Caring What It Costs: Subsidy Design in Child Care

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Abstract

Child care subsidies can generate large positive externalities by promoting human capital development. An important and understudied feature of their design is the extent to which recipients are shielded from price differences. Such shielding can generate social gains by encouraging the use of higher-quality care. When providers have market power, however, it may also lead to higher equilibrium prices, affecting both subsidized and unsubsidized families. Using administrative data and quasi-experimental variation from the United States, I show that reducing recipients' exposure to price differences shifts them toward providers with higher prices and higher quality ratings. Then, in a novel application of bunching methods, I estimate that providers charge 15 percent more when subsidy recipients are fully shielded from price differences than when they are fully exposed. To quantify the welfare effects of subsidy design, I estimate a structural model of child care demand and supply. I compare a benchmark policy based on federal guidelines, which substantially shields recipients from price differences, to an equally costly fixed subsidy that leaves them fully exposed to those differences. The benchmark policy shifts more subsidized children into high-quality care, but reduces both overall child care use and consumer surplus for subsidized and unsubsidized families. Finally, when quality is observable, I find that tying subsidies directly to quality is more efficient than tying subsidies to price across a wide range of policy specifications.

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1 Introduction

Child care subsidies have the potential to generate substantial efficiency gains. They can encourage investment in child development, for which the social returns may exceed the private returns [Heckman and Masterov, 2007; Heckman et al., 2010]. By lowering the cost of working, they can also offset the employment disincentives created by taxes and transfers [Domeij and Klein, 2013; Kleven, 2014]. Further gains may arise when providers have market power or families face credit constraints [Auerbach and Hines, 2003; Caucutt and Lochner, 2020]. Determining how much to subsidize child care requires weighing these gains against the distortionary costs of raising public funds.

The welfare effects of child care subsidies also depend on how they are designed. This paper studies a central design choice: the extent to which subsidies should shield recipients from price differences. This choice involves a trade-off arising from three features of child care markets. First, evidence suggests that the social returns to child care depend on its quality [Araujo et al., 2019; Andrew et al., 2024]. Shielding recipients from price differences may therefore generate social gains by encouraging families to choose higher-quality care. Under a fixed budget, however, this involves raising subsidies for high-priced care and lowering them for low-priced care, having an ambiguous effect on families' overall participation. Second, child care markets are typically characterized by imperfect competition. If shielding recipients from price differences causes providers to face less elastic demand, providers may raise prices in equilibrium and capture a larger share of the subsidy. Third, evidence suggests that expanding access to child care generates larger gains for disadvantaged children [Cascio and Schanzenbach, 2013; Havnes and Mogstad, 2015]. Consistent with this, child care subsidies are typically means tested, leaving ineligible families directly exposed to subsidy-induced price increases that may reduce their welfare and utilization of care. Whether shielding recipients from price differences improves welfare therefore depends on whether the gains from inducing subsidized families to choose higher-quality care outweigh the associated reductions in consumer welfare and use of care.

I study this trade-off using both quasi-experimental evidence and a structural model of the child care market. Using variation from child care subsidy reforms, I show that shielding recipients from price differences shifts families toward higher-quality care and that participation responds strongly to out-of-pocket prices. In a novel application of bunching methods, I estimate that providers charge 15 percent more when subsidy recipients are fully shielded than when they are fully exposed to price differences. These findings establish the importance of families' participation and quality choices and providers' pricing responses. To assess their joint welfare implications, I estimate an equilibrium model of the child care

market and use it to evaluate alternative subsidy designs, accounting for effects on both subsidized and unsubsidized families.

