110	-0.0432
	(0.0103)	(0.0311)
1 - 1.5 miles	-0.00664	-0.0397
	(0.00854)	(0.0456)
1.5 - 2 miles	-0.00225	-0.0506
	(0.00788)	(0.0513)
Observations	158,211	158,211
R-squared	0.881	0.881
Housing Characteristics	Y	Y
School Characteristics	Y	Y
School Fixed-Effects	N	N
Census Block Fixed-Effects	Y	Y

Notes: See Table C.4 for a description of baseline sample and controls. Robust standard errors clustered by elementary school zone in 2002 in parentheses. *, **, and *** denote significance at the 10%, 5% and 1% levels, respectively.

Table C.7: Effect of Charters Within the Home's School District on Log Sale Prices for Los Angeles County Excluding LAUSD

	Number of charters	Charter seats as percentage of enrollment
0 - 0.5 miles	-0.0387	-0.118***
	(0.0284)	(0.0431)
0.5 - 1 mile	-0.0123	-0.0786*
	(0.0149)	(0.0409)
1 - 1.5 miles	-0.0178*	-0.0758*
	(0.0098)	(0.0433)
1.5 - 2 miles	-0.0186	-0.0670
	(0.0156)	(0.0541)
B. Condensed 0-2 miles		
0 - 2 miles	-0.0192**	-0.0292
	(0.0090)	(0.1540)
Observations	93,041	93,041
Housing Characteristics	Y	Y
School Characteristics	Y	Y
School Fixed-Effects	N	N
Census Block Fixed-Effects	Y	Y

Sample includes property sales from April 2009 through September, 2011. The independent variable denotes either the number of charters within the home's zoned school district in operation or the share of enrollment in operating charters as of the sale date in various distance rings from the property. Housing chracteristics include number of bedrooms, bathrooms, square footage, and quality. School chracteristics include API levels overall, lags and second lags of overall API scores, % of students of each race, % free lunch, % gifted, % English language learners, % disabled, and parent education levels for elementary school zoned to the property in 2002. All regressions include month-by-year fixed-effects. Robust standard errors clustered by elementary school zone in 2002 in parentheses. *, **, and *** denote significance at the 10%, 5% and 1% levels, respectively.

Table C.8: Impacts of Charters on Exogenous Observables

	LA County			
Number of charters	Square footage	# of Beds	# of Baths	Quality
0 - 0.5 miles	3.899	0.0147	0.0186	0.0124
	(14.0)	(0.0247)	(0.0192)	(0.0172)
0.5 - 1 mile	-6.593	-0.0112	-0.0183	-0.0096
	(7.4)	(0.0147)	(0.0112)	(0.0098)
1 - 1.5 miles	4.954	0.0116	0.0045	-0.0086
	(5.0)	(0.0098)	(0.0074)	(0.0066)
1.5 - 2 miles	-2.357	-0.0015	0.0001	-0.0120*
	(4.8)	(0.0097)	(0.007)	(0.0065)
Observations	158,211	158,211	158,211	158,211
Charter seats as percentage of enrollment	Square footage	# of Beds	# of Baths	Quality
0 - 0.5 miles	-22.710	-0.005	0.023	0.025
	(37.3)	(0.048)	(0.040)	(0.046)
0.5 - 1 mile	25.590	0.011	0.023	-0.037
	(40.6)	(0.062)	(0.050)	(0.05)
1 - 1.5 miles	28.490	0.011	-0.009	-0.021
	(39.5)	(0.057)	(0.051)	(0.050)
1.5 - 2 miles	-23.360	-0.044	-0.046	0.000
	(43.2)	(0.052)	(0.052)	(0.049)
Observations	158,211	158,211	158,211	158,211
R-squared	0.67	0.55	0.59	0.80
School Fixed-Effects	N	N	N	N
Census Block Fixed-Effects	Y	Y	Y	Y

Notes: See Table C.4 for a description of baseline sample. Robust standard errors clustered by elementary school zone in 2002 in parentheses. *, **, and *** denote significance at the 10%, 5% and 1% levels, respectively.

Table C.9: Effect of Lags and Leads of Charter Penetration

	LA County	
	Number of charters	Charter seats as percentage of enrollment
36 months prior to sale	-0.0005	-0.001
	(0.0100)	(0.027)
24 months prior to sale	-0.0039	-0.017
	(0.0093)	(0.031)
12 months prior to sale	-0.0055	-0.008
	(0.0089)	(0.030)
Time of sale	-0.0010	0.006
	(0.0088)	(0.031)
12 months after sale	0.0038	0.033**
	(0.0054)	(0.015)
24 months after sale	-0.0082	-0.019
	(0.0080)	(0.025)
36 months after sale	0.0190*	0.013
	(0.0102)	(0.037)
Observations	158,211	158,211
R-squared	0.88	0.88

Notes: See Table C.4 for a description of baseline sample and controls. Robust standard errors clustered by elementary school zone in 2002 in parentheses. *, **, and *** denote significance at the 10%, 5% and 1% levels, respectively.

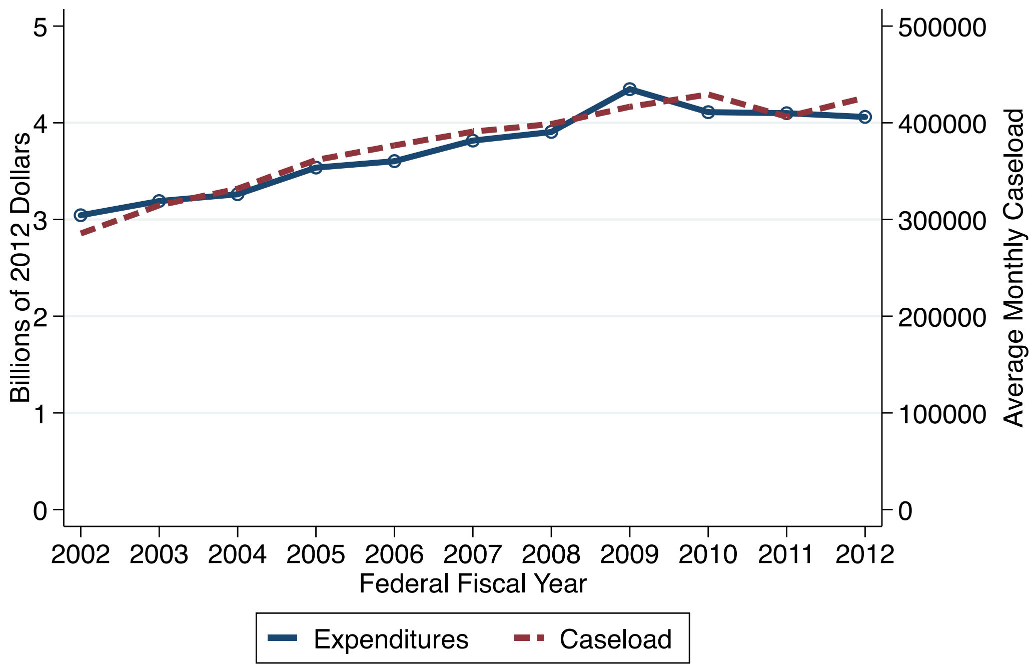
Table C.10: Relationship Between Charter Penetration and the Number of Annual Sales in Census Block

	Quantity of House Sales within Census Block			
	Counting Most Recent House Sale		Counting Three Most Recent House Sales	
	Number of Charters	Charter seats as percentage of enrollment	Number of Charters	Charter seats as percentage of enrollment
0 - 0.5 miles	0.0828	0.354	0.0901	0.346
	(0.168)	(0.414)	(0.174)	(0.417)
0.5 - 1 mile	0.119	0.346	0.130	0.391
	(0.110)	(0.533)	(0.110)	(0.537)
1 - 1.5 miles	0.139*	0.581	0.171**	0.801
	(0.0776)	(0.647)	(0.0794)	(0.721)
1.5 - 2 miles	0.0972*	-0.0104	0.121**	0.120
	(0.0547)	(0.607)	(0.0554)	(0.659)
Observations	87,683	87,683	87,683	87,683
Census Tract Fixed-Effects	N	N	N	N
Census Block Fixed-Effects	Y	Y	Y	Y

Sample includes property sales from September 2008 through September, 2011. The independent variable denotes either the number of charters in operation or the share of enrollment in operating charters as of the sale date in various distance rings from the property. Robust standard errors clustered by elementary school zone in 2002 in parentheses. *, **, and *** denote significance at the 10%, 5% and 1% levels, respectively.

APPENDIX D Figures for “State Responses to a Federal Matching Grant and Adoption from Foster Care”

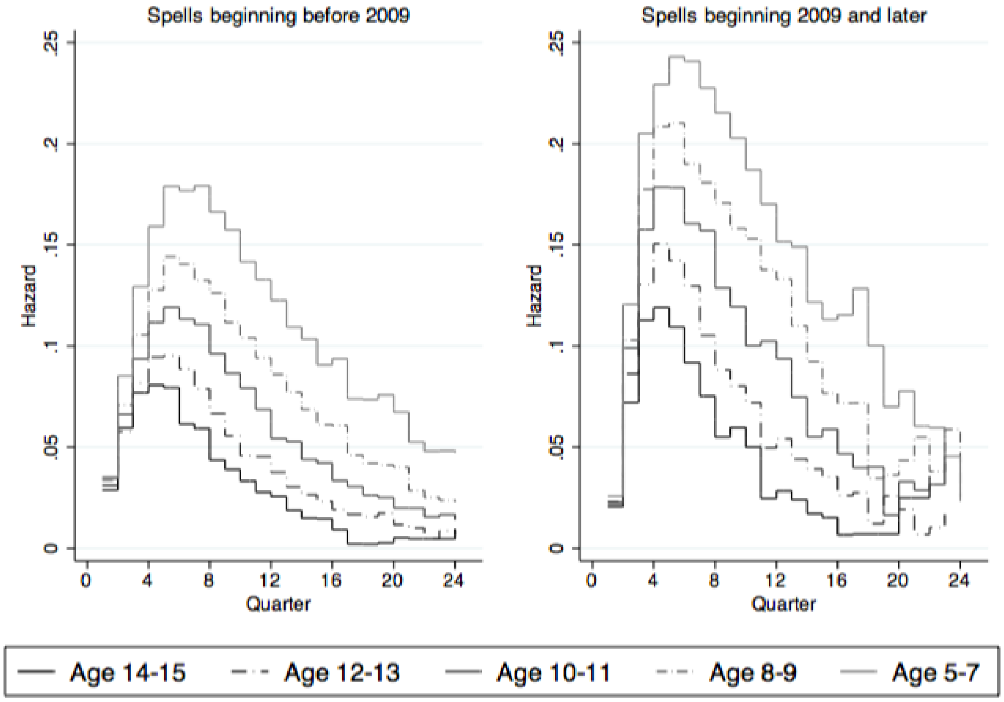
Figure D.1: Total Title IV-E Adoption Assistance Expenditures and Caseload



Sources: ACF Congressional Budget Justification (FFYs 2004-2014); Form ACF-IV-E-1 Part 1, Adoption Assistance Line 2 (FFYs 2002-2010); and Form CB-496 Part 1, Line 45 (FFYs 2011-2012).

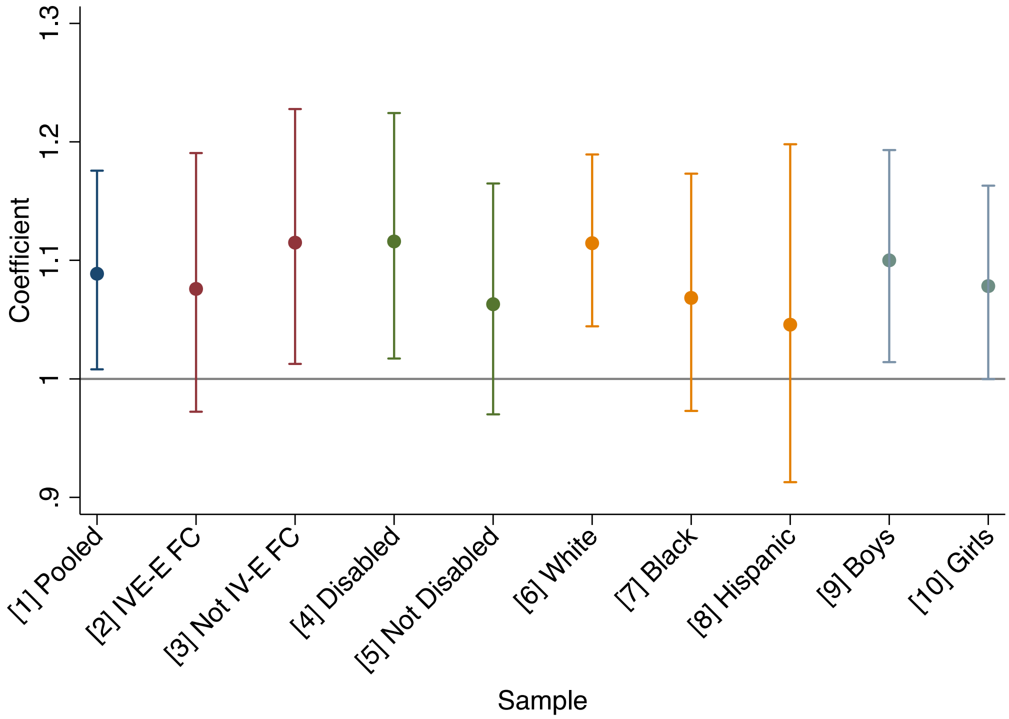
Figure plots aggregated data nationwide based on Title IV-E expenditure claims as submitted by states.

Figure D.2: Empirical Hazards by Age



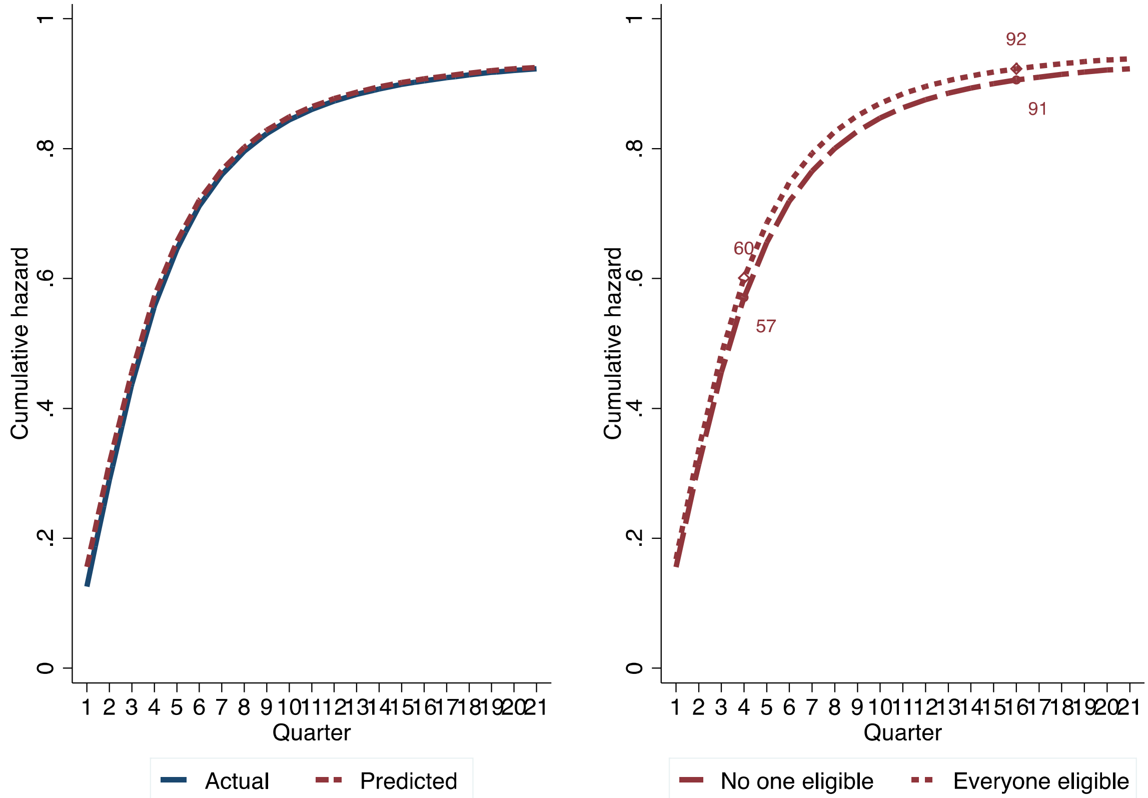
The figures plot empirical hazards separately by age at the spell start, or at mother's rights termination. The hazard in each quarter represents the percentage of children remaining in care each quarter that exits to an adoptive placement. Figure on the left uses sample of spells with mother's rights termination in fiscal years 2000–2008; figure on the right uses sample of spells with mother's rights termination in fiscal years 2009–2014. "Quarter" on the x–axis refers to the quarter following mother's rights termination. Source: AFCARS Foster Care files 2000–2014.

