Do Religious Exemption Laws Hurt or Help Government Contracting? Evidence from Foster Care

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ABSTRACT

Religious organizations have long been known for their selectivity in who they serve and employ. This exclusivity allows them to lower the search costs of recruiting participants, but only by rejecting services to those outside their community. As an intermediary that matches government services with participants, religious organizations' exclusivity can be problematic, since government contracts funding these services carry an expectation of equal access for all participants, not a self-selected few. This paper tests the effect of governments partnering with selective religious intermediaries using state religious exemption laws in U.S. foster care. Sixteen states have passed laws permitting faith-based foster care agencies to recruit foster families who share their religious beliefs while declining others based on sexual orientation, gender identity, or differing religious views. The central question is whether this specialization improves an intermediary's capacity to recruit and match foster parents with children, or instead, reduces matching capabilities by narrowing the pool it can serve.

Using a stacked difference-in-differences design and AFCARS data from 2012–2023, the analysis finds that religious exemption laws are associated with an increase in non-relative foster care placements, concentrated among groups who typically face greater search and matching barriers, including Black children and, with weaker statistical significance, older youth. Religious exemptions are also associated with improved placement stability among children who remain in foster care for over a year. These findings suggest that permitting government contractors to specialize by narrowing whom they serve can expand service delivery capacity, though this comes with an equity tradeoff that falls outside the scope of these findings.

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1 Introduction

Across areas such as healthcare, workforce development, refugee resettlement, and foster care, governments increasingly contract with organizations that serve as matchmakers between public agencies and the populations they serve. Rather than delivering services directly, these "matching intermediaries" use localized knowledge, community trust, and social networks to connect public programs with appropriate providers and recipients. Some connect seniors and vulnerable populations to Medicaid providers, match refugees with community sponsors, recruit contractors and service providers on behalf of public agencies or help address public-sector staffing shortages by linking potential employees to government jobs. Their value lies in reducing search costs and information asymmetries by reaching populations and mobilizing resources that governments often struggle to access through traditional bureaucratic channels.

A defining feature of many matching intermediaries is *specialization*. Rather than serving the general public, they frequently focus on particular communities or populations (e.g., seniors, formerly incarcerated individuals, or co-religionists), developing expertise, trust, and recruitment networks that improve matching in contexts characterized by high search costs and information asymmetries. Yet the same selectivity that enables this specialization may also limit access for those outside the communities on which these organizations concentrate. Governments therefore face a fundamental implementation dilemma when contracting with these organizations: *does permitting intermediary specialization improve public service delivery by strengthening recruitment and matching, or does it ultimately reduce service capacity by narrowing the pool of eligible participants?*

Among the specialized networks that intermediaries draw on to build this kind of trust and reach, the religious community is among the oldest and most significant. Religion has been central to human society and cooperation throughout recorded history, as some scholars argue that humans' capacity to create and sustain shared belief systems, including religion, enabled cooperation among large numbers of unrelated individuals in a way no other species has achieved (Harari, 2015). This significance is not merely historical. Religion continues to shape social and political life globally, with an estimated 75.8 percent of the world's population religiously affiliated today (Pew Research Center, 2025). This gap matters for understanding intermediary specialization specifically. Religion has historically enabled cooperation among large numbers of unrelated individuals. That same capacity explains why faith-based intermediaries can mobilize exclusive but dense, trust-based recruitment networks that secular organizations may struggle to replicate (Berger, 2003).

Foster care offers a useful setting for examining this dynamic, since public child welfare systems depend heavily on faith-based Foster Family Agencies (FFAs) who recruit foster families by mobilizing this same religious community. Each year, more than 500,000 children enter or remain in the U.S. foster care system and persistent shortages of foster parents have led states to contract extensively with private FFAs, which recruit, screen, and train foster and adoptive families on behalf of public child welfare systems (Bremner, 1970). A substantial share of these agencies is faith-based, drawing on religious communities whose shared values, congregational networks, and community trust provide access to highly motivated foster parents. In fact, religious foster parents have been found to complete the approval process at higher rates than other applicants, and be more responsive to recruitment efforts when reached out by staff who share their faith (OCFS – New York State, n.d.; Casey Family Programs, 2020). At the same time, these agencies have often limited recruitment or services to parents whose beliefs align

with their religious mission, creating conflict between religious selectivity and federal nondiscrimination policies that assume equal access (Bewkes, 2018).

Sixteen states have responded to this tension by enacting Religious Exemption (RE) laws that permit faith-based foster care agencies to decline service to prospective parents who do not satisfy their religious criteria, most notably LGBTQ+ families, while continuing to receive public funding (Spoto, 2021). Supporters argue that these laws preserve agencies' exclusive access to congregational networks and reduce legal liability that might otherwise discourage faith-based organizations from participating in public foster care contracting (Goodnow, 2018). Critics argue that they reduce the pool of prospective parents and ultimately diminish the supply of foster homes by excluding otherwise qualified families (Lund, 2020; Bewkes et al., 2018; Spoto, 2021). Because these laws alter the degree to which faith-based intermediaries may specialize while leaving the broader foster care system intact, they provide a natural policy setting for evaluating the implementation consequences of intermediary selectivity.

The legal status of government contractors helps explain why this policy variation across states exists in the first place (Uvaydov et al., 2021). Unlike government agencies, contractors are not automatically bound by the constitutional equal protection obligations that apply to the state (Rosenbloom and Piotrowski, 2005). Courts have generally held that contracting with a private organization, even one receiving substantial public funding, does not by itself transform that organization into a state actor subject to the same constitutional nondiscrimination requirements (*Rendell-Baker v. Kohn,* 1982). At the same time, intermediary selectivity is not legally unconstrained. The scope of this discretion also varies across protected characteristics. Race-based discrimination has been subject to robust statutory limits even outside government contracting (*Runyon v. McCrary*, 1976), whereas faith-based foster care agencies have received greater latitude to decline service to same-sex couples while continuing to participate in publicly funded child welfare systems (*Fulton v. City of Philadelphia*, 2021). Comparable tensions also arise internationally, where faith-based schools, hospitals, and social service organizations continue to navigate competing demands between religious autonomy and public nondiscrimination requirements (European Union, 2018; Hondros, 2015).

As a result, government contractors occupy a legally interesting position in public service delivery as they are expected to advance public purposes while, in some circumstances, retaining greater choice over whom they serve than government agencies themselves. Although this discretion remains constrained by civil rights statutes governing organizations that serve the public or receive government funding (Khan, 2024), research on faith-based social service organizations suggests that specialization around co-religionists may also improve recruitment among harder-to-reach populations (Monsma, 2006; Padot, 2014; Lipsky, 2011; Cnaan and Boddie, 2006), a possibility examined in this paper.

Using state religious exemption laws as a test case, this article asks whether legally permitting intermediary specialization increases or decreases the number and quality of matches between public service providers and recipients. Specifically, *when matching intermediaries are allowed to recruit selectively from value-aligned communities, do they place fewer children because the eligible pool of caregivers becomes smaller, or more, and more stably placed, children because specialization strengthens recruitment and matching?* To answer this question, the article employs a stacked difference-in-differences design using 2012–2023 Adoption and Foster Care Analysis and Reporting System (AFCARS) data.

The analysis finds that states adopting religious exemption laws experienced higher numbers of non-relative foster care placements than comparable states without such laws. These

increases are concentrated among Black children and, with weaker evidence, older youth, which are groups that typically face greater search and matching barriers in foster care (Kahn and Hansen, 2017; Brehm, 2018). The adopting of the law is also associated with improved placement stability among children in foster care over a year, a group whose stability is already lower than that of more recently placed children.

These findings should not be read as evidence that non-relative placement is preferable to kinship care, which remains the placement of choice in child welfare policy and practice when safe and available (Hegar and Scannapieco, 2005). A supplementary analysis in this paper finds no clear evidence that the increase in non-relative placements came at the expense of relative placements specifically. Nor do these findings imply that the selectivity enabling this increase is normatively unproblematic. The same religious criteria that allow faith-based agencies to recruit effectively within congregational networks are also the basis on which they may decline service to prospective parents outside those networks, including LGBTQ+ and non-religious families. This article documents an association between legally permitted selectivity and placement outcomes without assessing, or endorsing, the equity and access costs this selectivity may impose on excluded caregivers.

Religious exemption laws provide a rare opportunity to examine this question empirically by isolating how legally permitted specialization influences service delivery in a recruitment-intensive policy domain. The answer has implications not only for foster care but also for how policymakers evaluate the tradeoffs between implementation capacity, equity, and accountability when governments rely on specialized contractors.

2 Literature Review and Theory

2.1 Intermediaries in Public Service Delivery

Existing academic literature in public service delivery highlights a range of intermediaries, including those that help citizens navigate e-government systems and reduce administrative burdens (Benish et al., 2024), mobilize citizen participation to supplement public resources in emergencies (Krogh and Lo, 2024), assist individuals in lowering medical costs, advocate for nonprofit organizations in government contractual relationships (Tsukamoto and Nishimura, 2006), and coordinate services across sectors to improve health outcomes (Stein, 2006).

In practice, intermediaries across public and social service delivery take diverse forms, including those that link job seekers to government employment; connect government agencies with contractors; support the implementation of government programs as financial and technical intermediaries; operate grantor-grantee matching platforms; administer donor-advised funds; and facilitate public-private advocacy (Fitzgerald, 2004; Löbel et al., 2016; Liu et al., 2019; Batey, 2017; Mosley, 2014; Tsukamoto and Nishimura, 2006). Across these settings, their effectiveness derives from their network of participants and trust they cultivate among actors, enabling them to bridge institutional boundaries and function as coordinators in arranging cross-sectoral governance (Haug, 2024; Brandsen and Pestoff, 2006; Briggs, 2003).

Notwithstanding their prevalence, intermediaries have received relatively limited theoretical attention in the literature on government contracting, partly because the term "intermediary" itself is defined so broadly as to encompass a wide range of organizational forms and functions (Shea, 2011; Löbel et al., 2016; Smith, 2008; Tsukamoto and Nishimura, 2006).

As a result, intermediaries are often described in terms of *what* they do rather than *how* they operate and shape service outcomes (Brandsen and Pestoff, 2006; Shea, 2011; Ansell and Gash, 2018; Smith, 2008). In fact, Briggs (2003) catalogues the diverse roles intermediaries play, such as, "public educators" who fill information gaps, "capacity builders" who coach and train stakeholders, "organizers" who mobilize coalitions and advocacy, and "performance investors and monitors" who screen and match recipients to public goods and services. Social scientists have primarily focused on the first three types (Briggs, 2003; Shea, 2011; Ansell and Gash, 2018). Ansell and Gash (2018) conceptualize collaborative platforms in which intermediaries coordinate multilateral relationships among actors. Shea (2011) shows how nonprofit intermediaries help governments engage and strengthen local faith-based and community-based organizations, emphasizing their role in community empowerment within implementation processes.