In practice, the extent to which subsidy recipients are shielded from price differences varies considerably across contexts. For example, in countries such as the United States and Singapore, families receiving a child care subsidy typically pay a co-payment that is independent of the provider's posted price up to a threshold. In other words, recipients often face no additional cost to choosing a more expensive provider. In Australia and France, by contrast, subsidized families typically pay a share of the price up to a threshold, preserving recipients' financial incentive to choose a lower-priced provider.[1] Federal policy documents shed light on the rationale for the US approach. One of the stated purposes of the federal child care subsidy program is to "increase the number and percentage of low-income children in high-quality child care settings."[2] This objective is consistent with a large literature finding that high-quality child care has positive long-run effects on education, employment, health and crime [Schweinhart et al., 2005; Havnes and Mogstad, 2011; Rossin-Slater and Wüst, 2020; García et al., 2023]. Federal guidance then connects the decision to shield families from price differences to this goal, stating that a co-payment that depends on the price of care "incentivizes families to use lower cost care and impedes access to higher cost care."[3] Despite this rationale, there is little evidence on whether reducing recipients' exposure to price differences in fact shifts children into higher-quality care or whether unintended consequences arise.

I begin with a stylized framework to clarify the forces underlying the trade-off between alternative subsidy designs. Define the marginal price faced by a subsidy recipient as the change in the out-of-pocket price associated with choosing a provider charging one dollar more. A classic result from the literature comparing specific and ad valorem taxes and subsidies is that the instrument leaving consumers more exposed to price is superior on welfare grounds [Wicksell, 1896; Suits and Musgrave, 1953]. In a tractable monopoly model, I extend these results to targeted subsidies, showing that for a fixed budget, lowering the marginal price raises the prices paid by both subsidized and unsubsidized consumers and reduces social surplus. Like most of the existing literature, however, these results are derived in a setting with a single market price. When prices differ across providers, a budget-neutral reduction in the marginal price also shifts recipients toward more expensive providers and may raise or lower overall participation. Whether these changes improve welfare depends

[1] Similar variation also exists in contexts other than child care. Housing subsidies in France, the United Kingdom and the United States and food assistance through WIC in the United States all make recipients fully insensitive to price up to a threshold. Housing subsidies in Australia, Germany and Italy and health insurance subsidies in Australia, on the other hand, cover a share of the price.

[2] 45 C.F.R. § 98.1(a)(7).

[3] 81 Fed. Reg. 67438, 67515.

on how price relates to quality and which providers families on the margin of participating would choose, leaving the welfare-maximizing subsidy design an empirical question. The paper therefore proceeds by estimating how families and providers respond to marginal price and evaluating these trade-offs in an equilibrium model of the child care market.

This requires data on families' child care choices and on providers' prices and quality, all of which are difficult to observe. Data on child care use is limited, particularly among unsubsidized families, and providers often do not disclose their prices publicly. Measuring child care quality poses a further challenge, as observable provider characteristics may not fully capture the aspects of care that shape children's development. In addition, the state-level administration of US child care subsidies leaves administrative data fragmented across state agencies. I combine several data sources to address these challenges. In Vermont, I use administrative records on the universe of subsidy recipients, their chosen providers each month, and providers' prices and quality ratings to estimate how subsidy design affects families' choices and providers' prices. Estimating the structural model further requires observing care use among unsubsidized families. Although such data is unavailable in Vermont, I obtain it for Georgia from provider inspection records covering 2017–2026. Along with administrative data from Georgia on subsidized attendance, prices, and quality ratings, these records allow me to model demand from both subsidized and unsubsidized families.

To estimate how exposure to price differences affects families' choice of provider, I exploit a 2022 reform of Vermont's child care subsidy program that sharply reduced the marginal price faced by some recipients but had little effect on the lowest-income families, who already faced a low marginal price. Using a difference-in-differences design, I find that affected families shift toward providers that are both more expensive and have higher quality ratings. The reform increases treated families' probability of attending a center with the highest quality rating by 10.9 percentage points, relative to a pre-reform baseline of 21.9%.

Exploiting a parallel reform that sharply reduced out-of-pocket prices for subsidized families with more than one child, I also find that overall participation in subsidized care responds strongly to the price level. As the framework highlights, however, a budget-neutral reduction in the marginal price lowers out-of-pocket prices for expensive care while raising them for cheaper care. Its effect on overall participation therefore depends on which providers families on the margin of participating would choose. While the reduced-form evidence does not resolve this ambiguity, the structural analysis that follows allows me to evaluate the overall effects of alternative subsidy designs on participation.

