The Effect of Same-Sex Marriage Legalization on Adoptions and Family Formation in the U.S.

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In this paper we explore the role that same-sex marriage legalization had on the number of adoptions of children from foster care in the United States. We do so by employing a synthetic difference-in-differences estimator which leverages both the differential timing of these laws across states and the subsequent wave of state-level legal protections which give foster-care agencies the right to deny service to same-sex couples based on religiously-held beliefs. Using highly detailed, county-level data of nearly 20 million children in the foster care system from 1995-2020, our findings reveal that same-sex marriage legalization led to a 3.8%-5.9% increase in the annual number of adoptions. We show that this is driven by an asymmetric substitution in the composition of adoptive household types away from unmarried couples and single women and towards recognized families. Back-of-the-envelope calculations suggest that 1) same-sex marriage indirectly caused an additional 10,000-17,500 additional adoptions within an average four-year time window after its passage and 2) that there would have been 2,900-4,500 more adoptions from 2016 to 2019 without laws which grant foster-care agencies the right to refuse service to same-sex couples.

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1 — Introduction

There exists a persistent shortage of individuals who are willing or able to either adopt or foster children. Within the United States in particular, over 5% of all children interact with the foster care system at least once – well-over 100,000 of whom annually report neither the ability nor goal of reunification with their biological parent(s) (Wildeman and Emanuel, 2014; Children's Bureau, 2019). These children are disproportionately likely to be racial or sexual minorities (Putnam-Hornstein et al., 2013; Yi et al., 2020; Grooms, 2020) and are orders of magnitude more likely to have health complications such as worse mental and physical health, physical disabilities, visual or hearing impairments, or severe emotional or interpersonal difficulties (Zito et al., 2001; Wildeman et al., 2014; Turney and Wildeman, 2016).

Recent estimates reveal that each year more than 17,000 of these children age out of the system before matching with long-term foster or adoptive parents (Child Welfare Information Gateway, 2020).[1] The consequences of children who age out of the system are stark and well documented. These include increased probabilities of homelessness (Dworsky et al., 2013), unemployment (Macomber, 2008), unplanned pregnancy (Dworsky and Courtney, 2010) and reduced educational attainment (Valset, 2018).

In this paper we analyze policy changes which helped to ameliorate these negative outcomes. By analyzing states which extended the legal right to marriage for same-sex couples, we provide evidence that these laws significantly increased the number of adoptions relative to states without gay marriage protections. We document these findings by analyzing approximately 20 million case-specific observations from 1995 to 2020 which cover all children in foster care systems which receive funding from the federal government in the United States. These data contain

1. "Aging out" in this context refers to the process through which children in foster care system transition towards living independently once reaching the age of 18 and lose eligibility for title IV-E foster care payments.

detailed personal and demographic information of both the children and caretakers, goals of each foster placement, the sources and amounts of foster subsidies received, and the dates of movement into, between and out of foster care.

We employ a stacked, synthetic difference-in-differences estimation strategy which leverages the differential timing of state-level same-sex marriage laws. We add to this identification strategy by incorporating “never-treated” control units due to the fact that ten states (in response to same-sex marriage becoming legal) extended legal protections for foster care agencies to refuse service to same-sex couples based on religious beliefs.

Using these methods, we show that counties within states which legalized same-sex marriage saw significant increases in the the number of adoptions of children from foster care. Event study analyses, a placebo test with randomized treatment timing and robustness checks which reveal that these effects are largest in areas which were likely to have had less favorable policies and attitudes towards LGBT-adoptive parents suggest a causal relationship. Testing for potential mechanisms of these results, we provide evidence that these findings are driven in part by an asymmetric substitution away from single female and unmarried partner households adopting towards recognized families. We additionally demonstrate that these effects are largest in counties which had the highest shares of same-sex couples per child in foster care.

Our estimates of this effect suggest that same-sex marriage legalization increased the number of annual adoptions by 3.8%-5.9%. Within a four year time window of our analysis, this corresponds to a cumulative effect of approximately 10,000-17,5000 additional adoptions than would have occurred otherwise. Back-of-the-envelope calculations which extrapolate these estimates suggest that there would have been approximately 2,900-4,500 more adoptions from 2016 to 2019 without laws which grant foster-care agencies the right to refuse service to same-sex couples.

2 — Literature Review

2.1. Economics of Adoption & Foster Care

There are three primary focuses in the literature regarding the economics of adoption and foster care: research which focuses on the supply of children in the foster care system, the demand for adoptable children and outcomes for children who interact with the system. Bitler and Zavodny (2002) studies the supply-side showing that abortion legalization in the U.S lead 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; Grogger and Bronars, 2001) while Biehl and Hill (2018) shows that more generous federal earned income tax credits lowers rates of foster care entry via reductions in financial distress. Relatedly, Cunningham and Finlay (2013) shows that substance higher levels of abuse cause foster care caseloads to increase through greater amounts of parental physical neglect and abuse.

A considerable amount of attention has been focused on the demand side as well. 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 have also been shown to decrease the amount of time children spend in foster care while increasing rates of both adoption and fostering by lowering both the absolute and relative cost of adoption (Doyle and Peters, 2007; Argys and Duncan, 2013; Buckles, 2013; Brehm, 2021). Theoretically, these subsidies could act as a price mechanism to equalize both sides of this market. In practice, however the magnitude of these subsidies are neither cost-of-living adjusted, tied to inflation, nor adjusted frequently enough to achieve this purpose (Horwitz et al., 2014).

There have been conflicting findings regarding the effects of foster care on child welfare. Doyle Jr (2007), Doyle (2008) and Lindquist and Santavirta (2014) find a large negative effects of foster care placement including substantially lower earnings and large increases in teen motherhood, delinquency, and unemployment and adult criminality while Baron and Gross (2022) show that foster care placements increased children's safety, academic and behavioral outcomes in the short-run while reducing the probability of a wide array of later-in-life criminal outcomes in the long-run. Warburton et al. (2014) shows that placement into care delays high school graduation and increases income assistance receipts for 16-18 year old boys while Bald et al. (2022) show that removal from abusive or neglectful homes significantly increases test scores and reduces grade repetition, but that these effects are only statistically significant for girls. Gross and Baron (2022) provide evidence that many of these observed improvements emerged were reunified with their parents which may resolve these seemingly contradictory findings.

2.2. Discrimination against LGBT Individuals in the Foster Care System

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 both different legal, social and biological constraints than heterosexual individuals, a new literature has emerged which studies how these restrictions deferentially impact bi- and homosexual individuals' family outcomes and their corresponding secondary, spillover effects.

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, of a sample couples raising children in the United States, the children of same-sex couples are more than seven times as likely to be adopted than those of different-sex couples. Black et al. (2007) provide suggestive evidence that lesbian couples are more likely to adopt children than gay couples. Aldén et al. (2015) suggests that this could be driven, in part, by the fact that female same-sex partners are more likely to take up marriage than gay men following expansions of legal rights.

There is evidence to suspect that the gap in the probability of adopting or fostering a child would be greater in the absence of discrimination against same-sex couples. While openly LGBT individuals and unmarried partners within every state from 2000-2015 (with the exception of Florida) were legally eligible to adopt or foster children, both the applications of "best interest of the child" criteria which were openly hostile towards sexual minorities and explicit judicial precedent which barred unmarried same-sex couples from receiving joint-custody of adoptive children effectively made either adopting or fostering children illegal for LGBT individuals in a majority of states (National Center for Lesbian Rights, 2014) Mackenzie-Liu et al. (2021) preform 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.[2]

2.3. *Same-Sex Marriage, Family Formation & the Demand for Children*

Differences in the probability of raising children in the household between married and non-married couples and are apparent as can be seen in Figure 1. The top portion of this figure shows that, across all coupled household types, married couples are significantly more likely to be raising children. The bottom portion 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

2. 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 in order to demonstrate that this effect is significantly different from zero.