Figure D.3: Hazard Model Estimates: Heterogeneity by Child Characteristics



Results are from a complementary-log-log specification; figure plots exponentiated coefficients and 95 percent confidence intervals from separate regressions of equation (1.1) using samples selected by indicated child's characteristics. Specification includes child and state covariates and sets of indicators for age (in years), duration from spell start, duration from entry, state, and quarter-by-year. Term "IV-E FC" in samples [2] and [3] refers to samples of children who are and are not receiving matched foster care payments. Sample sizes by sample: [1] 3,751,686; [2] 1,365,428; [3] 2,386,258; [4] 1,398,414; [5] 2,353,272; [6] 1,676,897; [7] 1,183,376; [8] 661,857; [9] 1,964,004; and [10] 1,787,682. Source: AFCARS Foster Care Files, 2000–2014.

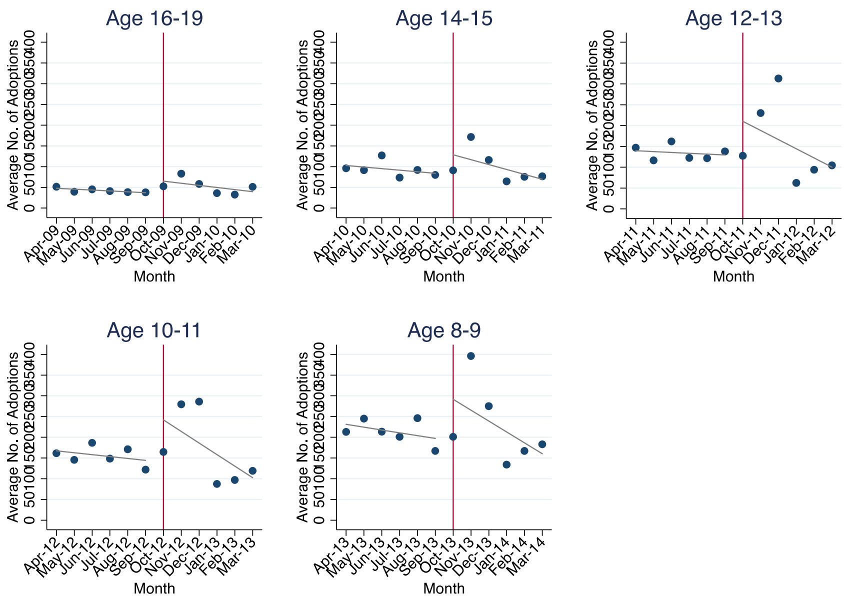
Figure D.4: Cumulative Proportion of Children Adopted by Quarter from Spell Start



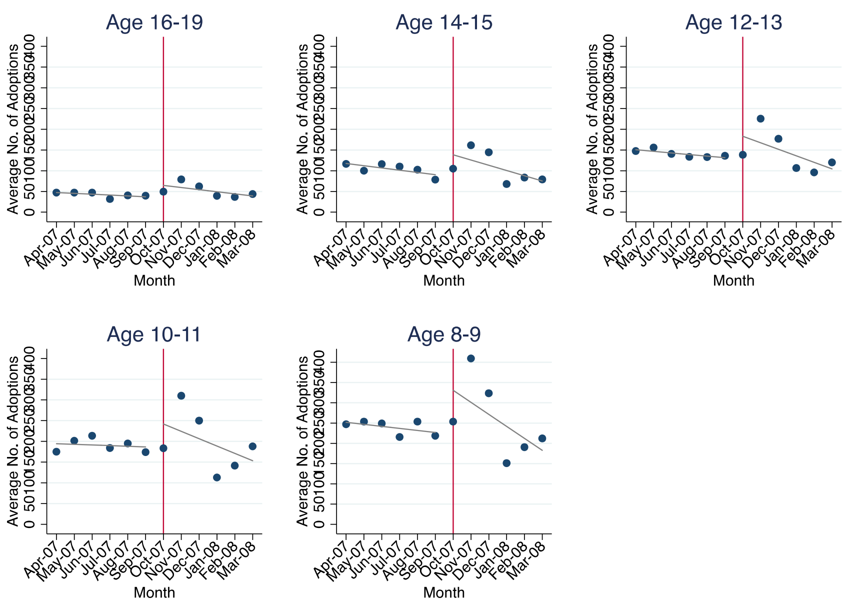
Left figure plots the actual cumulative proportion of children adopted by quarter from the time of mother's rights termination against the predicted proportion using the complementary-log-log estimate of equation (1.1). Right figure plots two counterfactual cumulative proportions of children adopted by quarter of duration, one for which no child is newly eligible and one for which every child is treated by new eligibility. The two labeled points are for quarter 4 and quarter 16 following mother's rights termination. Source: AFCARS Foster Care files 2000–2014.

Figure D.5: Monthly Number of Adoptions by Age Group

a) In age-criteria applicable years



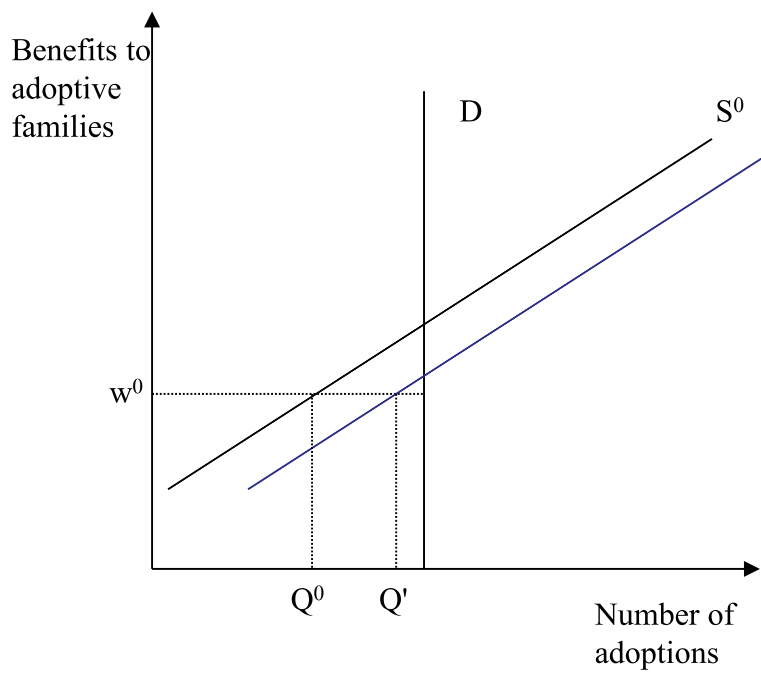
b) From April 2007 to March 2008 (across the beginning of fiscal year 2008)



Figures plot the mean number of monthly adoptions across the ages in each age group. In (a), the mean number of counts are given for each month from the April six months prior to when the federal criteria applied to that age group to the March six months after. The vertical line denotes the October fiscal year start when the applicable age criteria are in effect for the indicated age group in each figure title. Figures in (b) are for each age group in the last six months of fiscal year 2007 and first six months of fiscal year 2008, for comparison. Source: AFCARS Adoption Files, 2000-2014.

APPENDIX E Figures for "The Effects of Federal Adoption Incentive Awards for Older Children on Adoptions from U.S. Foster Care"

Figure E.1: Market for Adoptions



The figure illustrates that if states respond to the incentive payments in ways that increase the supply of adoptive families for older children, then holding benefits offered to families constant at w^0, the number of adoptions finalized increases from Q^0 to Q^1.

Figure E.2: Distribution of Annual Change in Number of Older Child Adoptions

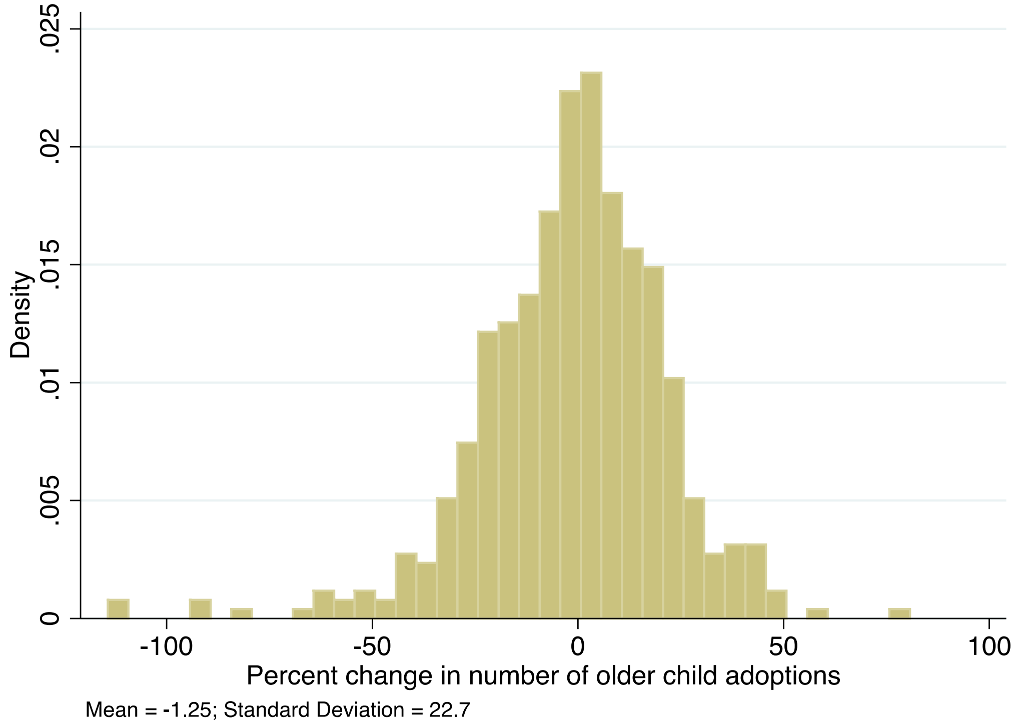
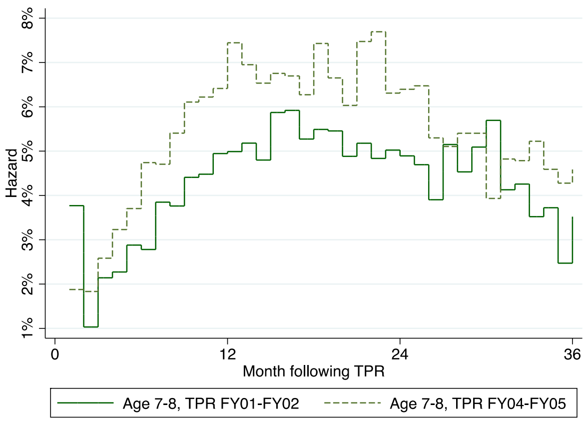


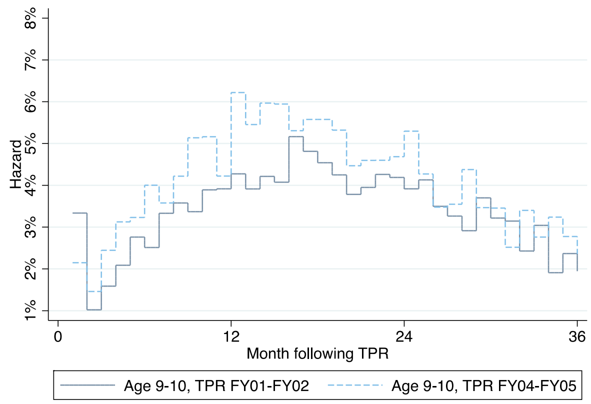
Figure E.3: Empirical Hazards

Empirical Hazards Before and After the Introduction of the Incentives

(a) Empirical hazard rates for children aged 7 through 8 at the time of termination of mother's parental rights, in the two fiscal years before and after the introduction of the incentive payments in FY03. (b) Empirical hazard rates for children aged 9 through 10.

a. b.

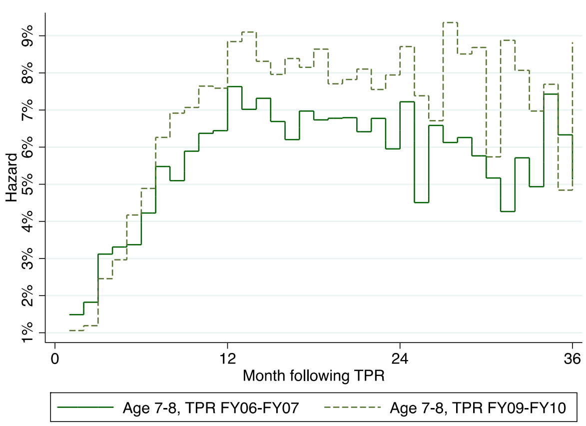




Empirical Hazards Before and After the 2008 Update of the Incentives

(a) Empirical hazard rates for children aged 7 through 8 at the time of termination of mother's parental rights, in the two fiscal years before and after the reauthorization of the incentive payments in FY08. (b) Empirical hazard rates for children aged 9 through 10.

a. b.