The 2022 reform also prompted providers to rapidly raise prices below the threshold up to which recipients were fully shielded from price differences. It had little effect, however, on providers charging above this threshold, whose subsidized customers remained exposed

to price differences. To isolate the effect of reduced price exposure from concurrent changes in subsidy policy, such as the reduction in out-of-pocket prices, I exploit the kink in the subsidy schedule at this threshold. Below the threshold, the subsidy increases dollar-for-dollar with the provider's price, while above it, the subsidy remains fixed and families bear the full cost of any additional price increase. The marginal price faced by subsidy recipients therefore jumps from zero to one at the kink price. This abrupt increase in recipients' price sensitivity generates an observable pricing response, with providers bunching at the kink price. This bunching is substantial, with 4.22 times as many providers located at the kink price as predicted by an estimated counterfactual density. Using this bunching, I estimate that the subsidy schedule raises the marginal bunching provider's weekly price by \$45.68, or 15%, relative to a counterfactual subsidy under which recipients face a marginal price equal to one throughout the price distribution (i.e., a lump-sum transfer conditional on the purchase of child care). I also document similar bunching in response to a kink in Georgia's subsidy schedule.

Applying bunching methods to firms' pricing decisions also raises a methodological question regarding their interpretation and validity when agents' decisions are interdependent. While standard bunching methods treat agents' decisions as independent, firms choose prices in response to those of their competitors. Competition can therefore alter firms' responses to the subsidy schedule and may also bias the bunching estimator. I assess these effects using three approaches. First, I show that bunching estimates are stable across providers facing different degrees of local competition. Second, using the equilibrium model introduced below, I find that providers' price responses change little when competitors' prices are held fixed rather than allowed to adjust. Third, the marginal buncher's price response calculated directly from the model closely matches the estimate obtained by applying bunching methods to the model-generated price distribution. Together, these exercises suggest that competition has little effect on the estimates.

I then estimate a structural model of Georgia's child care market to assess how these demand and pricing responses jointly shape welfare under different subsidy designs. Families choose among child care centers and an outside option, with demand depending on out-of-pocket prices, travel time, provider characteristics, and heterogeneous preferences for center-based care. Providers set prices under multiproduct Bertrand competition. Using the model, I compare a benchmark subsidy based on federal guidelines that substantially reduces recipients' exposure to price differences with an equally costly alternative that preserves full exposure to those differences. Consistent with the reduced-form evidence, the benchmark policy shifts subsidized children toward higher-quality care. However, it also raises provider prices by 20%, increasing profits while reducing consumer surplus for both subsidized and

unsubsidized families. It also reduces the overall utilization of center-based care among both groups. The welfare comparison therefore depends on how the social benefits of care vary by quality and subsidy status. If only subsidized attendance at high-quality centers generates positive externalities, the benchmark is more efficient than the alternative when the value of these externalities per child-year exceeds $34,587. Placing no weight on producer profits, however, raises this threshold to $120,929. The threshold also rises if other child care generates external benefits, either by encouraging parental employment or by also promoting child development. Even with full weight on producer profits, valuing each additional child in other care at $20,000 per child-year raises the threshold to $206,670.

These results suggest that, in order to justify the benchmark policy, external benefits from subsidized care at high-quality centers must be both substantial in magnitude and much larger than those from other care. While some evaluations of intensive early childhood programs do imply benefits of up to $180,000 per year of attendance when accounting for effects on earnings, crime and health, these estimates do not establish how external benefits vary across providers and children [Heckman et al., 2010; García et al., 2020].[4] This variation is critical because the benchmark policy becomes difficult to justify even when external benefits from other care are only a small fraction of those from subsidized high-quality care.

Finally, while this paper primarily focuses on the extent to which subsidy recipients should be shielded from price differences, governments can also encourage higher-quality choices by linking subsidies directly to provider quality ratings [Berlinski et al., 2024; Bodéré, 2026]. Exploiting a second reform in Vermont, I show that families' choice of quality responds to such incentives. I then use the structural model to compare quality-dependent and price-dependent subsidies, holding government spending fixed. Across a wide range of subsidy specifications, subsidies tied to quality ratings that preserve recipients' exposure to price are more efficient than subsidies tied to provider prices. Importantly, this finding relies on quality ratings, which may not capture all aspects of care relevant to children's development, such as how well a provider suits a particular child. If parents recognize these benefits, shielding them from price differences may encourage them to choose care that better supports their child's development.