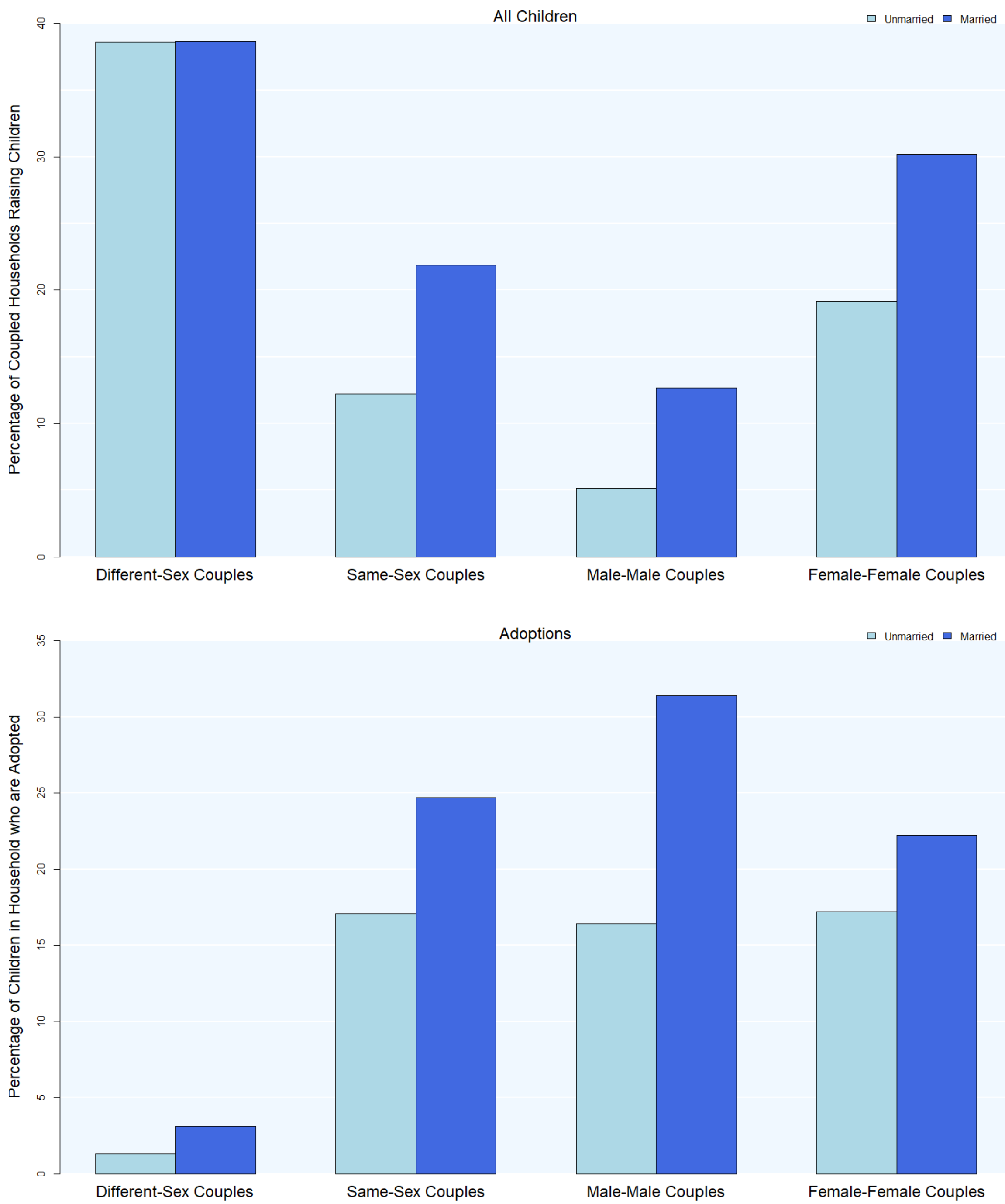
household. However, the causes of these differences have been the subject of much research and debate.

Though there have been large reductions in the biological constraints that same-sex couples face in reproduction via technological advancements in donor insemination, reciprocal IVF, surrogacy, etc., these options remain either prohibitively expensive or generally undesirable for many same-sex couples. Thus, this section looks at potential alternative pathways through which there same-sex marriage legalization could causally affect the number of adoptions via increases in family formation.

One potential starting point for considering the many factors which could influence this relationship is differences in the populations of lesbian, gay and bisexual individuals. In order for there to be detectable changes in the local demand for adoptable children, there must likely first be a robust dating market for LGB individuals to meet. Marcén and Morales (2022) analyze the impact of marriage legalization on the migratory behavior of gay and lesbian individuals – finding that states which legalized same-sex marriage saw significant increases the migratory flow of gay men, but that these effects were transitory. While important, migration is unlikely to determine the entirety of the variation given the economic barriers to migration – particularly for the disproportionately high costs of living in cities which contain larger numbers LGB individuals.

Additionally, marriage legalization could lead to longer and more stable relationships which would increase the demand for children in the household. Chen and van Ours (2020) study registered partnership and marriage laws for same-sex couples in the Netherlands. They find that these couples living in areas which first introduced these marriage laws saw substantially increased couple stability by lowering separation rates. Given that there were identical benefits in terms of the rights awarded to registered and married partners, they credit this finding to the effect of marital symbolism. Further, Carpenter (2020) shows that following same-sex marriage legalization in Massachusetts, there were was significant take-up of marriage and that

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 children who are adopted. Data comes from the 2014-2016 American Community Survey as calculated and reported in (Goldberg and Conron, 2018).

this effect was largest for lesbians and households with children.

Last, as proposed by Gary Becker, the demand for children could be a normal good (Becker and Lewis, 1973). Many studies show that same-sex couples respond to the financial incentives of marriage and that it can improve their financial welfare. Friedberg and Isaac (2022) estimate a small, but significant marriage elasticity for same-sex couples which suggests that these effects will be greatest for high-earning cohabiting couples. Miller and Park (2018) show that the legalization of same-sex marriage was associated with a 6-16% increase in applications for mortgages and that this effect is stronger than were anti-discrimination policies in housing. Sansone (2019) shows that same-sex marriage legalization increased the employment of individuals in same-sex households via improvements in attitudes towards – and decreases in discrimination for – sexual minorities.[3] Aldén et al. (2015) study the effects of registered partnerships for same-sex couples in Sweden on labor earnings and fertility. They document that the reasons for take-up differed significantly by gender – with gays primarily valuing the added benefits of resource pooling and lesbians being particularly responsive adoption incentives and family formation.

3 — Data

3.1. AFCARS

We gather 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 which is both housed at – and made freely available to researchers via – Cornell University. AFCARS is a federally mandated data collection system with case-specific information on all children in foster care systems and adoption agencies which receive funding from the federal government's

3. See Badgett (2020) Ch. 3 for an excellent overview of related stigma and LGBT welfare research.

Title IV-B/E Foster Care and Child Welfare funding services.[4] This effectively requires states to record data on on all children within the state's welfare agencies (or by private, contracted agencies) from whom they have the responsibility for either placement, care, or supervision.[5]

We combine data from each of the AFACRS' Foster Care datasets from 1995 to 2020. Each of these files includes detailed information on children who had interacted with the foster care system in that period of time.[6] 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 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 the most up-to-date information regarding the date their most recent move either within or out of the foster care system and their county of residence. However, within every state, there is a population of children whose county-level location is not reported for privacy concerns.[7] This is determined by children residing in counties with fewer than 1,000 annual observations. We assign these observations a rural "county"-by-state identifier, allowing these observations to act as both a treatment and control unit in the analysis.[8]

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4. 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.