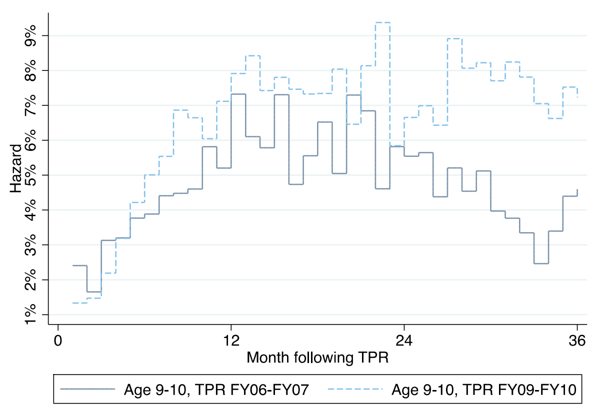
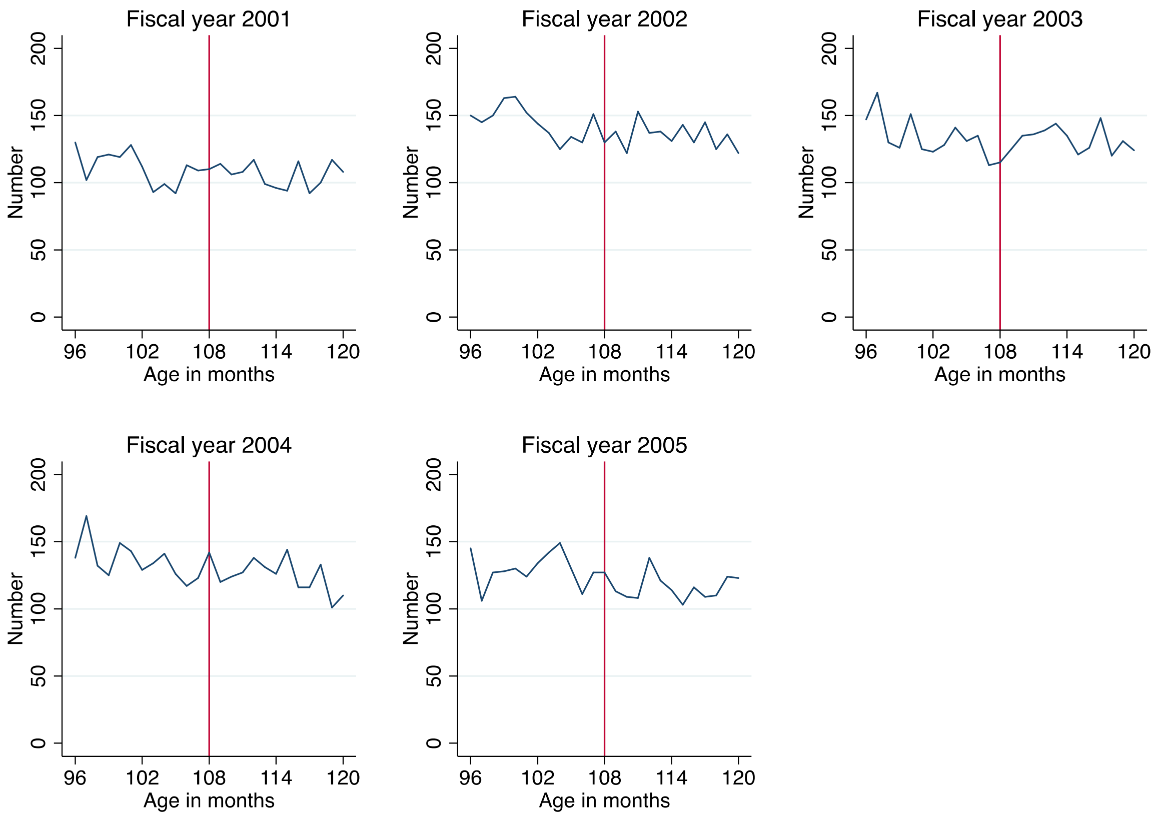


Figure E.4: Number of Adoptions by Age, 2001-2005



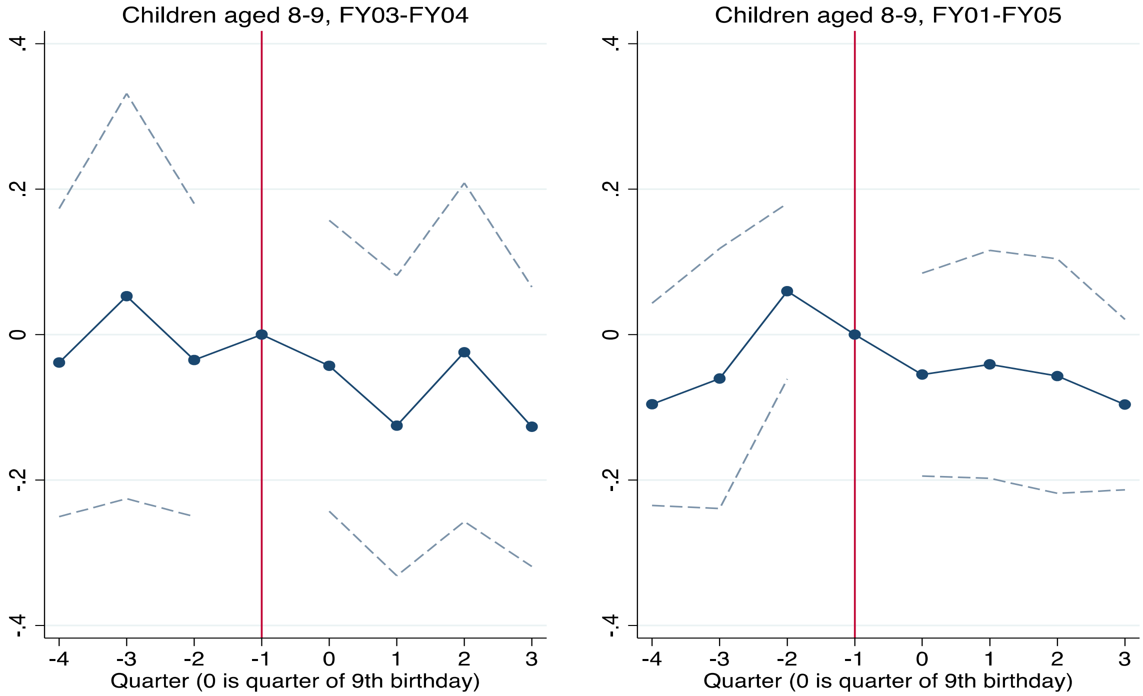
Figures show the raw number of adoptions of children aged 8 through 9, where age is measured in months at the time of adoption. Counts are tabulated across all states separately for each fiscal year. Figures exclude adoptions occurring in the +/ two week window around the end of each fiscal year. The vertical line is on 108 months, or age 9.

Figure E.5: Average Number of Adoptions by Age by Award Earning State-Years

Source: AFCARS foster care data files.

Figures show the mean number of adoptions by age for ages 8 through 9, where age is measured in months at the time of adoption, across the given year periods and state samples. The vertical line is on 108 months, or age 9. Counts are tabulated separately for each age bin, state, and fiscal year, then averaged across time periods (either 2001-2002, 2003-2005) and groups of states (all states in 2001-2002 and separately by award earning states and non award earning states in 2003-2005). Figures exclude adoptions occurring in the +/ two week window around the end of each fiscal year.

Figure E.6: Timing Estimates of the Probability of Adoption by Age



Figures shows the effect of age, as measured in quarters, relative to the quarter in which the child turns age 9, on the probability of adoption over the post period (October 2003 and later). The excluded indicator is the interaction indicating a child is one quarter shy of their 9th birthday quarter (quarter 0). The sample includes child-month observations with children aged 8 up to, but not including, age 10. The figure shows estimated logit coefficients on the post x age interaction terms from equation (2.2) and standard errors clustered at the state level. Controls include child demographics, state controls, indicators for duration month from mother's date of rights termination, indicators for age as measured in months, state indicators, and month-by-year indicators.

Figure E.7: Timing Estimates of the Probability of Adoption by Age in Award-Earning States

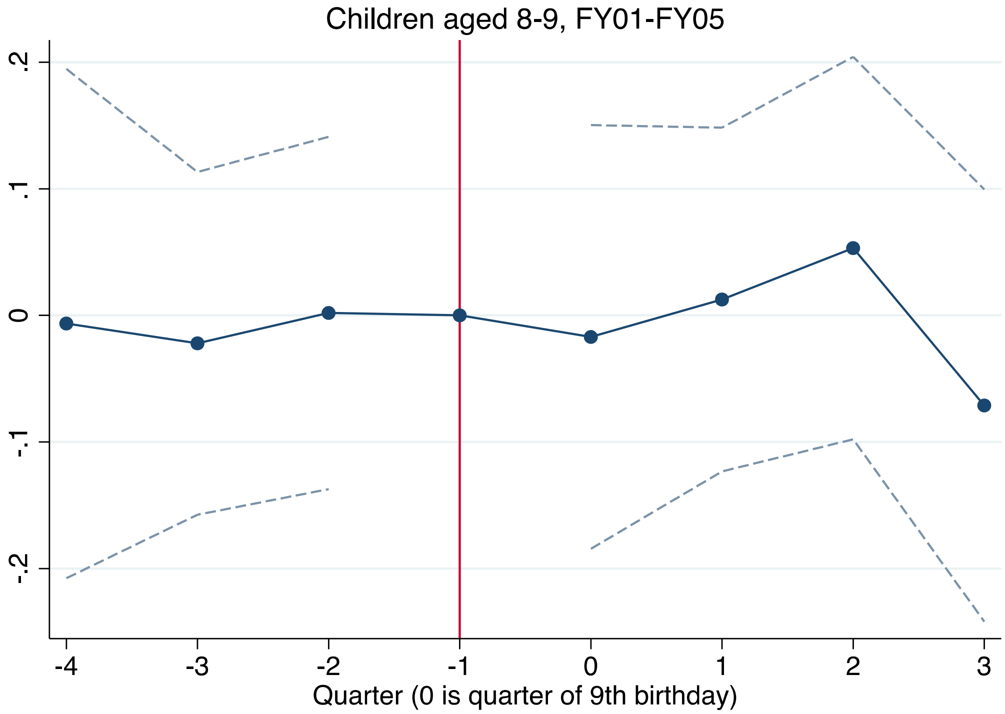


Figure shows the effect of age, as measured in quarters, relative to the quarter in which the child turns age 9, on the probability of adoption in state-years in which states exceed the applicable baseline and earned an award for older child adoptions (in October 2002 and later). The excluded indicator is the interaction between whether the state earned the award in that year and the indicator indicating a child is one quarter shy of their 9th birthday quarter (quarter 0). The sample includes child-month observations with children aged 8 up to, but not including, age 10. The figure shows estimated logit coefficients and standard errors clustered at the state level. Controls include child demographics, state controls, indicators for duration month from mother's date of rights termination, indicators for age as measured in months, state indicators, and month-by-year indicators.

Figure E.8: September-October Adoptions Among 8 and 9 Year Olds, 2003–2010

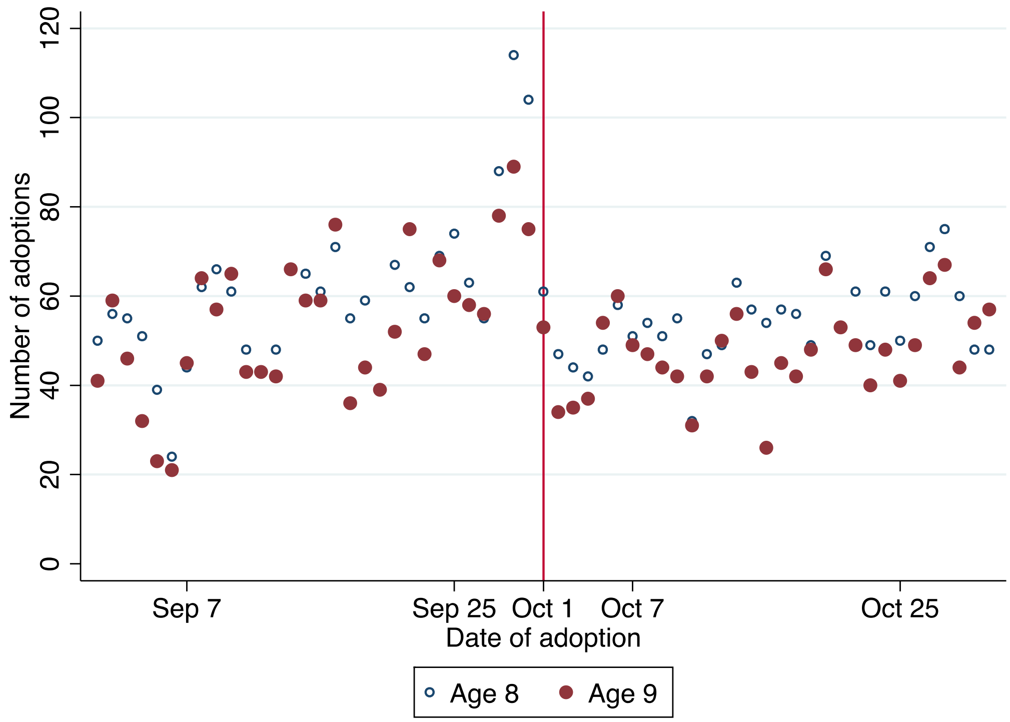


Figure show the number of adoptions of children aged 8 and 9 at the time of adoption, by day from all September and October adoptions from calendar years 2003 through 2010. Source: AFCARS Adoption Data files, fiscal years 2003 through 2010.

APPENDIX F Figures for "Capitalization of Charter Schools into Residential Property Values"

Figure F.1: Case-Shiller House Price Index for Greater Los Angeles

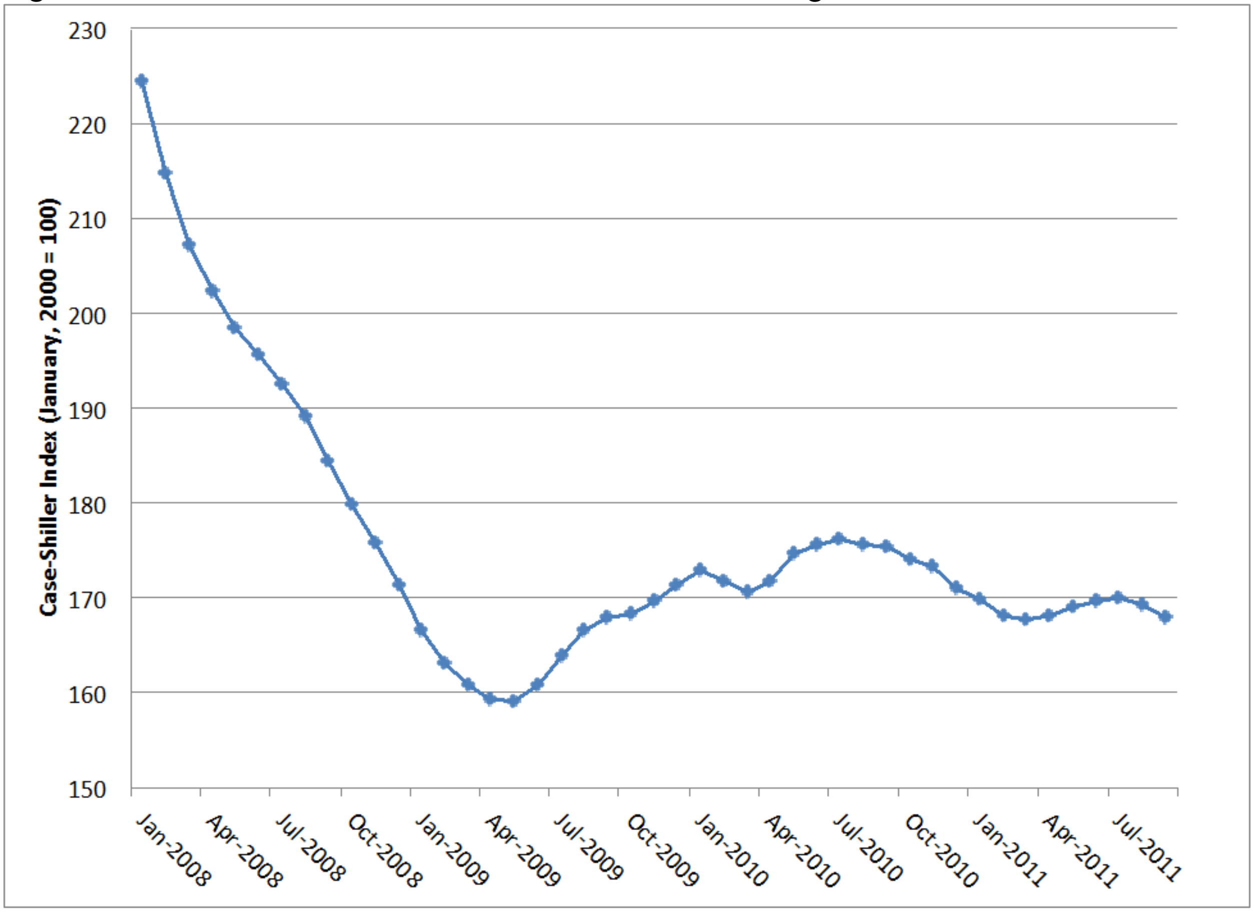
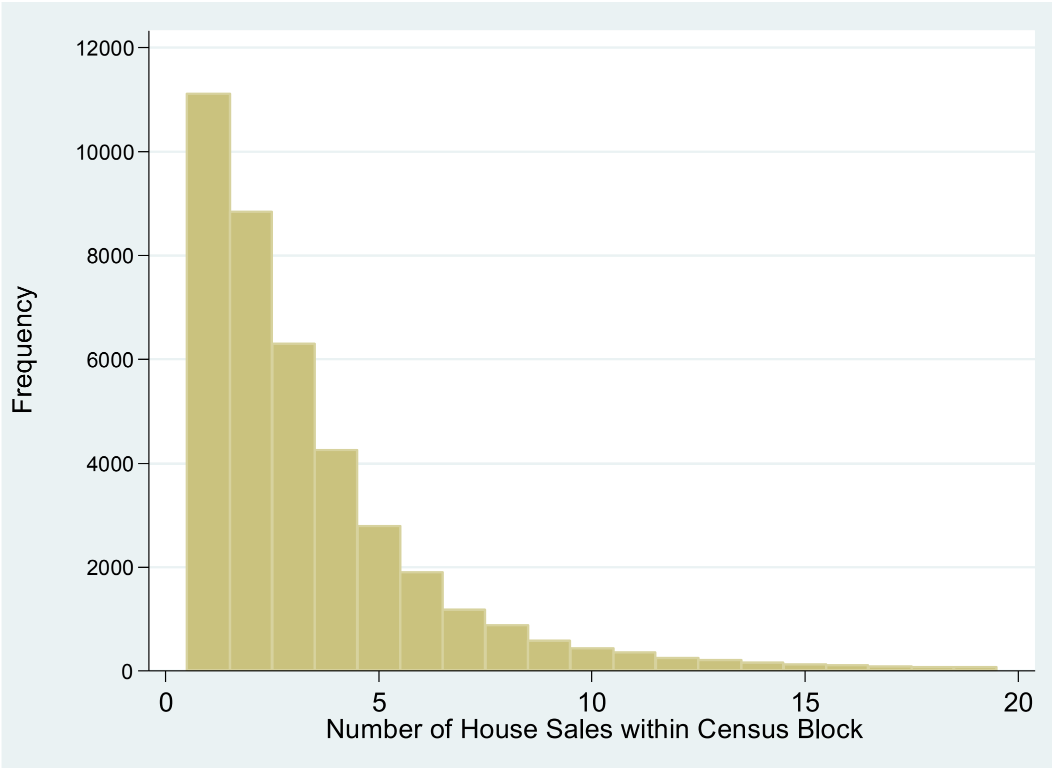