Related Literature. This paper contributes to the broad literature on the design of taxes, subsidies and insurance. First, it builds on work examining how subsidies shape recipients' choices. Existing studies find mixed evidence. Subsidy expansions have been found to affect the quality of home appliances consumers purchase [Houde and Aldy, 2017],

[4]This approximate value is expressed in 2026 dollars. Further caveats are that these estimates include private benefits that families may already account for when choosing care and that they come from programs that combined early education with other interventions such as home visits or health care.

but to have limited effects on college choices [Carruthers and Welch, 2019]. In housing, Collinson and Ganong [2018] find that increasing subsidy generosity has limited effects on neighborhood quality, whereas linking subsidies to neighborhood quality shifts recipients toward better neighborhoods. Related work studies how cost sharing in health insurance affects utilization [Manning et al., 1987; Chandra et al., 2010; Einav et al., 2016] and price shopping [Brot-Goldberg et al., 2017; Whaley et al., 2017]. My contribution is to examine how recipients' choice of quality depends on the extent to which subsidies shield them from price differences, a dimension of subsidy design distinct from overall generosity. I also examine how these effects compare with those of subsidies tied directly to provider quality.

Second, this paper contributes to research on how tax and subsidy design affect equilibrium prices and incidence. A theoretical literature compares specific and ad valorem taxes and subsidies under alternative market structures [Wicksell, 1896; Suits and Musgrave, 1953; Delipalla and Keen, 1992; Adachi and Fabinger, 2022; Kiso, 2022; Kroft et al., 2024b,a]. This paper extends these findings to *targeted* policies, accounting for equilibrium effects on unsubsidized consumers. This paper also contributes to empirical work on the equilibrium price effects of taxes, subsidies, and insurance. Empirical work has largely focused on how the scale of taxes, subsidies, and insurance affects equilibrium prices [Besley and Rosen, 1999; Gibbons and Manning, 2006; Cellini and Goldin, 2014; Jaravel, 2018], with less attention to the role played by exposure to price differences. Notable exceptions include Cutler and Reber [1998] and Whaley and Brown [2018], who study how consumers' exposure to price differences affects prices in the contexts of employer contributions to health insurance and insurance cost sharing, respectively. Related work by Miravete et al. [2018] uses a structural model to evaluate alternative commodity tax designs. I contribute a novel empirical approach exploiting provider bunching at kinks in subsidy schedules to isolate the effect of exposure to price differences from other policy channels.

Finally, this paper contributes to the literature on optimal tax and subsidy design [Allcott et al., 2015; Fowlie et al., 2016; Griffith et al., 2019; Polyakova and Ryan, 2019; Decarolis et al., 2020; Jaffe and Shepard, 2020; Meckel, 2020; Tebaldi, 2025; Bodéré, 2026]. I develop a framework for evaluating the extent to which subsidies should vary with price or quality under imperfect competition when higher-quality consumption generates positive externalities. The framework captures how these designs shape families' choices and providers' prices, and the resulting effects on welfare. One closely related paper is Meckel [2020], which studies how prohibiting vendors from charging different prices to subsidized and unsubsidized consumers raises prices for the latter on products covered by WIC, a US nutrition assistance program that also largely shields recipients from price differences. Another is Bodéré [2026], which estimates a dynamic structural model of the child care market in Pennsylvania to evaluate

the effects of quality-dependent subsidies and expanded subsidy eligibility, accounting for provider entry, exit, and quality investment. My contribution is to study recipients' exposure to price differences as a dimension of subsidy design and to compare the welfare effects of price-dependent and quality-dependent subsidies.

The remainder of the paper proceeds as follows. Section 2 develops the conceptual framework. Section 3 describes the institutional context and data. Sections 4 and 5 estimate the effects of recipients' marginal prices on their care choices and providers' pricing, respectively. Section 6 presents the structural model, and Section 7 compares alternative subsidy designs. Section 8 concludes.