5. States are additionally encouraged to report other private adoptions that are finalized within the state.

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

7. These consist of approximately 35% of all observations.

8. There is a rural county indicator for each of the 50 states.

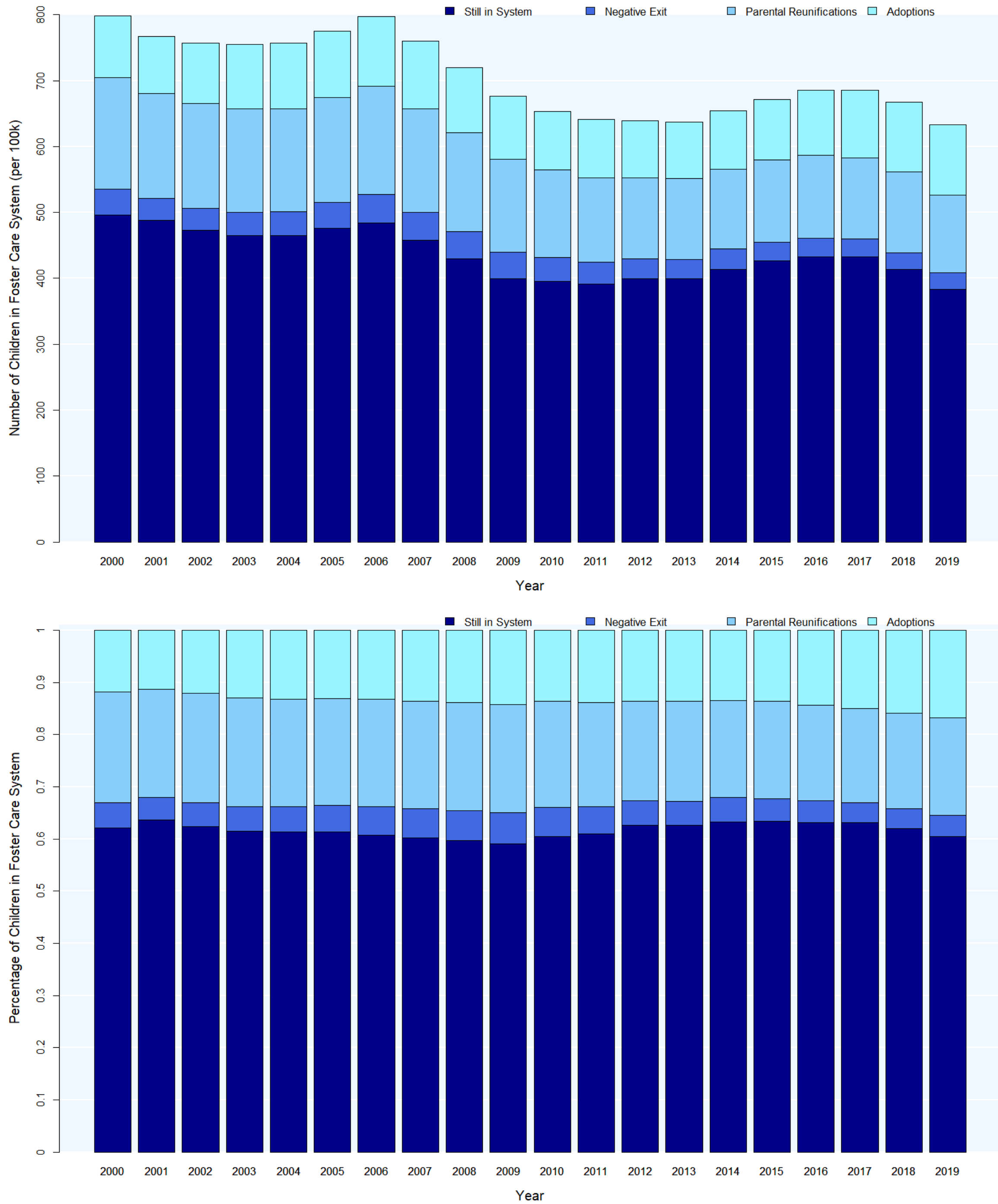
Each observation contains a tremendous amount of information regarding the timing of each move into, within and out of the foster care system. Determining which of these dates to use for the purpose of analysis is crucial for identification. We work backwards in time order to construct a date which measures the timing of when each child entered their current setting. We first begin with exit dates from the foster care system. Less than 40% of all children within the system exit it for each year in our sample.[9] For children who remain in the system, we match in the date of their most recent move into their current foster setting. We then match in the latest date of removal from their previous foster home in instances where there still remain missing values. Finally, for children who are still residing in the first foster home that they entered, we match in the date of when the child was first removed from their original home for the purpose of placement in a foster care setting.

We then restrict this sample to contain only observations where the timing of each child's movement into their current setting occurred from 2000-2019. We do this for three main 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 in data files after 1999 given increases in compliance and data reporting. Third, we wish to avoid any confounding factors resulting from the COVID-19 pandemic in 2020.

We conclude by aggregating this data to a county-by-year level. The primary reason for this is for the ease of interpreting the outcome variable. Analyses which look at changes in the total number of children who have been adopted in a county are both simpler to measure and interpret than would be changes in the probability of being adopted (in the case that the data were left at an individual level). For the purposes of establishing causality, this second alternative is particularly difficult to

9. Figure 2 shows that this percentage is relatively stable over time.

Figure 2 — Movement of Children Into & Out of the Foster Care System



"Adoptions" correspond to any exit from the foster care system resulting from legal adoption, guardianship or permanent residence with another relative. "Parental Reunifications" correspond 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 death.

use given that it is unclear how to test for parallel trends assumptions.

3.2. Summary Statistics

The results of the aggregation process can be seen in Tables 1, 2 and 3. We observe 146 counties over 20 time periods. However, we omit any county which has fewer than 30 observations of children in foster care for any year within the time window for our analysis. This is done in order to provide variables of the average characteristics of the children in the county at that period of time which are less sensitive to outliers.[10] To provide the most accurate sense of the distributions of the variables used in our analysis, we omit any observation from in Tables 1, 2 and 3 which contains fewer than 30 children in foster care for that year.[11]

The first section of Table 1, *Exits & Children in Foster Care*, reports the totals for each observation. *Foster Care* represent the number of overall observations for each child in each county per year. *Total Exits* corresponds to the number of children who are discharged from the foster care system per county per year. *Parent Reunification* correspond 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. The sum of these three variables is equal to *Total Exits*. *In System* is calculated as the difference between *Foster Care* and *Total Exits*.

We choose to leave the *Exits & Children in Foster Care* variables as totals rather than means two two reasons. First, *Adoptions* serves as our primary dependent variable. Leaving this as a continuous variable provides for easier interpretation and minimizes the concerns of extrapolation that are common with linear probability mod-

10. 30 observations is a popular rule-of-thumb in statistics for assuming that a variable contains a normal distribution.

11. We omit 22 such of these observations from these tables.

Table 1 — Summary Statistics (1 of 3)

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
Total 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
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
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
Disability	2,898	0.238	0.159	0	0.976
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
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

Table 2 — Summary Statistics (2 of 3)

Statistic	N	Mean	St. Dev.	Min	Max
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 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
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
Same Race as Child	2,374	0.542	0.147	0.052	0.983

Table 3 — Summary Statistics (3 of 3)

Statistic	N	Mean	St. Dev.	Min	Max
Subsidies					
IV-EF	2,867	0.360	0.151	0.006	0.968
IV-EA	2,820	0.026	0.041	0	0.910
IV-A AFDC	2,870	0.049	0.076	0	0.461
IV-D	2,827	0.061	0.095	0	0.809
XIX	2,886	0.659	0.319	0	1
XVI	2,886	0.056	0.043	0	0.373
No Subsidy	2,866	0.261	0.242	0	0.989
Subsidy Amount	2,798	876.924	863.001	0	12,341.470

els. Second, the remainder of these variables are important for the construction of synthetic control groups. Leaving these variables as totals allows us to capture any important differences in both total populations or degrees of urbanization across treatment and control groups beyond measures of overall (non-foster) population or binary indicators or urban/rural status.

The remaining variable categories in Table 1 provide information contain information on the characteristics of the children which were in the foster care system at the time. *Child Characteristics* provides variables on the average age, sex, race and disability status of the children in question.[12] We additionally create a variable measuring the average amount of time spent in foster care as measured by the difference between when the child first was removed from their home until when they were moved to their current setting. This ideally aids in capturing additional unobservables across the children, the institutions which are responsible for their placements, and the willingness and ability to foster or adopt among the populations of

12. *Disability* is set equal to zero, but receives a value of one a 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.

potential caretakers which are not explicitly measured.

Reasons for Removal include one of three categories regarding the reasons for the child's removal from home. *Abused* is set equal to zero, but receives a value of one if there was 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 which does not receive a value of one for one of these two categories receives a value of one for the variable *Unknown Removal*.

The *Foster Care Outcome Goal* section provides information regarding the goal of the child's stay in the foster care system as determined by the social worker and original legal guardians(s). Once within the foster care system, 51% of children have the explicit goal of being reunified with their initial parent(s). The remainder either have the explicit goal of adoption or do not yet have a stated goal.