Figure F.2: Distribution of House Sales by Census Block During Sample Period Conditional on Census Block Having Any Sales



APPENDIX G Appendices for "State Responses to a Federal Matching Grant and Adoption from Foster Care"

Alternative functional form choices for the hazard specification

Tables G.1 and G.2 below provide the results from estimating equation (1.1) using a logit functional form and linear probability model (LPM) for the hazard, respectively. The estimated coefficients of the logit specification have no natural interpretation, however, the calculated average partial effects are similar in magnitude to those from the complementary–log–log specification.

The estimates using the LPM are directly analogous to the average partial effects from the nonlinear specifications. In contrast to the positive estimates in the nonlinear specifications, the LPM estimates are negative. The estimates are both statistically significant at the 1 percent level and economically meaningful. The estimate in column [3], including the full set of age, duration, quarter x year, and state indicators and child and state covariates, is –0.013, indicating new eligibility reduces the per-period probability of adoption by 1.3 percentage points. This represents 10 percent of the per-period mean of 13 percent, similar in magnitude to the positive treatment effect estimated using the nonlinear functional forms.

While falsification tests cannot rule out the results of the logit specification, this is not true for the LPM specification. The falsification test results (described in section 5.6) are in Panel A for logit and Panel B for the LPM of Table G.3. Similar to the results using complementary–log–log, the estimates using the logit functional form are small in magnitude and not statistically different from zero. However, the falsification estimates for the LPM are negative and statistically significant, casting doubt on the validity of the LPM estimates in Table G.2. The estimate using the 2005 rollout is –0.007 and –0.008 using the 2004 rollout and both are statistically significant at the 5 percent level. The magnitudes of these estimates are about half the size of the estimates using the actual dates of the rollout.

Together with the complementary–log–log and logit results, the LPM results are a clear outlier and suggest the LPM is an inappropriate functional choice for the hazard specification in equation (1.1). Lewbel, Dong, and Yang (2012) provide an example where estimation using a LPM can provide a wrong-signed estimate with the same magnitude as the true positive treatment effect. Their example illustrates that wrong-signed treatment effects are possible in highly non-linear models. This point is relevant in the setting here with a 13 percent per-period mean (87 percent of the dependent variables are 0's). Further, the LPM implies an additive hazard rather than a proportional form.

In this setting, the LPM does not provide a good approximation of the true marginal effects. However, the results from the logit and complementary–log–log forms provide internally consistent results and pass the falsification tests.

Table G.1: Hazard Model Estimates of the Effect of New Federal Match Eligibility on the Probability of Adoption (Logit)

	Fiscal years 2000–2014			
	[1]	[2]	[3]	[4]
Age- or duration- eligible	0.056	0.074*	0.075*	
	(0.045)	(0.045)	(0.043)	
	[0.006]	[0.008]	[0.008]	
Age- or duration- eligible x 16-19				0.016
				(0.073)
				[0.002]
Age- or duration- eligible x 14-15				0.089
				(0.055)
				[0.010]
Age- or duration- eligible x 12-13				0.085*
				(0.049)
				[0.009]
Age- or duration- eligible x 10-11				0.103**
				(0.042)
				[0.011]
Age- or duration- eligible x 8-9				0.054
				(0.043)
				[0.006]
Age- or duration- eligible x 5-7				0.108
				(0.069)
				[0.012]
Age, duration from spell start, duration from entry, and quarter x year indicators	Y	Y	Y	Y
State indicators	Y	Y	Y	Y
Child covariates		Y	Y	Y
State covariates			Y	Y
Child-quarter observations	3,751,686	3,751,686	3,751,686	3,751,686
Number of children	618,150	618,150	618,150	618,150
Log pseduolikelihood	-1,313,639	-1,304,537	-1,304,167	-1,304,157
Mean of dep. var.	0.13	0.13	0.13	0.13

Source and notes: Adoption and Foster Care Analysis and Reporting System (AFCARS) Foster Care Files, 2000–2014. Results are from a logit specification; coefficients shown with marginal effects in brackets. Baseline hazard comprises 21 parameters and there are 72 duration from entry indicators. Child covariates are: whether or not the child receives matched foster care payments, male, and 6 race/ethnicity categories. State covariates are percent of the population aged 25 to 64, percent of the population black, unemployment rate, and log of per capita median income. Standard errors are clustered at the state level and shown in parentheses. Marginal effects of are computed as the average derivative of the probability of adoption with respect to age or duration eligibility. *** p< 0.01, ** p<0.05, * p<0.10.

Table G.2: Hazard Model Estimates of the Effect of New Federal Match Eligibility on the Probability of Adoption (Linear Probability Model)

	Fiscal years 2000–2014			
	[1]	[2]	[3]	[4]
Age- or duration- eligible	-0.016***	-0.013***	-0.013***	
	(0.005)	(0.005)	(0.004)	
Age- or duration- eligible x 16-19				-0.023***
				(0.006)
Age- or duration- eligible x 14-15				-0.016***
				(0.005)
Age- or duration- eligible x 12-13				-0.011**
				(0.005)
Age- or duration- eligible x 10-11				-0.004
				(0.004)
Age- or duration- eligible x 8-9				-0.004
				(0.004)
Age- or duration- eligible x 5-7				0.008
				(0.007)
Age, duration from spell start, duration from entry, and quarter x year indicators	Y	Y	Y	Y
State indicators	Y	Y	Y	Y
Child covariates		Y	Y	Y
State covariates			Y	Y
Child-quarter observations	3,751,686	3,751,686	3,751,686	3,751,686
Number of children	618,150	618,150	618,150	618,150
Mean of dep. var.	0.13	0.13	0.13	0.13

Source and notes: Adoption and Foster Care Analysis and Reporting System (AFCARS) Foster Care Files, 2000–2014. Results are from a linear probability model. Baseline hazard comprises 21 parameters and there are 72 duration from entry indicators. Child covariates are: whether or not the child receives matched foster care payments, male, and 6 race/ethnicity categories. State covariates are percent of the population aged 25 to 64, percent of the population black, unemployment rate, and log of per capita median income. Standard errors are clustered at the state level and shown in parentheses. *** p< 0.01, ** p<0.05, * p<0.10.

Table G.3: Falsification Tests of Logit and Linear Probability Model Hazard Model Estimates

	2005 rollout, 2000–2009	2004 rollout, 2000–2008	2010 rollout for younger ages, 2000–2014
	[1]	[2]	[3]
A. Logit			
Age- or duration- eligible	0.036	0.016	0.018
	(0.036)	(0.043)	(0.029)
	[0.004]	[0.002]	[0.002]
B. LPM			
Age- or duration- eligible	-0.007**	-0.008**	-0.002
	(0.003)	(0.004)	(0.003)
Child-quarter observations	2,518,926	2,221,844	3,751,686
Number of children	418,648	374,978	618,150
Mean of dep. var.	0.12	0.12	0.13

Source and notes: Adoption and Foster Care Analysis and Reporting System (AFCARS) Foster Care Files, 2000–2014. Results in Panel A are from logit estimation of the specification in column 3 of Table A.6; coefficients shown with marginal effects in brackets. Results in Panel B are from the linear probability model of the same specification. Eligibility in [1]–[2] defined by starting rollout based on age and duration rules in the named year; eligibility in [3] defined by starting rollout in 2010 where eligibility defined by age at end of the fiscal year as 4+ in 2010 and later; 3+ in 2011 and later; 2+ in 2012 and later; age 1+ in 2013 and later; and ages 0+ in 2014. Standard errors are clustered at the state level and shown in parentheses. Marginal effects are computed as the average derivative of the probability of adoption with respect to age or duration eligibility. *** p< 0.01, ** p<0.05, * p<0.10.

Table G.4: State Characteristics, 2000–2009

State	Match Rate	Percent receiving matched foster care payments	Percent receiving matched adoption assistance	Percent designated special needs	Percent receiving adoption assistance	Standard deviation of adoption assistance amounts	AFDC income standard for a family of 3
	2000–2009						
	[1]	[2]	[3]	[4]	[5]	[6]	[7]
Alabama	70	22	36	61	50	244	673
Alaska	56	21	74	100	95	438	1028
Arizona	67	45	62	85	77	412	964
Arkansas	74	49	65	83	81	228	705
California	51	60	82	98	96	505	723
Colorado	51	34	49	80	72	363	421
Connecticut	51	61	52	49	85	458	745
Delaware	51	25	43	89	73	279	338
D.C.	70	23	37	99	81	430	712
Florida	59	27	56	85	75	239	1082
Georgia	62	35	48	70	65	352	424
Hawaii	57	53	66	93	86	365	1140
Idaho	71	55	77	95	90	196	991
Illinois	50	41	74	93	92	435	963
Indiana	64	42	60	82	60	373	320
Iowa	64	34	51	60	72	454	849
Kansas	61	38	64	79	86	216	403
Kentucky	71	52	87	77	92	393	526
Louisiana	72	52	69	71	90	193	658
Maine	66	45	73	56	97	475	553
Maryland	51	35	62	93	98	233	517
Massachusetts	51	27	44	100	86	263	579
Michigan	57	23	72	72	92	336	551
Minnesota	51	38	56	90	70	302	532
Mississippi	77	22	77	93	78	218	368
Missouri	62	31	69	75	94	195	846
Montana	72	36	66	83	91	214	558
Nebraska	60	22	34	60	84	406	364
Nevada	54	31	78	96	93	1251	699
New Hampshire	51	46	86	100	86	389	2034
New Jersey	51	39	75	88	94	297	985
New Mexico	73	53	83	97	90	449	381
New York	55	57	72	98	98	442	577
North Carolina	64	40	65	95	95	275	544
North Dakota	68	40	45	69	63	486	431
Ohio	60	58	96	98	96	532	950
Oklahoma	70	45	57	93	89	188	645
Oregon	61	53	77	80	98	201	460
Pennsylvania	55	60	79	77	90	380	587
Rhode Island	54	26	60	46	96	488	554
South Carolina	71	39	55	91	81	296	524
South Dakota	65	38	66	100	93	136	507
Tennessee	65	48	57	88	75	632	677
Texas	61	50	62	89	79	216	751
Utah	72	41	52	86	82	228	568
Vermont	61	59	74	100	82	552	1173
Virginia	51	52	68	73	92	308	322
Washington	54	35	59	76	76	481	1252
West Virginia	75	29	62	100	87	268	991
Wisconsin	59	36	80	99	96	538	647
Wyoming	57	17	49	33	86	241	674
Median	61	39	65	88	86	352	

Source and notes: Adoption and Foster Care Analysis and Reporting System (AFCARS) Foster Care Files, 2000–2009 and Adoption Files, 2000–2009. Figures represent mean in each state over fiscal years 2000–2009. Highlighted cells are those above the median match rate in column [1], and below the median in columns [2]–[6]. Figures for the AFDC income standard for a family of 3 are from U.S. Congressional Research Service (2012a).

Table G.5: Short–Run Effects of the Introduction of Age Criteria on Monthly Adoptions – Average Partial Effects

	+/- 1 months	+/- 2 months	+/- 3 months	+/- 4 months	+/- 6 months
	[1]	[2]	[3]	[4]	[5]
Dependent variable is number of adoptions					
Not age-eligible					
April					0.383
					(0.135)
May					-0.222
					(0.332)
June				-0.262	-0.163
				(0.170)	(0.156)
July			-0.295	-0.143	-0.047
			(0.240)	(0.243)	(0.228)
August		-0.118	-0.223	-0.071	0.024
		(0.306)	(0.327)	(0.332)	(0.315)
September	-0.206	-0.571**	-0.662***	-0.509***	-0.405***
	(0.197)	(0.234)	(0.210)	(0.195)	(0.182)
Age-eligible					
October	0.710***	0.442**	0.322*	0.472***	0.556***
	(0.152)	(0.184)	(0.192)	(0.181)	(0.153)
November		0.781***	0.652***	0.801***	0.878***
		(0.230)	(0.240)	(0.261)	(0.254)
December			1.162***	1.309***	1.377***
			(0.400)	(0.414)	(0.429)
January				-0.167	-0.149
				(0.362)	(0.311)
February					0.035
					(0.251)
March					0.018
					(0.252)
N	28,560	57,120	85,680	114,240	171,360
Change in adoptions before	-0.206	-0.689*	-1.181**	-0.984*	-0.430
	(0.197)	(0.415)	(0.533)	(0.586)	(0.679)
Change in adoptions after	0.710***	1.223***	2.135***	2.415***	2.716***
	(0.152)	(0.342)	(0.500)	(1.432)	(0.714)
Difference	0.504*	0.534	0.955	1.432	2.286**
	(0.278)	(0.633)	(0.811)	(0.945)	(1.061)

Source and notes: Adoption and Foster Care Analysis and Reporting System (AFCARS) Adoption Files, 2000–2014. Dependent variable is state monthly adoption counts by age, as measured at the end of the fiscal year at the time of adoption. Sample is monthly adoptions within the relevant window. Window denotes the number of months before and after the start of the fiscal year (October). Average marginal effects and corresponding standard errors shown in parentheses from Poisson regressions reported in Table A.9. All specifications include age, calendar month, year, and state fixed effects, and an age-specific linear time trend. Change in adoptions before and after are calculated as the sum of the average partial effects across each set of months. The difference is calculated as the sum of the change in adoptions before and after. *** p< 0.01, ** p<0.05, * p<0.10.

