The Effect of Same-Sex Marriage Legalization on Adoptions and Family Formation

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The stability and availability of legal rights are known to be crucial factors influencing investment decisions. In this paper, we extend this framework to the family by estimating the impact of same-sex marriage (SSM) legalization on the demand for households' most significant investment – children. To do so, we employ a stacked difference-in-differences estimator, which leverages the differential timing of these laws across states. Using highly detailed, case-level data of nearly 20 million children in the foster care system from 1995-2019, we demonstrate that SSM led to substantial increases in the annual number of adoptions. Same-sex households drive this effect. Secondary analyses highlight the role of uncertainty reductions as an important mechanism – particularly through these laws' granting of presumptive joint parental rights.

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1 — Introduction

Researchers have long identified uncertainty as one of the primary factors exacerbating allocational inefficiencies. Investments for the future change in proportion with perceptions of its predictability. While this rationale has rightfully highlighted the importance of the stability of the rule of law for promoting economic activity among firms, little attention has been paid to the role of legal protections in affecting a household's most significant investment decisions – children.

In this paper, we examine policy changes that plausibly affected this demand by analyzing the staggered rollout of same-sex marriage (SSM) legalization across different states within the United States. More specifically, we posit that these marriage rights served to increase adoptions through both increases in the stability of partnerships and reductions in the legal uncertainty of awarding joint parental rights. To test our primary hypothesis, we utilize data covering the near full universe of children within the foster care system of the United States from 1995 to 2019. This data contains detailed personal and demographic information of approximately 20 million children, their caretakers, the goals of each foster placement, the sources and amounts of foster subsidies received, and the dates of movement into, within, and out of foster care. This level of detail within the data enables us to isolate changes in adoptions resulting from demand-side responses and explore the mechanisms behind these changes.

We employ a stacked difference-in-differences framework to estimate this impact. Our results indicate that adoptions of children from foster care rose by approximately 4%-6% due to the legalization of SSM. The magnitude of this effect is striking due to the highly inelastic nature of demand for children within the household. For example, our conservative back-of-the-envelope estimates indicate that it would cost governments approximately \$91-\$215 million in subsidies annually to generate as comparably large of an increase in adoptions as did SSM. Event study analyses of this effect reveal a sharp, persistent increase in adoptions which suggests that there was pent-up demand for these legal protections and recognition. These estimates translate to 2,500-4,250 additional adoptions per year than would have occurred otherwise. We confirm the robustness of these analyses with data from the U.S. Census Bureau's American Community Survey (ACS), which shows that same-sex households drive these increases.

We next examine the importance of reduced uncertainty as a mechanism through which SSM increases adoptions. First, we examine supply-side responses to these laws by gathering information on case managers' goals for each child's stay within the foster care system. We identify that SSM caused significant reductions in the frequency of uncertain foster care goals. These reductions are explained by substitutive increases in the goals of being adopted. These results suggest that children who were at the margin of whether to remain in the foster care system change their goal (in conjunction with their case manager) to exit via adoption in response to the increase of parents who were newly willing and able to adopt. We also document that SSM is associated with fewer unmarried couples adopting from the foster care system. This finding is noteworthy as, prior to SSM, same-sex couples would often have first to adopt and then legally fight for joint parental rights – the results of which were often left to the whims of courts that had long discriminated against them. This observed reduction in the number of adopting unmarried couples, coupled with evidence that same-sex households drive corresponding increases in adoptions, combine to provide evidence of the importance of presumptive joint parental rights in affecting the demand for children within the household.

This paper contributes to the literature that examines natural experiments and policy shocks to better understand the causes and consequences of family formation. These shocks are crucial to circumvent the endogeneity arising from the relationship between marriage and the demand for children within the household. For instance, Goldin and Katz (2002) and Bailey (2006) pioneered the study of the impact of the introduction of birth control pills on women's education, labor supply, marriage, and fertility decisions. This paper differs from these aforementioned studies in its focus on the impacts of the *removal* of legal barriers for disenfranchised groups. In this sense, we follow in the footsteps of research into the impacts of divorce and antimiscegenation law liberalization. For example, Stevenson and Wolfers (2006) and Stevenson (2007) advanced research into the topics of domestic violence, female suicide and marriage-specific capital investments by examining changes in divorce laws while Fryer Jr (2007), Gevrek (2014) and Houseworth and Fisher (2024) help us to better understand migration, the marriage wage premium and the forces impacting interracial marriages by studying cross-state variation induced by the U.S. Supreme Court's ruling in *Loving v. Virginia*.

We also provide evidence in this paper that policies that enfranchise minoritized groups can aid the intended beneficiaries while having sizeable social welfare spillovers, as there is a global shortage of individuals willing and able to foster or adopt children in need. Within the United States specifically, this issue is both widespread and persistent across regions and time. More than 5% of all children interact with the foster care system at least once before the age of eighteen, and over 100,000 annually indicate either no intention or ability to reunite with their biological parent(s) (Wildeman and Emanuel, 2014; of Health & Human Services, 2019). These children and adolescents are significantly more likely to struggle with mental and physical health concerns, disabilities, and emotional difficulties (Zito et al., 2001; Wildeman et al., 2014; Turney and Wildeman, 2016) and are far more likely to be racial or sexual minorities (Putnam-Hornstein et al., 2013; Yi et al., 2020; Grooms, 2020). More than 17,000 children age out of the foster care system each year without finding long-term foster or adoptive parents (Child Welfare Information Gateway, 2020). These consequences of aging out are severe and well-documented – leading to higher rates of homelessness, unemployment, unplanned pregnancy, and reduced educational attainment (Dworsky et al., 2013; Macomber, 2008; Dworsky and Courtney, 2010; Valset, 2018). Our estimates show that SSM legalization reduced the number of children within the foster care by four percent.

The following section discusses how the findings we present in this paper are informed by the broader literature on household economics. Sections three through five contain detailed discussions of the data, identification strategy, and methods we use to derive our estimates. We discuss these results in section six and then conclude by exploring the public finance ramifications of these impacts, our study's limitations, and possible future research directions.

2 — Literature Review

While Gary Becker's seminal work on the economics of the household (Becker and Lewis, 1973; Becker, 1981, 1991) began the formal study of how differences in constraints and changes in incentives faced by individuals could be used to provide deep insights into family organization and structure, more recent research has extended this framework into analyzing households which differ by sexual orientation and gender composition (Black et al., 2007). Working from observations that gay men and lesbian women face different legal, social, and biological constraints than heterosexual couples, a new literature has emerged which studies how these restrictions deferentially impact the family outcomes of sexual minority individuals and their corresponding spillover effects.

Studying how same-sex marriage restrictions affect adoption requires a better understanding of the demand for children among same-sex households. The biological constraints of same-sex couples for reproducing have historically made adoption a relatively attractive pathway to parenthood. Baccara et al. (2014) find that same-sex couples submit applications for adoption at nearly three times the rate of heterosexual couples. Goldberg and Conron (2018) show that the children of same-sex couples are more than seven times as likely to be adopted than those of different-sex couples. These differences can be seen in Figure 1. The top panel of this figure shows that married couples are significantly more likely to raise children across all coupled household types. The bottom part of this figure shows that, of the subset of coupled households raising children, same-sex couples are approximately six times as likely to have at least one adopted child within the home. The pronounced differences in adoption rates between married and unmarried same-sex couples provide suggestive evidence of the mechanism behind the hypothesis of this paper.

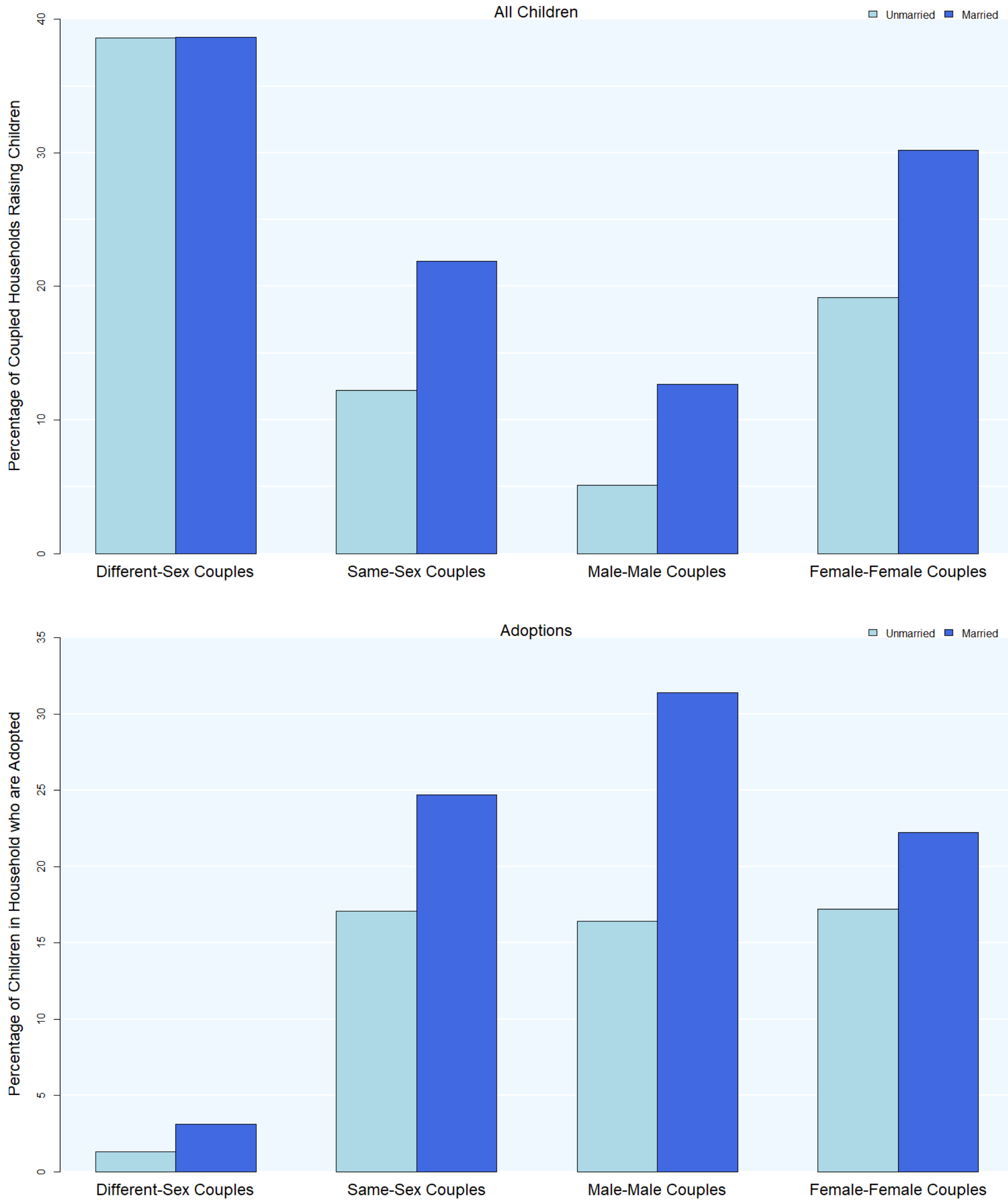
There is evidence that the gap in the probability of adopting or fostering a child would be even greater in the absence of discrimination against same-sex couples. While openly LGBT individuals and unmarried partners within every state from 2000-2015 (except for Florida) were legally eligible to adopt or foster children, the applications of "best interest of the child" criteria were openly hostile towards sexual minorities. In areas where same-sex couples had more certainty of the legality of their adopting rights, it was seldom positive news as explicit judicial precedent often barred these individuals from receiving joint custody of any adopted children (National Center for Lesbian Rights, 2014).