We know far less about matching intermediaries that function as "performance investors and monitors" that screen, validate, sort, and connect individuals to public services. These include public workforce intermediaries, Medicaid managed care organizations, and government contracting and grant platforms. In spite of their central role in facilitating access and bypassing red tape (Löbel et al., 2016), scholarship has paid limited attention to how their matchmaking strategies influence performance outcomes.

Rather, matching intermediaries are documented in economics and finance literature as "two-sided marketplaces" that connect distinct groups and reduce search frictions between them (Rochet and Tirole, 2003; Rysman, 2009). Unlike traditional retail platforms, which simply provide a marketplace for producers and consumers, intermediaries enable users to find one another, creating value by reducing information asymmetry and search costs associated with matching (Rochet and Tirole, 2003; Rysman, 2009; Allen and Santomero, 1997). They also act as delegated monitors, collecting and verifying information on behalf of participants, lowering verification costs, monitoring opportunistic behavior, and ultimately enhancing match qualities (Diamond, 1984; Allen and Santomero, 1997).

The logic of intermediaries builds on earlier work on matching, specifically Becker's (1973, 1974) analysis of marriage markets, where individuals sort potential partners through assortative matching based on observable characteristics. Extending this insight, modern economics conceptualizes intermediaries, such as online dating platforms and card merchants, as entities that collect and process information on participants to reduce search costs and connect individuals who might otherwise struggle to find suitable matches (Rochet and Tirole, 2003; Rysman, 2009). While this literature illuminates the general logic of intermediary matching, the discussion below draws more directly on institutional accounts of specialization in government contracting.

2.2 Intermediary Specialization and Selectivity

Matching intermediaries frequently concentrate on a defined segment of participants rather than serving a general population of eligible clients. Because intermediaries face heterogeneous participants and high search and information costs (Allen and Santomero, 1997), narrowing focus to a specific population allows them to develop domain-specific knowledge and denser relational networks. That way, participants can more easily signal relevant attributes and intermediaries can more effectively verify them, reducing uncertainty in identifying suitable matches.

Certain online dating platforms, for instance, specialize in serving specific communities like farmers or the LGBTQ+ individuals; organizations such as the Center for Employment Opportunities (formerly incarcerated individuals), Hire Heroes USA (veterans), and the National Organization on Disability (people with disabilities) similarly concentrate matching jobs with a specific population.

These specialized intermediaries' partnership with the government can be both effective and problematic, since their exclusivity may conflict with democratic values of equal access and nondiscrimination even as it improves service delivery. Weighing these effects, first, requires understanding how intermediaries actually operate. Analyzing these organizational processes is essential, as it informs policies that draw boundaries on practices that undermine public goals while preserving the organizational capacities necessary for effective service delivery. Existing scholarship, however, has focused more on the contracting component and the need to outsource democratic values to contractors (Eikenberry, 2009), and less on the underlying mechanisms of specialization that make intermediaries good matchmakers. Specifically, it has not fully incorporated theoretical insights from across disciplines on how intermediaries build and rely on specific relational networks to carry out their matchmaking abilities more efficiently and effectively.

Faith-based organizations provide a useful case for illustrating these dynamics because their selective practices simultaneously facilitate effective service delivery and generate concerns about equitable access. Rather than treating particularism solely as a limitation, some scholars find that religiously-affiliated organizations are often effective at the relational, trust-building dimensions of service delivery (Monsma, 2006; Lipsky, 2011), a pattern also documented in public health vaccination outreach (Syed et al., 2023) and to foster care placement outcomes (Padot, 2014, pp. 53–54). This same particularism, however, can also restrict access as organizations built around a specific faith community sometimes limit services to those who share that affiliation, "naturally exclud[ing] some potential beneficiaries (Lipsky, 2011, p. 33).”

2.3 Government Contractor’s Selectivity and Its Limits

The coexistence of effectiveness and exclusion also raises questions about the legal limits of government contractor’s selectivity. Although private organizations often administer publicly funded programs, they are not automatically subject to the constitutional constraints that apply to government agencies. Courts have generally held that contracting with a private organization, even one that is substantially publicly funded, does not by itself transform that organization into a state actor bound by the Fourteenth Amendment's Equal Protection Clause (*Rendell-Baker v. Kohn,* 1982). At the same time, recent cases have expanded constitutional protections for religious organizations participating in publicly funded programs, reinforcing the principle that governments may not condition participation in public funding on the relinquishment of certain religious rights (*Carson v. Makin,* 2022).

Outside government contracting relationships, however, courts have increasingly scrutinized identity-based selection criteria used by private organizations. In American *Alliance for Equal Rights v. Fearless Fund Foundation* (2024), the court held that a venture capital fund's grant contest, which was limited to Black women-owned businesses, was substantially likely to violate the same federal statute prohibiting private racial discrimination in contracting that was at issue in *Runyon*, rejecting the fund's argument that the contest constituted protected expressive conduct under the First Amendment. The fund subsequently discontinued the program. Notably,

Fearless Fund involved no government funding or contracting relationship, suggesting that statutory constraints on nonprofit selectivity differ substantially depending on the protected characteristic and legal framework.

Fulton v. Philadelphia (2021) illustrates this complexity. Unlike race-based discrimination, which is generally prohibited under federal civil rights law, *Fulton* involved a faith-based foster care agency's refusal to certify same-sex couples as foster parents, raising questions about how nondiscrimination requirements should be balanced against religious freedom protections. Overall, these cases illustrate that the legality of government contractors' selectivity depends not only on whether an organization is treated as a state actor, but also on the statutory framework and the type of discrimination at issue.

Even when selective practices are legally permissible, however, their practical consequences for access barriers are not uniform. Whether selectivity meaningfully restricts access depends on two related but distinct dimensions: *market dominance* (the extent to which an organization controls service provision) and *numerical alternatives* (the number of other providers available to affected individuals). Miracle Hill Ministries, South Carolina's largest state-contracted foster care agency, illustrates these dynamics. After the agency denied a married same-sex couple's foster care application on religious grounds, the plaintiffs argued that the state and HHS had authorized taxpayer-funded exclusion through the state's dominant provider (*Rogers v. HHS*, 2023). Although the court rejected the equal protection claim because Miracle Hill was not a state actor, it also emphasized that the plaintiffs could obtain the same services from 26 other private agencies in the state. Similar reasoning has not always prevailed in other contexts. In *Stormans v. Wiesman* (2015), for example, the court upheld a nondiscriminatory dispensing requirement for pharmacies, rejecting the argument that the availability of nearby alternatives justified a pharmacy's refusal to dispense emergency contraception. In terms of market dominance, Catholic hospital mergers present a related structural concern as religiously affiliated health systems become dominant or even sole providers in some regions, and they can restrict access to reproductive and gender-affirming care (Sanders et al., 2025). Jointly, these cases suggest that evaluating government contrators' selectivity requires attention to both legal doctrine and market structure. The consequences of selective practices depend not only on whether they are legally permissible, but also on how intermediary organizations are positioned within service delivery systems and the extent to which meaningful alternatives exist for those they exclude.

3 U.S. Child Welfare Policy and Religious Exemptions

These dynamics are notably pronounced in child welfare, where governments contract with Foster Family Agencies (FFAs), which are private, and often nonprofit organizations engaged in the recruiting, certifying, and training of foster parents, and in coordinating the placement of children in need of care. Persistent shortages of non-relative foster parents and increasing case complexity make effective matching especially critical, creating a setting in which intermediary specialization may play an important role in shaping placement numbers.

3.1 Overview of Current Child Welfare System

Each year, approximately 500,000 children enter state custody, with the goal of placing them in safe, temporary homes until reunification with biological parents, or if necessary,

adoption (ACF, 2025). When a child is removed due to factors such as parental incarceration or substantiated neglect and abuse, child welfare practice and federal policy prioritize kinship care. Federal law requires states to exercise due diligence in identifying and notifying relatives within 30 days of a child's removal, and to consider giving relatives preference over non-related caregivers where they meet applicable safety standards, reflecting a general policy preference for kinship placement (Congressional Research Service, 2015). Kinship care and family preservation are broadly favored in both child welfare policy and developmental research, associated with greater placement stability and, in some studies, better behavioral outcomes relative to non-relative foster care, though evidence on physical and mental health outcomes specifically remains more mixed (Hegar and Scannapieco, 2005).

Where no suitable kin are identified or available, the state places the child with a recruited, compensated non-relative foster family. Children generally remain in care until reunification, adoption, or emancipation at age 18, though many states have opted into a federal option extending eligibility for care and support to age 21 (ACF, n.d.).

For foster youth who disproportionately face challenges such as mental health issues, incarceration, homelessness, and gender identity-related vulnerabilities, placement instability can exacerbate existing trauma (Rosenberg and Kim, 2018; Mallon et al., 2017). This risk grows with time in care, as national data show that children in foster care for less than 12 months achieve placement stability (two or fewer placement settings) at a median rate of 83.5%, compared to just 41.1% among children in care for 24 months or more (Administration for Children and Families, 2018). Even with the importance of timely placements, a nationwide decline in foster parents has strained system capacity, at times forcing states to house children in temporary settings such as detention centers, parks, or hotels in the absence of an available family placement (Rodriguez, 2023).

FFAs are therefore essential partners in public service delivery, recruiting and training foster families, administering subsidies, monitoring licensed parents, and facilitating placements (Andrews et al., 2022). Privatization has been linked to improved placement access for more difficult-to-place children, including older youth, racial minorities, and those with medical or behavioral needs (Deutsch et al., 2017; Huggins-Hoyt et al., 2019). As of 2017, roughly thirty-three states have contracted with FFAs and created a mixed system of public and private recruitment (Committee on Finance, U.S. Senate, 2017; Fellmeth, 2021). States like Kentucky and Florida have privatized nearly all child placements (U.S. HHS, 2007).

3.2 Historical Role of Faith-Based FFAs

Since the 1800s, many states opted to publicly subsidize existing private child protection institutions that were predominantly faith-based, motivated by cost reduction and administrative convenience (Bremner, 1970, pp. 269–276). Religious FFAs reduce search costs by tapping into local communities of faith, which provide a consistent pool of highly motivated foster families whose religious beliefs encourage caring for vulnerable children (Kirby, 1997; Howell-Moroney, 2013). Targeted recruitment is further facilitated through referrals within these networks, providing a cost-effective and reliable source of new foster families (OCFS New York State, n.d.; U.S. GAO, 2018). In turn, prospective parents are more responsive when approached by agencies staffed with individuals who share their faith or have lived experience as foster parents (OCFS New York State, n.d.; Howell-Moroney, 2013). The same relational networks that make

faith-based FFAs effective recruiters, however, can also shape who is encouraged to apply and who gains access to services.

Although the national scope of these exclusions remains unknown, available evidence suggests that differential treatment is not confined to a small number of documented cases. Correspondence studies find that faith-based agencies respond less frequently and less informatively to inquiries from same-sex couples (Mackenzie-Liu et al., 2022), consistent with documented cases of differential treatment (Bewkes et al., 2018). Same-sex couples are approximately seven times more likely than different-sex couples to be raising an adopted or fostered child (21.4% vs. 3% for adoption and 2.9% vs. 0.4% for foster care), indicating that LGBTQ prospective parents are disproportionately engaged in foster care and adoption (Williams Institute, 2018). Consequently, even modest barriers affecting LGBTQ applicants could meaningfully reduce the pool of prospective caregivers available to an already overburdened child welfare system.