Table G.6: Falsification Test: Short–Run Effects of the Introduction of Age Criteria on Monthly Adoptions

	+/- 1 months	+/- 2 months	+/- 3 months	+/- 4 months	+/- 6 months
	[1]	[2]	[3]	[4]	[5]
Dependent variable is ln(number of adoptions)					
Not age-eligible					
April					0.006
					(0.046)
May					-0.013
					(0.039)
June				-0.073**	-0.066**
				0.031	(0.026)
July			-0.089	-0.063*	-0.055*
			(0.033)	0.035	(0.033)
August		-0.011	-0.004	0.023	0.030
		(0.031)	(0.029)	0.030	(0.028)
September	-0.022	-0.073*	-0.066*	-0.039	-0.032
	(0.040)	(0.039)	(0.036)	0.034	(0.031)
Age-eligible					
October	0.074	0.023	0.031	0.057	0.064
	(0.059)	(0.059)	(0.058)	(0.059)	(0.059)
November		0.142***	0.149***	0.1756***	0.183***
		(0.046)	(0.047)	(0.053)	(0.055)
December			0.108***	0.134***	0.141***
			(0.040)	(0.042)	(0.044)
January				-0.034	-0.027
				(0.075)	(0.073)
February					0.016
					(0.040)
March					-0.006
					(0.030)
N	18,360	36,720	55,080	72,420	107,100
Log pseduolikelihood	-34,251	-74,156	-109,649	-143,975	-206,163
Mean of dep. var.	3.87	4.49	4.44	4.36	4.22

Source and notes: Adoption and Foster Care Analysis and Reporting System (AFCARS) Adoption Files, 2000–2009. Dependent variable is state monthly adoption counts by age, as measured at the end of the fiscal year at the time of adoption. Sample is monthly adoptions within the relevant window. Window denotes the number of months before and after the start of the fiscal year (October). Estimates are based on a Poisson regression, with standard errors shown in parentheses. All specifications include age, calendar month, and year fixed effects, state fixed effects, and an age-specific linear time trend. Falsification test defines eligibility by starting rollout based on age and duration rules in fiscal year 2005 rather than 2010. *** p< 0.01, **

Table G.7: Falsification Test: Short–Run Effects of the Introduction of Age Criteria on Monthly Adoptions – Average Partial Effects

	+/- 1 months	+/- 2 months	+/- 3 months	+/- 4 months	+/- 6 months
	[1]	[2]	[3]	[4]	[5]
Dependent variable is number of adoptions					
Not age-eligible					
April					0.025
					(0.196)
May					-0.054
					(0.164)
June				-0.306**	-0.268***
				(0.125)	(0.102)
July			-0.378***	-0.265*	-0.228*
			(0.137)	(0.144)	(0.134)
August		-0.050	-0.017	0.100	0.128
		(0.137)	(0.129)	(0.132)	(0.122)
September	-0.085	-0.317**	-0.282*	-0.168	-0.133
	(0.151)	(0.161)	(0.151)	(0.141)	(0.128)
Age-eligible					
October	0.297	0.105	0.138	0.256	0.280
	(0.247)	(0.269)	(0.263)	(0.273)	(0.264)
November		0.682***	0.713***	0.836***	0.847***
		(0.235)	(0.237)	(0.272)	(0.277)
December			0.505**	0.626***	0.641**
			(0.196)	(0.210)	(0.212)
January				-0.146	-0.114
				(0.318)	(0.299)
February					0.070
					(0.173)
March					-0.025
					(0.124)
N	18,360	36,720	55,080	72,420	107,100
Change in adoptions before	-0.085	-0.367	-0.677**	-0.639*	-0.530
	(0.151)	(0.247)	(0.279)	(0.328)	(0.406)
Change in adoptions after	0.297	0.786**	1.356***	1.573***	1.515**
	(0.247)	(0.367)	(0.416)	(0.537)	(0.738)
Difference	0.212	0.419	0.679	0.934	0.985
	(0.323)	(0.496)	(0.547)	(0.676)	(0.868)

Source and notes: Adoption and Foster Care Analysis and Reporting System (AFCARS) Adoption Files, 2000–2009. Dependent variable is state monthly adoption counts by age, as measured at the end of the fiscal year at the time of adoption. Sample is monthly adoptions within the relevant window. Window denotes the number of months before and after the start of the fiscal year (October). Average marginal effects and corresponding standard errors shown in parentheses from Poisson regressions reported in Table G.6. All specifications include age, calendar month, year, and state fixed effects, and an age-specific linear time trend. Falsification test defines eligibility by starting rollout based on age and duration rules in fiscal year 2005 rather than 2010. Change in adoptions before and after are calculated as the sum of the average partial effects across each set of months. The difference is calculated as the sum of the change in adoptions before and after. *** p< 0.01, ** p<0.05, * p<0.10.

APPENDIX H Appendices for "The Effects of Federal Adoption Incentive Awards for Older Children on Adoptions from U.S. Foster Care"

Figure H.1: Event Study Estimates of the Probability of Adoption by Quarter

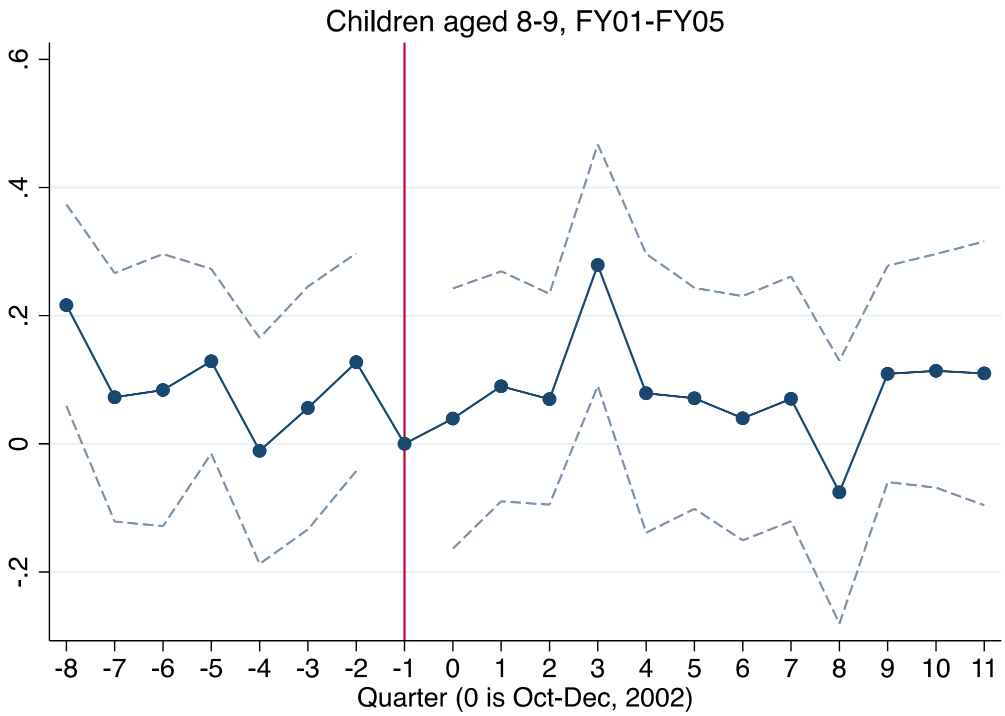


Figure shows the effect of being 9 in each quarter over federal fiscal years 2001 through 2005 relative to the quarter prior to the start of federal fiscal year 2003, October, 2002 (quarter 0), in which the Act of 2003 became retroactively effective. The sample includes child-month observations with children aged 8 up to, but not including, age 10. The figure shows estimated logit coefficients and standard errors clustered at the state level. Controls include child demographics, state controls, indicators for duration month from mother's date of rights termination, indicators for age as measured in months, state indicators, and month-by-year indicators.

Figure H.2: Event Study Estimates of the Probability of Adoption by Quarter – 2008 Update

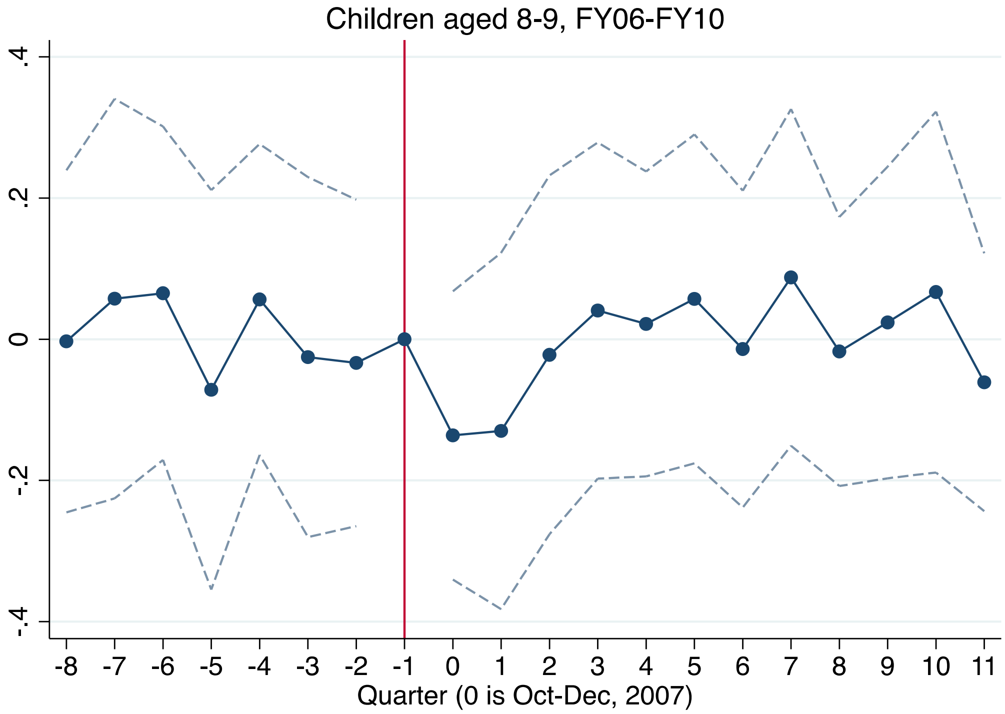
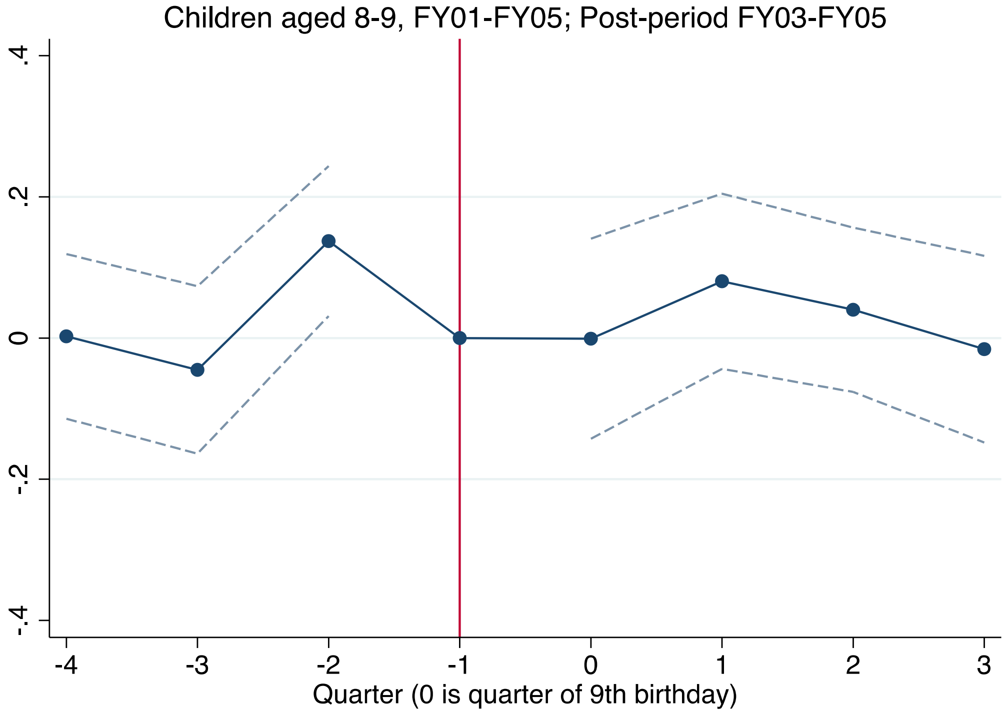


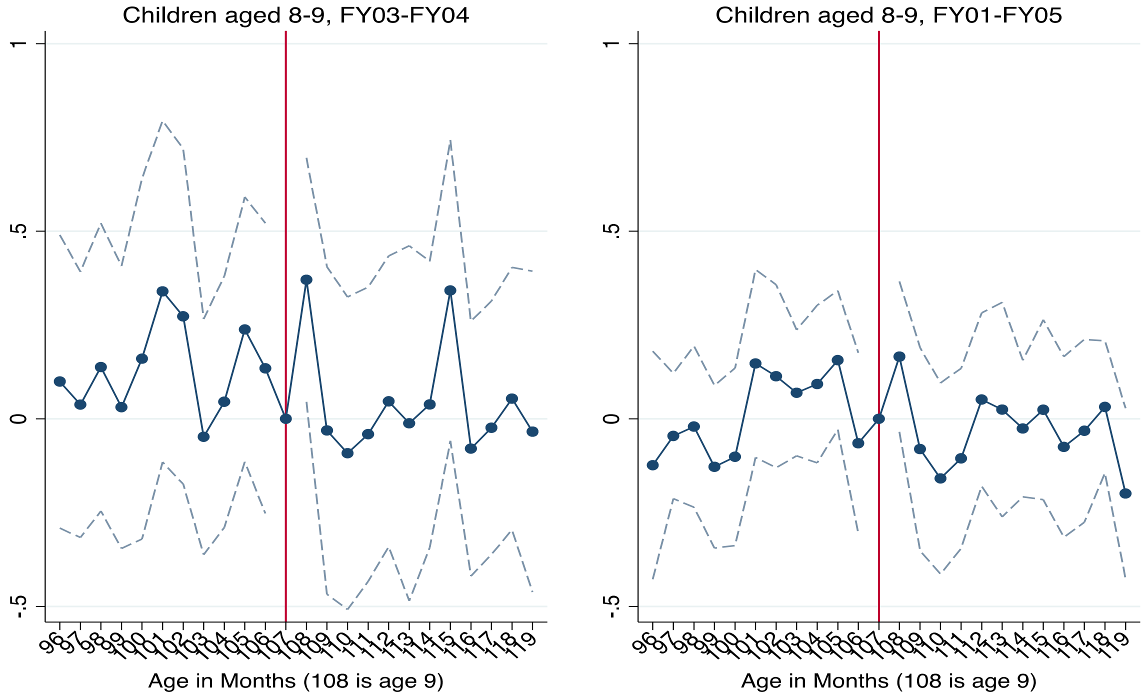
Figure shows the effect of being 9 in each quarter over federal fiscal years 2006 through 2010 relative to the quarter prior to the start of federal fiscal year 2008, October 2007 (quarter 0), in which the Act of 2008 became retroactively effective. The sample includes child-month observations with children aged 8 up to, but not including, age 10. The figure shows estimated logit coefficients and standard errors clustered at the state level. Controls include child demographics, state controls, indicators for duration month from mother's date of rights termination, indicators for age as measured in months, state indicators, and month-by-year indicators.