Table 2 provides information on the current setting of the children with the foster care system. These are mutually exclusive categories where *Pre-Adoption* corresponds to a home in which 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 adoptions (permanent; exits from the system) or foster care (temporary; remaining within the system). Both *Group Home* and *Institution* correspond to homes which 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.[13] Similarly, *Supervised Independent* provides 24-hour support for children in foster care, but are often intended for children who are older – providing them with opportunities for increased responsibilities for self-care. *Trial Home* corresponds to settings where the

13. Examples of institutions include child care institutions, residential treatment facilities, maternity homes; etc.

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 either runaway or there was a failure in data entry.

Caretaker Family Structure in Table 2 is very important for our analysis. Given that we do not directly observe the gender of the adoptive parents within the data, we can infer changes in family formation based on changes in the composition of caretaker family structures. "Caretaker" in this context refers to those who are legally responsible for watching over the child in their current setting. Importantly, their current setting could either be within or out of the foster care system. The five variables in this section are mutually exclusive.

Given that every setting aside from *Pre-Adoption*, *Relatives* and *Foster Home* are unlikely able to specify a caretaker family structure, *Unknown Family* serves primarily as a "none of the above" category. However, given that a non-trivial number of observed adoptions are categorized as being completed by unknown family types, we suspect that non-traditional families fall into this category as well. One potential piece of supporting evidence for this hypothesis is that, while the AFCARS foster care files contain information of the caretakers as "Parent 1" and "Parent 2", the Adoption files include binary "Mother" and "Father" variables – regardless of the gender composition of the household.

Caretaker Characteristics in Table 2 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 a biracial couple. We also match the racial and and ethnic information of the caretakers to the children – finding a strong relationship.

Last, Table 3 details information on both the types of subsidies received by caretakers and the average monthly amount (in USD) of said subsidies. *IV-EF* and *IV-EA* are binary indicators representing whether there are Title IV-E foster care maintenance payments being paid on behalf of the child; the only difference is that the later is for children who are in pre-adoptive homes. *IV-A AFDC*, *XIX* and *XVI* are each binary variables which correspond to differing 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). Over one quarter of children in the system recieve no subsidies. The mean amount of monthly subsidies is $877 per month though there remains considerable variation.

4 — Identification

The essence of our identification strategy relies upon a difference-in-differences framework. Difference-in-differences estimators compare changes in outcomes between otherwise similar groups which differ primarily according to their as-if random assignment to an intervention. The two most common requirements for establishing causality when employing these estimators are, first, that the outcomes of these groups would continue trending parallel of one another over time without the intervention and, second, that the determinants of which groups receive the intervention are independent of any variable which could affect the outcome of interest.

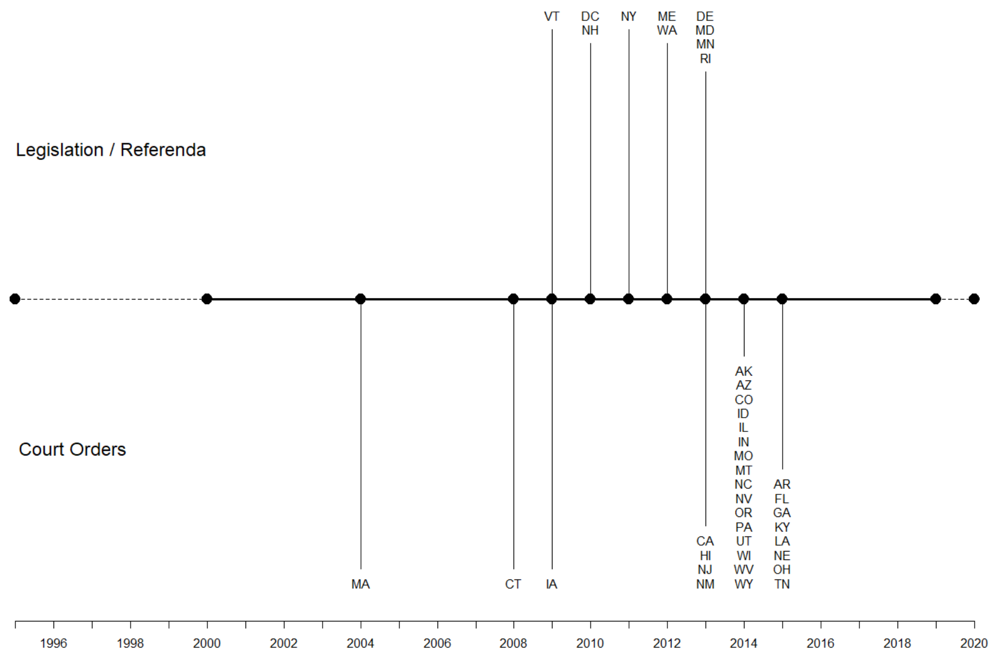
The intervention that we study in this paper is same-sex marriage laws.[14] The staggered introduction of these laws across U.S. states over time provides a unique natural experiment for estimating their causal impact on a number of outcomes. While states which house a greater number of same-sex couples such as Vermont, New York and California were more likely to have legalized same-sex marriage before states 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.[15] As such, the timing of much of

14. The timing of these laws can be seen in Figure 3.

15. Justice Robert Kennedy commented on this in his opinion in *Obergefell v. Hodges*: "The dynamic

these interventions were unlikely to have been either expected or largely demanded by the general public and, thus, should not threaten our identification.

Figure 3 — Timeline of Same-Sex Marriage Legalization for Treated States



Dashed lines correspond to years which were used to construct the data sample used for analysis. The solid, horizontal line represents the years included in the analysis. Each vertical line represents a year which same-sex marriage has been *effectively* legalized for at least one month within a state. The top portion of the graph represents those states which achieved this via legislation or popular vote. The bottom portion demonstrates that the majority of these laws came to be as as a result of court intervention. States which implemented laws which allow for foster and adoption agencies to refuse service to same-sex couples are not included.

One major assumption of this identification strategy is that these legal changes do not have unintended, offsetting behavior for different-sex couples. While we cannot directly rule out this hypothesis given the inherent limitations of our data, a number of studies suggest that this effect is unlikely to even exist. For exam-

of our constitutional system is that individuals need not await legislative action before asserting a fundamental right. [...] The idea of the Constitution 'was to withdraw certain subjects from the vicissitudes of political controversy, to place them beyond the reach of majorities and officials and to establish them as legal principles to be applied by the courts'. [...] This is why "fundamental rights may not be submitted to a vote; they depend on the outcome of no elections. [...] It is of no moment whether advocates of same-sex marriage now enjoy or lack momentum in the democratic process."

ple, Dillender (2014) studies legalization of same-sex marriage in the United States and finds no evidence that these legal protections reduce different-sex marriage rates. Trandafir (2015) uses causal inference techniques to demonstrate that there were no changes in same-sex marriage laws and family formation, divorse or extramaritial births for different-sex couples living in OECD member countries. Using representative, confidential data with a variable of self-reported sexuality from Massachusetts, Carpenter (2020) similarly finds that legal same-sex marriage led to neither significant effects in coupling nor reductions in marriage for heterosexual individuals. When analyzed in an international setting, Trandafir (2014) finds no evidence of a change in ther overall or different-sex marriage rate in the Netherlands following both their registered partership law and legal same-sex marriage.

There is also a growing body of related research which similarly leverages changes in same-sex marriage and partnership laws. These studies include a wide array of topics ranging from discrimination and employment (Sansone, 2019), household income (Delhommer and Hamermesh, 2021), LGBT hate crimes (Valencia et al., 2019; Nikolaou, 2022b), attitudes and polarization (Flores and Barclay, 2016; Aksoy et al., 2020), mental health (Chen and van Ours, 2022), health care and insurance usage (Hatzenbuehler et al., 2012; Dillender, 2015; Carpenter et al., 2021) and sexual health (Nikolaou, 2022a).[16]

While our identification strategy is quite similar to these listed papers, it differs primarily in two ways. First, the majority of U.S.-based studies analyze state-level outcomes whereas our data is available at the county-level. This presents tradeoffs in the form of increased ability to explore heterogeneous treatment effects while requiring more attention towards selecting appropriate comparison groups.