Mackenzie-Liu et al. (2021) perform a correspondence study and demonstrate that, while there are comparable response rates from foster care agencies to fictitious same- and different-sex couples, gay men receive much shorter responses which take longer to receive and include less information about the process of becoming a foster parent.[1]

Much research has contributed to the literature on the economics of adoption and fostering. Bitler and Zavodny (2002) studies the supply side of these markets by showing that abortion legalization in the U.S. led to sizeable reductions in the number of adoptions. Researchers have generally found no detectable effects of welfare generosity with child-rearing decisions (Acs, 1996; Hoffman and Foster, 2000;

1. In a follow-up paper, Mackenzie-Liu et al. (2022) suggest that this discrimination is larger for faith-based foster care agencies, but are underpowered statistically to demonstrate that this effect is significantly different from zero.

Figure 1 — Households Raising Children by Couple Type



The first sub-figure represents the percentage of households currently raising either a biological, step, foster, or adopted child in the household. The second graph shows (of the subset of households currently raising children) the percentage of adopted children. Data comes from the 2014-2016 American Community Survey, calculated and reported in (Goldberg and Conron, 2018).

Grogger and Bronars, 2001) while Biehl and Hill (2018) and Ginther and Johnson-Motoyama (2022) show that more generous welfare policies lower rates of foster care entry via reductions in family financial distress. Cunningham and Finlay (2013) show that higher levels of substance abuse cause foster care caseloads to increase through greater parental physical neglect and abuse.

A considerable amount of attention has focused on the demand side. Baccara et al. (2014) analyze parental preferences for adopted children – finding that prospective adoptive parental preferences generally favor girls and unborn children close to birth and disfavor African-American children. They show that these preferences hold for same-sex couples as well. Subsidies also decrease the amount of time children spend in foster care while increasing rates both of adoptions and fostering by lowering the absolute and relative cost of adoption (Doyle and Peters, 2007; Buckles, 2013; Brehm, 2021).[2] Theoretically, these subsidies could act as a price mechanism to equalize both sides of this market (Argys and Duncan, 2013). In practice, however, the magnitude of these subsidies is neither cost-of-living adjusted, tied to inflation, nor adjusted frequently enough to achieve this purpose (Horwitz et al., 2014).

3 — Data

3.1. AFCARS

To account for the many factors influencing fostering and adoption decisions, we utilize data from the *Adoption and Foster Care Analysis and Reporting System* (AFCARS) from the U.S. Department of Health and Human Services' National Data Archive on Child Abuse and Neglect (of Health & Human Services, 2019). AFCARS is a federally mandated data collection system with case-specific information on all chil-

2. However, Brehm (2018) provides evidence that subsidies for children aged nine and older were ineffective.

dren in foster care systems and adoption agencies that receive funding from the federal government's Title IV-B/E Foster Care and Child Welfare funding services. These provide funds for states and tribal governments to provide foster care, transitional independent living programs, guardianship, and adoption assistance for children – particularly those with special needs. In exchange for these funds, states must record data on all children within the state's welfare agencies (or by private, contracted agencies) from whom they are responsible for placement, care, or supervision. States are also encouraged to report other private adoptions finalized within the state.

We combine data from each AFACRS Foster Care dataset from 1995 to 2020. Each of these files includes detailed information on children who had interacted with the foster care system in that time.[3] This information includes variables measuring each child's age, sex, race, ethnicity, and disability status, the reasons for entrance into foster care, social worker and family goals for the child's foster care outcomes, the child's current setting, the structure of the caretaker's household, the age, race, and ethnicity of the caretaker(s) (when applicable), and both the type and the monthly amount of subsidies received by the foster/adoptive households.

In total, these data consist of approximately 20 million observations. Each observation corresponds to one child and contains a tremendous amount of information regarding the timing of each move into, within, and out of the foster care system, including the date of their most recent move. However, within every state, there is a population of children whose county location is not reported for privacy concerns. These consist of approximately 35% of all observations and are determined by children residing in counties with fewer than 1,000 annual observations. We assign these observations to one rural county identifier for each state. We then restrict

3. There is often a considerable lag between when these transitions into or out of the foster care system finalize and when they occur. Thus, gathering datasets for years outside our analysis window is important for ensuring accurate estimates of the number of foster care exits and entrances and the time spent in the system.

this sample to only observations where the timing of each child's movement into their current setting occurred from 2000-2016.[4] We aggregate the data from these case files to construct a county-by-year level panel dataset. We observe 146 counties over 20 time periods.[5] We present and discuss the data used for the primary analyses in Table 1 and Table A2 of the appendix.

3.2. *Summary Statistics*

The first section of Table 1, *Exits & Children in Foster Care*, reports the totals for each observation. *Foster Care* represents the number of children interacting with the foster care system in one year. *Total Exits* corresponds to the number of children discharged from the foster care system per county per year. *Parent Reunification*, Adoptions and *Negative Exits* are mutually exclusive subsets of the *Total Exits* variable. *Parent Reunification* corresponds to the number of children returning to live with their principal caretaker(s). *Adoptions* correspond to any exit from the foster care system resulting from legal adoption, guardianship, or permanent residence with another relative. *Negative Exits* corresponds to children who exit the system via aging out, transferring to another agency, running away, or death. *Still In System* is calculated as the difference between *Foster Care* and *Total Exits*.

The next variable category, *Child Characteristics*, provides variables on the children's average age, sex, and disability status.[6] We additionally create a variable measuring the average amount of time spent in foster care as measured by the difference

4. We do this for two reasons. First, AFCARS cautions against using adoption data prior to 1998 as not every state had yet developed the necessary electronic information systems, nor had financial penalties associated with poor quality data yet been levied. Second, the number of observable counties jumps considerably following data files after 1999, given compliance and data reporting increases.

5. However, we omit any observations with fewer than 30 observations of children in foster care for any year within the time window for our analysis in order to the sensitivity of the aggregation to the presence of outliers.

6. *Disability* is set equal to zero but receives a value of one if the child has a clinical diagnosis of a significant intellectual or physical disability, visual or hearing impairment, severe emotional or interpersonal difficulties or any other disability status which qualifies the child's caretakers for a subsidy under Title IV-E.

Table 1 — Summary Statistics

Statistic	N	Mean	St. Dev.	Min	Max
Exits & Children in Foster Care					
Foster Care	2,898	4,629.929	4,953.400	30	57,424
Foster Care Exits	2,898	1,771.320	1,887.183	0	23,007
Parent Reunifications	2,898	920.118	1,009.163	0	12,238
Adoptions	2,898	627.179	709.766	0	7,646
Negative Exits	2,898	224.024	288.699	0	4,301
Still In System	2,898	2,858.609	3,164.184	29	34,417
Child Characteristics					
Time in Foster Care	2,869	2.197	0.613	0.473	5.420
Age	2,830	8.259	1.193	4.181	12.171
Male	2,897	0.519	0.026	0.312	0.706
Disability	2,898	0.238	0.159	0	0.976
Same Race / Ethnicity	2,374	0.542	0.147	0.052	0.983
Foster Care Outcome Goal					
Parent Reunification	2,898	0.518	0.150	0	0.953
Adoption	2,898	0.356	0.121	0.030	1
Unknown Goal	2,898	0.099	0.112	0	0.932
Caretaker Family Structure					
Married	2,898	0.363	0.124	0	0.789
Unmarried	2,898	0.041	0.055	0	0.328
Single Woman	2,898	0.212	0.116	0	0.794
Single Man	2,898	0.023	0.019	0	0.228
Unknown Family	2,898	0.361	0.174	0	1
Subsidies					
No Subsidy	2,866	0.261	0.242	0	0.989
Subsidy Amount	2,857	710.72	418.97	0	2,304.93

between when the child first was removed from their home and when they moved to their current setting. This variable aids in capturing additional unobservable factors across the children, the institutions responsible for their placements, and the willingness and ability to foster or adopt among the populations of potential caretakers that are not explicitly measured.

The third section in Table 1 captures the child's goal for remaining in foster care. This information is reported by their case manager, who selects one of two options approximately ninety percent of the time: parental (or broader family) reunification or adoption. Instances are recorded as an unknown goal when the manager fails to report either (or any) option.

The remaining sections are related to the caretakers responsible for overseeing the child upon exiting the foster care system via either short-term fostering or permanent adoption. For purposes of identification in this paper, the largest shortcoming of this data is that we cannot observe the gender of adopting parents. The best we can do in this regard is to study the family composition of the adopting caretaker. This structure is recorded into one of five mutually exclusive categories: married, unmarried, single woman, single man, or unknown.[7] Conditional upon exit from the foster care system, approximately seventy-five percent of families in our sample receive some governmental financial support. The average amount of this subsidy is $710 per month.

3.3. *American Community Survey*

Given that we cannot observe the sex composition of the adoptive families within the AFCARS data, we supplement our analysis with individual-level demographic data from the 2000-2019 waves of the U.S. Census Bureau's American Community Survey (ACS). Crucially, these data contain geographic identifiers for all households, the sex of each household member, and each household member's relationship to

7. The unknown family structure variable both captures missingness in reporting and serves a "none-of-the-above" function such as in instances of the child aging out of the foster care system.

the head of household.

We identify same-sex households in the ACS by matching household members to the gender of the reported spouse of the head of household (Gates, 2010; Goodnature and Neto, 2021). We next aggregate household characteristics such as the number of individuals within the home, whether there are any children, the number of said children, and whether the child is adopted or fostered to their county of residence for each year.[8] We repeat this process separately for both different and same-sex households. The summary statistics for this dataset can be seen in Table A1 in the Appendix.

4 — Identification

This paper estimates the causal impact of same-sex marriage (SSM) laws on adoptions. The staggered implementation of these laws across states and over time provides a unique natural experiment for understanding the relationships between otherwise endogenous variables. Massachusetts was the first state to extend this legal right to same-sex couples in 2004. Several other states followed by legalizing SSM via either judicial rule or legislative means, which continued until when, during the summer of 2015, the Supreme Court ruled in their decision of the case *Obergefell v. Hodges* that the failure of states to license or recognize marriages between individuals of the same sex was unconstitutional. Figure A1 in the Appendix provides an timeline of these state-level law changes.

We consider treatment beginning in the first full year following legal SSM. We do this for two reasons. First, adoptions of children from foster care require long-term planning by any prospective parent in order to satisfy the state and agency's background information checks. This process involves intensive interview components and investigations of the household's financial, emotional, and safety characteris-

8. See Figure A1 for a discussion of the shortcomings of adoption and county identification in the ACS.

tics. This process often takes up to 12 months to finalize. Second, due to legislative and judicial cycles, SSM was disproportionately legalized during the fall of calendar years. A consequence of this is that SSM is often legal for less than fifteen percent of the calendar year for any state in which it is legalized. Taken together, this means that one should not expect to see SSM-induced changes in adoptions until at least one year after the date of SSM legalization.