2 Conceptual Framework

This section develops two theoretical frameworks to examine how shielding subsidy recipients from price differences affects both providers' pricing incentives and consumers' choices. First, in Section 2.1, I extend the classic comparison of specific and ad valorem subsidies under monopoly to targeted subsidies. For a given government budget, both subsidized and unsubsidized consumers face lower prices when subsidies preserve recipients' exposure to price. Like the broader theoretical literature, however, this analysis assumes a single equilibrium price and therefore does not capture important differential effects across providers as well as effects on consumers' choices between providers. In Section 2.2, I focus on this choice response, considering a framework with multiple providers whose prices differ but are held fixed for tractability. I show that a budget-neutral reduction in recipients' exposure to price differences shifts them toward more expensive providers and derive conditions under which overall recipient participation rises or falls. Neither framework alone captures the full set of channels through which subsidy design affects market outcomes, so quantifying the overall effect requires an equilibrium model in which prices, choice of provider, and participation respond together, as estimated in Section 6.

It is useful to begin with the case of perfect competition. In such a market, a demand subsidy raises the equilibrium price when supply is upward sloping, with the extent of the price increase determined by the relative elasticities of supply and demand. In the standard homogeneous-good model, the subsidy has equivalent effects whether paid as a fixed amount per unit purchased (a specific subsidy) or as a fraction of the posted price (an ad valorem subsidy), even though only the latter reduces consumers' exposure to price changes [Auerbach and Hines, 2002].

Under monopoly, however, this equivalence breaks down. A classic result established by Wicksell [1896] and Suits and Musgrave [1953] is that an ad valorem tax raises more revenue

than a specific tax that induces the same reduction in output. For subsidies, the ranking reverses, with specific subsidies achieving a given output at lower cost to the government [Hwang et al., 1997; Kiso, 2022; Dertwinkel-Kalt and Wey, 2025]. Both results reflect the same mechanism, whereby the preferred design, when implemented as a demand-side instrument, leaves consumers more exposed to price changes. An ad valorem tax increases consumers' exposure to changes in the posted price relative to a specific tax, while an ad valorem subsidy reduces it relative to a specific subsidy. In both cases, reducing consumers' exposure to price strengthens the monopolist's incentive to restrict output, so achieving a given output requires a higher equilibrium posted price, resulting in less tax revenue or greater subsidy spending.

2.1 Targeted Subsidy Design

This section extends this problem to targeted subsidies, whereby some consumers are eligible while others are not. While a long literature has compared specific and ad valorem taxes and subsidies, how these results apply when the tax or subsidy varies by consumer remains unclear. In addition, rather than comparing only the two pure instruments, this analysis considers a general linear subsidy schedule $S(p) = a + sp$, which nests the specific ($s = 0$) and ad valorem ($a = 0$) subsidies as special cases. Note that the comparison of *untargeted* ad valorem and specific taxes and subsidies does not depend on whether the statutory incidence is on consumers or suppliers. For a targeted subsidy, however, if firms cannot price discriminate by eligibility, statutory incidence is no longer irrelevant.[5] In practice, targeted subsidies typically lower the price paid by eligible consumers, even when payments are made to sellers on their behalf. I therefore model the subsidy as a demand-side subsidy paid to eligible consumers.

Suppose a monopolist sets a single price p for all consumers. Subsidized consumers receive a subsidy of $S(p) = a + sp$ on each unit they purchase and face the price $p_c = p - S(p)$, while the monopolist receives p on every unit sold. Demand from both groups is strictly decreasing, demand and cost functions are twice differentiable, marginal cost is non-decreasing, and the monopolist's problem has a unique interior optimum at which subsidized consumers purchase a positive quantity. The subsidy satisfies $s \in [0, 1]$ and $a + s\,C'(0) \geq 0$, which ensures it is never negative at a price the monopolist would charge.[6] Define social surplus as the value

[5] Since the monopolist sets a single price, eligible consumers pay less than ineligible consumers when the subsidy is paid to them directly. If the same subsidy is instead paid to the monopolist on eligible sales, and the monopolist is prohibited from price discriminating, then all consumers must pay the same price. So the two cases lead to different equilibria.