Figure H.3: Timing Estimates of Probability of Adoption – Robustness to Post-Period Beginning in Fiscal Year 2003



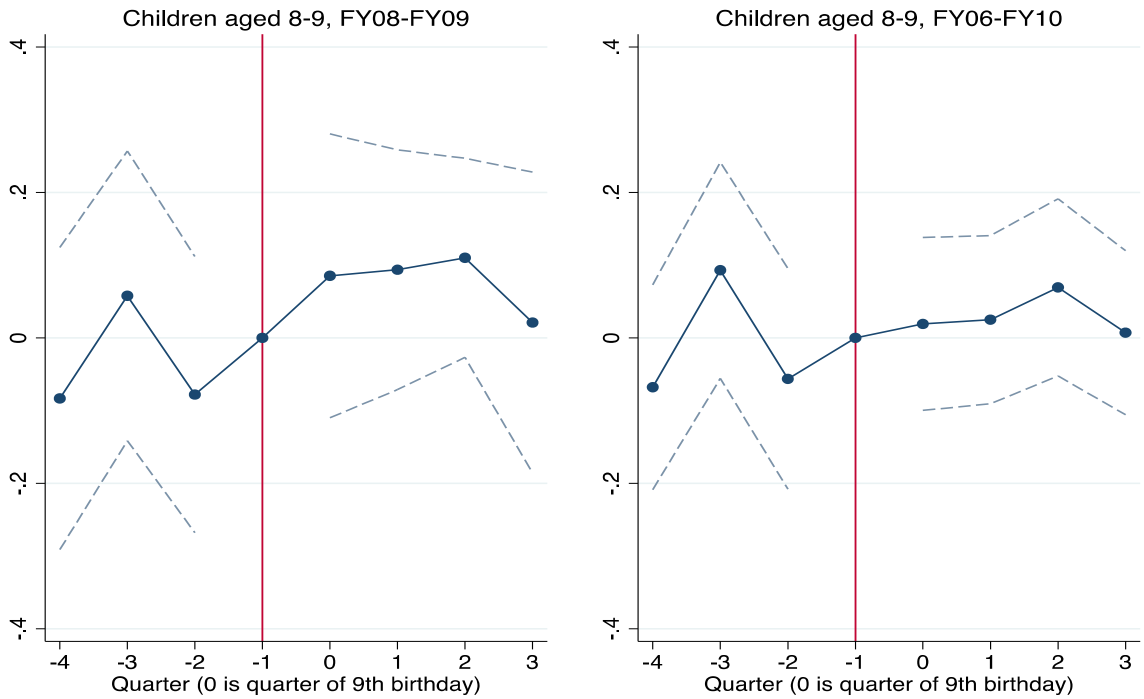
Figures shows the effect of age, as measured in quarters, relative to the quarter in which the child turns age 9, on the probability of adoption over the post period (October 2002 and later). The excluded indicator is the interaction indicating a child is one quarter shy of their 9th birthday quarter (quarter 0). The sample includes child-month observations with children aged 8 up to, but not including, age 10. The figure shows estimated logit coefficients on the post x age interaction terms from equation (2.2) and standard errors clustered at the state level. Controls include child demographics, state controls, indicators for duration month from mother's date of rights termination, indicators for age as measured in months, state indicators, and month-by-year indicators.

Figure H.4: Timing Estimates of Probability of Adoption by Age in Months



Figures shows the effect of being a given age as measured in months relative to age 9 in the post-incentives introduction period, where the post-period is October 2003 and later. The excluded indicator is the interaction indicating a child is aged 107 months in the post-period. The sample includes child-month observations with children aged 8 up to, but not including, age 10 in fiscal years 2003 through 2004 on the left, and fiscal years 2001 through 2005 on the right. The figures plot estimated logit coefficients and standard errors clustered at the state level. Controls include child demographics, state controls, indicators for duration month from mother's date of rights termination, indicators for age as measured in months, state indicators, and month-by-year indicators.

Figure H.5: Timing Estimates of Probability of Adoption by Age Around the 2008 Update of the Incentives



Figures shows the effect of age, as measured in quarters, relative to the quarter in which the child turns age 9, on the probability of adoption over the post period (October 2008 and later). The excluded indicator is the interaction indicating a child is one quarter shy of their 9th birthday quarter (quarter 0). The sample includes child-month observations with children aged 8 up to, but not including, age 10. The figure shows estimated logit coefficients on the post x age interaction terms from equation (2.2) and standard errors clustered at the state level. Controls include child demographics, state controls, indicators for duration month from mother's date of rights termination, indicators for age as measured in months, state indicators, and month-by-year indicators.

Figure H.6: Timing Estimates of Probability of Adoption by Age Around the 2008 Update of the Incentives – Robustness to Post-Period Beginning in Fiscal Year 2008

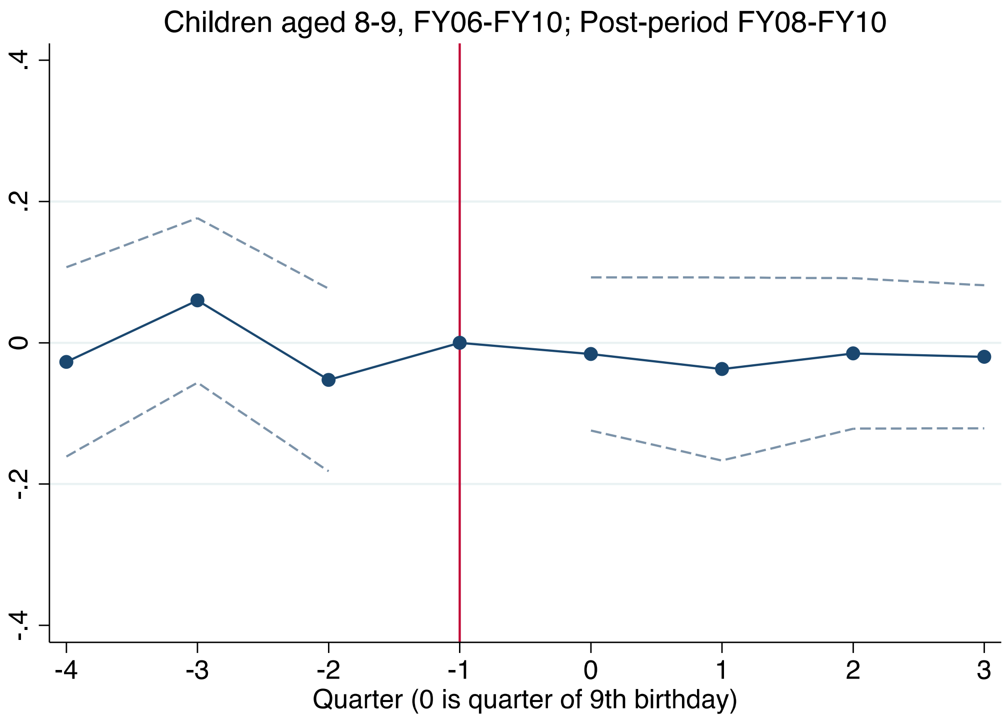


Figure shows the effect of age, as measured in quarters, relative to the quarter in which the child turns age 9, on the probability of adoption over the post period (October 2007 and later). The excluded indicator is the interaction indicating a child is one quarter shy of their 9th birthday quarter (quarter 0). The sample includes child-month observations with children aged 8 up to, but not including, age 10. The figure shows estimated logit coefficients on the post x age interaction terms from equation (2.2) and standard errors clustered at the state level. Controls include child demographics, state controls, indicators for duration month from mother's date of rights termination, indicators for age as measured in months, state indicators, and month-by-year indicators.

Table H.1: Effect of the Incentives on Adoptions of Older Children – Controls Shown

Time period (fiscal years)	2003-2004		2001-2005
Ages	8-9	7-10	8-9
	[1]	[2]	[3]
Post x older	-0.0430	-0.0048	-0.0291
	(0.0515)	(0.0435)	(0.0363)
Child characteristics			
Male	-0.144***	-0.164***	-0.153***
	(0.0290)	(0.0209)	(0.0200)
Black	-0.385***	-0.361***	-0.333***
	(0.0517)	(0.0618)	(0.0738)
Hispanic	-0.0752**	-0.102***	-0.0843**
	(0.0308)	(0.0376)	(0.0415)
Receiving matched foster care payments	-0.1200	-0.0972	-0.0420
	(0.1090)	(0.1060)	(0.1140)
State covariates			
Log population 0 - 17 year old	29.1500	23.6800	-11.7900
	(27.4100)	(21.6100)	(12.5500)
Log population 18 - 29 year old	17.48**	15.57**	6.609*
	(7.7320)	(7.8960)	(3.7350)
Log population 30 - 49 year old	-41.8000	-35.3900	13.1700
	(30.6300)	(24.3400)	(14.3700)
Log black population	2.4980	1.1830	-2.296*
	(2.1880)	(1.9280)	(1.3230)
Unemployment rate	-0.5210	-0.4560	0.0857
	(0.3690)	(0.2810)	(0.1290)
Log median income	-3.188*	-2.237*	-0.3650
	(1.6320)	(1.3580)	(1.0450)
Mean of dependent variable	0.043	0.043	0.042
Child-month observations	148,857	301,005	390,920
Number of children	18,146	31,202	37,895
Log likelihood	-25,542	-51,195	-65,281

Source: Adoption and Foster Care Analysis and Reporting System (AFCARS). See text for description of baseline sample. Fiscal year restrictions in columns [1] and [2] are for child-month observations in FY03 through FY04. The fiscal year restriction in column [3] is for child-month observations in FY01 through FY05. Age restrictions in columns [1] and [3] are for child-month observations with children aged 7 up to, but not including, age 11, and in columnn [2], for child-month observations with children aged 8 up to, but not including, age 10. The sample size in column [1] does not perfectly align with that in Table 2 due to dropped observations for perfectly predicting outcomes. All specifications also include state indicators, month-by-year indicators, month of duration indicatros, and age indicators. Logit coefficients with standard errors clustered at the state level shown in parentheses. *** p< 0.01, ** p<0.05, * p< 0.10.

Table H.2: Effect of the 2008 Update to Incentives for Adoptions of Older Children – Controls Shown

Time period (fiscal years)	2008-2009		2006-2010
Ages	8-9	7-10	8-9
	[1]	[2]	[3]
Post x older	0.0927**	0.0377	0.0222
	(0.0418)	(0.0353)	(0.0227)
Child characteristics			
Male	-0.0298	-0.0622***	-0.0717***
	(0.0246)	(0.0189)	(0.0216)
Black	-0.300***	-0.300***	-0.267***
	(0.0405)	(0.0358)	(0.0368)
Hispanic	-0.0482	-0.0586	-0.0692**
	(0.0435)	(0.0396)	(0.0276)
Receiving matched foster care payments	0.0626	0.0594	0.0572*
	(0.0422)	(0.0427)	(0.0307)
State covariates			
Log population 0 - 17 year old	-4.5080	-7.0510	-4.8500
	(8.0130)	(7.2530)	(3.2510)
Log population 18 - 29 year old	9.919*	6.1500	2.4210
	(5.8010)	(5.3800)	(2.4360)
Log population 30 - 49 year old	2.5310	7.9700	5.569*
	(8.1400)	(7.5810)	(3.1370)
Log black population	0.0442	0.2690	0.8880
	(2.6610)	(2.2730)	(0.8220)
Unemployment rate	-0.0161	-0.0250	-0.0116
	(0.0574)	(0.0551)	(0.0253)
Log median income	-1.6760	-0.9700	-0.6760
	(1.1640)	(0.9770)	(0.5210)
Mean of dependent variable	0.054	0.055	0.055
Child-month observations	127,175	254,865	309,685
Number of children	16,601	28,363	33,165
Log likelihood	-25,645	-51,620	-63,451

Source: Adoption and Foster Care Analysis and Reporting System (AFCARS). See text for description of baseline sample. Fiscal year restrictions in columns [1] and [2] are for child-month observations in FY08 through FY09. Fiscal year restriction in column [3] is for child-month observations in FY06 through FY10. Age restrictions in columns [1] and [3] are for child-month observations with children aged 8 and up to, but not including, age 10, and in columnn [2], for child-month observations with children aged 7 up to, but not including, age 11. All specifications also include indicators for duration month from mother's date of rights termination, indicators for age as measured in months, state indicators, and month-by-year indicators. Logit coefficients with standard errors clustered at the state level shown in parentheses. *** p< 0.01, ** p<0.05, * p<0.10.

APPENDIX I Appendices for “Capitalization of Charter Schools into Residential Property Values”

Table I.1: Relationship Between Charters in a School Zone and Elementary School Characteristics

	Count of Open Charters within Local School Zone	Count of Open Charters within Local School Zone
Enrollment	-0.0001	-0.0001
	(0.0001)	(0.0003)
API Score	-0.0010	0.0006
	(0.0007)	(0.0006)
Percent Black	-0.0030	0.0118**
	(0.0036)	(0.0053)
Percent Native American	0.0363	-0.0004
	(0.0346)	(0.0037)
Percent Asian	-0.0138***	0.0041
	(0.0025)	(0.0030)
Percent Filipino	-0.0204**	-0.0093
	(0.0095)	(0.0069)
Percent Hispanic	-0.00920***	0.0019
	(0.0027)	(0.0027)
Percent Pacific Islander	-0.0340*	-0.0167
	(0.0201)	(0.0137)
Percent Gifted	0.0479***	0.0031
	(0.0082)	(0.0025)
Percent Free or Reduced Lunch	-0.0006	0.00340*
	(0.0020)	(0.0018)
Percent ELL	0.00680***	-0.0007
	(0.0024)	(0.0016)
Percent Disabled	0.00603*	0.00520**
	(0.0035)	(0.0022)
Percent HS Graduate	-0.0043	0.0001
	(0.0027)	(0.0007)
Percent Bachelors Degree	-0.0015	0.00216*
	(0.0049)	(0.0012)
Percent Graduate School	-0.0034	-0.0040
	(0.0049)	(0.0034)
Observations	5,858	5,858
R-squared	0.126	0.974
School Fixed-Effects	N	Y
Census Block Fixed-Effects	N	N

Notes: See Table C.4 for a description of baseline sample. Robust standard errors clustered by elementary school zone in 2002 in parentheses. *, **, and *** denote significance at the 10%, 5% and 1% levels, respectively.

Table I.2: Heterogeneity by Neighborhood Income and Public School API (Excluding LAUSD)

	Median Household Income in Census Tract		API Score of Zoned Elementary School	
	Number of charters	Charter seats as percentage of enrollment	Number of charters	Charter seats as percentage of enrollment
0 - 0.5 miles x Tercile 1	-0.0216	0.0788	-0.00899	-0.00894
	(0.0297)	(0.107)	(0.0260)	(0.0608)
0 - 0.5 miles x Tercile 2	0.00447	-0.0713	-0.0266	-0.0440
	(0.0366)	(0.0802)	(0.0204)	(0.0378)
0 - 0.5 miles x Tercile 3	-0.00412	-0.0401	0.0432*	0.0222
	(0.0183)	(0.0304)	(0.0236)	(0.0647)
0.5 - 1 mile x Tercile 1	-0.0221*	0.0349	-0.0101	0.0294
	(0.0133)	(0.0590)	(0.0131)	(0.0352)
0.5 - 1 mile x Tercile 2	-5.21e-05	0.0222	-0.00150	0.00373
	(0.0186)	(0.0526)	(0.00952)	(0.0536)
0.5 - 1 mile x Tercile 3	0.0119	-0.0146	-0.00128	-0.0299
	(0.0126)	(0.0379)	(0.0131)	(0.0458)
1 - 1.5 miles x Tercile 1	0.00495	0.0608	-0.00461	0.00164
	(0.0125)	(0.0941)	(0.00937)	(0.0440)
1 - 1.5 miles x Tercile 2	-0.0154	-0.0164	0.00475	0.0273
	(0.0109)	(0.0554)	(0.00823)	(0.0631)
1 - 1.5 miles x Tercile 3	-0.00496	0.0127	-0.0162	0.0267
	(0.0108)	(0.0572)	(0.00985)	(0.0572)
1.5 - 2 miles x Tercile 1	-0.00514	0.0439	-0.00279	0.0284
	(0.00843)	(0.0537)	(0.00720)	(0.0356)
1.5 - 2 miles x Tercile 2	-0.000721	0.0520	-0.00480	-0.00102
	(0.00998)	(0.0542)	(0.00620)	(0.0382)
1.5 - 2 miles x Tercile 3	0.00205	0.00948	0.00592	0.0817**
	(0.00771)	(0.0301)	(0.00814)	(0.0362)
Observations	93,041	93,041	93,041	93,041
R-squared	0.881	0.880	0.880	0.880
Housing Characteristics	Y	Y	Y	Y
School Characteristics	Y	Y	Y	Y
School Fixed-Effects	N	N	N	N
Census Block Fixed-Effects	Y	Y	Y	Y

Notes: See Table C.4 for a description of baseline sample. API scores are from the year of sale for the school that was zoned to the property in 2002. Robust standard errors clustered by elementary school zone in 2002 in parentheses. *, **, and *** denote significance at the 10%, 5% and 1% levels, respectively.