Second, our identification strategy uniquely incorporates an important consideration given our outcome variable of interest. Following the Supreme Court's ruling in *Obergefell v. Hodges* in 2015, it became illegal to prevent same-sex couples from

16. The effect of SSM laws and LGBT mental health is contested (Anderson et al., 2021).

receiving the same legal marital status or benefits enjoyed up until then exclusively by different-sex couples. While these benefits should have spilled over into the fostering and adoption process, a number of states, in direct response to same-sex marriage legalization, passed religious exemption laws which allowed foster care and adoption agencies which received public funding to refuse services to couples and individuals based on religious beliefs.[17] These discriminatory laws have remained in effect since the Supreme Court's ruling in their favor in *Fulton v. City of Philadelphia* in 2021. As a result, each of these states serve as a "never-treated" group which allows us to extend our analysis beyond 2015.

5 — Methodology

We employ a stacked, synthetic difference-in-differences estimator in order to provide causal estimates for the effect of same-sex marriage legalization on the number of adoptions of children from foster care in the U.S.[18,19] Recent work has begun employing this method given the numerous advantages to combining these popular empirical tools in this setting (Caselli et al., 2020; Wiltshire, 2022; Mann et al., 2022). One such benefit is that stacked regressions have been demonstrated as a viable alternative in avoiding the biased-estimate issues which can arise from from difference-in-differences estimators which have staggered treatments and hetero-

17. These states are Alabama, Kansas, Michigan, Mississippi, North Dakota, Oklahoma, South Carolina, South Dakota, Texas and Virginia (Movement Advancement Project, 2022).

18. Stacking involves restructuring data into relative event time rather than calendar time. Specifically, a window of time periods relative to treatment timing is chosen so as to balance the tradeoff between maximizing the number of both (1) pre- and post-treatment periods and (2) suitable control units. A "slice" of the dataset is then taken in this event time window so to contain only one treated unit with the maximum number of (not-yet or never-treated) control units. This dataset "slicing" process is completed for each treated unit where they are then finally "stacked" together with a unique dataset-identifying variable. The eventual result is that the treatment for each unit is centered at the same relative treatment time as if treatment were no longer staggered. Unbiased, event study estimates can be obtained via OLS with dataset, unit and time fixed-effects.

19. For more information or to see examples of this used in research please see Gormley and Matsa (2011), Cengiz et al. (2019) and Deshpande and Li (2019).

geneous treatment effects (either across groups or over time) as demonstrated in Goodman-Bacon (2021). Baker et al. (2022) preform a simulation analysis of the most commonly used estimation techniques for correcting these issues. Their analysis shows that each of the alternative estimators (stacking included) is able to recover the true treatment path when in event-study settings.[20]

While alternative difference-in-differences methods and estimators are excellent tools for uncovering causal relationships when treatment is staggered over time, few are of any use if suitable comparison units cannot be found for treated units. Synthetic control methods were first created for use in causal inference primarily for this reason (Abadie and Gardeazabal, 2003). Given that this method constructs as similar of a counterfactual as possible given the observable covariates of a donor pool of control units for each treated unit, iterated synthetic control calculations for treated groups naturally complement stacking methods.

Each of the advantages of using synthetic control for estimating the dynamic treatment effects of only one unit are also present when jointly estimated via stacking. For instance, synthetic control methods enjoy the benefits of reducing the both the uncertainty of, and degree to which, control units affect the construction and trajectory of the counterfactual. Additionally, these methods can overcome the arbitrary nature of researcher choice when it comes to removing variables from serving as control units due to lack of similarity in observables with treated groups. Finally, synthetic controls helps to safeguard against linear extrapolation beyond of the range of the outcomes of the control units (Abadie et al., 2010).

Recently researchers have extended the original one-unit, one time-period synthetic control method due to its versatility. Arkhangelsky et al. (2021) propose a synthetic difference-in-differences estimator which aids in relaxing the strictness of exogeneity and parallel trends assumptions, but is limited in its ability to analyze set-

20. Baker et al. (2022) show that one important constraint of stacking is that it exhibits both greater bias (point-estimates) and efficiency (narrower distributions) when employed in static estimation settings. This issue is discussed further in the Results section.

tings with treatment timing in more than one time period. Ben-Michael et al. (2021) introduce an augmented synthetic control method which pools synthetic estimators – handling multiple treated units simultaneously.[21] Most recently, Porreca (2022) combined the strengths of each of these methods into a staggered difference-in-differences estimator.

The primary difference between our method and that proposed by Ben-Michael et al. (2021) is the tradeoff between separate and pooled synthetic control units. Estimating a synthetic control fit for each treated unit allows exhibits less bias when estimating unit-specific treatment effects at the risk of bias in estimation of the average treatment effect as discussed in Baker et al. (2022). The primary difference between our preferred estimation method and that proposed by Porreca (2022) is the use of "not-yet-treated" control groups. Given that variation in assignment to both treated and "never-treated" groups in the setting of our analysis occurs at the state-level, carefully selecting control groups likely matters considerably in terms of minimizing unobservable differences between treatment and control groups. For example, Massachusetts is the first state to legalize same-sex marriage in 2004. Using a "never-treated" control group selection approach would draw from a donor pool of counties in Alabama, Kansas, Michigan, Mississippi, North Dakota, Oklahoma, South Carolina, South Dakota, Texas and Virginia rather than from Connecticut, Delaware, Maine, New Hampshire, New York, Rhode Island and Vermont if using a "not-yet treated" approach.

Our empirical approach takes the following steps. First, we select a time window for our analysis of nine years. This provides five years of pre-treatment observations for the construction of the counter-factual with four years remaining for estimation of dynamic, post-treatment effects. Second, we construct a donor pool consisting of all never-treated units and include any not-yet treated units whose treatment timing

21. Pooled synthetic control methods minimize the average pre-treatment difference between all treated units and their corresponding synthetic controls rather than doing so seperately for each treated unit.

occurs outside of the time window. Third, we subtract the mean from the dependent variable for each treatment and control unit so that the synthetic control method matches on trends rather than levels.[22] Fourth, we make minor modifications to the donor pool. We remove all observations of any control unit from the donor pool which fails to contain at least thirty observations of children in foster care for any period within the time window.[23] Additionally, we replace any singular missing value for a control unit's independent variable with its mean. Control units with more than one missing value within the same variable within the time window are removed from the donor pool. Last, we preform synthetic control analysis with this donor pool which includes each of covariates referenced in Tables 1, 2 and 3. Steps one through five are repeated for each treated unit. The results are combined ("stacked") into a dataset – resulting in treatment for each unit that is centered at the same relative treatment time.

6 — Results

6.1. Primary Results

Table 4 presents the the primary regression result. The variable of interest, *SSM*, is a binary variable set equal to zero – taking a value of one once same-sex marriage is legalized for the treated group. Coupled together with unit (*County*) and time (*Year*) fixed-effects, the coefficient for this variable represents the difference-in-differences estimate. A stack fixed-effect is crucially included in thi model in order to ensure that treated groups are being compared with their synthetic counterfactual. Standard errors are clustered at the state level in order to account for potential autocorrelation in the residuals of the counties within treated states.

We estimate that there was a statistically significant increase of approximately

22. This is similar to Arkhangelsky et al. (2021) and Porreca (2022)'s use of unit-fixed effects.

23. Thirty is often cited as the rule-of-thumb as the minimum number of units needed to ensure a normal distribution – ideally minimizing the impact of outliers on the mean.

23.6 adoptions of children from foster care per county per year in the three years following the legalization of same-sex marriage. This represents a 3.8% increase relative to the average number of yearly adoptions.[24] Back-of-the-envelope calculations suggest that this corresponds to an addition 10,000 adoptions over the course of three years.

Using stack, state and year fixed-effects (with standard errors clustered at the state-level), we estimate an event study analysis whereby we interact a variable indicating treated status with a relative treatment time variable in order to test for the differences between treatment and control groups over time. These results can be seen in Figure 4.