There are two major threats to this identification strategy. One concern is the endogeneity of selection into treatment. While it is true that states that house a greater number of same-sex couples, such as Vermont, New York, and California, were more likely to have legalized same-sex marriage before states with smaller sexual minority populations, such as Arkansas, Louisiana, or Alabama, a majority of these legal protections arrived via decisions made by a group of largely non-elected judges in state and federal courts rather than by popular referenda. As such, the timing of many of these interventions was unlikely to have been expected or differentially demanded by the general public. Further, any possible anticipatory behavior is unlikely to bias our results significantly due to the slow-moving nature of the adoption process. Another primary concern is that these legal changes lead to offsetting behavior for different-sex couples. The literature suggests that this effect is doubtful to either exist or bias our results as there is no evidence that SSM affected different-sex marriage coupling, family formation, divorce, or extramarital birth rates in either domestic or international contexts (Dillender, 2014; Trandafir, 2014, 2015; Carpenter, 2020).

This identification strategy follows a growing body of related research that similarly leverages changes in legal SSM. These studies include a wide array of topics ranging from discrimination and employment (Sansone, 2019), household income (Delhommer and Hamermesh, 2021), LGBT hate crimes (Pettis et al., 2022; Nikolaou, 2022b), attitudes and polarization (Flores and Barclay, 2016; Aksoy et al., 2020), health care and insurance usage (Hatzenbuehler et al., 2012; Dillender, 2015; Carpenter et al., 2021), and sexual and mental health (Anderson et al., 2021; Chen and van Ours, 2022; Nikolaou, 2022a).

Though the identifying variation we leverage does not differ from that employed within the literature, our empirical approach diverges primarily through its focus on analyzing outcomes at the county (rather than state) level. We believe that this is an important theoretical and statistical consideration. First, the spatial segregation of minoritized groups is a well-known, thoroughly documented, and persistent social phenomenon (Ihlanfeldt and Scafidi, 2002; De la Roca et al., 2014; Bayer et al., 2014). Geographic segregation is particularly pronounced for sexual and gender minority (SGM) individuals living within highly urbanized areas (Black et al., 2000, 2002). Given that SGM individuals can simultaneously make up comparatively small (large) overall percentages of state (county) populations, researchers studying these populations increasingly risk Type II statistical errors in analyses when aggregating data to larger geographic levels.[9]

5 — Methods

We follow the literature empirically by employing a difference-in-differences estimator to recover the impacts of legal same-sex marriage. The primary idea of this approach is to compare changes in adoptions between counties, which, holding all else constant, only differ in the timing with which SSM is legalized. Given that SSM became legal countrywide in the summer of 2015, this empirical strategy relies upon comparing these effects to the outcomes of "not-yet treated" control counties.

Our method for estimating difference-in-differences models can be seen in the two-way fixed effects linear regression model in Equation 1. The data is analyzed at the stack (s) by county (c) by year (t) level. This stack indicator is created to address the bias that can arise from OLS' handling of binary staggered treatment timing

9. See Buzzelli (2020) for more discussion on the sensitivity of results and standard errors with differing levels of aggregation.

variables (Goodman-Bacon, 2021; Baker et al., 2022).

$$Y_{sct} = \alpha_{sc} + \delta_{st} + \beta T_{sct} + \omega_j Z_{sct} + \epsilon_{sct} \tag{1}$$

This stack indicator captures the fact that the data has been restructured into relative event time rather than calendar time to ensure that already treated counties do not act as a control group for those not yet treated. This restructuring occurs as follows: There is some time (E_c) at which each county receives legal same-sex marriage. Counties treated in a unique period (E_c) act as a treated unit, while those who receive treatment at any point after n years ($E_j > E_c + n$) act as the control group.[10,11] This process is repeated for all unique treatment periods (E_c) except the final period for whom no control group would exist in this setting. Each iteration is considered one stack and is given a unique indicator. Finally, each stack is combined ("stacked") into one final data frame.

Thus, Equation 1 includes stack-by-county and stack-by-year fixed effects along with a treatment indicator variable (T_{sct}), which is equal to zero but takes a value of one after same-sex marriage legalization. Finally, there exists a number (j) of control variables (Z_{sct}) that describe detailed demographic and case-specific background information about each child as well as variations in the generosity of subsidies offered to adopt parents. Importantly, we also control for the number of children within the foster care system in specifications that analyze changes in adoptions. We posit that any remaining variation in the adoptions of children from foster care that cannot be explained by changes in the overall foster care population can most plausibly be attributed to demand-side changes in the population of potential adoptive parents.

10. This n-year cut-off is an arbitrary, researcher-made decision that varies depending upon the specifics of the scenario. We select three years to balance the tradeoff that maximizes the number of periods and suitable control units.

11. If no control group can be selected within n periods, we restrict the post-treatment periods by $n - k$ where $k = E_c - E_j - 1$ for both treatment and control groups. For instance, Texas (treated in 2015) can act as a control unit for Arizona (treated in 2014), but only for one year post-treatment. Thus, the entirety of the data stack for 2014 legalizing states was cut off in 2015.

One crucial assumption within difference-in-differences models is that the outcomes between treated and control groups would continue trending parallel over time without treatment intervention. We test for the plausibility of this assumption by employing an event study model that dynamically estimates the treatment effects of SSM on adoptions. This model can be seen in equation 2.

$$Adoptions_{sct} = \alpha_{sc} + \delta_{st} + \sum_{e} \beta_e \, \mathbb{I}[t - E_c] + \omega_j Z_{sct} + \epsilon_{sct} \tag{2}$$

where $\{ \, e \in \{-5 : 3\} : e \neq -1 \}$

Finally, we both replicate and expand upon our findings with data from the ACS. This data allows us to identify the sex composition of household members and their spouses. Thus, we categorize households (h) as belonging to a different-sex or same-sex household ($SSHH$). We interact this treatment indicator with a binary variable representing whether the household is same-sex. This model, when estimated with stack-by-county-by-year (α_{sct}), stack-by-household type-by-year (δ_{sht}) and stack-by-county-by-household type (γ_{sch}) fixed effects becomes a triple difference estimator. The coefficient of interest (β) tests whether the effect of SSM legalization varies between same-sex and different-sex households while holding constant a wide array of outcome differences across counties, household types, and periods.[12] The interpretation of this model is to subtract the difference-in-difference estimate of control counties (comparing same-sex to different-sex households before and after SSM legalization in the treated counties) from the equivalent estimate in treated counties. The staggered nature of treatment timing means that the coefficient β represents the average of these triple difference calculations across stacks.

$$Y_{scht} = \alpha_{sct} + \delta_{sht} + \gamma_{sch} + \beta(T_{sct} \times SSHH_{sh}) + \epsilon_{sct} \tag{3}$$

12. The restrictiveness of the fixed effects of this model means that only the interaction of the SSM treatment indicator and household type can be estimated due to multi-colinearity.

Standard errors are calculated by clustering at the stack-by-state level for each empirical specification. This is done to account for potential autocorrelation in the residuals of the counties within treated states.

6 — Results

6.1. Primary Analyses

Models 1 and 2 of Table 2 showcase our baseline specifications. Model 1 is estimated at the county level, while Model 2 is analyzed on an equivalent dataset that only differs in that it has been aggregated to the state level. Each indicates positive and statistically significant impacts. Models 3 and 4 reproduce these results with added demographic and subsidy controls. These controls include the child's age, sex, race, disability status, the amount of time spent in foster care, the amount of subsidy awarded, and whether their race and ethnic group match with their adoptive parent(s). The addition of the controls reduces the coefficients of the SSM indicator variable, but only marginally while maintaining statistical significance.

The baseline models suggest SSM legalization caused an additional 45-50 (284-327) adoptions per county (state) per year. Relative to the pre-treatment mean of adoptions for treated counties (states), this represents a 9-13 (16-18) percent increase. Notably, an interesting tradeoff between data quality and statistical power is evident from this table. The addition of control variables from models 1 to 3 reduces the number of employable observations – largely due to complete missingness in reporting parental race/ethnicity for select counties early in the analysis window. No such issue appears when analyzing state-level observations, where the magnitude of the effect is almost twice as large but is far less precisely estimated.

Figure 2 examines this issue further by piloting out the dynamic treatment effects of models 1 and 2 from Table 2. For presentational simplicity, the output from these regressions is divided by the average number of adoptions prior to legal SSM in

Table 2 — The Effect of Same-Sex Marriage on Adoptions

Dependent Variable:	Adoptions			
	Baseline		Baseline + Controls	
Model:	(1)	(2)	(3)	(4)
Variables				
Treatment (SSM)	50.89***	327.09*	45.80**	284.14*
	(19.51)	(194.79)	(19.03)	(163.22)
Effect Size				
Mean of Dep. Var.:	500	1820	500	1820
Change rel. to. Dep. Var:	0.10	0.18	0.09	0.16
Fixed-effects				
Stack by County	✓		✓	
Stack by Year	✓	✓	✓	✓
Stack by State		✓		✓
Fit statistics				
Observations	9,332	2,325	8,810	2,325

*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

The coefficient is the estimated average treatment effect of same-sex marriage legalization on adoptions. The odd columns are analyzed using data that is aggregated to the county level whereas the even columns are aggregated to the state level. The first two models are the baseline specifications. The second two models include a wide array of control variables such as the children's age, sex, race, disability status, amount of time in foster care and the subsidy amount. Each model controls for the population of children remaining in the foster care system. Each model's standard errors are clustered at the state-level.

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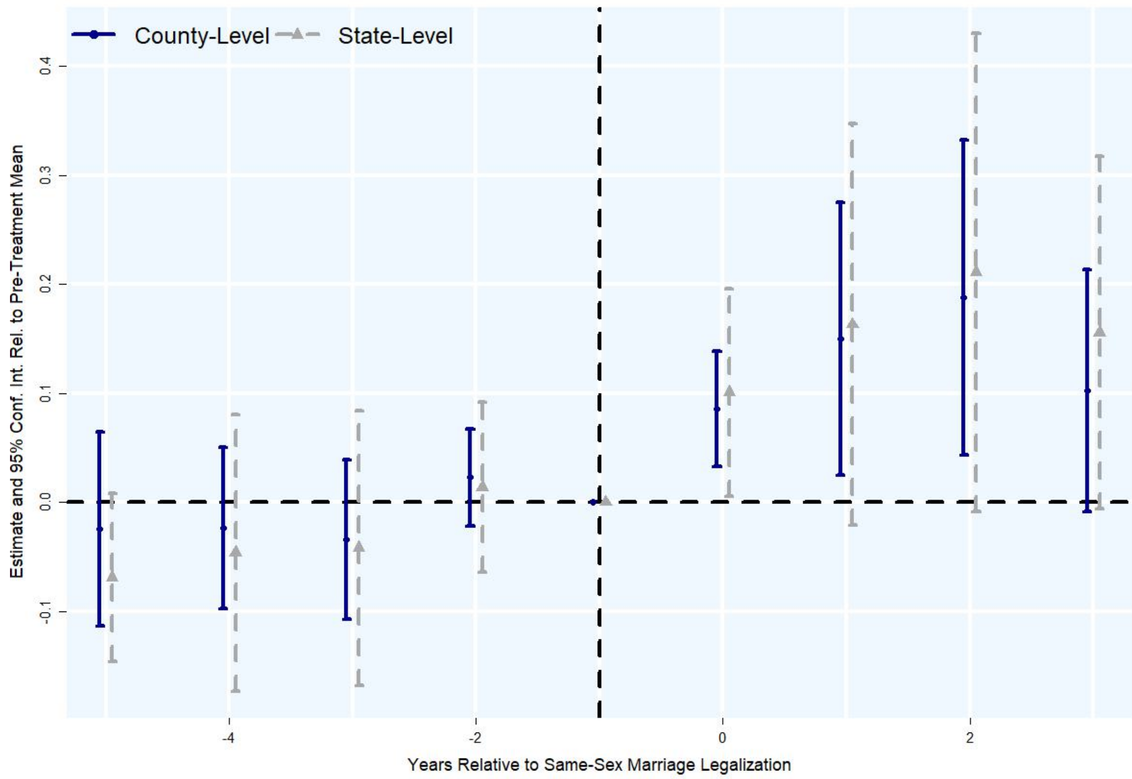
Figure 2 — Event Study of the Effect of Same-Sex Marriage on Adoptions



treated areas. Thus, each coefficient in Figure 2 represents the average percent change in adoptions for the treatment group relative to the control group at each period relative to SSM legalization.