3.3 Religious Exemptions in Foster Care

Federal policy has oscillated between broad anti-discrimination protections and targeted exemptions, shaped by both administrative actions and judicial decisions. After the Supreme Court's 2015 decision recognizing same-sex couples, the U.S. Department of Health and Human Services (HHS) issued a grants rule adding explicit nondiscrimination protections based on sexual orientation and gender identity (see 45 C.F.R. § 75.300(c)–(d)). Importantly, these nondiscrimination provisions were understood to flow down to subrecipients such as private child-placing agencies, thereby extending federal nondiscrimination obligations beyond direct grantees (U.S. HHS, 2016).

Multiple states then independently passed exemptions to waive state-level nondiscrimination mandates for faith-based FFAs, allowing them to retain public funding while limiting services to parents who share their religious beliefs. Beginning with North Dakota in 2003, at least sixteen states enacted RE laws by 2026 (see table 1). Then, the Trump administration granted waivers to additional states, again allowing agencies to operate according to religious criteria and maintain Title IV-E funds (Meckler, 2019). Moreover, the Supreme Court's decision in *Fulton v. City of Philadelphia* (2021) held that the city had violated the Free Exercise Clause by refusing to contract with a Catholic FFA that would not certify same-sex couples. However, that same year, the Biden administration rescinded the waivers from the past administration and ceased federally approved waivers (Weixel, 2021).

Table 1. States with targeted religious exemption on publicly funded child welfare agencies

States	Enacted	Bill Title	States	Enacted	Bill Title
ND	2003. 1.	§50-12-07.1	OK	2018. 3.	SB 1140
VA	2012. 1.	HB 189	SC	2018. 3.	HB 4950 (Rescinded by the HHS 2021.11)
MI	2015. 2	HB 4188 (Rescinded by the HHS 2021.11)	TN	2020. 2.	SB 1304
MS	2016. 2.	HB 1523	AZ	2022. 1.	SB 1399
SD	2017. 2.	SB 149	UT	2023. 3.	SB 154
AL	2017. 2.	HB 24	ID	2024. 2.	HB 578

TX	2017. 3.	HB 3859 (Rescinded by the HHS 2021.11)	AR	2025. 4.	HB 1669
KS	2018. 1.	SB 284	IN	2026. 2.	HB 1389

Various media reports claimed that RE prevent states from expanding the pool of prospective parents, especially in recruiting LGBTQ+ parents (Lund 2020; Bewkes et al. 2018). In contrast, advocates of the waiver point to the historical interdependency between the government and religious FFAs and assert that these organizations are too valuable to lose, taking the example of large Christian FFAs that ended its services or lost licenses for refusing child placement with LGBTQ+ parents (Goodnow, 2018; Rutledge, 2008). Furthermore, compliance with state inclusion requirements is expected to provoke criticism and backlash from core constituencies, including faith communities, churches, and donors, placing organizations in a precarious position (Williams, 2022).

Legal scholarship has largely focused on the constitutionality of religious exemptions and faith-based FFAs' selective practices. Spoto (2021) argues that RE laws violate the Establishment Clause by requiring government funding of religiously discriminatory child welfare organizations, favoring certain religious viewpoints over others, and imposing burdens on LGBTQ prospective parents and youth. Woods (2019) situates these laws within a longer historical pattern of child welfare interventions that have subordinated LGBTQ youth, and documents that LGBTQ youth of color, particularly, are overrepresented in foster care and face elevated rates of abuse and instability within the system. Similar unresolved doctrinal questions are related to the state-actor status of foster care agencies. Legal commentators have documented a persistent split among federal district courts over whether private foster care agencies qualify as state actors for purposes of constitutional liability, with the issue still awaiting resolution (Uvaydov et al., 2021). While this body of scholarship engages closely with the constitutional and doctrinal dimensions of religious exemptions, quantitative evidence on their policy effects remains limited.

Existing quantitative studies show that religious FFAs are less responsive to same-sex applicants (Mackenzie-Liu et al., 2022), and that state antidiscrimination laws are associated with a higher likelihood of children finding foster and permanent homes, as well as shorter time to placement (Barak-Corren et al., 2022). However, Barak-Corren et al. (2022) did not examine the effects of religious exemption laws, and their analysis examined total foster care placements rather than non-relative placements, a distinction that obscures faith-based FFAs' influence on foster care capacity. Because kinship placements are typically identified and facilitated by public child welfare agencies rather than recruited by contracted foster care agencies, total foster care measures are less sensitive to changes in the recruitment capacity of these matching intermediaries. Consequently, the effects of religious exemption laws on the segment of foster care most directly shaped by intermediary's recruitment remain largely unexamined.

In addition, little is known about whether these effects vary across children with different demographic and service needs, such as race, age, or disability status. Nor has prior research examined whether increases in non-relative placements are accompanied by greater placement stability and corresponding changes in kinship placements, a possibility that is particularly relevant given the child welfare system's longstanding preference for kinship care.

4 Hypotheses

State-level variation in religious exemption laws provides a quasi-experimental setting to examine how intermediary specialization among contractors affects non-relative placement volume and quality. By reducing legal liability associated with serving narrowly defined subgroups, these policies generate plausibly exogenous variation in agency recruitment and matching practices. Reducing this liability exposure may affect matching performances through three related channels.

First, agencies facing lower compliance risk may be less likely to limit their foster care operations or exit the system entirely, preserving overall placement capacity, as some faith-based agencies have done in jurisdictions without such exemptions (Watson and McNeil, 2018). Second, RE laws formally authorize agencies to decline service to prospective parents whose values are incongruent with their religious mission, a legal accommodation that in turn may grant these agencies greater legitimacy to recruit directly within congregations and denominational networks with a strong religious commitment to child welfare work. Third, operating within these tightly connected religious communities provides reputational information, repeated interactions, and informal monitoring, potentially reducing information asymmetries about prospective foster parents' commitment and capacity relative to generalist intermediaries recruiting across a broader, less densely connected pool (OCFS– New York State, n.d.).

> Hypothesis 1: Religious exemption status is positively associated with the number of non-relative foster care placements.

Such advantages may be particularly consequential for children who face greater barriers to placement, for whom effective recruitment, screening, and matching are especially important. These groups include older youth, children with disabilities, and Black children (Brehm, 2018; Kahn and Hansen, 2017; Foster et al., 2011). For instance, the federal government offers adoption incentives for children aged nine and older, reflecting the greater difficulty of securing permanent placements for older youth (Brehm, 2018). Children with disabilities may similarly require additional time and effort to identify families with the capacity and resources to meet their needs.

Black children also face substantial disparities in achieving permanency. Cho et al. (2023) find that Black children are less likely to be adopted than White children and experience longer waits between termination of parental rights and adoption finalization. Even after accounting for child, family, and system characteristics, White children had 1.27 times the likelihood of adoption compared with Black children. These disparities are particularly relevant to the recruitment and matching mechanism considered here, as prior research also documents longer waits for Black children to be matched with adoptive families. Although Black children constitute approximately 14% of the U.S. child population, they account for roughly 24% of children in foster care in 2023 (ACF, 2025). These disparities extend beyond entry into care. Foster et al. (2011) find that Black children experience greater placement instability and more out-of-home placements than White children. Kahn and Hansen (2017) further find a significant Black–White disparity in the predicted probability of foster care placement in their Texas analysis, while the corresponding difference between Hispanic and White children is not statistically significant. These studies provide a basis for examining Black children separately, suggesting that effective recruitment and matching may be especially consequential for this population.

Faith-based agencies may help expand the pool of prospective foster and adoptive parents by recruiting through congregations and religious networks. This additional recruitment capacity may be particularly consequential for children who face greater barriers to placement. Cho et al. (2023), for example, emphasize the importance of partnerships with Black communities and targeted recruitment of Black foster and adoptive parents to address disparities in the adoption process. Faith-based agencies may be positioned to expand that pool by recruiting prospective foster parents through religious congregations and networks, including individuals whose willingness to foster is motivated by religious beliefs or a sense of calling. Regulatory accommodations such as religious exemption laws may facilitate this recruitment and specialization, potentially expanding the set of families available for children who otherwise face greater placement barriers.

> Hypothesis 2: Religious exemption status is positively associated with the number of placements for children with disabilities.

> Hypothesis 3: Religious exemption status is positively associated with the number of placements for older children.

> Hypothesis 4: Religious exemption status is positively associated with the number of placements for Black children.

If reduced information asymmetry and stronger reputational monitoring within religious networks improve an agency's ability to identify committed, capable foster parents, this same mechanism may also affect placement quality, not simply placement volume. Length of stay in care is itself a marker of placement difficulty: nationally, children in foster care for less than 12 months achieve placement stability (two or fewer placement settings) at a median rate of 83.5%, compared to just 41.1% among children in care for 24 months or more (Administration for Children and Families, 2018). If specialized recruitment through religious networks improves the durability of matches for harder-to-place cases, this effect should be most detectable among children whose longer stays in care reflect greater placement difficulty, rather than among children whose placements are already largely stable in the short term.

> Hypothesis 5: Religious exemption status is positively associated with placement stability among children in foster care for one year or more.

5 Data and Methods

5.1 Data

This article analyzes AFCARS data for federal fiscal years (FFY) 2012–2023, obtained from the National Data Archive on Child Abuse and Neglect (NDACAN). States have been required to report detailed child welfare information to AFCARS since FFY 1995, following federal legislation enacted in 1986. The underlying AFCARS case-level data include 7,684,126 child-year observations for all states between 2012 and 2023, prior to excluding Puerto Rico for missing data, restricting the sample to non-relative placements, and removing Tennessee's 2023 observation due to the anomaly discussed above.

NCCS Organization Exit Count. To account for the market supply of FFAs, which may affect aggregate placement volume apart from any individual intermediary's performance, and to address potential endogeneity between the number of religious FFAs and RE enactments, the analysis includes state-level counts of nonprofit foster care and adoption organizations drawn from the Urban Institute's NCCS Core Files for 2012–2023. The NCCS data do not disclose which organizations are faith-based, making it impossible to construct a direct count of religious FFAs specifically. As a proxy, the analysis uses total NCCS-registered foster care and adoption organizations (NTEE codes P31 and P32). After filtering to organizations classified under these NTEE codes, the resulting dataset includes 9,159 organization-year records. Then, an organization's EIN is coded as exiting in a given year if it fails to appear in the NCCS Core Files for the three years immediately following its last observed appearance, consistent with the Internal Revenue Service's automatic revocation policy, under which tax-exempt organizations lose their exempt status after failing to file required annual returns for three consecutive years (Blackwood and Roeger, 2011). Because confirming an exit requires observing three subsequent years of absence, this measure cannot be constructed for the final three years of the sample period, and instead, counts exit between 2012 to 2020. This measure may better capture short-run organizational responses to RE adoption than organizational count alone, since the closure of an existing organization is likely to occur, or be detectable in the data, sooner than the formation and registration of a new one.