Table 4 — Primary Result

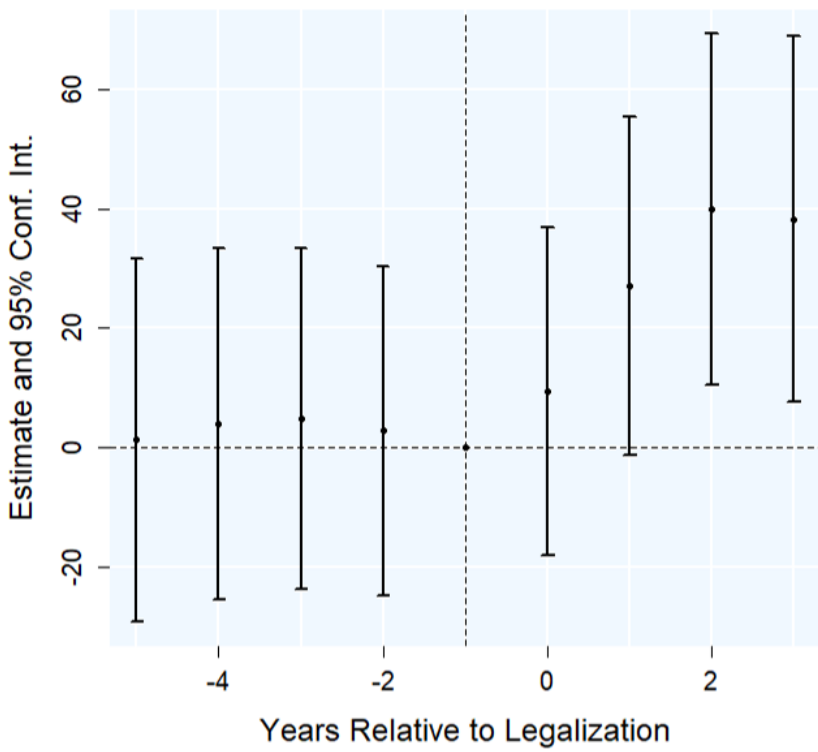
Dependent Variable:	Adoptions
Variables	
SSM	23.56**
	(11.63)
Fixed-effects	
Stack	✓
State	✓
County	✓
Year	✓
Fit statistics	
Observations	2,232
R^2	0.26011
Within R^2	0.00374

The results of Figure 4 suggest a strong casual relationship between same-sex marriage legalization and the number of adoptions within a county. Additionally, this figure reveals that the estimated effect from the static model in Table 4 is likely downwardly biased from the true effect given that the dynamic estimates do not begin diverging until at least one year after the law change.

We suspect that this divergence does not begin until one year after the law is passed primarily for two reasons. First, the majority of the timing of the law changes occur near election cycles and courts of appeals sessions in the late Fall of each year. Second, going through the adoption process itself can take regularly anywhere

24. The post-treatment mean of the number of yearly adoptions for treated units is approximately 604 which is smaller than the full-sample mean. This implies that the true effect is likely closer to a 3.9% increase.

Figure 4 — Event Study Analysis



This figure represents the dynamic estimates of differences in the yearly number of adoptions between treated counties and their synthetic counterfactuals relative to the timing of the legalization of same-sex marriage. Standard errors are clustered at the state level.

from six to eighteen months to legally complete.[25] In either of these two cases, this means that the immediate effects of the laws are likely to spill-over into the following calendar year – particularly if the law change is unexpected.

6.2. Robustness Checks

We preform a number of robustness checks and report these results in Table 5. We begin by both testing for differences in the pretends for each of the 124 treated units as well as calculating each of their average treatment effects. In model 1 of Table 5, we remove the stacks of 14 treated units from the sample which have statistically significant differences in the pre-trends of the outcome between treated units and their synthetic counterfactual. Both the point estimate and standard error in the model remain largely unchanged the results in Table 4.

In model 2, we restrict the sample to include counties within states whose assignment to treatment status was the most plausibly exogenous. We do this by restricting our sample to states where the legalization of same-sex marriage occurred due to court orders arising from legal disputes. We do this by omitting counties whose states either explicitly passed legislation or held referenda on same-sex marriage.[26] This is the strongest effect that we estimate. We speculatively credit this result to that fact that states which made the purposeful, political choice to pass these laws plausibly already had strong legal protections, culture, norms and attitudes towards same sex couples. Thus, the *marginal* incentive to form a family or adopt in these settings was likely lower that it was for same-sex couples living in states which were more politically hostile towards the rights of LGBT individuals.

In model 3 we employ a series of controls variables in order to ensure that the

25. Adoptive parents must first receive certification from their state of residence which regularly requires foster-care specific training, gathering and submitting of appropriate legal documents and social-worker home studies. These are followed by an often lengthy matching process whereby children are paired with potential adoptive parents. Finally, the process of the adoption itself can often be significantly delayed given the status of the parental rights of children's biological parents.

26. These states include Vermont, New Hampshire, Washington D.C., New York, Rhode Island, Delaware, Minnesota, Maryland, and Washington.

Table 5 — Regression Results: Robustness Tests

Dependent Variable:	Adoptions			
Model:	(1)	(2)	(3)	(4)
Variables				
SSM	23.01**	35.37***	25.49**	27.14**
	(10.64)	(11.93)	(10.43)	(11.85)
Fixed-effects				
Stack	✓	✓	✓	✓
State	✓	✓	✓	✓
County	✓	✓	✓	✓
Year	✓	✓	✓	✓
Fit statistics				
Observations	1,980	2,016	2,232	1,980
R^2	0.26194	0.26195	0.33910	0.34979
Within R^2	0.00336	0.00452	0.11010	0.12199

Notes. * p<0.1; ** p<0.05; *** p<0.01
Clustered (*State*) standard-errors in parentheses. *SSM* corresponds to the timing of same-sex marriage legalization. The *County* fixed-effect refers to a binary control variable indicating the treated unit for each stack. This acts a traditional unit fixed-effect when employed with in conjunction with stack fixed-effects. Model 1 omits fourteen treated units which fail parallel trends tests. Model 2 omits treated units from states which legalized same-sex marriage via popular vote. Model 3 includes twenty-seven additional control variables which differ significantly between treatment and control groups pre-treatment. The coefficients of these variables are not reported. Model 4 includes each of the control variables from model 3 and omits the treated units mentioned in model 1.

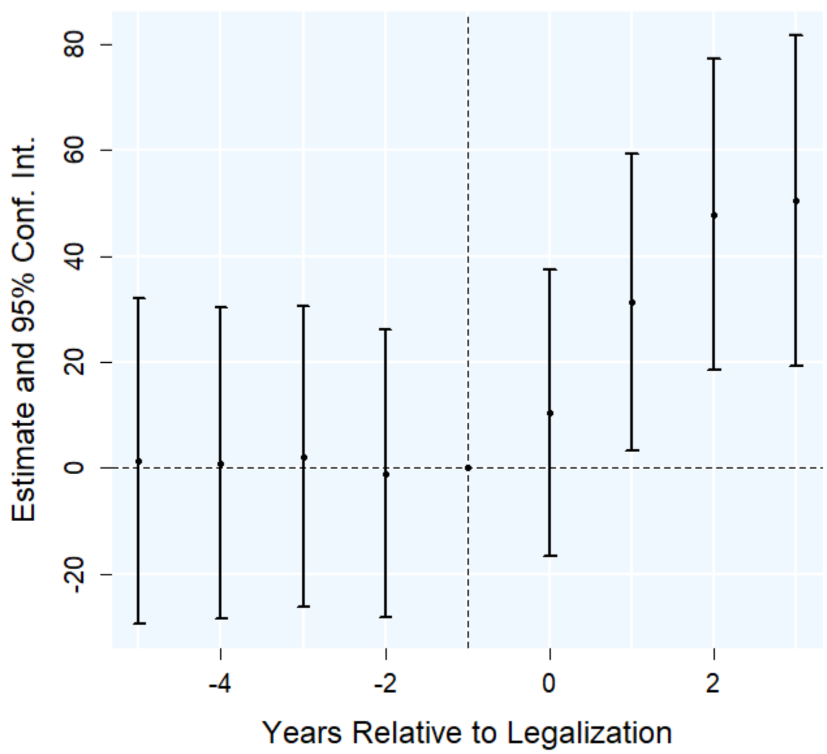
result is not being driven by differences in observables. In order to avoid over-fitting the model, we filter out a group of potential control variables by preforming a series of simple regressions which test for differences between treated and control units in the pre-treatment values of each independent variable in the sample variable. We find that 27 of the 55 independent variables were statistically different from one another prior to same-sex marriage legalization.[27] We include each of these variables in model 3, but omit their estimates for tractability. Model 4 includes both the regressors of model 3 and removes the stacks mentioned in model 1. Each produce consistent estimates with minimal changes to the standard errors.