Figure 2 demonstrates that the average treatment effects calculated in Table 2 lend themselves to a causal interpretation. This effect can be seen in that the divergence between treated and control groups only happens following SSM legalization. In other words, the lack of pre-treatment differences in outcomes between the two groups suggests that the trends in the number of adoptions of children from foster care would have continued parallel to one another, absent SSM legalization. These results contrast the sharp, statistically significant divergence from the pre-treatment trend in the first year following legal SSM at period zero. This effect persists and grows in magnitude for the following two years.

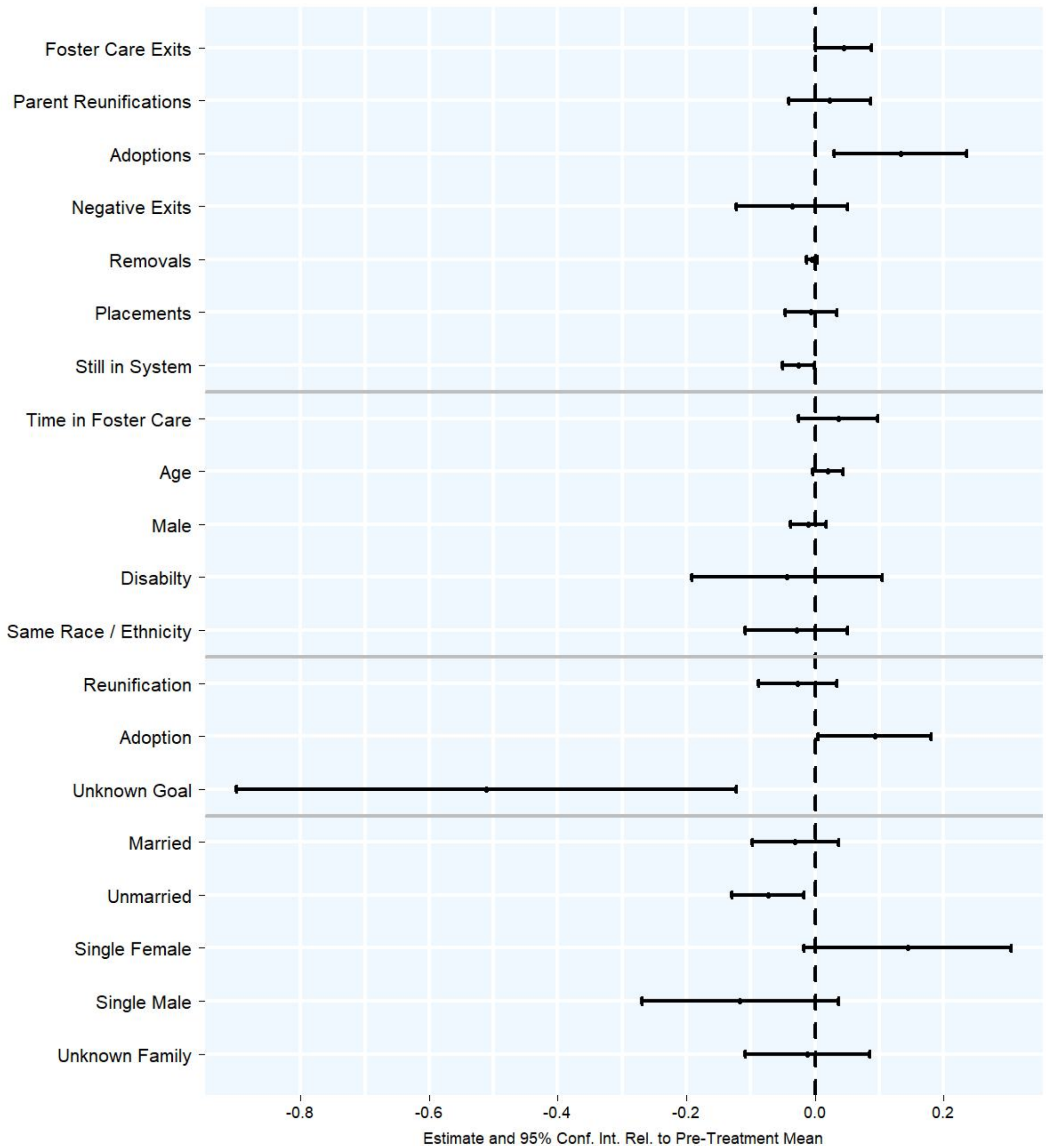
While the effect is seemingly beginning to revert to the trend by the third period, we do not think it is correct to extrapolate that these effects were short-lived for two reasons. First, in the final period of the event-time window, the estimated effect sizes are still very large ($10 \sim 20$ percentage points above the pre-treatment mean). Second, the relatively clustered timing of SSM legalization within the United States means that there are few counties or states that can act as a control group for four whole years following another state's SSM legalization. This clustering mechanically inflates the standard errors quickly after the first post-treatment period. The combination of these two events suggests that SSM legalization within the U.S. is an imperfect natural experiment for estimating long-run outcomes.

6.2. Robustness Checks

We next turn our attention to checking the consistency of the results from our primary analyses. We begin this by re-estimating model 1 from Table 2 with alternative dependent variables related to foster care. Figure 3 reports the dependent variables and their corresponding estimated effects. The output of each regression is scaled for presentational simplicity such that it can be interpreted as a percent change.

Figure 3 is presented in four sections, representing natural groupings: overall foster care outcomes, child demographics, foster care goals, and adoptive family types. The key takeaway from this first section is that SSM legalization increased the number of children exiting the foster care system by approximately 4 percent. This change in exits can neither be explained by parental reunifications, negative exits (i.e., aging out of foster care, transferring to another agency, running away, or death), removals from children's pre-foster care household for reasons of abuse or neglect, or increased numbers of placements within the foster care system. Only increases in adoptions can explain the observed reduction in foster care exits. These increases correspond to an overall reduction in the number of children remaining

Figure 3 — The Effect of Same-Sex Marriage on Alternative Outcomes



within the foster care system year-over-year by approximately 3%.

The second group of outcomes shows that the children being adopted neither spent more time in the foster care system, were older, nor were more likely to be male, disabled, or belonging to the same major racial or ethnic group as their adoptive parent(s). While we fail to detect any changes in demographics amongst the children within the system, we document substantial changes in their goals. While there was no detectable change in the aims of reunifying with one's biological parent(s), we estimate a fifty percent reduction in the incidence of children with unknown goals. This reduction is driven by a substitution in the number of children whose case managers report their goal as being adopted, increasing by 10% percent. This finiding provides suggestive evidence that one mechanism through which legal SSM increased the number of adoptions is by reducing uncertainty for children in the foster care system through visible increases in the populations of parents who are willing and able to adopt.

The final section of Figure 3 analyzes adoptive family type. We turn to this because this is the most commonly reported data in AFCARS regarding adoptive parents. Family types are married, unmarried, single female, single male, or unknown. Between each of these categories, we find significant reductions in the number of unmarried adoptive parents. While this finding is consistent with the idea that same-sex couples are now able to jointly adopt as legally married couples, we fail to document any significant corresponding increase in the number of adoptive married couples or unknown family types.

Given the imperfect ability to observe adoptive family structure with the AFCARS data, we supplement our analysis with the U.S. Census Bureau's American Community Surveys (ACS). We replicate model 1 of Table 2 in model 1 of Table 3. This coefficient means that SSM caused an overall increase of 0.02 adopted or fostered children per household.

Model 2 restricts the sample to only include same-sex households. The mag-

Table 3 — The Effect of Same-Sex Marriage on Adoptions (ACS)

Dependent Variable:	Adoptions			
	Full	SSHs	Full	
Model:	(1)	(2)	(3)	(4)
Variables				
Treatment (SSM)	0.02***	0.04***	-0.01***	
	(0.01)	(0.01)	(0.00)	
Same-Sex Household			0.01***	
			(0.00)	
Treatment (SSM) × Same-Sex Household			0.05***	0.04***
			(0.01)	(0.01)
Mean of Dep. Var.:	0.02	0.03	0.03	0.03
% Change rel. to. Dep. Var:	0.67	1.09	-	1.14
Fixed-effects				
Stack by County	✓	✓	✓	
Stack by Year	✓	✓	✓	
Stack by County by Year				✓
Stack by Household Type by Year				✓
Stack by County by Household Type				✓
Fit statistics				
Observations	32,388	16,194	32,388	32,388

*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

The coefficient for the treatment variable is the estimated average treatment effect of same-sex marriage legalization. Model 2 uses a data set only including same-sex coupled households. Model 3 interacts this treatment variable with an indicator representing whether the household is a same-sex couple. Model 4 includes county-by-year, household-by-year and county-by-household fixed effects. Each model's standard errors are clustered at the stack-by-state-level.

nitude of the result increases by forty-two percent when making this restriction. Taken together with the fact that same-sex households were already significantly more likely to have adopted children within the household, this finding from model 2 strongly suggests that same-sex couples drove the increase in adoptions we document. Model 3 provides evidence in support of this hypothesis. Interacting the SSM treatment indicator with a same-sex household dummy demonstrates that these households drive the overall positive treatment effect.

We explore the robustness of this result by estimating a triple difference model in model 4 in Table 3. We accomplish this by adding stack-by-county-by-year, stack-by-household type-by-year, and stack-by-county-by-household type fixed effects to the SSM treatment and household type interaction. This model tests the degree to which the treatment effect result can be described by differences in changes in adoption between same- and different-sex households. The output reveals that adoptions among same-sex households increased by one hundred and fourteen percent due to SSM.

We next investigate whether these increases in adoptions can be detected within other related outcome variables in Table 4 where we study both the overall number of children within the household and the presence of any children. Though the signs of the coefficients are theoretically consistent with adoptions increasing household sizes, our estimates are modest in magnitude and are statistically insignificant across each specification. However, there are reasons to be hesitant that the absence of evidence in this table is evidence of absence.