Time Window. Early adopters, such as North Dakota (2003) and Virginia (2012), enacted exemptions prior to *Obergefell v. Hodges* (2015) and subsequent federal interpretations of sex discrimination extending protections to sexual orientation and gender identity. The pre-2015 period was characterized by substantial variation in the legal and regulatory environment governing foster care, including uneven recognition of same-sex couples and limited federal guidance on nondiscrimination. As a result, states during this period do not provide a consistent counterfactual for later adopters operating under a more uniform post-*Obergefell* legal environment. To improve comparability across sub-experiments, these early adopters are not used as treatment cohorts in the stacked DiD design. The analysis begins in 2012 to provide a consistent three-year pre-treatment period for Michigan (2015), the first cohort included in the estimation. The observation window extends through 2023, consistent with the availability of AFCARS data and last treatment year.

Variable Construction. The AFCARS data includes two child identifier codes to track children in state custody. The number of observations in the public foster care system includes children reported to be served by the public foster care system on the last day of each fiscal federal year (as of September 30) and during the fiscal federal year. This case-level data provides national information on reasons for removal, the characteristics of foster families and children (including demographics, length of days spent in foster care since last removal from home and in current placement, and whether that is a non-relative foster care).

A binary control was added to identify states that adopted a RE law for publicly funded child welfare organizations at various years and durations. The RE status is coded by year, according to the House or Senate Bills that were passed in each state. For the organizational density variable, a subset of the NCCS Core Files was extracted by filtering organizations classified under NTEE codes P31 (Adoption) and P32 (Foster Care), which identify human

service nonprofits operating in the foster care and adoption sectors. The number of such organizations was then aggregated by state and year to construct annual measures of intermediary density. It is important to note that not all organizations coded under P31 or P32 focus exclusively on child placement; many provide ancillary support services such as counseling, mentoring, or summer programming. Likewise, some child-placing FFAs may be coded under adjacent NTEE categories. Although some foster care contractors are privately run and therefore do not appear in IRS nonprofit filings, nonprofit organizations comprise the majority of contracted providers (Committee on Finance, U.S. Senate 2017). Accordingly, variation in the number of nonprofit FFAs following the enactment of religious exemptions still offers a meaningful proxy for shifts in the broader landscape of religious intermediaries.

Dependent Variables. The key dependent variables are the number of *non-relative* placements and *stable* non-relative placements (2 or less moves since the most recent removal from home), aggregated at the state-year level. Total placements do not isolate the role of private FFAs, as they reflect both public and private recruitment efforts. In contrast, non-relative placements more directly capture the contribution of private agencies, since governments prioritize kinship care and typically rely on FFAs when no suitable relatives are available.

To capture placement quality alongside placement volume, two additional measures using the AFCARS variables “NumPlep” (number of placement settings in the child's current foster care episode) and “LatRemLOS” (days since the child's most recent removal) were constructed, following the general logic of the federal Children's Bureau's Child and Family Services Review (CFSR) placement stability standard (ACF, 1999). Both measures were restricted to non-relative placements and calculated only for children with non-missing values. The first measure, *stable under one year*, is the share of children in care for fewer than 365 days with two or fewer placement settings, mirroring the CFSR indicator for children in care less than 12 months. The second, *stable one year or more*, applies the same threshold to children in care for 365 days or longer.

The raw individual data was aggregated by state, identifying the total number of non-relative placements by state and year, and Puerto Rico was dropped because of multiple missing data. Additional key variables used in the analyses include the total number of children categorized by disability status, age group (e.g., 0-8, 9-18), and race. Building on the precedent that the U.S. federal foster care system incentivized families to foster children aged 9 and older (Brehm, 2018), the ‘older’ variable examines non-relative placements for children in this age group.

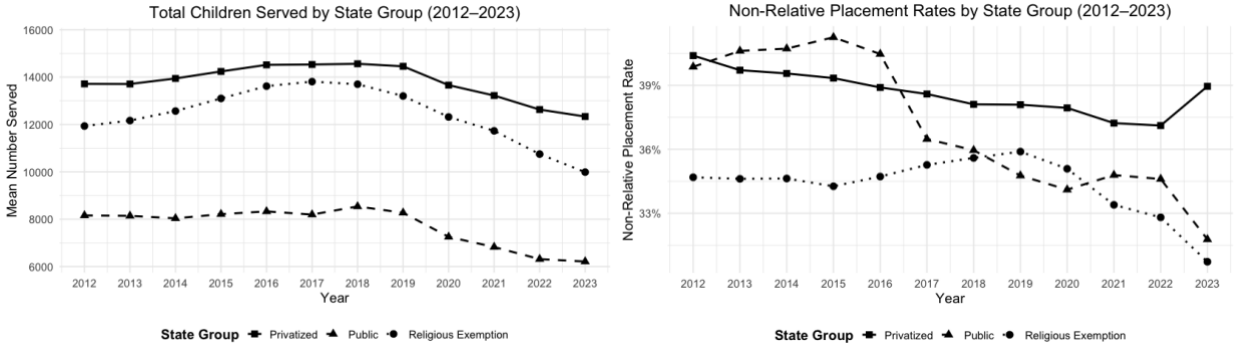
5.2 Covariate Balance

Balancing checks. This section provides descriptive evidence and baseline comparisons to illustrate variation across states and assess the comparability of treated and control groups prior to policy adoption. The proportion of non-relative placements out of the national total number of children served is on average, about 36% in 2023 (appendix 1).

Turning to figure 1, states are grouped into three, based on their child welfare system structure for comparison across time (2012-2023). The public group includes states with public child placement services only, whereas the privatized states are those that have privatized child placement services but have not passed a religious exemption law, and lastly, religious

exemption groups are those with privatized child placement services and religious exemption law passed.

Figure 1. Children served in foster care and non-relative families by state group (2012-2023)



Source: Adoption and Foster Care Analysis and Reporting System (AFCARS), Foster Care AB File 2012-2023, obtained from the National Data Archive on Child Abuse and Neglect (NDACAN).

While the number of children served follows a broadly similar downward trajectory across all three groups from 2019 onward, non-relative placement rates diverge more distinctly. Privatized states maintain an elevated rate throughout the period, while Public and Religious Exemption states, which track closely together before 2016, begin to separate thereafter; Religious Exemption states experience a late-period rise but still end the period below Public states' non-relative placement rate. As this raw group-level pattern may reflect changes in cohort composition and state-specific anomalies rather than a clean treatment effect, this study uses the stacked event-study design introduced below to isolate the effect of RE adoption more precisely.

Table 2. Pre-Treatment Covariate Balance Between Ever-Treated and Never-Treated States

Panel A. Characteristics of Non-Relative Placements (FFA-Relevant Population)

variable	control_mean	treated_mean	difference	std_diff	p_value	sig
older_nr_share	0.320	0.312	-0.009	-0.115	0.403	
disability_nr_share	0.243	0.273	0.026	0.135	0.249	
white_nr_share	0.652	0.743	0.091	0.504	0.000	***
black_nr_share	0.293	0.237	-0.056	-0.299	0.001	***
stable_nr_share_lt1yr	0.830	0.812	-0.014	-0.021	0.026	**
stable_nr_share_1yrplus	0.480	0.430	-0.050	-0.055	0.000	***

Panel B. Broader Foster Care System Characteristics

variable	control_mean	treated_mean	difference	std_diff	p_value	sig
served	12,526.024	13,175.600	649.576	0.047	0.615	
nonrelative_count	4,820.017	4,334.038	-485.979	-0.088	0.250	
relative_count	3,728.755	3,668.362	-60.393	-0.013	0.894	

Panel C. Organizational Context

variable	control_mean	treated_mean	difference	std_diff	p_value	sig
org_exit	1.784	1.312	-0.472	-0.139	0.091	*

Signif. codes: + p < 0.1, * p < 0.05, ** p < 0.01, *** p < 0.001

Note: (Treated = States that eventually adopt RE; Pre-Treatment = Years before state's adoption date)
Source: Adoption and Foster Care Analysis and Reporting System (AFCARS), Foster Care AB File 2012-2023, obtained from the National Data Archive on Child Abuse and Neglect (NDACAN).

Table 2 presents covariate balance tests comparing mean characteristics between states that ever adopt an RE law and those that never do, using only pre-treatment years for the treated group and all available years for the control group. Panel A focuses on non-relative placements and examines the composition of child and foster family attributes. Treated states exhibit higher pre-treatment shares of White children and lower shares of Black children in non-relative foster homes, and worse placement stability for children by having a lower proportion of children who moved foster homes two or less times than control states.

Panel B extends the comparison to overall foster care populations and shows that, before adopting REs, treated states show no statistically meaningful differences with control states in the number of children served and those placed with relatives or non-relatives.

The differences in Panel A likely reflect underlying structural and demographic differences. States that eventually adopt REs tend to be more rural, politically conservative, less racially diverse, and characterized by less Black children in foster care. Because these pre-existing differences could confound comparisons between treated and control states, the main analyses use inverse propensity score weighting based on pre-treatment covariates measured two years before adoption (event time $t = -2$) to improve covariate balance prior to estimating treatment effects. Details of the weighting procedure and balance diagnostics are provided in the empirical strategy section.

The average number of foster care and adoption nonprofit organizations exiting in the sector (Panel C) shows that eventually-treated states already exhibited significantly lower organizational exit rates than never-treated states prior to adoption (1.31 vs. 1.78 exits per state-year; $p < 0.05$), though the standardized difference is modest in magnitude (-0.139). Over the 2012–2023 organizational data period and consistent with the characteristics of treated states described above, organizations in thinner markets may face less competitive turnover, allowing existing agencies to remain active even under financial strain, whereas denser markets with greater organizational competition may exhibit higher baseline rates of exit and replacement. This pre-existing difference reinforces caution in interpreting the organizational exit results as evidence of a causal religious exemption-effect.

5.3 Empirical Strategy

Stacked Difference-in-Differences. Recent research has demonstrated that varied treatment effects over time and between states could introduce bias in the two-way-fixed-effects estimator (Wing et al., 2025). Each state legalized the waiver for publicly funded child welfare agencies under various scopes, timings, and durations. As a result, a stacked DiD model is employed to formulate statistical inferences that the traditional DiD model cannot capture the variation in timing of policy adoption. In the following discussion and table 3, (a) refers to the adoption year defining each sub-experiment (equivalent to the stack index (e), in the equations), whereas (t) denotes calendar year in the estimation models.

The stacked DiD estimator involves dividing states that adopt policies at different years into distinct yearly "sub-experiments" (Freedman et al., 2023). Each sub-experiment comprises states that adopted the policy in a specific year (a), along with any states serving as "clean controls" for this group. These clean controls are states that have not adopted the policy prior to or during the specified interval. The event time (e) lines up various treatment timings according to the adoption year (a) and ideally, each sub-experiment is adjusted to be balanced, ensuring an equal number of pre and post periods.