Table I.3: Heterogeneity by Neighborhood Income and Public School API Using Counts of Charters in Home School District (Excluding LAUSD)

	Median Household Income in Census Tract		API Score of Zoned Elementary School	
	Number of charters	Charter seats as percentage of enrollment	Number of charters	Charter seats as percentage of enrollment
0 - 0.5 miles x Tercile 1	-0.0706*	-0.086	-0.044	-0.084
	(0.039)	(0.096)	(0.032)	(0.055)
0 - 0.5 miles x Tercile 2	-0.008	-0.156*	-0.0556*	-0.188**
	(0.055)	(0.090)	(0.031)	(0.095)
0 - 0.5 miles x Tercile 3	-0.002	-0.001	0.0325*	-0.027
	(0.031)	(0.052)	(0.018)	(0.096)
0.5 - 1 mile x Tercile 1	-0.0391*	-0.063	-0.019	-0.071
	(0.021)	(0.102)	(0.016)	(0.058)
0.5 - 1 mile x Tercile 2	-0.010	-0.126	-0.010	-0.035
	(0.026)	(0.143)	(0.016)	(0.094)
0.5 - 1 mile x Tercile 3	0.016	-0.027	0.000	-0.079
	(0.022)	(0.049)	(0.026)	(0.091)
1 - 1.5 miles x Tercile 1	-0.012	-0.092	-0.019	-0.074
	(0.019)	(0.073)	(0.016)	(0.061)
1 - 1.5 miles x Tercile 2	-0.061	-0.081	-0.012	-0.056
	(0.051)	(0.108)	(0.020)	(0.074)
1 - 1.5 miles x Tercile 3	-0.029	-0.083	-0.016	-0.131*
	(0.037)	(0.080)	(0.030)	(0.068)
1.5 - 2 miles x Tercile 1	-0.022	-0.103	-0.012	-0.062
	(0.027)	(0.074)	(0.026)	(0.101)
1.5 - 2 miles x Tercile 2	0.024	0.140	-0.005	-0.054
	(0.022)	(0.087)	(0.018)	(0.052)
1.5 - 2 miles x Tercile 3	0.006	-0.060	0.029	0.039
	(0.031)	(0.095)	(0.029)	(0.082)
Observations	93,041	93,041	93,041	93,041
R-squared	0.88	0.88	0.88	0.88
Housing Characteristics	Y	Y	Y	Y
School Characteristics	Y	Y	Y	Y
School Fixed-Effects	N	N	N	N
Census Block Fixed-Effects	Y	Y	Y	Y

Notes: See Table C.4 for a description of baseline sample. API scores are from the year of sale for the school that was zoned to the property in 2002. Robust standard errors clustered by elementary school zone in 2002 in parentheses.
*, **, and *** denote significance at the 10%, 5% and 1% levels, respectively.

Table I.4: Heterogeneity by Public School District API Using Charters in Home School District (Excluding LAUSD)

	API Score of School District	
	Number of charters	Charter seats as percentage of enrollment
	(iii)	(iv)
0 - 0.5 miles x Tercile 1	-0.0459*	-0.111**
	(0.026)	(0.055)
0 - 0.5 miles x Tercile 2	-0.034	-0.039
	(0.060)	(0.117)
0 - 0.5 miles x Tercile 3	0.0293**	0.006
	(0.013)	(0.070)
0.5 - 1 mile x Tercile 1	-0.0235*	-0.063
	(0.014)	(0.059)
0.5 - 1 mile x Tercile 2	-0.005	-0.069
	(0.027)	(0.073)
0.5 - 1 mile x Tercile 3	0.031	0.060
	(0.027)	(0.130)
1 - 1.5 miles x Tercile 1	-0.017	-0.108*
	(0.011)	(0.062)
1 - 1.5 miles x Tercile 2	-0.027	-0.096
	(0.025)	(0.064)
1 - 1.5 miles x Tercile 3	0.013	0.113*
	(0.022)	(0.068)
1.5 - 2 miles x Tercile 1	-0.020	-0.098
	(0.020)	(0.075)
1.5 - 2 miles x Tercile 2	0.031	0.042
	(0.021)	(0.048)
1.5 - 2 miles x Tercile 3	0.010	0.090
	(0.060)	(0.154)
Observations	93,041	93,041
R-squared	0.88	0.88
Housing Characteristics	Y	Y
School Characteristics	Y	Y
School Fixed-Effects	N	N
Census Block Fixed-Effects	Y	Y

Notes: See Table C.4 for a description of baseline sample. API scores are from the year of sale for the district of the elementary school that was zoned to the property in 2002. Robust standard errors clustered by elementary school zone in 2002 in parentheses. *, **, and *** denote significance at the 10%, 5% and 1% levels, respectively.

Table I.5: Effect of Charters on Log Sale Prices by Charter Grade Levels

	LA County	
	Number of charters	Charter seats as percentage of enrollment
Elementary - high school		
0 - 0.5 miles	0.00454	0.0161
	(0.0190)	(0.0229)
0.5 - 1 mile	0.00448	0.0282**
	(0.0109)	(0.0134)
1 - 1.5 miles	-0.00673	0.0126
	(0.0102)	(0.0122)
1.5 - 2 miles	-0.00706	-0.00861
	(0.00984)	(0.0126)
Middle school		
0 - 0.5 miles	-0.00538	0.00362
	(0.0248)	(0.0264)
0.5 - 1 mile	0.0204*	-0.00197
	(0.0118)	(0.0155)
1 - 1.5 miles	0.0161	0.0104
	(0.0101)	(0.0158)
1.5 - 2 miles	0.0110	-0.00336
	(0.00744)	(0.0189)
High school		
0 - 0.5 miles	0.00184	-0.00533
	(0.0115)	(0.0194)
0.5 - 1 mile	0.00515	-0.000962
	(0.00735)	(0.0128)
1 - 1.5 miles	-0.00250	0.0130
	(0.00541)	(0.0140)
1.5 - 2 miles	-0.00362	0.00529
	(0.00489)	(0.0121)
Elementary school		
0 - 0.5 miles	-0.0173	-0.0162
	(0.0132)	(0.0226)
0.5 - 1 mile	-0.0159*	-0.0418*
	(0.00861)	(0.0251)
1 - 1.5 miles	0.00152	-0.00958
	(0.00636)	(0.0356)
1.5 - 2 miles	-0.00457	0.00664
	(0.00559)	(0.0305)
Observations	158,211	158,211
R-squared	0.881	0.881
Housing Characteristics	Y	Y
School Characteristics	Y	Y
Census Block Fixed-Effects	Y	Y

Notes: See Table C.4 for a description of baseline sample and controls. Robust standard errors clustered by elementary school zone in 2002 in parentheses. *, **, and *** denote significance at the 10%, 5% and 1% levels, respectively.

Table I.6: Effect of Charters on Log Sale Prices – Heterogeneity by Year

	LA County	
	Number of charters	Charter seats as percentage of enrollment
0 - 0.5 miles x 2008	-0.00164	-0.00106
	(0.0144)	(0.0273)
0 - 0.5 miles x 2009	-0.00305	0.00532
	(0.00957)	(0.0220)
0 - 0.5 miles x 2010	-0.000975	0.0132
	(0.00855)	(0.0200)
0 - 0.5 miles x 2011	-0.0106	-0.0258
	(0.00955)	(0.0235)
0.5 - 1 mile x 2008	0.0102	0.00924
	(0.00809)	(0.0312)
0.5 - 1 mile x 2009	-0.00327	-0.0304
	(0.00581)	(0.0213)
0.5 - 1 mile x 2010	0.00127	-0.0116
	(0.00489)	(0.0228)
0.5 - 1 mile x 2011	0.00244	-0.00873
	(0.00563)	(0.0238)
1 - 1.5 miles x 2008	0.0141***	0.0135
	(0.00472)	(0.0324)
1 - 1.5 miles x 2009	0.00109	-0.0318
	(0.00372)	(0.0272)
1 - 1.5 miles x 2010	0.00116	-0.00301
	(0.00318)	(0.0307)
1 - 1.5 miles x 2011	0.00346	-0.0109
	(0.00350)	(0.0313)
1.5 - 2 miles x 2008	0.00166	0.0461
	(0.00474)	(0.0394)
1.5 - 2 miles x 2009	-0.00204	-0.0137
	(0.00354)	(0.0294)
1.5 - 2 miles x 2010	-0.000481	-0.0141
	(0.00322)	(0.0312)
1.5 - 2 miles x 2011	-0.000747	0.000509
	(0.00305)	(0.0299)
Observations	158,211	158,211
R-squared	0.881	0.881
Housing Characteristics	Y	Y
School Characteristics	Y	Y
Census Block Fixed-Effects	Y	Y

Notes: See Table C.4 for a description of baseline sample. Robust standard errors clustered by elementary school zone in 2002 in parentheses. *, **, and *** denote significance at the 10%, 5% and 1% levels, respectively.

Table I.7: Heterogeneity by Neighborhood Income, Public School API, and Percent Minority

	LA County							
	Median Household Income in Census Tract		API Score of Zoned Elementary School		Percent Minority in Elementary School		Percent Minority in Census Tract	
	Number of charters	Charter seats as percentage of enrollment	Number of charters	Charter seats as percentage of enrollment	Number of charters	Charter seats as percentage of enrollment	Number of charters	Charter seats as percentage of enrollment
	(i)	(ii)	(iii)	(iv)	(v)	(vi)	(vii)	(viii)
0 - 0.5 miles x Quartile 1	0.011	0.079	0.001	-0.007	-0.026	-0.047	-0.032	-0.065
	(0.014)	(0.053)	(0.012)	(0.032)	(0.040)	(0.042)	(0.050)	(0.051)
0 - 0.5 miles x Quartile 2	-0.0530***	-0.068	-0.006	0.003	-0.001	0.027	-0.020	-0.055
	(0.017)	(0.056)	(0.012)	(0.028)	(0.019)	(0.039)	(0.020)	(0.036)
0 - 0.5 miles x Quartile 3	0.024	0.004	-0.015	-0.012	-0.001	0.022	-0.007	0.0812*
	(0.018)	(0.035)	(0.017)	(0.039)	(0.012)	(0.031)	(0.016)	(0.049)
0 - 0.5 miles x Quartile 4	-0.026	-0.052	0.017	0.000	-0.002	-0.005	0.004	0.019
	(0.032)	(0.042)	(0.026)	(0.042)	(0.011)	(0.032)	(0.012)	(0.034)
0.5 - 1 mile x Quartile 1	-0.002	0.039	-0.005	-0.006	0.003	0.001	0.013	-0.020
	(0.008)	(0.049)	(0.007)	(0.034)	(0.014)	(0.037)	(0.024)	(0.041)
0.5 - 1 mile x Quartile 2	0.000	0.029	-0.002	-0.027	-0.002	-0.027	-0.009	0.019
	(0.010)	(0.048)	(0.006)	(0.036)	(0.009)	(0.027)	(0.011)	(0.043)
0.5 - 1 mile x Quartile 3	-0.006	-0.069	0.008	0.028	-0.004	-0.033	-0.016	0.026
	(0.010)	(0.071)	(0.007)	(0.044)	(0.006)	(0.041)	(0.011)	(0.052)
0.5 - 1 mile x Quartile 4	0.007	-0.007	0.000	-0.017	0.000	0.034	0.002	-0.027
	(0.012)	(0.032)	(0.011)	(0.033)	(0.006)	(0.048)	(0.007)	(0.059)

Notes: See Table C.4 for a description of baseline sample. API scores are from the year of sale for the school that was zoned to the property in 2002. Robust standard errors clustered by elementary school zone in 2002 in parentheses. *, **, and *** denote significance at the 10%, 5% and 1% levels, respectively.

Table I.7 (cont'd)

	LA County							
	Median Household Income in Census Tract		API Score of Zoned Elementary School		Percent Minority in Elementary School		Percent Minority in Census Tract	
	Number of charters	Charter seats as percentage of enrollment	Number of charters	Charter seats as percentage of enrollment	Number of charters	Charter seats as percentage of enrollment	Number of charters	Charter seats as percentage of enrollment
	(i)	(ii)	(iii)	(iv)	(v)	(vi)	(vii)	(viii)
1 - 1.5 miles x Quartile 1	0.005	0.021	0.005	-0.022	-0.008	-0.008	-0.012	-0.052
	(0.005)	(0.073)	(0.004)	(0.037)	(0.010)	(0.031)	(0.014)	(0.039)
1 - 1.5 miles x Quartile 2	0.0163**	0.097	0.00790*	0.032	-0.005	-0.022	0.003	0.034
	(0.008)	(0.083)	(0.005)	(0.044)	(0.007)	(0.045)	(0.010)	(0.065)
1 - 1.5 miles x Quartile 3	-0.007	-0.049	0.001	0.006	0.008	-0.001	0.007	0.074
	(0.008)	(0.046)	(0.006)	(0.035)	(0.006)	(0.050)	(0.010)	(0.047)
1 - 1.5 miles x Quartile 4	-0.015	-0.012	-0.009	-0.016	0.00694*	0.043	0.005	0.008
	(0.011)	(0.038)	(0.008)	(0.038)	(0.004)	(0.056)	(0.004)	(0.066)
1.5 - 2 miles x Quartile 1	-0.002	-0.055	-0.002	0.021	0.002	-0.013	-0.005	-0.034
	(0.004)	(0.097)	(0.003)	(0.039)	(0.009)	(0.043)	(0.013)	(0.057)
1.5 - 2 miles x Quartile 2	-0.005	0.006	-0.003	-0.016	-0.003	-0.023	0.003	-0.009
	(0.007)	(0.084)	(0.005)	(0.044)	(0.007)	(0.039)	(0.008)	(0.049)
1.5 - 2 miles x Quartile 3	0.005	0.035	0.000	0.002	-0.004	0.002	-0.012	0.013
	(0.006)	(0.052)	(0.005)	(0.040)	(0.004)	(0.056)	(0.008)	(0.046)
1.5 - 2 miles x Quartile 4	-0.004	-0.026	-0.004	-0.030	-0.002	-0.007	0.000	0.062
	(0.007)	(0.042)	(0.007)	(0.055)	(0.003)	(0.070)	(0.003)	(0.070)
Observations	158,211	158,211	158,211	158,211	158,211	158,211	158,211	158,211
R-squared	0.85	0.85	0.85	0.85	0.85	0.85	0.85	0.85
Housing Characteristics	Y	Y	Y	Y	Y	Y	Y	Y
School Characteristics	Y	Y	Y	Y	Y	Y	Y	Y
School Fixed-Effects	N	N	N	N	N	N	N	N
Census Block Fixed-Effects	Y	Y	Y	Y	Y	Y	Y	Y

Notes: See Table C.4 for a description of baseline sample. API scores are from the year of sale for the school that was zoned to the property in 2002. Robust standard errors clustered by elementary school zone in 2002 in parentheses. *, **, and *** denote significance at the 10%, 5% and 1% levels, respectively.