Most notably, one concern about causally interpreting the models for Table 4 is the issue of statistical power. Though we estimate a treatment effect of over one hundred percent for adoptions into same-sex households, adopted and fostered children make up only three percent of children within households during our sample periods. Assuming that the regression output from model 4 (2) of Table 4 reflects the true effects of SSM reveals that the effects on adoptions would have to be 3

Table 4 — The Effect of Same-Sex Marriage on Household Composition

Dependent Variables:	Number of Children		Any Children	
Model:	(1)	(2)	(3)	(4)
Variables				
Treatment (SSM)	0.00		0.01	
	(0.02)		(0.01)	
Treatment (SSM) × Same-Sex Household		0.00		0.01
		(0.03)		(0.01)
Mean of Dep. Var.:	0.66	0.41	0.35	0.23
% Change rel. to. Dep. Var:	0.00	0.01	0.02	0.04
Fixed-effects				
Stack by County	✓		✓	
Stack by Year	✓		✓	
Stack by County by Year		✓		✓
Stack by Household Type by Year		✓		✓
Stack by County by Household Type		✓		✓
Fit statistics				
Observations	32,388	32,388	32,388	32,388

*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

The coefficient for the treatment variable is the estimated average treatment effect of same-sex marriage legalization. Models 1 and 2 analyze the number of children within the household. Model 3 and 4 analyze changes in the probability that there are any children within the household. Models 1 and 3 use a traditional two-way fixed effects approach with stack-by-county and stack-by-year fixed effects. Models 2 and 4 include county-by-year, household-by-year and county-by-household fixed effects. Each model's standard errors are clustered at the stack-by-state-level.

(25) times larger in order to produce statistically significant results.

Last, we examine relative differences in the presence of children within the household between male and female same-sex households in Table 5. In order to perform this analysis, we further stratify the panel dataset of same-sex couples used in Tables 3 and 5 by the sex composition of the heads of the household – omitting different-sex households so that they do not act as a control group. The results from this table reveal that the effect of SSM on male same-sex households was larger than it was for female ones but that there were no discernible differences in either the number or presence of children once again.

7 — Discussion and Conclusion

We provide evidence in this paper of an important indirect effect of policies that relax the legal, economic, and social constraints on sexual minority individuals. We hypothesize that reductions in the legal uncertainty of receiving joint parental rights increased the demand for children in the household. Given same-sex couples' biological constraints for reproducing, one potential method for satisfying this demand is through the adoption process. We find evidence supporting this hypothesis via analyses that employ foster care case files and demographic census data. Estimates from difference-in-differences models suggest that these laws increased the number of adoptions significantly and that same-sex households drove this increase. This increase was driven in part by the adoptions of children from the foster care system – resulting in 15,700-16,700 more exits from the foster care system in four years than would have occurred otherwise.

Interpreting the magnitude of these findings from a public finance perspective can be illuminating. The highly inelastic demand for fostered children often makes subsidizing their adoptions extremely expensive. For instance, Hansen (2007) finds that the elasticity of adoptions is 0.16. This estimate implies that adoptions would

Table 5 — Differential Effects of Same-Sex Marriage among Same-Sex Households

Dependent Variables:	Adoptions	Number of Children	Any Children
Model:	(1)	(2)	(3)
Variables			
Treatment (SSM) × Male-Male Household	0.01***	-0.01	0.00
	(0.00)	(0.01)	(0.00)
Fixed-effects			
Stack by County by Year	✓	✓	✓
Stack by Household Type by Year	✓	✓	✓
Stack by County by Household Type	✓	✓	✓
Effect Size			
Mean of Dep. Var.:	0.02	0.84	0.5
% Change rel. to. Dep. Var:	0.42	-0.01	-0.01
Fit statistics			
Observations	32,388	32,388	32,388

*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

The household type variable as described in Equation 3 is broken down into three categories for this exercise: male-male, female-female and male-female households. We exclude different-sex households to test for the difference in average treatment effects of same-sex marriage legalization between male and female same-sex households. Each model measures a different dependent variable: adoptions, the number of children and the presence of any children. Each model includes county-by-year, household-by-year and county-by-household fixed effects. Standard errors are clustered at the stack-by-state-level.

increase by approximately 1.6% for every 10% increase in the subsidy amount awarded to prospective parents. It then follows that, considering our findings that SSM increased the adoptions of children from foster care by 10-18%, subsidies to adoptive families would have had to increase by 62.5-112.5% ($444~$800 per month) in order to lead to a comparable increase in the number of adoptions. Given that the average age at adoption within our sample is 8.3 years old and that the length of these subsidies is likely to persist until the child reaches the age of 18, our conservative back-of-the-envelope calculation estimates that it would have cost federal, state, and local governments approximately $365-$863 million in cumulative subsidies to adoptive parents to generate as comparably large of an increase in the number of adoptions as did SSM.

Our secondary analyses speak to the role of reductions in uncertainty in driving these increases in adoptions. On the demand side, the larger overall changes that we document when employing census data speak to the possibility that same-sex households have responded to SSM by adopting through channels other than domestic foster care agencies, which have historically discriminated against them. This effect is particularly pronounced for male same-sex couples who have faced greater amounts of rejection from social workers based on sexual orientation and legal uncertainty over the awarding of joint parental rights (Ryan, 2000; Wald, 2006; Mackenzie-Liu et al., 2021). On the supply side, we provide evidence of important substitutions in the case manager's reports of children's goals in the foster care system from uncertainty to adoption – presumably in response to visible increases in the number of parents willing to adopt.

These findings both complement and are informed by two existing literature. The most obvious of these is research focused on understanding the impacts of SSM. For instance, for there to be detectable, persistent changes in the local demand for children within the same-sex households, both existing couples must adopt and new couples must meet and form. A large and growing literature studying these laws shows they increased marriage take-up, monogamous sexual behavior and the stability of existing partnerships (Dee, 2008; Chen and van Ours, 2020; Carpenter, 2020; Nikolaou, 2022a). Marcén and Morales (2022) demonstrates that states that legalized same-sex marriage saw significant, transitory increases in the migratory flow of gay men. Miller and Park (2018) and Eilam and Shahid (2023) show that both the legalization of same-sex marriage and the Supreme Court's ruling in *Obergefell v. Hodges* significantly increased the demand for mortgage credit among same-sex couples via reductions in the uncertainty of the legal recognition of their relationship status. This recurring point of uncertainty also speaks to early work on the economics of the household. Early work in this area focused on uncertainty with respect to child-specific characteristics and outcomes to sex preferences, length of time to conception, and the likelihood of mortality (Schultz, 1969; Becker, 1992) while later work focused more broadly on the risk associated with financial well-being (Santos and Weiss, 2016; Sommer, 2016). To our knowledge, our paper is the first analyzing the role of policy-specific induction of uncertainty on outcomes.

However, our results come with some important caveats. First, the results regarding changes in adoptive household type in the AFCARS data are inconclusive. Although we document significant reductions in the proportion of unmarried adopting families (as one would expect of adopting same-sex couples following SSM), we find no evidence of substitution to other family types.[13] Second, we are wary of the small samples we use to construct the panel dataset from the ACS. Although same-sex couples grow in representation over time, these households make up fewer than 2% of our sample's population each year. Further stratifying these populations by county (and later by sex composition) only increases the likelihood that our results are driven by statistical noise. Thus, we stress caution in definitively interpreting the

13. We believe that a potential explanation for this inconsistency could be due to changes in the recording of parental information over time arising from the structure of the data that only allows for one "mother" and "father" category regardless of the sex composition of the household. However, we have no means of testing this hypothesis.

results from the ACS data, as the purpose of their inclusion is primarily to replicate what we find in the AFCARS data. For instance, we find inconclusive evidence as to whether these adoptions occurred at the extensive margin (i.e., starting new families) or the intensive margin (i.e., expanding families) for same-sex households. However, this could plausibly reflect an issue of statistical power in this setting rather than serve as evidence of an actual null effect.

The results from this paper provide valuable lessons for foster care policy today. Suppose the growing number of same-sex families persists due to either increasing social acceptance or greater certainty of the recognition of their legal rights. In that case, evidence from this paper suggests that it is reasonable to assume that adoptions will correspondingly increase. However, the connection between same-sex families and adoption is likely to considerably weaken as technological advancements in the ability for same-sex couples to reproduce will increase the relative price of adoption. Thus, explicitly discriminatory policies, such as the wave of religious exception laws passed following SSM in order to further extend foster care agencies' right to disqualify parents due to their sexual orientation, are likely to impose greater social burdens today than tomorrow.

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Appendix

A1 — Additional Tables (Descriptive)

We construct a county-by-household type panel dataset of the proportion of same-sex couples per year from 2005 to 2019. Household types represent either different or same-sex households. We report a series of difference-in-means tests across four household variables of interest: household size, the number of children, the number of adopted children, and the presence of any children. The results confirm the priors that, on average, different-sex households are significantly larger and that this difference can be described primarily by the number of children. However, as seen in Figure 1, we find evidence that same-sex couples are twice as likely than different-sex households to have adopted children. The data begins in 2005 because the former is the first year for which county information is available. Beginning in 2005 rather than 200 means that only post-treatment effects for Massachusetts can be calculated within the total sample.

Table A1 — Summary Statistics (ACS)

	Different-Sex Households	Same-Sex Households	Difference in Means
Household Size	2.991	2.553	-0.438***
Number of Children	0.842	0.35	-0.493***
Number of Adopted Children	0.022	0.045	0.023***
Any Children	0.447	0.198	-0.249***

Notes. * p<0.1; ** p<0.05; *** p<0.01
Each row represents an average value from the corresponding household variables. These are calculated from a county-level, yearly dataset spanning 2000-2019. The third column represents the difference in these averages. The stars in this column represent the statistical difference as calculated by a two-sample t-test.

There are two common objections to using the ACS to identify same-sex couples within our sample period. First, the ACS changed its methodology for identifying same-sex couples in 2008. This change led to a sharp reduction in the share of reported same-sex households. See Figure A4 for visualization of this event, demonstrating that the share of same-sex households fell by 20% (0.3 percentage points) from 2007 to 2008. Additionally, 2008 represented the first year adopted children could be identified with the survey. See Figure A5 for visualization of this event, which demonstrates that the average number of adopted or foster children within the household rose 90% (0.027 percentage points) from 2007 to 2008. Prior to 2008, only foster children could be identified within the household.

The second concern often raised is that the ACS can only infer sexual minority individuals by the sex of their cohabiting partner. While this undoubtedly leads to dramatic under-representations of the broader LGBT community, cohabiting same-sex couples who respond to census surveys represent the most likely group to adopt in response to same-sex marriage legalization. Thus, the commonly understood bias from the ACS surprisingly helps rather than harms us in this study.

Given these data concerns, we test for the robustness of the results from Table 3 by dropping all observations before 2008. These results can be seen in Table A3. The only noteworthy change between these tables is that the effect sizes decrease marginally.

Table A2 — Summary Statistics, cont.