Table 3. Sub-experiments in the religious exemption staggered adoption design

Sub-experiment (a)	Starting Year	Ending Year	Treated States	Control States	N_Treat	N_Control
2003	Trimmed	Trimmed	ND	-	-	-
2012	Trimmed	Trimmed	VA	-	-	-
2015	2012	2018	MI	TN, AZ, UT, ID, AR, IN, Never Adopted	1	41
2016	2013	2019	MS	TN, AZ, UT, ID, AR, IN, Never Adopted	1	41
2017	2014	2020	AL, SD, TX	AZ, UT, ID, AR, IN, Never Adopted	3	40
2018	2015	2021	OK, SC, KS	AZ, UT, ID, AR, IN, Never Adopted	3	40
2020	2017	2023	TN	ID, AR, IN, Never Adopted	1	38
2022	Trimmed	Trimmed	AZ	-	-	-
2023	Trimmed	Trimmed	UT			
2024	Trimmed	Trimmed	ID	-	-	-
2025	Trimmed	Trimmed	AR			
2026	Trimmed	Trimmed	IN			

Event-Study Specifications. For this article's event-study, the pre-period was defined as three years prior to policy adoption $(a-3)$ and the post-period as three years after $(a+3)$. This window was selected to maximize the inclusion of treated states while maintaining a balanced comparison between treated and control states across presidential regimes with shifting federal policies. The reference year is set to $(e=-1)$ to account for variation in the specific months when states enacted the laws within a federal fiscal year and to better capture policy changes that may begin during the legislative process, prior to the law's formal effective date. States with exemptions adopted after the last calendar year included in the data, namely AZ, UT, ID, AK, and IN fall outside the event-study window, and therefore, are not included as treated states. Tennessee's 2023 observation is also excluded from its cohort's post-treatment window due to a policy-unrelated disruption, evidenced by a sharp, anomalous increase in group home placements (see Appendix 3). For each sub-experiment *e* defined by adoption year *a,* the model is:

$$(1.1)\ Y_{ste} = \sum_{k \in K, k \neq -1} \beta_k 1(EventTime_{ste} = k) + u_{se} + v_{te} + \varepsilon_{ste}$$

Where:

- Y_{ste}= the outcome of interest (non-relative placements, or placement stability among a specified subgroup) in state *s,* year *t,* in sub-experiment *e*
- $EventTime_{ste} = t - a_e$ =event time aligned to the adoption year
- k indexes relative years (e.g., -3, -2, 0, +1, +2, +3)
- Omitted category: $k = -1$ (reference year)
- u_{se}= state x sub-experiment fixed effects
- v_{te}= year x sub-experiment fixed effects
- Clustered SEs at: state

The event-study specification in Equation 1.1 estimates a series of coefficients β_k, each measuring the effect of *k* years from adoption relative to the omitted period $k = -1$. These effects estimate how the outcome variables, including non-relative placements and placement

stability change in treated states relative to clean-control states in each year *k* around adoption. The fixed effects (u_{se}) and (v_{te}) control for time-invariant differences across states and common shocks within each adoption cohort across calendar years.

$$(1.2)\ Y_{ste} = \beta_{stacked} RE_{ste} + u_{se} + v_{te} + X_{st}q + \varepsilon_{ste}$$

Equation (1.2) is a compact stacked DiD form in which the variable $\mathrm{RE}_{\mathrm{ste}}$ is a dummy equal to 1 for states *s* in calendar year *t* within a stack *e* if a RE law is in effect. The coefficient $\beta_{stacked}$ represents the stacked DiD estimator, capturing the average treatment effect of RE laws on non-relative placements relative to non-treated states.

Parallel pre-trends. The difference-in-differences (DiD) design relies on the assumption that, absent treatment, treated and control states would have followed parallel outcome trends. Because states adopting religious exemption laws differ systematically from those that never adopt them (Table 2), the analysis applies the two-stage weighting procedure proposed by Wing et al. (2025), which reweights control states to more closely resemble treated states based on pre-treatment characteristics measured before RE law adoption. This procedure improves covariate balance and reduces bias arising from observable pre-existing differences between treated and control states (see appendix 2). Consistent with the parallel trends assumption, the estimated pre-treatment event-study coefficients remain close to zero and are statistically indistinguishable from zero, providing no evidence of differential pre-treatment trends.

Addressing Covariate Imbalance: Two-stage reweighting procedure. The first stage uses inverse propensity score weighting to improve balance on observed pre-treatment characteristics, while the second stage applies the ATT reweighting procedure to aggregate staggered adoption cohorts appropriately. These steps strengthen the plausibility of the conditional parallel trends assumption before estimating treatment effects. This procedure involves multiplying the inverse propensity score weight$\left(w_{s_{ae}}^{ATT}\right)$ by a reweighting factor $(Q_{s_{ae}}^{w^{ATT}})$ as depicted in Equation (2).

$$(2)\ w_{Final} = w_{s_{ae}}^{ATT} \times Q_{s_{ae}}^{w^{ATT}}$$

The first stage, inverse propensity score weighting (w_{sae}^{ATT}), adjusts for baseline covariate imbalances between treated and control groups to satisfy the conditional common trends assumption (see figure below for the pre-matching event-study). These weights give greater weight to control units that are more similar to treated units based on pre-treatment covariates $X_{s'_a}$ measured at $e = -2$. As depicted in Equation (3), $D_{s_{ae}}$ is equals 1 if state *s* in the sub-experiment *a* is treated at event time, *e* and 0 if not.

$$(3)\ w_{s_{ae}}^{ATT} = D_{s_{ae}} + \left(1 - D_{s_{ae}}\right)\frac{p_{s_{ae}}}{(1-p_{s_{ae}})},$$

The propensity score $p_{s_{ae}}$ is estimated using a model (e.g., logistic regression) that includes covariates such as the total number of children served (proxy for state size) and the racial composition of the served population. The propensity score is defined as:

$$(4)\ p_{s_{ae}} = \Pr\left(D_{s_a} = 1 \middle| X_{s'_a}, e = -2\right)$$

The second stage applies the aggregation weight $Q_{s_{ae}}^{w^{ATT}}$, as shown in Equation (5), to appropriately combine estimates across stacked sub-experiments and event times while presering the target ATT estimand (Wing et al., 2025).

$$(5)\ Q_{s_{ae}}^{w^{ATT}} = D_{s_a} \times \left[\frac{N_a^{D(w^{ATT})}/N_{\Omega_k}^{D(w^{ATT})}}{N_{ae}^{D(w^{ATT})}/N_e^{D(w^{ATT})}}\right] + (1 - D_{s_a}) \times \left[\frac{N_a^{D(w^{ATT})}/N_{\Omega_k}^{D(w^{ATT})}}{N_{ae}^{C(w^{ATT})}/N_e^{C(w^{ATT})}}\right]$$

Where:

- $N_a^{D(w^{ATT})}$: Number of weighted treated states from sub-experiment *a.*
- $N_{\Omega_k}^{D(w^{ATT})}$: Total number of weighted treated states across all sub-experiments.
- $N_{ae}^{C(w^{ATT})}$: Number of weighted control states from sub-experiment *a* in event time *e.*
- $N_e^{C(w^{ATT})}$: Number of weighted control states in event time *e*.
- $N_{ae}^{D(w^{ATT})}$: Number of weighted treated states in sub-experiment *a* at event time *e*
- $N_e^{D(w^{ATT})}$: Total number of weighted treated states at event time *e* across all sub-experiments.

Control units that are more similar to treated units receive higher weights (closer to 1), whereas less similar controls receive lower weights. This approach creates a control group that is more comparable to the treated group in terms of pre-treatment covariates, thereby improving the validity of the parallel pre-trends assumption and leading to more credible treatment effect estimates.

6 Results

6.1 Impact of RE Laws on Non-Relative Placements

Table 4 (Column 1) and Figure 2 present the weighted stacked event-study estimates of the effect of religious exemption laws on non-relative foster care placements. The pre-treatment event-study coefficients provide little evidence of differential trends between treated and comparison states. Specifically, the estimated coefficients at event times −3 and −2 are negative but statistically indistinguishable from zero, lending support to the conditional parallel trends assumption after applying the covariate-balancing weights.

Following adoption, religious exemption laws are associated with increases in non-relative placements. The average post-treatment estimate indicates approximately 398 additional non-relative placements per treated state-year relative to comparison states ($p < .05$). The effect develops gradually rather than occurring immediately after adoption. Although the estimated effect at event time 0 is positive, it is not statistically significant. The estimated effect increases

to approximately 328 additional placements one year after adoption ($p < .10$), peaks at roughly 488 additional placements two years after adoption ($p < .05$) and remains positive three years after adoption ($p < .10$). Relative to a pre-adoption baseline of approximately 5,297 non-relative placements per treated state (measured in the reference year, event time = −1), the estimated average post-treatment effect of 398 additional non-relative placements corresponds to an approximately 7.5% increase following the adoption of religious exemption laws. Overall, the results suggest that religious exemption laws are associated with a short-term increase in non-relative placements, providing support for Hypothesis 1.

Table 4. Estimated effect of religious exemption on non-relative placements by event time

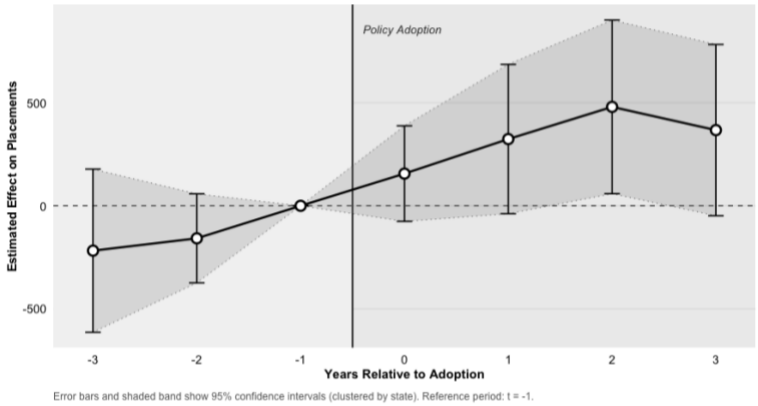
	(1) nonrelative	(2) disability	(3) older	(4) black
A. Post-Treatment Avg. Effect (df=50)				
Treated (=1) x Post (=1)	398* (188)	112 (119)	134+ (71.6)	156* (75.6)
B. Event-studies				
Treated x event_time = -3	-208.8 (198.8)	-235.7 (197.2)	-99.73* (48.28)	-82.79 (59.91)
Treated x event_time = -2	-155.7 (108.1)	-279.9 (170.7)	-47.39+ (27.16)	-64.21+ (32.84)
Treated x event_time = 0	157.2 (116.0)	62.48 (72.43)	29.51 (37.79)	73.30* (33.16)
Treated x event_time = 1	327.7+ (181.6)	174.8 (138.2)	89.22 (60.55)	118.6* (60.24)
Treated x event_time = 2	487.9* (211.3)	142.6 (138.2)	138.3+ (79.94)	191.8* (92.15)
Treated x event_time = 3	378.9+(208.9)	19.72 (128.7)	175.2* (79.67)	158.1+ (89.41)
Observations	1,462	1,462	1,462	1,462
FE: state-sub_exp	Yes	Yes	Yes	Yes
FE: year-sub_exp	Yes	Yes	Yes	Yes
S.E.: clustered by	state	state	state	state

Signif. codes: + p < 0.1, * p < 0.05, ** p < 0.01, *** p < 0.001

Source: Authors' calculations using Adoption and Foster Care Analysis and Reporting System (AFCARS) data, 2012–2023, obtained from the National Data Archive on Child Abuse and Neglect (NDACAN).