[6] The monopolist prices above marginal cost, and marginal cost is never below $C'(0)$, so $p > C'(0)$. As the subsidy is non-decreasing in p, it is therefore at least $a + s\,C'(0) \geq 0$.

of consumption, including an external benefit $e \geq 0$ per unit of subsidized consumption, less its production cost.[7]

Proposition 1. *Suppose the monopolist's profit from unsubsidized consumers, at any fixed level of sales to subsidized consumers, is strictly concave in p. If two subsidies $S(p) = a + sp$ and $S'(p) = a' + s'p$ with $s < s'$ result in the same price for subsidized consumers, then, relative to S, the subsidy S' results in:*

(i) a strictly higher price for unsubsidized consumers,

(ii) strictly higher government spending,

(iii) strictly higher profit for the monopolist, and

(iv) strictly lower social surplus.

Proof. See Appendix A.1. □

Holding fixed the price faced by subsidized consumers, and thereby also their consumption, a subsidy that rises more steeply with price leads to a higher equilibrium price, benefiting the monopolist at the expense of unsubsidized consumers and taxpayers. Unlike in the untargeted case, where the steeper subsidy simply transfers more money from taxpayers to the firm, here it also distorts the allocation to unsubsidized consumers. Since this holds for any level of subsidized consumption the policymaker might target, it holds in particular at the socially optimal level. Whatever that level is, a flatter subsidy achieves it at lower cost to taxpayers and unsubsidized consumers.

This analysis can then be extended to the case of a budget-neutral reform.

Proposition 2. *Maintain the assumptions of Proposition 1. Suppose two subsidies $S(p) = a + sp$ and $S'(p) = a' + s'p$ with $s < s'$ result in the same government spending $G > 0$, and that, at the rate s, delivering a lower price to subsidized consumers requires strictly higher government spending. Then, relative to S, the subsidy S' results in:*

(i) a strictly higher price for subsidized consumers,

(ii) a strictly higher price for unsubsidized consumers, and

(iii) strictly lower social surplus, provided that under S the price for subsidized consumers plus e is no lower than marginal cost.

[7] The analysis is partial equilibrium and abstracts from the distortionary cost of raising the funds for the subsidy.

For a government with a fixed budget, both subsidized and unsubsidized consumers are better off under a subsidy that retains recipients' sensitivity to price than one that reduces it.[8]

These results, like the vast majority of theoretical comparisons of specific and ad valorem policies, are established only in a setting featuring a single equilibrium price, such as monopoly, symmetric oligopoly, or oligopoly with a homogeneous good.[9] By abstracting from price heterogeneity, these analyses overlook an important mechanism. When prices differ across providers, a budget-neutral increase in the ad valorem component s necessarily reduces the subsidy for low-priced providers and increases it for high-priced providers. This has two implications. First, the effect on equilibrium prices may differ across the price distribution, with low-priced providers potentially even lowering their prices. Second, the resulting shift in subsidized demand toward high-priced providers has ambiguous effects on consumer surplus when consumers value providers differently, and on social welfare when consumption externalities differ across providers.

2.2 Subsidy Design and Consumer Choice

To isolate how subsidy design affects consumer choice when prices differ across providers, I now consider a framework with differentiated products and fixed posted prices. I hold posted prices fixed for tractability and to separate consumers' responses from providers' pricing decisions. This allows me to characterize how shielding recipients from price differences affects both their choice of provider and their decision to purchase care. The equilibrium model estimated in Section 6 combines these demand responses with providers' pricing decisions to quantify the effects of alternative subsidy designs.

Consider a model in which families choose to purchase one unit of child care or choose an outside option (utility normalized to zero). Write their utility as

$$u_{ij} = v_{ij} - \alpha[p_j - S(p_j)],$$

where family i choosing option j receives value v_{ij}, faces posted price p_j and receives a potentially price-dependent subsidy $S(p_j) = a + sp_j$, where $s \in [0, 1]$ and $a + s \min_j p_j \geq 0$, so that the subsidy is non-negative at every provider. The subsidized price $p_j - S(p_j)$ can be

[8] Note, some papers also consider "inversely-related" subsidies that decrease with price [Kiso, 2022; Dertwinkel-Kalt and Wey, 2025]. As such subsidies are not observed in the context of child care, I restrict the consideration set to subsidies with $s \in [0, 1]$.