We estimate an event study model which tests for the differences in the number of adoptions between treated and control units relative to the timing of same-sex marriage legalization while controlling for the independent variables which were significantly different across these groups prior to treatment. Figure 5 shows these results. Controlling for these variables reduces some of the estimated pre-treatment coefficients which, in turn, increases the upper bounds relative to our estimations shown in Figure 4.

If Table 5 and Figure 5 demonstrate the stability of our estimates, then Figure 6 showcases the importance of the unique timing of same-sex marriage legalization in driving these results. We do so by performing a placebo test – where we assign the treatment timing of each treated unit to a randomized year from 2005-2016. We then perform a synthetic difference-in-differences with staggered, random treatment timing and report the results in Figure 6. The null result across each time period suggests that the timing of same-sex marriage legalization, rather than any inherent or random trends between either the treated and control groups, is likely to be be driving the results.

27. The 27 variables include *Time in Foster Care*, *White*, *Native*, *Asian*, *Hispanic*, *Abused*, *Abandoned*, *Pre-Adoption*, *Relatives*, *Group Home*, *Institution*, *Supervised Independent*, *Trial Home*, *Unknown Goal*, *Unmarried*, *Unknown Family*, *Parents Age*, *Parents White*, *Parents Native*, *Parents Hispanic*, *Parents Unknown*, *Parents Biracial*, *IV-EF*, *IV-EA*, *IV-A AFDC*, *No Subsidy* and *Subsidy Amount*.

Figure 5 — Event Study Analysis (w/ Controls)



This figure represents the dynamic estimates of differences in the yearly number of adoptions between treated counties and their synthetic counterfactuals relative to the timing of the legalization of same-sex marriage. Additionally, these point estimates include control variables which differ signficantly between treatment and control groups prior to treatment. Standard errors are clustered at the state level.

We calculate our back-of the envelop calculations for the number of additional adoptions that would have occurred in had no states granted foster care agencies the right to refuse service to same-sex couples. The point estimates from table 4 and model 2 of of table 5 serve as the lower and upper bounds for this calculation. The mean number of adoptions in the twenty two counties that we observe in these from 2016-2019 was 685. Thus, we multiply this number with the number of years in the post and average yearly treatment effect of same-sex marriage legalization (3.8% and 5.9%). This corresponds to an increase of 2,900-4,500 additional adoptions.[28]

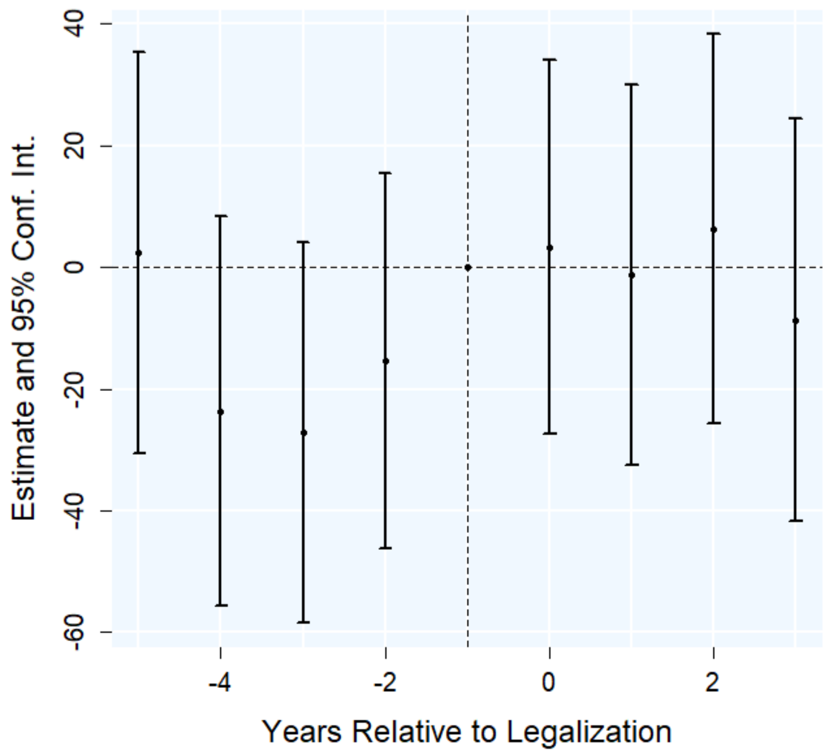
6.3. *Mechanism*

We begin to explore what could be driving these results by analyzing changes in the characteristics of the adoptive parent(s). For the purposes of our analysis, this could be most easily accomplished by observing the sex or gender identity of these these individuals in order to measure the proportion of same-sex couples in the sample. Unfortunately, the AFCARS Foster Care data does not contain this information as it only reports age and race of "Parent 1" and "Parent 2". Thus, we proxy for changes in the number of same-sex couples who are adopting by measuring changes in adoptive family type.

Our hypothesis is that we would see a substitution in the share of adoptive families away from unmarried couples and single households towards married couples. There are two primary pieces of evidence which support this idea. First, as shown by Figure 1, married couples are many times more likely to be raising adoptive children in the household – a fact that is particularly true for same-sex couples. Second, Badgett (2003) and Black et al. (2007) discuss the common, stressful and time-consuming process whereby lesbian, gay or bisexual individuals living in states where

28. There are competing sources of bias which could both bias these estimates up or down. These counties very likely had the most discriminatory practices towards same-sex couples prior to same-sex marriage being legalized. The marginal benefit of the relaxation of these constraints for same-sex couples in these counties would be very large. However, given the persistence of these constraints, there are far fewer same-sex couples residing in these counties.

Figure 6 — Event Study: Placebo Test



This figure represents the dynamic estimates of differences in the yearly number of adoptions between treated counties and their synthetic counterfactuals relative to the timing a randomized year between 2005 and 2019. Standard errors are clustered at the state level.

it was illegal for same-sex couples to jointly adopt would complete the process of adoption alone with the hope that their partner could be later legally recognized as a co-parent. Extending the legal right to marry to these individuals would grant same-sex couples the same legal protections in parenting as different-sex couples and thus reduce the proportion of individuals who adopt alone.

We test this by estimating changes in the composition of adoptive family types by preforming a difference-in-differences analysis as described in previous sections. Given that our control groups for these estimations are the weighted averages of the outcomes of a group of similar counties, we apply these weights towards the calculation of "synthetic" numbers of adoptive family types for each treated unit over time to create an appropriate comparison group. The results of this analysis can be seen in Figure 7.

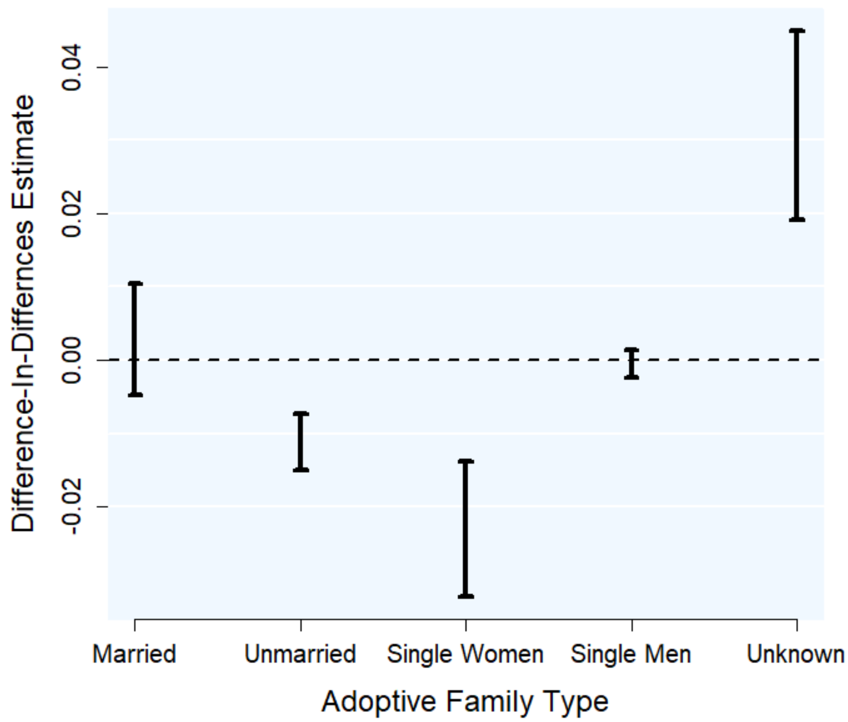
Figure 7 plots the confidence intervals demonstrating the average changes in the composition of adoptive households types in counties which legalized same-sex marriage relative to changes in their synthetic counterfactuals. From this figure, we can see that there was a significant decline in unmarried couples and single women relative to an increase in "unknown" family types.[29]