Figure 2 illustrates these dynamic effects. The pre-treatment coefficients remain statistically indistinguishable from zero, while the post-treatment coefficients become positive following policy adoption, reaching their largest estimated magnitude approximately two years after enactment before remaining positive through the third post-treatment year.

Figure 2. Event-study of RE on total non-relative placements



Note: Estimates are from a stacked difference-in-differences event-study specification, weighted using inverse propensity score weights, with state-by-cohort and year-by-cohort fixed effects. Standard errors are clustered by

state; shaded bands (or error bars) show 95% confidence intervals. Event time -1 is the omitted reference period. Tennessee's 2023 observation is excluded due to an anomalous increase in group home placements documented in Section 5.3, unrelated to religious exemption adoption.

6.2 Impact of RE Laws on Non-Relative Placements of Children with Disabilities

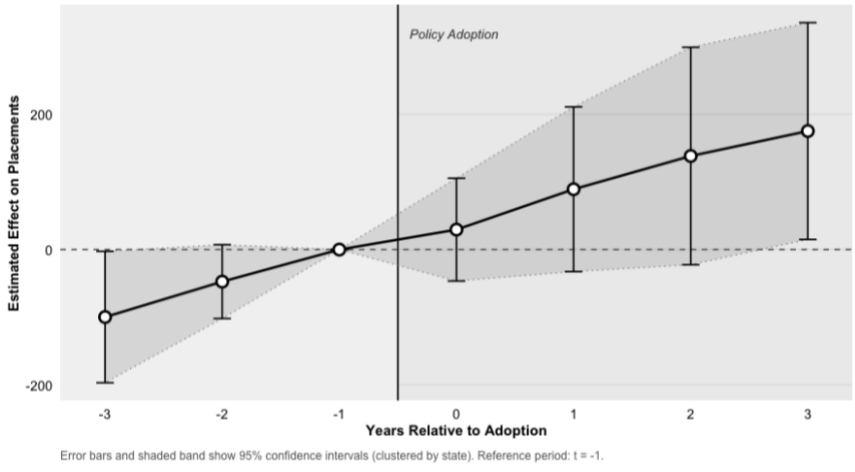
Unlike the overall results, the estimates for children with disabilities do not indicate a statistically detectable effect of religious exemption laws on non-relative placements. Neither the average post-treatment estimate nor any of the event-study coefficients are statistically significant (see Table 4 (Column 2)). Although the estimated effects are generally positive after policy adoption, the confidence intervals remain wide throughout the post-treatment period, suggesting substantial uncertainty around the magnitude of any potential effect.

6.3 Impact of RE Laws on Non-Relative Placements of Older Children

Table 4 (Column 3) presents the event-study estimates for non-relative placements among older children. Pre-treatment coefficients are negative and statistically significant at event time -3 (-99.73, $p < 0.05$) and marginally significant at event time -2 (-47.39, $p < 0.10$), indicating a pre-existing downward divergence between treated and control states in the years before RE adoption. This pattern raises concern about the parallel trends assumption for this outcome and should be weighed when interpreting the post-treatment estimates below.

Post-treatment coefficients are positive throughout the event window, though precision varies. Event time 0 is not statistically distinguishable from zero (29.51, n.s.), while event time 1 is positive but insignificant (89.22, n.s.). The estimated effect strengthens over time, reaching marginal significance at event time 2 (138.3, $p < 0.10$) and full significance at event time 3 (175.2, $p < 0.05$). The post-treatment average effect is 134 ($p < 0.10$), consistent with Hypothesis 3, though only marginally significant. Relative to a pre-adoption baseline of approximately 1,454 non-relative placements among older children per treated state (measured in the reference year, event time = −1), this corresponds to an approximately 9.2% increase following the adoption of religious exemption laws. Given the significant pre-treatment divergence at event time -3, he growing post-treatment effect could reflect a genuine treatment response building over time, or a continuation of a pre-existing trend rather than a discrete break caused by RE adoption.

Figure 4. Event-study of RE on non-relative placements for older children

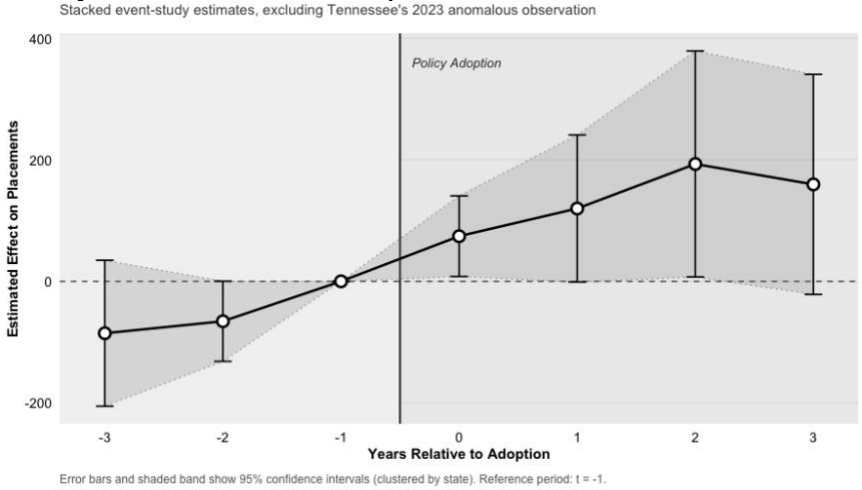


Note: Specification as in Figure 1.
Source: Authors' calculations using Adoption and Foster Care Analysis and Reporting System (AFCARS) data, 2012–2023, obtained from the National Data Archive on Child Abuse and Neglect (NDACAN).

6.4 Impact of RE Laws on Non-Relative Placements of Black Children

Event-study estimates for non-relative placements of Black children are reported in Table 4 (Column 4). Pre-treatment coefficients are negative but only marginally significant at event time -2 (-64.21, $p < 0.10$) and not statistically distinguishable from zero at event time -3 (-82.79, n.s.). Post-treatment coefficients are positive and statistically significant across nearly the entire event window. The effect is significant beginning in the adoption year itself (event time 0: 73.30, $p < 0.05$), and remains significant at event time 1 (118.6, $p < 0.05$) and event time 2 (191.8, $p < 0.05$), before falling to marginal significance at event time 3 (158.1, $p < 0.10$). The post-treatment average effect is 156 ($p < 0.05$), providing support for Hypothesis 4. Relative to the pre-adoption baseline of approximately 1,685 non-relative placements in the reference year (event time = −1), this corresponds to an increase of roughly 9.3%.

Figure 5. Event-study of RE on non-relative placements for Black children



Note: Specification as in Figure 1.
Source: Authors' calculations using Adoption and Foster Care Analysis and Reporting System (AFCARS) data, 2012–2023, obtained from the National Data Archive on Child Abuse and Neglect (NDACAN).

6.5 Impact of RE Laws on Non-Relative Placement Stability

Event-study estimates for placement stability show a pattern that diverges sharply between children in care for less than one year and those in care for one year or more (Table 5). Among children in foster care less than one year, no event-time coefficient is statistically distinguishable from zero, either before or after adoption (event time -3: 1.10, n.s.; event time 2: 0.0024, n.s.). This null pattern is consistent with a baseline stability among treated states' short-stay children which is already high (mean = 81.2%), close to the national median of 83.5% for this population (ACF, 2018).

Table 5. Estimated effect of religious exemption on placement stability share by event time

	(1) % stable_nr_share_lt1yr	(2) % stable_nr_share_1yrplus
C. Post-Treatment Avg. Effect (df=50)		
Treated (=1) x Post (=1)	0.589 (1.05)	2.6** (1.08)
D. Event-studies		
Treated x event_time = -3	1.101 (1.440)	0.1459 (0.8676)
Treated x event_time = -2	0.0024 (0.6531)	-0.5465 (0.6644)
Treated x event_time = 0	-0.4638 (0.7174)	1.207* (0.5286)
Treated x event_time = 1	0.1924 (0.8575)	1.951* (0.9498)
Treated x event_time = 2	0.7007 (1.122)	3.384** (1.066)
Treated x event_time = 3	0.8737 (1.407)	2.463+ (1.465)
Observations	1,462	1,462
FE: state-sub_exp	Yes	Yes
FE: year-sub_exp	Yes	Yes
S.E.: clustered by	state	state

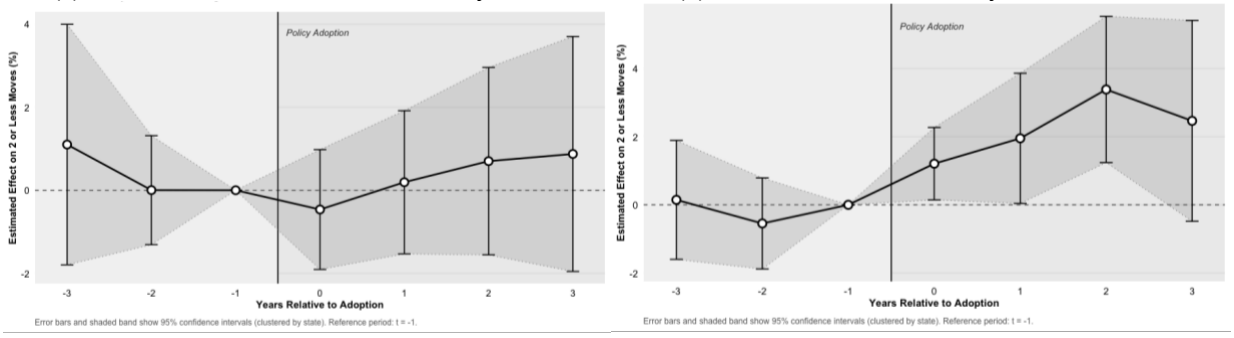
Signif. codes: + p < 0.1, * p < 0.05, ** p < 0.01, *** p < 0.001

Note: Placement stability is measured as the percentage of children with two or fewer placement settings during their current foster care episode, following the Children's Bureau's Child and Family Services Review (CFSR) placement stability standard (Administration for Children and Families, 1999).
Source: Authors' calculations using Adoption and Foster Care Analysis and Reporting System (AFCARS) data, 2012–2023, obtained from the National Data Archive on Child Abuse and Neglect (NDACAN).