[9] Kiso [2022] compares ad valorem and specific subsidies with heterogeneous prices, but allows the specific subsidy to differ across firms so that each firm's equilibrium price is the same under both policies. Anderson et al. [2001] show that with heterogeneous prices a unit tax can be more efficient than an ad valorem tax, in an example with perfectly inelastic aggregate demand.

negative. Now consider a budget-neutral increase in s. In order to keep the policy budget-neutral, any increase in s must be associated with a reduction in a. Thus, let $a = a(s)$ to maintain a fixed budget. Holding posted prices fixed, the change in utility is

$$\frac{du_{ij}}{ds} = \alpha \left[p_j + \frac{da}{ds} \right].$$

An increase in s lowers out-of-pocket prices at high-priced providers and raises them at low-priced providers, making the former more attractive. This is reflected in $\frac{du_{ij}}{ds}$ being increasing in p_j. Among families purchasing care both before and after the reform, choices therefore shift toward providers charging weakly higher prices. More specifically, $-\frac{da}{ds}$ is the price threshold below which out-of-pocket prices rise and above which they fall. The effect on overall participation, however, is ambiguous, as some providers become more attractive relative to the outside option while others become less attractive.

For a marginal reform, the sign of the effect on participation depends on which providers families at the extensive margin would choose. Let D_j denote the share of families choosing provider j, $D = \sum_j D_j$ the share purchasing care and $D_0 = 1 - D$ the share choosing the outside option. Let m_j denote the density of marginal families whose preferred provider is j, and $M = \sum_j m_j$. Since the utility of provider j changes at rate $\alpha[p_j + \frac{da}{ds}]$,

$$\frac{dD}{ds} = \alpha \sum_j m_j \left[p_j + \frac{da}{ds} \right] = \alpha M \left[\bar{p}^{\,\text{marg}} + \frac{da}{ds} \right],$$

where $\bar{p}^{\,\text{marg}} = \sum_j m_j p_j / M$ is the average price at marginal families' preferred providers. Budget neutrality requires $\sum_j S(p_j) D_j$ to remain constant, so

$$\sum_j \left[\frac{da}{ds} + p_j \right] D_j + \sum_j S(p_j) \frac{dD_j}{ds} = 0 \quad \Longrightarrow \quad -\frac{da}{ds} = \bar{p}^{\,\text{avg}} + \frac{1}{D} \sum_j S(p_j) \frac{dD_j}{ds},$$

where $\bar{p}^{\,\text{avg}} = \sum_j D_j p_j / D$ is the average price paid by families purchasing care. The second term is a fiscal externality: as families switch providers, or enter or leave the market, government spending changes, and a must adjust further to keep the budget fixed. Combining the two expressions,

$$\frac{dD}{ds} > 0 \quad \Longleftrightarrow \quad \bar{p}^{\,\text{marg}} - \bar{p}^{\,\text{avg}} > \frac{1}{D} \sum_j S(p_j) \frac{dD_j}{ds}.$$

That is, participation rises only if marginal families favor more expensive providers than the average family purchasing care by enough to offset the fiscal externality.

If the goal of a subsidy is to change consumption behavior, the optimal relationship between subsidy and price depends on three factors. First, if the goal is to increase the share of families purchasing care, the effect of increasing s is ambiguous: it depends on whether families at the extensive margin are mostly choosing between low-priced care and the outside option or between high-priced care and the outside option. Second, if the goal is to improve the quality of care, it is important to determine whether price is correlated with quality. If it is, raising s induces families to choose higher-quality care, conditional on purchasing care. Third, again if the goal is to improve quality, it matters how the quality of both low-priced and high-priced care compare with the outside option. It may be, for example, that children are best off in high-priced care but better off with the outside option than in low-priced care.