Statistic	N	Mean	St. Dev.	Min	Max
Race / Ethnicity					
White	2,898	0.414	0.215	0	0.967
Black	2,898	0.280	0.231	0	0.954
Native American	2,898	0.026	0.074	0	0.627
Asian	2,898	0.013	0.042	0	0.602
Biracial	2,898	0.059	0.067	0	0.574
Hispanic	2,898	0.185	0.182	0	0.992
Unknown Race	2,898	0.023	0.050	0	0.932
Reasons for Removal					
Abused	2,898	0.879	0.136	0	1
Abandoned	2,898	0.295	0.149	0	0.930
Unknown Removal	2,898	0.026	0.113	0	1
Current Setting					
Pre-Adoption	2,898	0.079	0.052	0	0.395
Relatives	2,898	0.242	0.113	0	0.656
Foster Home	2,898	0.396	0.107	0.032	0.906
Group Home	2,898	0.063	0.057	0	0.422
Institution	2,898	0.085	0.072	0	0.668
Supervised Independent	2,898	0.015	0.023	0	0.218
Runaway	2,898	0.017	0.017	0	0.126
Trial Home	2,898	0.092	0.094	0	0.465
Unknown Setting	2,898	0.006	0.031	0	0.603
Caretaker Characteristics					
Parents Age	2,069	47.258	1.675	40.123	53.725
Parents White	2,191	0.453	0.239	0.001	0.983
Parents Wlack	2,194	0.231	0.208	0	0.951
Parents Native	2,212	0.012	0.032	0	0.327
Parents Asian	2,193	0.015	0.072	0	0.696
Parents Hispanic	2,462	0.084	0.113	0	0.733
Parents Biracial	2,895	0.031	0.025	0	0.270
Parents Unknown	2,898	0.400	0.174	0.014	0.997

Table A2 provides information on the current setting of the children in the foster care system. These are mutually exclusive categories where *Pre-Adoption* corresponds to a home where the family intends to adopt the child. *Relatives* and *Foster*

Home are the most commonly occurring variables – representing almost two-thirds of the current settings. These are homes for the child where the only differentiation is the caretaker's pre-adoptive relationship with the child. Importantly for our identification, *Relatives* could correspond to either adoption (permanent; exits from the system) or foster care (temporary; remaining within the system). Both *Group Home* and *Institution* correspond to homes that provide 24-hour care for children in group-living experiences. The distinction is in their scale. Group homes typically have seven to twelve children whereas institutions are typically larger. Examples of institutions include child care institutions, residential treatment facilities and maternity homes. Similarly, *Supervised Independent* provides 24-hour support for children in foster care, but are often intended for older children – providing them with opportunities for increased responsibilities for self-care. *Trial Home* corresponds to settings where the child requires only a temporary stay with the explicit goal of returning to their primary caretaker. Approximately 2% of children's current setting is unknown as they have run away or there was a failure in data entry.

Reasons for Removal include one of three categories regarding the reasons for the child's removal from the home. *Abused* is set equal to zero but receives a value of one if there is evidence of physical, sexual, alcohol, or drug abuse. *Abandoned* similarly receives a value of one if there was evidence of child neglect or abandonment, legal relinquishment, or parental incarceration or death. These categories are not mutually exclusive. Any observation that does not receive a value for one of these two categories receives a value of one for the variable *Unknown Removal*.

Caretaker Characteristics in Table A2 reports the age and race of the caretaker(s). We choose to take the mean of the outcomes across each of the potential two caretakers. Caretakers from different racial and ethnic groups are coded as biracial couples. We also match the racial and ethnic information of the caretakers to the children – finding a strong relationship.

Last, Table A2 details information on both the types of subsidies received by caretakers and the average monthly amount (in USD) of said subsidies. We replace subsidy amount values that are more than three standard deviations above the mean with NA values to minimize the skew in the distribution of the data. *IV-EF* and *IV-EA* are binary indicators representing whether Title IV-E foster care maintenance payments are being paid on behalf of the child; the only difference is that the latter is for children who are in pre-adoptive homes. *IV-A AFDC*, *XIX* and *XVI* are each binary variables that correspond to different aspects of funding which comes from the Social Security Act of 1935 – Aid to Families with Dependent Children (AFDC) and Medicaid, Supplemental Security Income for the Aged, Blind and Disabled. *IV-D* indicates instances where child support funds are being paid to the state agency from the receiving parent(s).

A2 — Additional Tables (Results)

Table A3 — The Effect of Same-Sex Marriage on Adoptions (ACS; t $\geq$ 2008)

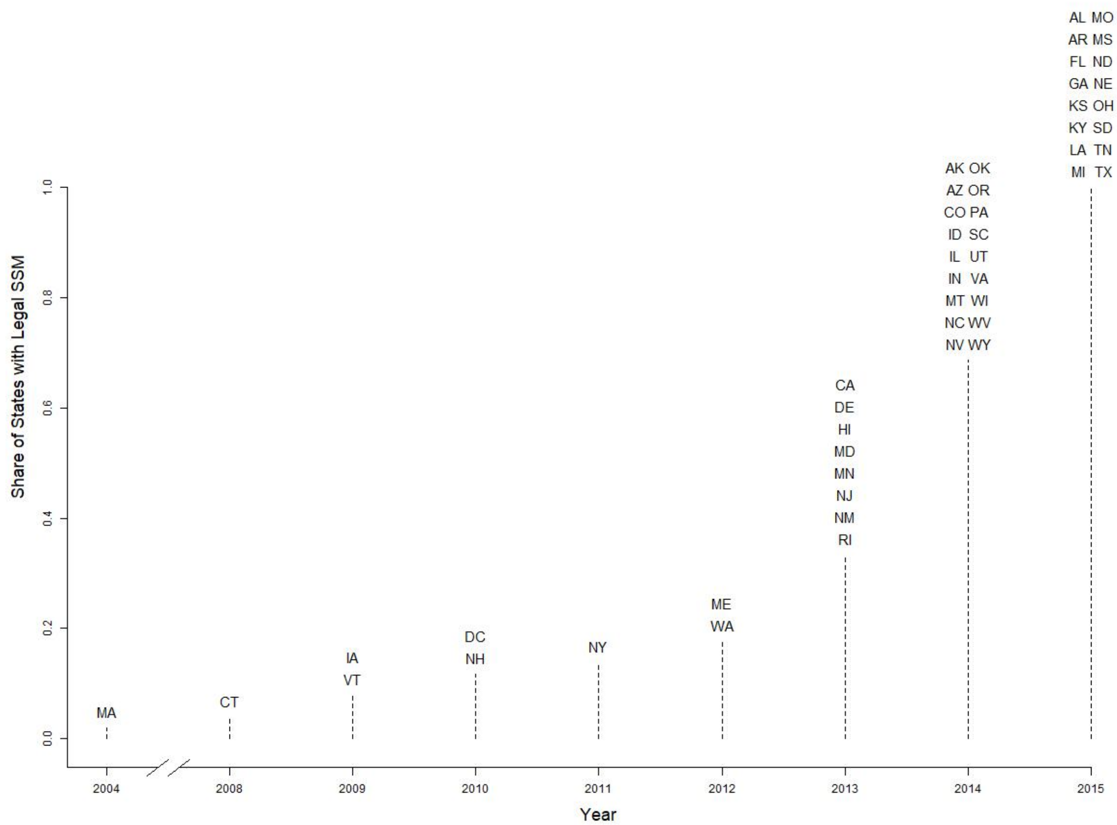
Dependent Variable:	Adoptions			
	Full	SSHs	Full	
Model:	(1)	(2)	(3)	(4)
Variables				
Treatment (SSM)	0.01**	0.03**	-0.01	
	(0.01)	(0.01)	(0.00)	
Same-Sex Household			0.01***	
			(0.00)	
Treatment (SSM) $\times$ Same-Sex Household			0.04***	0.03***
			(0.01)	(0.01)
Mean of Dep. Var.:	0.04	0.04	0.04	0.04
% Change rel. to. Dep. Var:	0.42	0.71	-	0.74
Fixed-effects				
Stack by County	✓	✓	✓	
Stack by Year	✓	✓	✓	
Stack by County by Year				✓
Stack by Household Type by Year				✓
Stack by County by Household Type				✓
Fit statistics				
Observations	22,040	11,020	22,040	22,040

*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

This table mirrors Table 3, but differs in that the beginning of the sample (t $\geq$ 2008) is omitted in order to avoid differences in reporting adoptions in the ACS as shown in figure A5. The coefficient for the treatment variable is the estimated average treatment effect of same-sex marriage legalization. Model 2 uses a data set only including same-sex coupled households. Model 3 interacts this treatment variable with an indicator representing whether the household is a same-sex couple. Model 4 is a fully specified model with county-by-year, household-by-year and county-by-household fixed effects. Each model's standard errors are clustered at the stack-by-state-level.

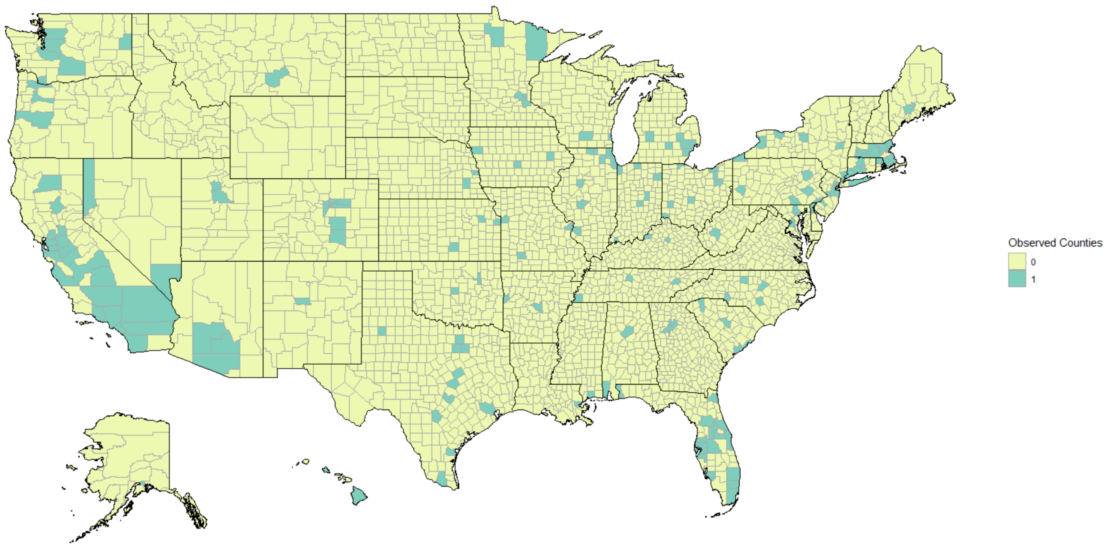
A3 — Additional Figures (Descriptive)

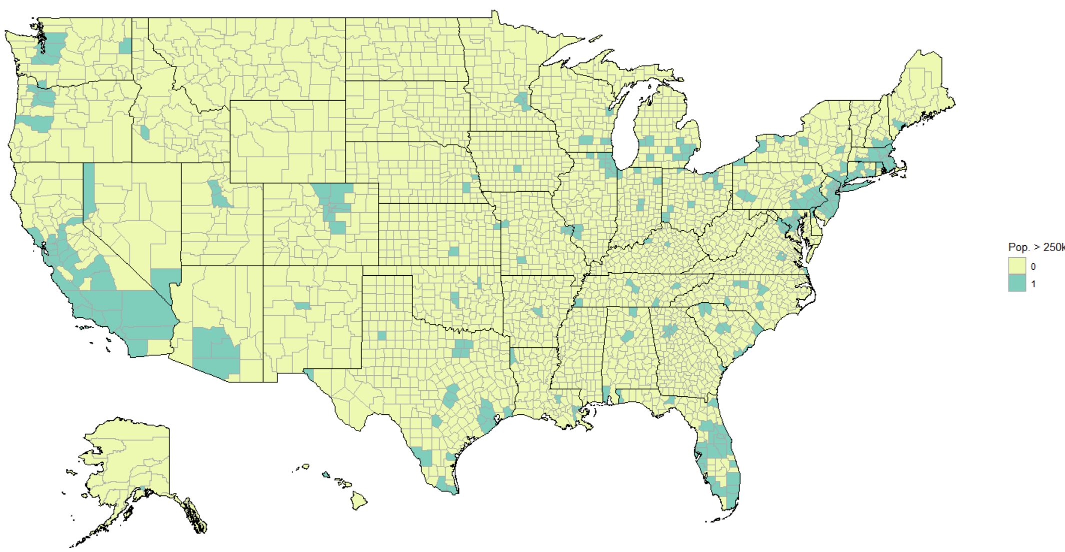
Figure A1 — Timeline of Same-Sex Marriage Legalization for Treated States



Each vertical line corresponds to the timing of when a state *effectively* legalized SSM. The length of each line corresponds to the share of states within the U.S. that had legalized SSM at that time. Note that these timings are recorded to include only the final instance of change in SSM legality. For example, we record California as first receiving treatment in 2013 (upon the overturning of a constitutional ban) rather than in 2008 (when a ban on SSM was first overturned only to be reinstated shortly after that). We chose to focus on the timing of the final instance of SSM legality rather than the first, as the passage of the former law likely conveyed more certainty in its awarding of legal rights than the former.