Among children in care for one year or more, pre-treatment coefficients are small and statistically insignificant (event time -3: 0.15, n.s.; event time -2: -0.55, n.s.), supporting the parallel trends assumption for this outcome. Following adoption of RE laws, the estimated effect turns positive and grows in both magnitude and precision: 1.21 percentage points in the adoption year ($p < 0.05$), 1.95 points in the first-year post-adoption ($p < 0.05$), and 3.38 points in the second-year post-adoption ($p < 0.01$), before declining slightly to 2.46 points in the third year ($p < 0.10$). Relative to a pre-adoption baseline stability rate of approximately 44.9% among treated states' longer-stay children, the peak effect represents an approximately 7.5% relative increase in the share achieving placement stability. This pattern is consistent with Hypothesis 5.

Figure 6. Event-study of RE on non-relative placement stability

(a) children in foster care less than 1 year (b) children in foster care 1 year or more



Note: Panel (a) shows results for children in non-relative foster care placements for fewer than 365 days; Panel (b) shows results for children in care for 365 days or more. Estimates are from a stacked difference-in-differences event-study specification, weighted using inverse propensity score weights, with state-by-cohort and year-by-cohort fixed effects. Standard errors are clustered by state; shaded bands show 95% confidence intervals. Event time -1 is the

omitted reference period. Tennessee's 2023 observation is excluded, as in all figures (see notes to Figure 1 for further detail).
Source: Adoption and Foster Care Analysis and Reporting System (AFCARS) data, 2012–2023, obtained from the National Data Archive on Child Abuse and Neglect (NDACAN).

6.6 Robustness Checks

Organizational Exit. Event-study estimates for organizational exit are reported in Table 6 and Figure 7. Pre-treatment coefficients are close to zero as shown in event time -3, being negative and marginally significant (-1.79, $p < 0.10$), while event time -2 is small and statistically insignificant (0.04, n.s.). Post-treatment estimates are mixed in sign and generally imprecise. The adoption-year coefficient is negative and statistically significant (-1.08, $p < 0.01$), and event time 2 is negative and marginally significant (-2.73, $p < 0.10$). Event times 1 and 3, however, are not statistically distinguishable from zero (0.12, n.s.; -0.70, n.s.), with event time 1 in fact positive rather than negative. Rather than a sustained, monotonic decline in exits following adoption, the pattern alternates between marginally significant negative estimates and null results of inconsistent sign across the event window. Combined with the marginally significant pre-treatment divergence at event time -3, this instability provides little confidence that religious exemption adoption is associated with a genuine reduction in organizational exit. The analysis does not find clear evidence that RE laws operate through preserving existing organizations that would otherwise leave the market, at least as measured by this proxy.

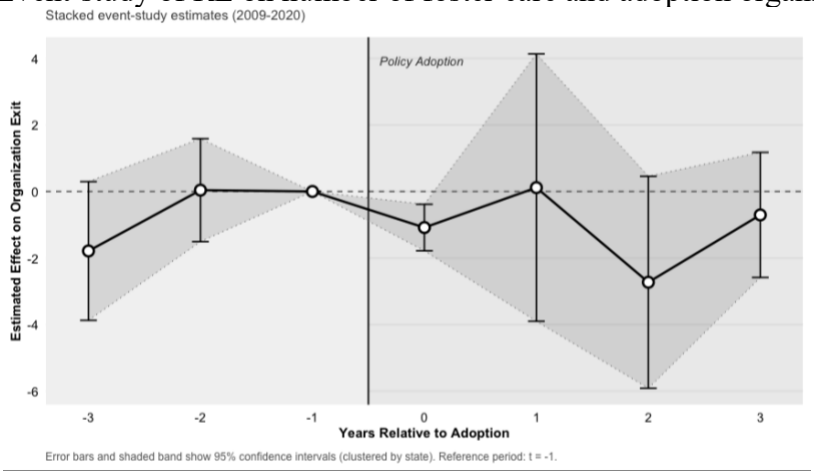
Table 6. Estimated effect of religious exemption on organization exit by event time (2012-2023)

	(1) org_exit
A. Post-Treatment Avg. Effect (df=50)	
Treated (=1) x Post (=1)	-1.11 (1.15)
B. Event-studies	
Treated x event_time = -3	-1.788+ (1.036)
Treated x event_time = -2	0.0396 (0.7689)
Treated x event_time = 0	-1.084** (0.3475)
Treated x event_time = 1	0.1162 (1.995)
Treated x event_time = 2	-2.728+ (1.582)
Treated x event_time = 3	-0.7046 (0.9341)
Observations	910
FE: state-sub_exp	Yes
FE: year-sub_exp	Yes
S.E.: clustered by	state

Signif. codes: + p < 0.1, * p < 0.05, ** p < 0.01, *** p < 0.001

Note: Organizational exit is defined as an organization's EIN failing to appear in the NCCS Core Files for the three years following its last observed appearance, consistent with the Internal Revenue Service's automatic revocation policy for tax-exempt organizations that fail to file for three consecutive years. Because confirming an exit requires three subsequent years of data, exits can only be observed through 2020, the final year for which three years of follow-up data are available within the sample period. Estimates are from a stacked difference-in-differences event-study specification, weighted using inverse propensity score weights, with state-by-cohort and year-by-cohort fixed effects.

Figure 7. Event-study of RE on number of foster care and adoption organization exit



Note: Standard errors are clustered by state; shaded bands show 95% confidence intervals. Event time -1 is the omitted reference period.
Source: Authors' calculations using organizational data from the National Center for Charitable Statistics (NCCS) Core Files, and state religious exemption adoption dates compiled by the authors, 2012–2020.

This null result should not be read as evidence that nondiscrimination requirements never prompt organizational exit. Well-documented cases exist of large faith-based agencies closing foster care and adoption programs in control states without religious exemptions (Goodnow, 2018). However, these closures are not necessarily timed to any particular treated state's adoption year, and this paper's event-study design specifically measures deviations in organizational exit relative to each cohort's own adoption timing. Exits occurring in control states at unrelated points in time would not be expected to produce a systematic pattern within this framework, even if they represent a real and documented phenomenon. The null result is therefore better understood as evidence that organizational exit in control states does not cluster in a way that would bias the comparison against treated states, rather than as evidence against the claim that nondiscrimination requirements can prompt organizational exit.

For the broader analysis, this pattern is genuinely inconclusive rather than clearly reassuring or damaging. One might worry that the placement increases documented above simply reflect a larger supply of organizations in treated states, whether because RE laws prevent existing agencies from closing, or because control states lose agencies at a different rate around the same time. The event-study estimates for organizational exit do not resolve this concern cleanly in either direction: two post-treatment coefficients are negative and at least marginally significant (event times 0 and 2), consistent with fewer exits in treated states, while the remaining post-treatment estimates are statistically indistinguishable from zero, and a marginally significant pre-treatment divergence further complicates a causal reading of any of these coefficients. Given this instability, organizational exit cannot be ruled out, nor confirmed, as a contributing mechanism. Regardless, the magnitude and inconsistency of these estimates make it unlikely that shifts in organizational exit alone could fully account for the substantially larger and more consistent increase in non-relative placements documented above, suggesting that specialized recruitment within existing organizations, likely play a meaningful role.

Relative Placements Because kinship care is generally regarded as the preferred placement option due to its association with greater placement stability and improved child well-being

(Hegar and Scannapieco, 2005), agencies are expected to exhaust available kinship options before seeking non-relative placements. Although federal policy and child welfare best practices prioritize placement with relatives whenever possible, another possible concern is that religious exemption laws could alter placement practices by shifting greater effort toward recruiting and placing children with non-relative foster families. If religious exemption laws instead encourage agencies to direct more children toward non-relative foster families, a corresponding decline in relative placements would be expected following policy adoption.

Table 7. Estimated effect of religious exemption on relative placements by event time

	(1) relative	(2) relative/served
C. Post-Treatment Avg. Effect (df=50)		
Treated (=1) x Post (=1)	-62 (222)	-0.0035 (0.0095)
D. Event-studies		
Treated x event_time = -3	553.7* (217.2)	0.0170+ (0.0098)
Treated x event_time = -2	225.9+ (123.6)	0.0080 (0.0048)
Treated x event_time = 0	-17.10 (116.8)	-0.0040 (0.0049)
Treated x event_time = 1	3.941 (182.5)	-0.0057 (0.0067)
Treated x event_time = 2	-166.9 (249.3)	-0.0095 (0.0116)
Treated x event_time = 3	-23.13 (324.0)	0.0047 (0.0132)
Observations	1,462	1,462
FE: state-sub_exp	Yes	Yes
FE: year-sub_exp	Yes	Yes
S.E.: clustered by	state	state

Signif. codes: + p < 0.1, * p < 0.05, ** p < 0.01, *** p < 0.001

Note: Specification as in Figure 1.
Source: Authors' calculations using Adoption and Foster Care Analysis and Reporting System (AFCARS) data, 2012–2023, obtained from the National Data Archive on Child Abuse and Neglect (NDACAN).

The evidence does not support a sustained decline, but the diagnostic value of this check is weakened by pre-treatment divergence present in both measures (Table 7). Using the raw count of relative placements, pre-treatment coefficients are positive and statistically significant at event times -3 (553.7, $p < 0.05$) and -2 (225.9, $p < 0.10$), indicating that treated states had significantly more relative placements than control states even before RE adoption occurred. The same pattern holds less consistently for relative placements as a share of total children served: event time -3 is marginally significant (0.017, $p < 0.10$), while event time -2 is not (0.008, n.s.). This pre-treatment divergence in both measures complicates the parallel trends assumption for this outcome and limits how confidently the post-treatment results can be interpreted as evidence against substitution.

With that caveat in mind, post-treatment coefficients for both measures are consistently small and statistically insignificant: the post-treatment average effect on the raw count is -62 (SE = 222, n.s.), and on the share is -0.0035 (SE = 0.0095, n.s.), with no individual post-treatment event-time coefficient reaching significance in either specification. Read alongside the pre-treatment estimates, this pattern suggests that treated states' initially elevated relative placement levels converge toward control-state levels following RE adoption, rather than falling below them. This potential pattern is inconsistent with a substitution account, which would predict relative placements falling below control-state levels rather than merely converging toward them.

7 Discussion

Evidence from a stacked difference-in-differences analysis of 2012–2023 AFCARS data shows that allowing intermediaries to concentrate on specific target populations increases non-relative placement volume in the short term. These gains are concentrated among children who typically require greater recruitment and matching effort. On average, treated states place approximately 398 ($p < 0.05$) more non-relative children per year than they would have in the absence of REs, relative to comparable control states, after accounting for fixed effects and weighting. Scaled across all RE-adopting states, this magnitude implies several thousand additional non-relative placements nationally in a typical post-adoption year.