The reductions in the share of unmarried couples and single women adopting confirm our hypothesis, but we do not detect any change for single adopting men. This lack of significant variation is perhaps unsurprising given that single men account for the smallest share of the adoptive family types. Where we would expect to see effects for married couples, we instead see large increases in the number of "unknown" family types adopting. Given that the AFCARS adoption files data have categories explicitly for the characteristics of one mother and one father (regardless of the gender composition of the household), we suspect that adoptive couples which did not fit into this binary were left coded as "unknown".

The larger observed changes for single women is also consistent with the find-

29. The sum of the differences should sum to one given that all adoptions must fall into one of these five categories.

Figure 7 — Differences in Adoptive Family Type Following SSM Legalization



This figure represents the confidence intervals demonstrating the average changes in the composition of adoptive households types in counties which legalized same-sex marriage relative to their synthetic counterfactuals. The estimates are in terms of percentage points.

ings of the effects of same-sex marriage on lesbian households. Black et al. (2007) show that female same-sex households are significantly more likely to have children in the household. Goldberg and Conron (2018) demonstrates that married lesbian households are significantly more likely to be raising adopted children than their non-married counterparts. Carpenter (2020), using data with self-reported sexuality, shows that marriage take-ups in Massachusetts were larger for lesbians than for bisexual women or gay men, and that this effect was greatest for households with children.

If these results are being driven by increases the demand for children in the household among same-sex couples marrying, then the results should be greatest in counties which contain the most same-sex couples. In order to test this hypothesis, we turn to the *LGBT Demographic Data* dataset provided by The Williams Institute (2019) which provides county-level estimates of the number of same-sex couples per county in the United States.[30]

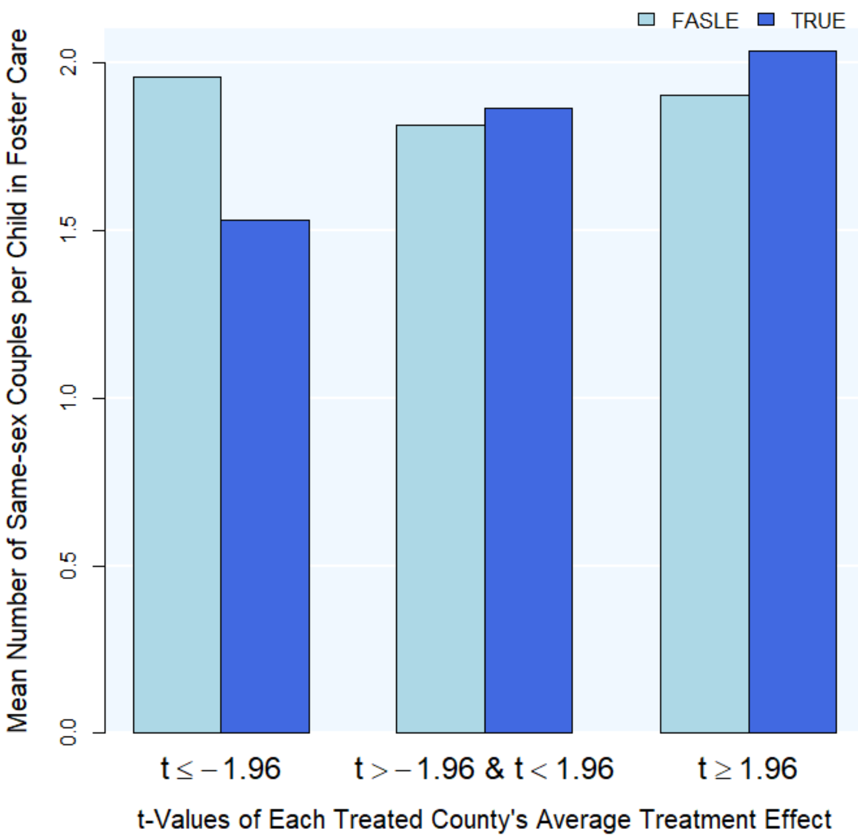
In order to capture less of the inherent correlation between the number of same-sex couples and the general population of an area, we calculate the number of same-sex couples per child in foster care for each county. This statistic allows us to identify areas which have higher *shares* of those who were potentially most affected by same-sex marriage legalization. We combine this statistic with data from the distribution of average-treatment effects from each treated county observed in the data.[31] Figure 8 shows these results.

The left-most set of columns in Figure 8 demonstrates that there were far fewer same-sex couples per child in foster care in counties where there were fewer adoptions after same-sex marriage was legalized. These differences were relatively minor in counties where there were no significant changes in the number of adoptions. Ad-

30. Note that this data is only available for one time period. Thus, we assume that the relative proportion of the number of same-sex couples across counties is constant over time.

31. All observations which are aggregated to the state-level for child-confidentiality are dropped from the analysis in Figure 8.

Figure 8 — Differences in Adoptive Family Type Following SSM Legalization



This figure combines information on the number of same-sex couples per child in foster care and the distribution of average treatment effects of same-sex marriage on adoptions. The left-most set of columns demonstrates that same-sex couples were a much smaller share of the population in counties which same-sex marriage legalization was estimated to have had a negative impact on adoptions. In counties which experienced a significant increase in the number of adoptions, same-sex couples were over-represented relative to sample averages. All observations which are aggregated to the state-level for child-confidentiality are dropped from this analysis. None of the estimates in this figure have differences in means which are statistically signficicant given the small sample size.

ditionally, areas which saw significant increases in the number of adoptions following the legalization of same-sex marriage had disproportionate numbers of same-sex couples per child in foster care.

While the findings in this figure are consistent with a story of effects being largest (smallest) in areas with the most (least) same-sex couples, the results of this figure should be interpreted with caution as none of these relationships presented are statistically significant due to the small sample size.

7 — Conclusion

In this paper we study the relationship between same-sex marriage legalization and the number of adoptions of children from the foster care system. We provide evidence that the former significantly increased the latter and that the relationship between these two variables is causal. Estimates from synthetic difference-in-differences models suggest that these laws increased the number of additional adoptions by 3.8%-5.9% – resulting in a 10,000-17,500 more children leaving the foster care system than would have otherwise occurred within four years. Extrapolating these estimates suggests that there were 2,900-4,500 fewer adoptions from 2016-2019 due to the ten states which granted legal protections for foster and adoption agencies to refuse service to same-sex couples.

We provide evidence of an important direct effect resulting from relaxations of the legal, economic and social constraints on LGBT individuals. While we cannot document which specific constraint was most binding, we are able to show that the composition of single female and unmarried couples declines significantly relative to an increase in recognized families. This suggests that one major component which is driving these results is increases in the take-up of marriage increasing the demand for children in the household. This finding is both timely and important poltically given that much of the (claimed) current resistance to expanding legal protections for same-sex families is on the grounds of their supposed *negative* effects on child-rearing.

While we are only able to document the direct effect of this law on the number of additional adoptions which occurred, it is plausible that the long-run social benefits of these adoptions could outweigh the personal gains to each child through reductions in homelessness, unemployment, criminal activity, unplanned pregnancies and school dropouts (Macomber, 2008; Dworsky and Courtney, 2010; Dworsky et al., 2013; Valset, 2018). We leave it to future researchers to document the long-run effects of *increases* in child wantedness (Donohue III and Levitt, 2001).

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