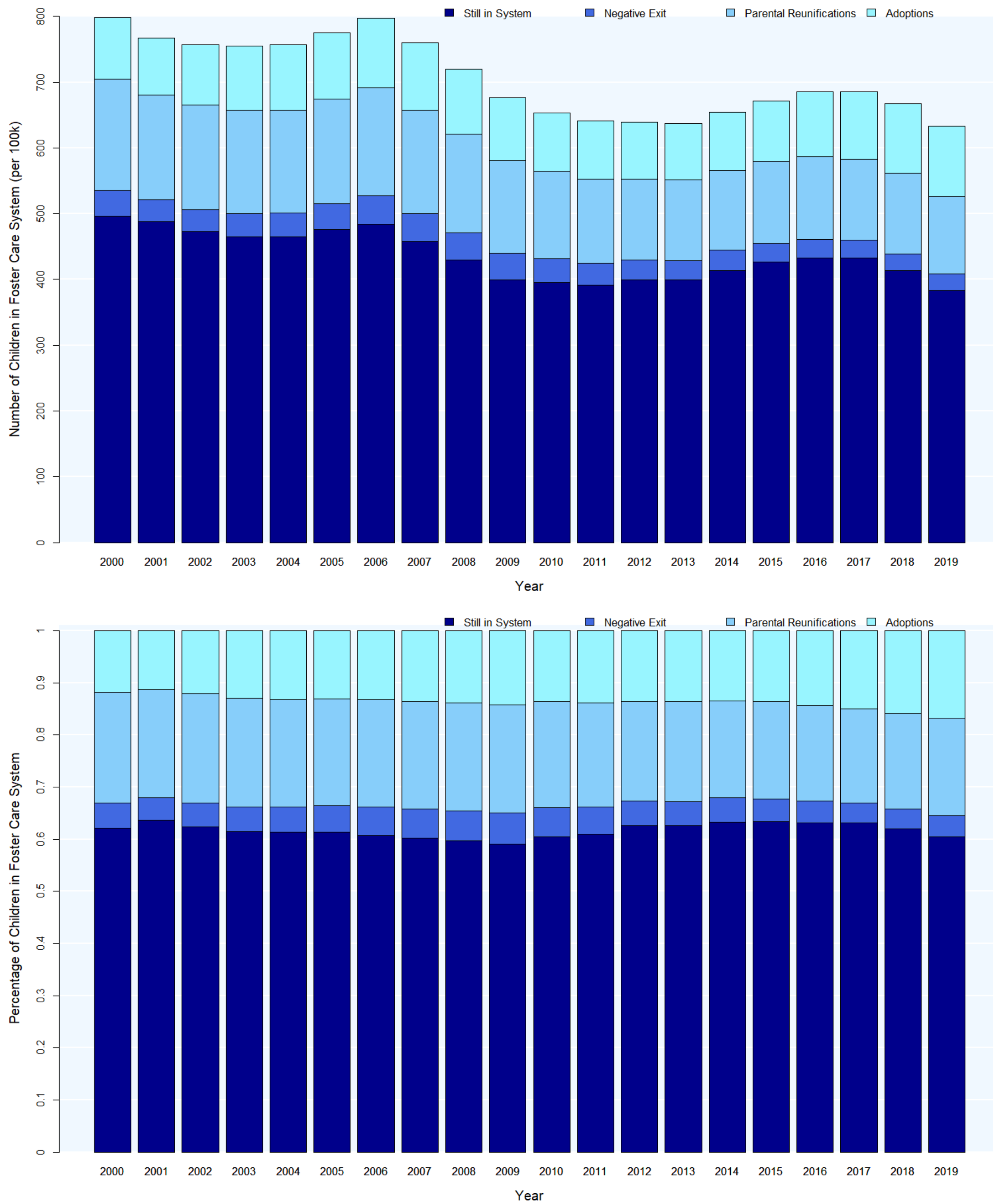
Figure A2 — Observed Counties in AFCARS Data vs. Population ≥ 250k





The top figure represents the counties that are observed within the AFCARS data. To preserve anonymity, AFCARS assigns observations as residing within a "rural" county of their state of residence only if there are not sufficiently high enough children within the foster care system that year. Contrast this map with the figure below showing counties with populations above two hundred and fifty thousand individuals. The striking similarities between the two figures suggest that there is neither systematic geographic missingness within the data nor that the identified counties reflect variation in child poverty or abuse.

Figure A3 — Movement of Children Into & Out of the Foster Care System



The following two graphs show the outcomes of children from our sample over time. The top graph displays this figure as an overall amount, whereas the bottom graph displays the share. These outcomes are grouped into four categories: still in the system, negative exits, parental reunification, and adoptions. "Still in System" corresponds to the number of children who have not yet exited the foster care system. "Parental Reunifications" correspond to returning to live with their principal caretaker(s). "Negative Exit" corresponds to children who exit the system via aging out, transferring to another agency, running away, or dying. "Adoptions" correspond to any exit from the foster care system resulting from legal adoption, guardianship, or permanent residence with another relative.

The top graph shows that the number of children interacting with the foster care system declined from eight hundred thousand in 2006 to approximately seven hundred thousand in 2019. The bottom figure reveals that the the proportion of these categories has remained relatively constant over time except for adoptions, which have increased from approximately eleven to fifteen percent of outcomes of the foster care population over our sample period.

Figure A4 — Share of Same-Sex Couples Identified in the ACS per Year

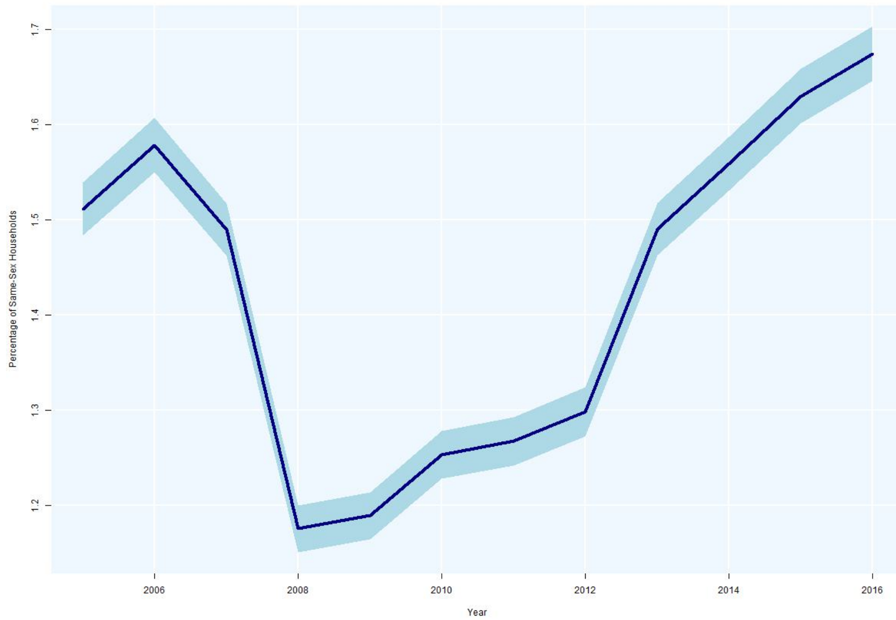
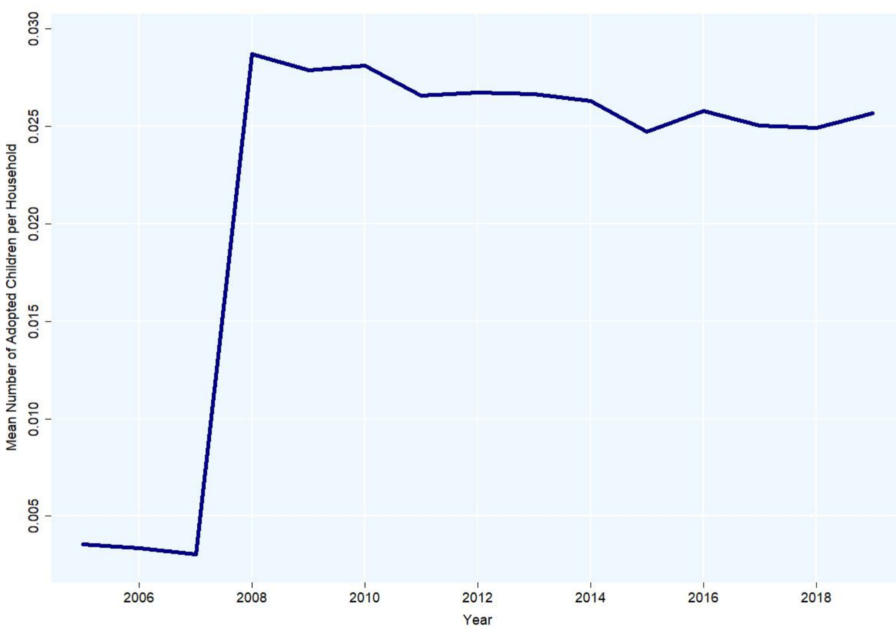


Figure A5 — Mean Adopted or Fostered Children per Household per Year (ACS)



A4 — Additional Figures (Results)