These gains are most pronounced among Black children and, to a lesser and less consistent degree, older children, suggesting that specialization may expand capacity specifically for harder-to-place populations rather than lifting placement volume uniformly across all groups. A natural concern is whether this increase came at the expense of kinship care. The relative-placement analysis offers only partial reassurance on this point. Treated states began the sample period with meaningfully higher relative-placement levels than control states, and this initial gap narrows rather than reverses following RE adoption, with no evidence that relative placements fall below control-state levels. One plausible driver of this narrowing is the broader family preservation movement that unfolded nationally over this period, including the 2018 Family First Prevention Services Act, which expanded support for kinship placement across all states regardless of RE status. If control states, which historically relied less on kinship placement, responded strongly to this shift, the resulting convergence would have little to do with RE adoption itself. Because this scenario involves states responding differently to a policy that applied nationally, rather than a shock common to all states in a given year, it is not something the fixed-effects structure used here can fully rule out, and it weakens confidence in treating the relative-placement results as clean evidence against substitution.

A second dimension of the analysis concerns not just how many children are placed, but how durably. Here, the results are more encouraging. Children who remain in care for a year or more show a meaningful gain in placement stability following RE adoption, climbing from roughly one percentage point in the first year to over three points by the second ($p < 0.01$), with no sign of pre-existing divergence between treated and control states. Children in care less than a year show no comparable movement, but this is unsurprising given how the national average for placement stability among children in foster care for a shorter amount of time is already high. The high national average for this group is partly because a child in care for only a few months has simply had fewer opportunities to experience a third placement change, whereas a child who remains in care for a year or more has had far more time, and more chances, for an early placement to break down. In other words, there is simply less room for a detectable gain among short-stay children, because their baseline is already high and because the "two or fewer moves" threshold is inherently easier to clear over a shorter window.

Length of stay is a plausible, if indirect, marker of placement difficulty. Children who remain in care longer necessarily age further into the system, converging with the "older youth" population already shown above to benefit most from specialization, and prior research documents that Black children in particular experience both longer waits for permanency and greater placement instability (Cho et al., 2023; Foster et al., 2011), suggesting the longer-stay population may disproportionately reflect the same subgroups examined elsewhere in this paper. This paper cannot fully disentangle these explanations with the data available, but either account

is consistent with the pattern observed here that the detectable stability gain concentrates among children whose placement, for whatever reason, has proven harder to stabilize quickly.

The analysis also asks whether RE adoption works partly by keeping existing faith-based FFAs in the market, rather than only by helping them recruit more effectively. The evidence here is unpersuasive either way. Estimates for organizational exit swing between marginal significance and null across the event window, and a divergence between treated and control states already present three years before adoption further undercuts any causal reading. Even taking the negative point estimates at face value, their size is too modest and inconsistent to account for an effect on placement volume as large as the one reported above. This is not to say agency survival plays no role at all as closures tied to nondiscrimination compliance are well documented in states lacking religious exemptions (Goodnow, 2018), but only that the present data cannot confirm it as a meaningful part of the story.

Overall, the pattern that emerges points less toward a larger or more stable population of organizations and more toward existing agencies doing more with the networks they already have. Faith-based FFAs sit inside dense religious communities that generate reputational information, repeated contact, and informal oversight, features prior work links to more effective outreach among populations that are otherwise hard to reach (Monsma, 2006; Lipsky, 2011; Syed et al., 2023; Padot, 2014). Yet the very ties that make this recruitment effective are the same ones agencies may use to decide who does not qualify for their services, and this study's empirical design does not allow the placement and stability gains to be cleanly separated from the exclusionary criteria that religious exemption laws permit.

The estimated effects on placement volume also fade over the post-adoption window. This could reflect events unfolding elsewhere during these later years, including the COVID-19 pandemic and the Biden administration's 2021 rescission of Trump-era exemption waivers. Regardless, the broader takeaway is that a narrow legal change, lowering liability for one type of selectively-serving intermediary, was enough to move both how many children were placed and how stably, while leaving open, and unresolved by this study, who was served in the process and who was not.

8 Conclusion and Limitations

Rather than taking a normative position on whether intermediary selectivity is justified, this article examines its implementation consequences. Government contracting rests on an uneasy premise, where contractors are expected to serve the public as the government itself would, yet they are rarely required, and often unable, to hold the same values as the state that funds them. Religious organizations make this tension especially visible, since their basis for effective service delivery, a shared community of belief, is also a basis for excluding those outside it. This arrangement has been repeatedly contested in courts and public debate while remaining a common feature of government-funded service delivery, raising a question beyond any single sector: *does permitting contractors to specialize by narrowing whom they serve reduce the capacity of public services delivered, or does specialization, through stronger relational networks and more effective matching, increase it?*

Governments often struggle to engage certain groups of recipients, such as seniors or non-English-speaking communities in public health programs, and to identify and mobilize the right providers, including contractors, refugee sponsors, housing providers, or foster families for children under state custody. Matching intermediaries serve as brokers in these settings through

specialized networks and expertise that reduce search costs and information asymmetries between matched agents that governments cannot easily replicate.

Religious exemption laws offer a useful setting for this broader question because they permit certain contractors to recruit from a narrower population than the government itself could require. Although these contractors often depend on substantial public funding, they are not automatically bound by the equal protection obligations that constrain government agencies directly. The result is a form of public service delivery in which the government's chosen instruments for reaching the public do not necessarily share the public's own commitment to equal access.

Exploiting the staggered adoption of religious exemption laws across states, this article evaluates whether legally permitted selectivity is associated with changes in non-relative foster care placements, where faith-based foster care agencies play a particularly important role in recruiting and licensing foster parents. The analysis finds that religious exemption laws are associated with an increase in non-relative placements among Black children with reasonable confidence, and among older children with weaker and less consistent support. Placement gains do not appear to come at the expense of kinship care overall. Religious exemption adoption is also associated with improved placement stability among children who remain in care for a year or more, though no comparable effect is found among more recently placed children. The analysis finds no confident evidence that these effects operate through changes in organizational exit, suggesting that effective recruitment and training practices within existing organizations, rather than a larger supply of organizations, is what drives the increase in placement numbers and stability. Beyond foster care, these findings speak to a tension at the heart of contracting out public services more broadly because gains from letting contractors specialize may be inseparable from a narrowing of who those contractors will serve, and that tradeoff is not unique to religious organizations.

Several caveats are important for interpreting these findings. First, the post-adoption period examined here is relatively short, and subsequent federal policy reversals and the COVID-19 pandemic limit assertions about whether the estimated effects would persist over a longer time horizon. Second, while this study's placement-stability measure offers some evidence on placement durability, it cannot determine whether children achieved improved well-being after placement. Results should not be read as a comprehensive account of child welfare. Third, the stacked difference-in-differences design assumes that, absent treatment, treated and control states would have followed comparable trends. State-by-cohort and year-by-cohort fixed effects and a two-stage weighting procedure strengthen this assumption's plausibility, but unobserved developments that coincide with adoption, including differential responses to concurrent national policy shifts such as the 2018 Family First Prevention Services Act, remain a potential source of bias.

One limitation deserves particular emphasis, because it bears directly on the equal-access side of the tradeoff this paper describes. The placement measures used here do not observe what happens to prospective caregivers turned away by faith-based agencies under religious exemption laws. Separate research documents differential treatment of LGBTQ applicants by such agencies (Mackenzie-Liu et al., 2020; 2022), a cost invisible to aggregate placement counts regardless of whether volume or stability rises. This study likewise cannot speak to the experiences of LGBTQ youth already in care. These are important dimensions of equity that warrant further study in future research.

In conclusion, the findings show that states that passed religious exemption laws experienced higher non-relative placement volume for children who are typically harder to place, alongside improved stability for children with the longest stays, without clear evidence of reduced kinship placement or organizational exit in those states. These results suggest that letting contractors specialize can expand implementation capacity, even as that same specialization narrows whom they serve. This tradeoff deserves equal weight when governments design public-private partnerships.

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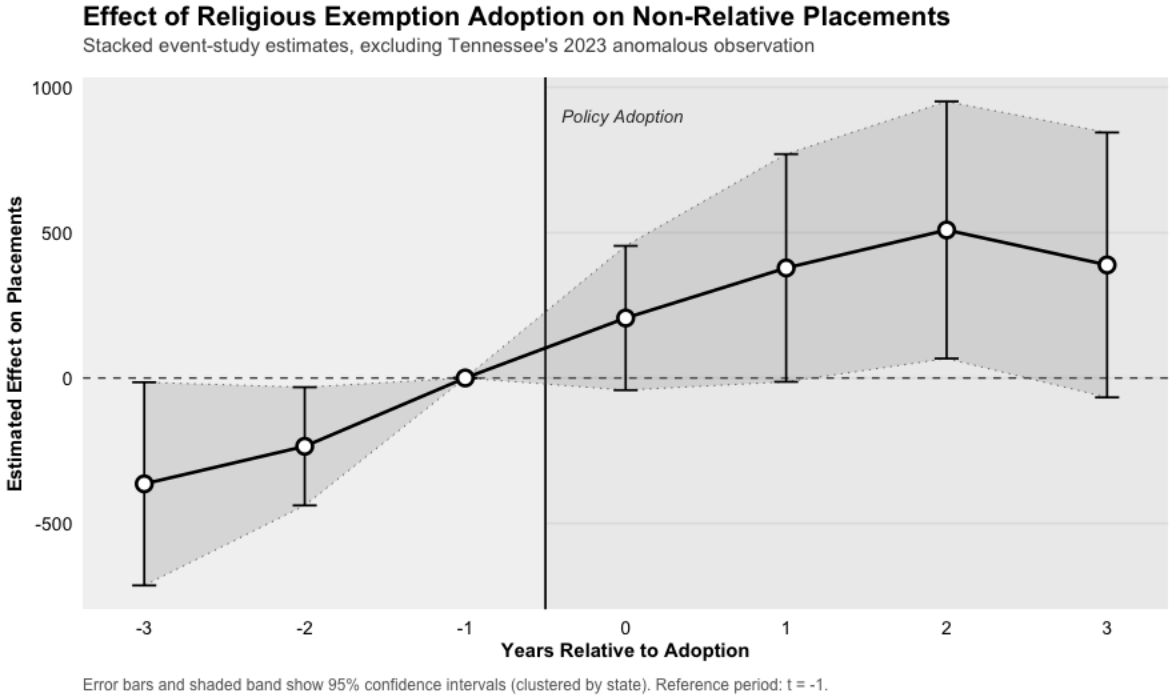
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Appendix 1. Total observations of children served and placed with non-relative families

Year	Total Served	Non-relative placement count	Non-relative placement rate (count/total served)
2012	634,713	245,104	38.62%
2013	638,117	244,146	38.26%
2014	649,903	248,018	38.16%
2015	667,996	253,567	37.96%
2016	684,866	258,485	37.74%
2017	687,116	256,277	37.30%
2018	689,257	255,505	37.07%
2019	676,019	250,383	37.04%
2020	631,254	231,374	36.65%
2021	606,187	216,978	35.79%
2022	569,871	202,672	35.56%
2023	548, 827	196,627	35.83%

(Source: AFCARS)

Appendix 2. Event study before weighting procedures



Appendix 3. Event-study of RE on total non-relative placements (including TN ’23 observations)