Table I.8: Effect of Charters on Log Sale Prices – Specification Checks

	Use sale levels	Limit to 0-2 Bedrooms	Limit to 3+ Bedrooms	Include Properties with > 8 Bedrooms	Drop Properties w/ > 5000 sf	Drop Multi-Unit	Summer Only	Add 2-5 mile ring	Include School FE
	(i)	(ii)	(iii)	(iv)	(v)	(vi)	(vii)	(viii)	(ix)
					A. Number of charters				
0 - 0.5 miles	-2181	-0.0043	-0.0019	-0.0050	-0.0061	-0.0029	0.0133	-0.0068	-0.0048
	(2422)	(0.0147)	(0.0109)	(0.0087)	(0.0083)	(0.0087)	(0.0262)	(0.0084)	(0.0083)
0.5 - 1 mile	565	0.0008	0.0067	0.0001	0.0009	-0.0016	0.0074	-0.0004	0.0016
	(1383)	(0.0075)	(0.0061)	(0.0049)	(0.0048)	(0.0048)	(0.0133)	(0.0049)	(0.0048)
1 - 1.5 miles	-272	0.0065	-0.0026	0.0040	0.0018	0.0026	0.0021	0.0011	0.0029
	(921.8)	(0.0056)	(0.004)	(0.0034)	(0.0031)	(0.0034)	(0.01020)	(0.0032)	(0.0031)
1.5 - 2 miles	-5	0.0063	-0.0050	-0.0021	-0.0013	0.0011	-0.0048	-0.0018	-0.0007
	(875.6)	(0.0059)	(0.0036)	(0.0032)	(0.0028)	(0.003)	(0.0092)	(0.0029)	(0.0028)
2 - 5 miles								0.0009	
								(0.0008)	
					B. Charter seats as percentage of enrollment				
0 - 0.5 miles	-3049	0.007	0.006	0.001	-0.002	-0.005	0.053	-0.002	-0.002
	(7929)	(0.037)	(0.025)	(0.023)	(0.019)	(0.021)	(0.06)	(0.019)	(0.02)
0.5 - 1 mile	-2389	0.022	-0.003	-0.009	-0.013	-0.019	0.047	-0.014	-0.012
	(9215)	(0.048)	(0.022)	(0.021)	(0.020)	(0.020)	(0.057)	(0.019)	(0.020)
1 - 1.5 miles	-4876	0.039	-0.026	-0.004	-0.019	-0.010	0.034	-0.013	-0.013
	(9456)	(0.049)	(0.029)	(0.025)	(0.022)	(0.024)	(0.058)	(0.024)	(0.024)
1.5 - 2 miles	3330	0.028	0.001	-0.013	-0.009	0.002	-0.001	-0.003	-0.006
	(9264)	(0.066)	(0.027)	(0.03)	(0.026)	(0.026)	(0.040)	(0.026)	(0.026)
2 - 5 miles								0.096	
								(0.082)	
Observations	158,211	49,432	108,779	159,906	157,783	151,797	42,962	158,211	158,211
R-squared	0.90	0.89	0.91	0.87	0.88	0.89	0.93	0.88	0.88

Note: The data cover sales from September 2008 through September 2011. All regressions control for the following: month by year fixed effects; census block fixed effects; housing characteristic controls - number of bedrooms, bathrooms, square footage, and quality; school characteristics - API levels overall, lags and second lags of overall API scores, % of students of each race, % free lunch, % gifted, % English language learners, % disabled, and parent education levels. Standard errors clustered at the school level are in parentheses. ***, **, and * indicate significance at the 1%, 5%, and 10% levels, respectively.

Table I.9: Effect of Charters on Log Sale Prices for Los Angeles County – All Controls Shown

	LA County								
	Number of charters			Charter seats as percentage of enrollment			Charter Penetration Variables Excluded		
	(i)	(ii)	(iii)	(iv)	(v)	(vi)	(vii)	(viii)	(ix)
A. Distance gradient									
0 - 0.5 miles	-0.00725	-0.0353***	-0.00543	0.0741*	0.00648	-0.00134			
	(0.0131)	(0.00800)	(0.00827)	(0.0438)	(0.0249)	(0.0194)			
0.5 - 1 mile	0.00858	-0.0253***	0.000950	0.117**	-0.0166	-0.0128			
	(0.00748)	(0.00609)	(0.00476)	(0.0564)	(0.0270)	(0.0195)			
1 - 1.5 miles	0.0252***	-0.0149***	0.00223	0.140**	-0.0442*	-0.0123			
	(0.00578)	(0.00387)	(0.00313)	(0.0616)	(0.0268)	(0.0239)			
1.5 - 2 miles	0.0239***	-0.00460	-0.00110	0.120	-0.0217	-0.00470			
	(0.00494)	(0.00309)	(0.00279)	(0.0770)	(0.0340)	(0.0255)			
Housing Characteristics									
Number of Bathrooms	-0.0406***	-0.0369***	0.00750*	-0.0413***	-0.0371***	0.00748*	-0.0416***	-0.0370***	0.00748*
	(0.00643)	(0.00381)	(0.00418)	(0.00666)	(0.00382)	(0.00418)	(0.00670)	(0.00383)	(0.00418)
Number of Bedrooms	0.00632	0.0513***	0.0356***	0.00187	0.0512***	0.0356***	0.000854	0.0511***	0.0356***
	(0.00664)	(0.00327)	(0.00289)	(0.00743)	(0.00327)	(0.00289)	(0.00752)	(0.00327)	(0.00289)
Square Feet of House	0.000358***	0.000332***	0.000220***	0.000369***	0.000333***	0.000220***	0.000370***	0.000333***	0.000220***
	(1.29e-05)	(7.88e-06)	(7.57e-06)	(1.34e-05)	(7.90e-06)	(7.58e-06)	(1.36e-05)	(7.89e-06)	(7.58e-06)
Quality of Housing Materials	0.00715	0.00569	0.0105***	-0.00123	0.00619	0.0105***	-0.00187	0.00617	0.0105***
	(0.00791)	(0.00456)	(0.00380)	(0.00840)	(0.00461)	(0.00380)	(0.00853)	(0.00462)	(0.00380)

Sample includes property sales from April 2009 through September, 2011. The independent variable denotes either the number of charters in operation or the share of enrollment in operating charters as of the sale date in various distance rings from the property. Housing chracteristics include number of bedrooms, bathrooms, square footage, and quality. School chracteristics include API levels overall, lags and second lags of overall API scores, % of students of each race, % free lunch, % gifted, % English language learners, % disabled, and parent education levels for elementary school zoned to the property in 2002. All regressions include month-by-year fixed-effects. Robust standard errors clustered by elementary school zone in 2002 in parentheses. *, **, and *** denote significance at the 10%, 5% and 1% levels, respectively.

Table I.9 (cont'd)

	LA County								
	Number of charters			Charter seats as percentage of enrollment			Charter Penetration Variables Excluded		
	(i)	(ii)	(iii)	(iv)	(v)	(vi)	(vii)	(viii)	(ix)
Local School Characteristics									
Number of Students Enrolled in Local School	-0.000576***	2.36e-05	-5.56e-05	-0.000559***	2.73e-05	-5.57e-05	-0.000560***	2.56e-05	-5.57e-05
	(9.23e-05)	(6.59e-05)	(4.76e-05)	(9.48e-05)	(7.24e-05)	(4.72e-05)	(9.75e-05)	(7.31e-05)	(4.71e-05)
Number of Students enrolled in Private School	0.00197***	0.000575***	0.000143	0.00190***	0.000590***	0.000143	0.00192***	0.000588***	0.000144
	(0.000325)	(0.000179)	(0.000437)	(0.000327)	(0.000180)	(0.000437)	(0.000326)	(0.000180)	(0.000436)
Academic Performance Index (API) Growth	0.00114**	-0.000102	-0.000197**	0.00121***	-0.000100	-0.000196*	0.00121***	-9.99e-05	-0.000196*
	(0.000452)	(9.78e-05)	(0.000100)	(0.000460)	(9.96e-05)	(0.000100)	(0.000452)	(9.99e-05)	(0.000100)
Lag of API	3.77e-05	0.000186**	6.78e-05	-2.88e-05	0.000200**	6.87e-05	-6.48e-05	0.000203**	6.93e-05
	(0.000388)	(9.41e-05)	(9.27e-05)	(0.000419)	(9.65e-05)	(9.26e-05)	(0.000417)	(9.67e-05)	(9.27e-05)
Double Lag of API	0.00173***	8.95e-05	0.000153	0.00163***	8.56e-05	0.000155	0.00163***	8.31e-05	0.000153
	(0.000477)	(9.40e-05)	(9.54e-05)	(0.000489)	(9.21e-05)	(9.58e-05)	(0.000495)	(9.17e-05)	(9.56e-05)
Percent Black	0.000800	-0.00340*	-0.00152	0.00173	-0.00280	-0.00155*	0.00261	-0.00269	-0.00152
	(0.00164)	(0.00199)	(0.000934)	(0.00165)	(0.00189)	(0.000934)	(0.00161)	(0.00190)	(0.000935)
Percent American Indian	-0.0493**	0.000368	-0.00125	-0.0563**	-0.00168	-0.00132	-0.0549**	-0.00157	-0.00128
	(0.0220)	(0.00425)	(0.00393)	(0.0224)	(0.00445)	(0.00393)	(0.0226)	(0.00448)	(0.00393)
Percent Asian	0.00203**	0.00151	0.000215	0.00241***	0.00136	0.000198	0.00228**	0.00141	0.000207
	(0.000892)	(0.00199)	(0.000884)	(0.000883)	(0.00223)	(0.000885)	(0.000891)	(0.00225)	(0.000886)

Sample includes property sales from April 2009 through September, 2011. The independent variable denotes either the number of charters in operation or the share of enrollment in operating charters as of the sale date in various distance rings from the property. Housing chracteristics include number of bedrooms, bathrooms, square footage, and quality. School chracteristics include API levels overall, lags and second lags of overall API scores, % of students of each race, % free lunch, % gifted, % English language learners, % disabled, and parent education levels for elementary school zoned to the property in 2002. All regressions include month-by-year fixed-effects. Robust standard errors clustered by elementary school zone in 2002 in parentheses. *, **, and *** denote significance at the 10%, 5% and 1% levels, respectively.

Table I.9 (cont'd)

	LA County								
	Number of charters			Charter seats as percentage of enrollment			Charter Penetration Variables Excluded		
	(i)	(ii)	(iii)	(iv)	(v)	(vi)	(vii)	(viii)	(ix)
Percent Filipino	-0.00199	0.00135	0.00178	-0.00234	0.00116	0.00176	-0.00266	0.00122	0.00178
	(0.00291)	(0.00182)	(0.00142)	(0.00291)	(0.00180)	(0.00142)	(0.00295)	(0.00179)	(0.00143)
Percent Hispanic	0.00663***	-0.00273**	-0.00121	0.00648***	-0.00264**	-0.00122	0.00633***	-0.00255*	-0.00120
	(0.00131)	(0.00139)	(0.000851)	(0.00132)	(0.00133)	(0.000847)	(0.00133)	(0.00133)	(0.000849)
Percent Pacific Islander	0.0311**	0.000137	-0.000251	0.0267**	-0.000726	-0.000257	0.0221*	-0.000591	-0.000242
	(0.0138)	(0.00355)	(0.00374)	(0.0136)	(0.00360)	(0.00374)	(0.0131)	(0.00361)	(0.00374)
Percent Gifted	0.00203	0.000416	4.41e-05	0.00222	0.000311	3.74e-05	0.00327**	0.000291	4.29e-05
	(0.00156)	(0.000593)	(0.000615)	(0.00157)	(0.000567)	(0.000615)	(0.00155)	(0.000563)	(0.000615)
Percent Free/Reduced Lunch	0.000946	-0.000273	-0.000116	0.00142	-0.000372	-0.000115	0.00190*	-0.000362	-0.000114
	(0.00117)	(0.000373)	(0.000308)	(0.00117)	(0.000395)	(0.000308)	(0.00115)	(0.000395)	(0.000307)
Percent English Language Learners	0.00334***	0.000391	-0.000159	0.00406***	0.000545	-0.000164	0.00428***	0.000549	-0.000163
	(0.00109)	(0.000742)	(0.000594)	(0.00115)	(0.000785)	(0.000589)	(0.00119)	(0.000790)	(0.000590)
Percent Disabled	0.00993***	-0.00195*	-0.00143*	0.00965***	-0.00221*	-0.00141*	0.0105***	-0.00226*	-0.00143*
	(0.00267)	(0.00112)	(0.000797)	(0.00274)	(0.00119)	(0.000799)	(0.00279)	(0.00120)	(0.000798)

Sample includes property sales from April 2009 through September, 2011. The independent variable denotes either the number of charters in operation or the share of enrollment in operating charters as of the sale date in various distance rings from the property. Housing chracteristics include number of bedrooms, bathrooms, square footage, and quality. School chracteristics include API levels overall, lags and second lags of overall API scores, % of students of each race, % free lunch, % gifted, % English language learners, % disabled, and parent education levels for elementary school zoned to the property in 2002. All regressions include month-by-year fixed-effects. Robust standard errors clustered by elementary school zone in 2002 in parentheses. *, **, and *** denote significance at the 10%, 5% and 1% levels, respectively.

Table I.9 (cont'd)

	LA County								
	Number of charters			Charter seats as percentage of enrollment			Charter Penetration Variables Excluded		
	(i)	(ii)	(iii)	(iv)	(v)	(vi)	(vii)	(viii)	(ix)
Percent High School Graduates	0.00795***	0.000774	-7.94e-05	0.00649***	0.000892	-8.54e-05	0.00601***	0.000919	-7.97e-05
	(0.00149)	(0.000606)	(0.000622)	(0.00153)	(0.000633)	(0.000621)	(0.00155)	(0.000638)	(0.000622)
Percent College Graduates	0.0193***	0.000101	-0.000152	0.0190***	0.000264	-0.000161	0.0187***	0.000309	-0.000153
	(0.00237)	(0.000729)	(0.000673)	(0.00243)	(0.000786)	(0.000673)	(0.00246)	(0.000795)	(0.000672)
Percent Graduate School Graduates	0.0143***	-0.000143	-0.000235	0.0143***	-0.000170	-0.000222	0.0149***	-0.000190	-0.000230
	(0.00221)	(0.000988)	(0.000723)	(0.00228)	(0.000998)	(0.000721)	(0.00231)	(0.00101)	(0.000722)
B. Condensed 0-2 miles									
0 - 2 miles	0.0193***	-0.0101***	-9.80e-05	0.328***	-0.0301	-0.00750			
	(0.00321)	(0.00255)	(0.00207)	(0.112)	(0.0609)	(0.0544)			
Observations	158,211	158,211	158,211	158,211	158,211	158,211	158,211	158,211	158,211
Housing Characteristics	Y	Y	Y	Y	Y	Y	Y	Y	Y
School Characteristics	Y	Y	Y	Y	Y	Y	Y	Y	Y
School Fixed-Effects	N	Y	N	N	Y	N	N	Y	N
Census Block Fixed-Effects	N	N	Y	N	N	Y	N	N	Y

Sample includes property sales from April 2009 through September, 2011. The independent variable denotes either the number of charters in operation or the share of enrollment in operating charters as of the sale date in various distance rings from the property. Housing chracteristics include number of bedrooms, bathrooms, square footage, and quality. School chracteristics include API levels overall, lags and second lags of overall API scores, % of students of each race, % free lunch, % gifted, % English language learners, % disabled, and parent education levels for elementary school zoned to the property in 2002. All regressions include month-by-year fixed-effects. Robust standard errors clustered by elementary school zone in 2002 in parentheses. *, **, and *** denote significance at the 10%, 5% and 1% levels, respectively.

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