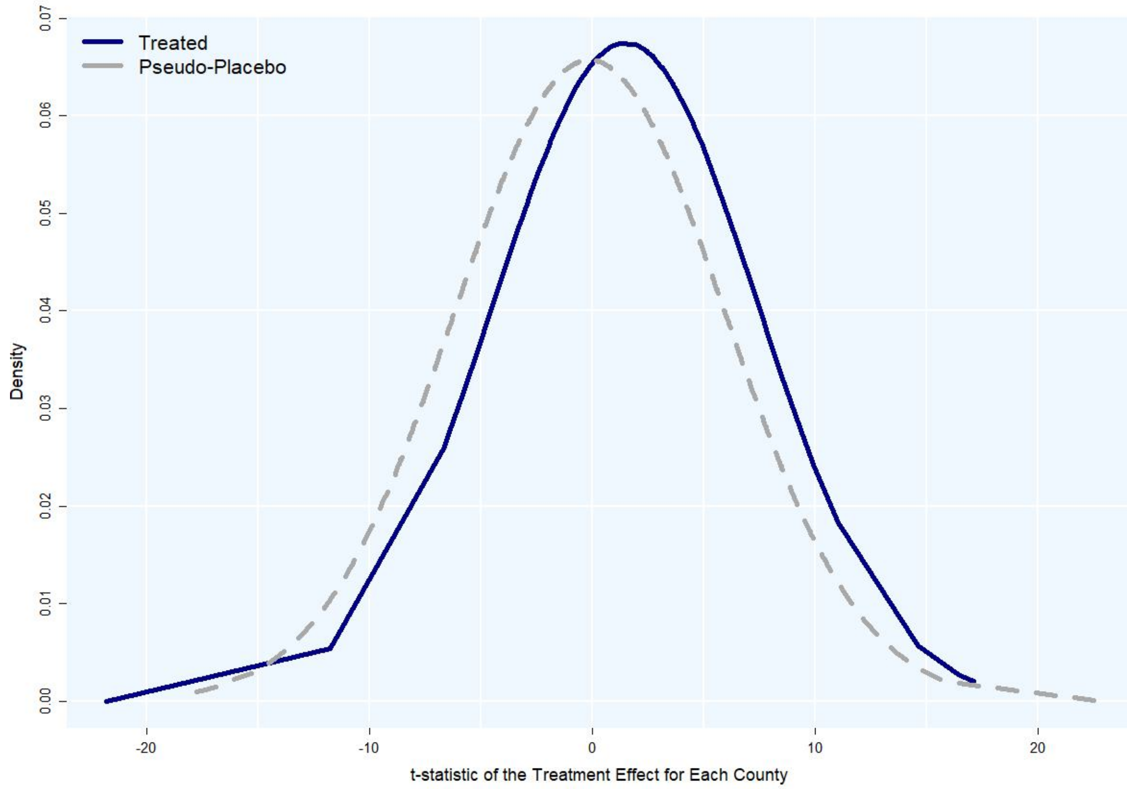
One potential concern is that statistical outliers may drive any detected effects. One context-specific justification for the importance of this analysis is due to (in)famous examples of unclear SSM legalization timings, such as California, where its legality was publicly contested within the legislature and judicial systems from 2008-2013. This uncertainty warrants an investigation into the sensitivity of the inclusion or exclusion of certain counties and states. We test for this possibility with Equation 4, which calculates each county's treatment effect individually rather than as one average. Analyzing the distribution of these effect sizes will speak to the role of outliers' impact in the analysis.

$$Adoptions_{sct} = \alpha_{sc} + \delta_{st} + \beta_c T_{sct} + \omega_n Z_{sct} + \epsilon_{sct} \tag{4}$$

We plot the distribution of treatment effects in Figure A6. However, rather than plot the distributions of coefficients (β_c) representing each county's treatment effect, we present the distributions of the t-statistics of these regressions. We believe the latter specification is preferable for two reasons. First, county-level differences in the magnitude of these coefficients (as captured by β_c) more likely reflect differences in total foster care populations than it does percentage changes in these populations. Second, coefficients alone cannot tell us about the degree of confidence that we can have in each estimate. t-statistics combine valuable information about the magnitude of the effect and the degree to which treated counties diverge from the control group.

The density of the t-statistics of β_c from equation 4 are displayed as a solid blue line in Figure A6. When contrasted with the dashed grey line of the pseudo-placebo (a distribution with the same variance but a mean equal to zero), treated counties display a clear rightward shift relative to expectation. This shift shows that our documented increase in the number of adoptions caused by SSM is not due to outliers

Figure A6 — Distribution of County Treatment Effects



driving the result). Removing complicated instances of the statewide legal timing of SSM, such as California, pushes our results back towards zero, but only modestly – decreasing the mean t-statistic by 0.2 (13%).

That the exclusion of California as a treated state reduces the overall effect of SSM on adoptions ought not to be surprising if we are correct in the hypothesis that same-sex households should drive effects. We further examine this relationship in Equation 5 where we take the average of our coefficients of interest (β_c) that are calculated in Equation 4. We differentiate these coefficients by their ranking in the national percentile of same-sex couples (p) in each treated county (c) at the time of legalization (E_c). We calculate these percentages by using data from the ACS. Our reasoning for using the yearly percentile (rather than the overall number) of same-sex couples is due to changing definitions of same-sex couples, as seen clearly in Figure A4. To avoid bias resulting from this change in methodology and bolster the results against the sensitivity of small samples, we use a five-year rolling average of percentiles. Finally, the treatment effects we estimate (B_c) are divided by the pre-treatment mean of the outcome. This process represents the effect in terms of a percentage change which is important in this context because same-sex households are disproportionately likely to live in highly urbanized areas.

$$\lambda_p = \frac{1}{n_p} \sum_{c=1}^{n_p} \beta_{ic} \tag{5}$$

where $p = Pctl(SSCs)_{E_c}$

Figure A7 plots out the mean of the treatment effects for counties with above and below average amounts of same-sex couples that the time of SSM legalization. The figure demonstrates that the effect is 30% percentage points larger in areas with more same-sex couples.

Figure A7 — Density of Same-Sex Couples & Changes in Adoptions after SSM

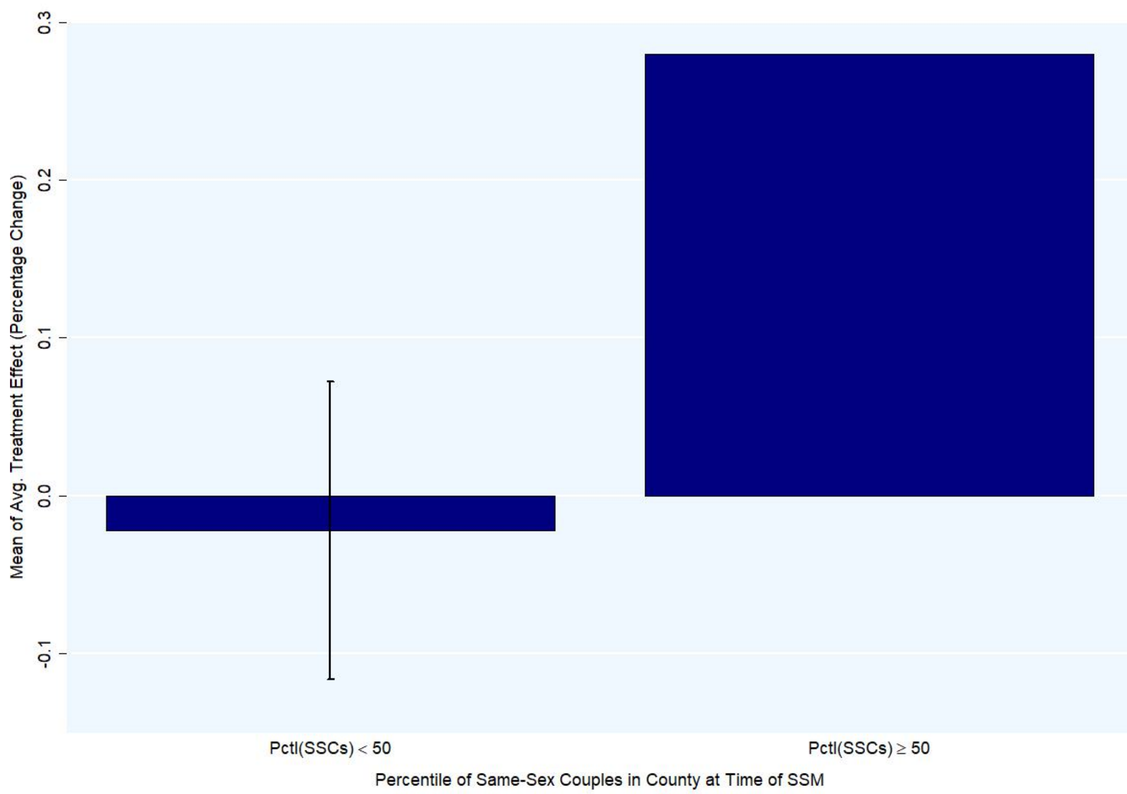


Figure A8 — Event Study of the Effect of Same-Sex Marriage on Adoptions (ACS)

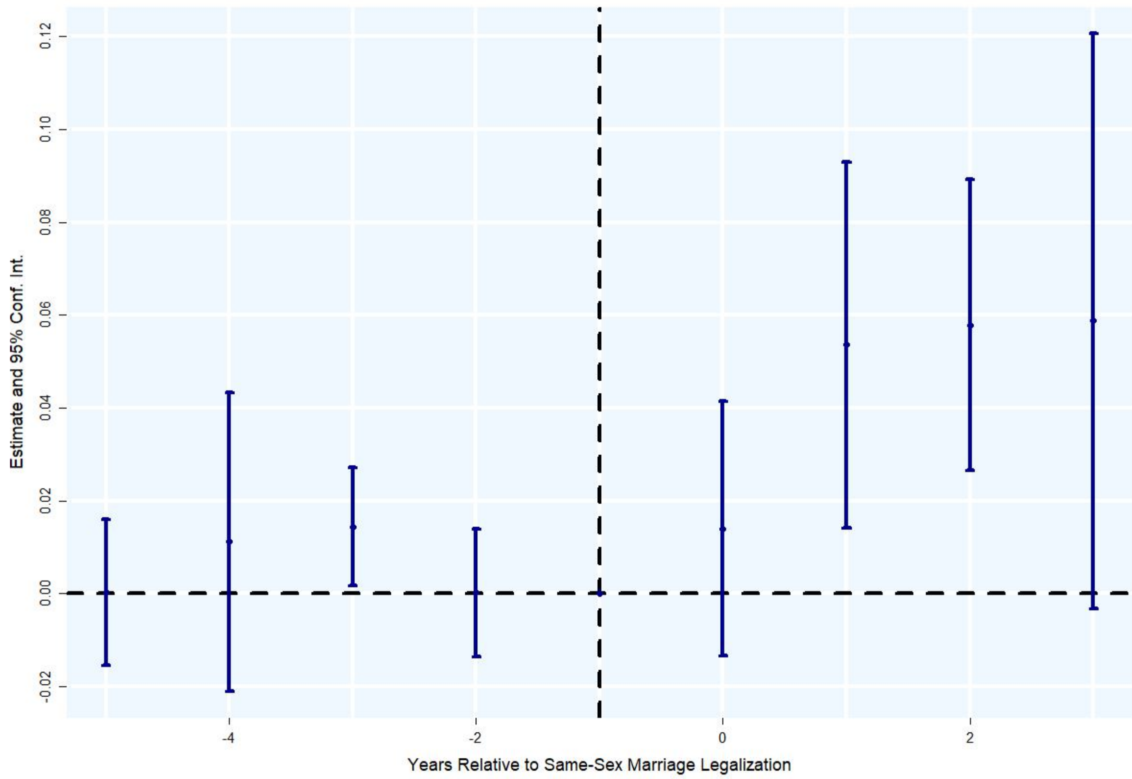


Figure A8 shows the event study analysis of model 4 in Table 3.