As shown in Section 2.1, making subsidy recipients less sensitive to price reduces social surplus under monopoly. With heterogeneous prices, however, the picture is less clear. Holding prices fixed, a steeper subsidy shifts recipients toward higher-priced providers while having an ambiguous effect on participation. When externalities differ across providers, this reallocation can improve welfare, for example if price is correlated with quality. Determining how subsidy design affects welfare therefore requires weighing these demand-side effects against the equilibrium price responses emphasized in Section 2.1, which in turn requires a model of supply and demand for differentiated products with heterogeneous prices. In Section 6, I estimate such an equilibrium model of the child care market.

3 Context and Data

3.1 Institutional Setting

In 2019, approximately 59.4% of US children aged 0–5 and not yet in kindergarten received some form of non-parental care [National Center for Education Statistics, 2024]. For those aged 3–5, the proportion was 74.4%. The most common form of non-parental care is center-based care, the primary non-parental child care arrangement for 32.1% of children aged 0–5. For 5.8% of children, the primary arrangement is child care in a non-relative's home. This mostly consists of registered child care providers who provide care for up to 6 children in their own home. This is commonly referred to as family child care and is typically cheaper than center-based care and more common for younger children.[10] The other widely-used form of care is with relatives, accounting for 17.3% of children aged 0–5. The US child care industry overwhelmingly consists of private rather than public operators. While chains exist

[10]This form of care can also serve up to 12 children when multiple caregivers operate together in a "group child care home."

and have been expanding, the majority of these providers are small, single-establishment firms or caregivers.

Child care subsidies in the United States are primarily funded through the Child Care and Development Fund (CCDF). In FY 2023, combined federal and state funding for CCDF was approximately $18 billion, providing child care assistance to around 1.62 million children per month. Despite providing the majority of the funding, the federal government only sets broad outlines for how states, territories and tribes should design their subsidy programs. As a result, the design of child care subsidy programs in the US varies greatly from state to state.

Eligibility. Federal regulations condition eligibility for federally-funded child care assistance on income, assets and labor market activity (45 C.F.R. §98.20). Specifically, states cannot use federal funds to assist children in households with assets above $1,000,000 or income above 85% of the state's median income for a family of the same size. Children must also reside with parents who are working or attending a job training or educational program.[11] States can set lower income and asset thresholds. They can also require a certain number of hours of labor market activity. While most states set their income thresholds at or below the maximum allowed by federal guidelines, a few, such as New Mexico, Maine and Vermont, set theirs above the maximum. Such states must use their own funds to assist these additional households.

Children are required to be US citizens or qualified non-citizens. Note, this makes the non-citizen children of many visa holders ineligible. Children must also be under the age of 13 upon determination or redetermination, with some exceptions. Redetermination of eligibility takes place at most once every 12 months, with some states applying longer periods. Within an eligibility period, income is typically allowed to rise above the state-specific eligibility threshold. However, a permanent rise in income above 85% of the state median income ends eligibility immediately in most states.

While recipients must be residents of the state they receive the subsidy from, some states allow residents to use the subsidy at out-of-state providers. For example, Vermont allows payments to out-of-state providers, subject to approval, while Georgia typically does not. Eligible providers can provide center-based, home-based or informal care, where informal care includes non-relative or relative care by an unregulated provider.

Administration. The subsidy application process typically occurs separately from the search for care. Families must submit an application to the responsible state government

[11] Note, eligibility requirements often differ for children who receive protective services.

agency, providing documentation including the parent's proof of identification, residency, income and employment or other activity as well as the child's proof of identification, age and citizenship.

In most states, including those used in this paper, child care assistance payments are made directly to the provider. Statutory incidence, however, is on the subsidy recipient, meaning the consumer pays the difference between the price charged by the provider and the amount the provider receives from the state.[12]

3.2 Data

Historically, provider-level data on enrollment and prices in US child care markets have been difficult to obtain. In particular, many child care providers, including large national chains, do not publicly advertise their prices. Instead, many providers disclose pricing details only upon inquiry or during an in-person tour. Fortunately, the administrative processes surrounding the implementation of child care subsidies in the US often generate useful data. Again, as each state is responsible for the administration of their own subsidy program, the extent of data available varies greatly by state.

For this paper's main empirical analyses, in Sections 4 and 5, I exploit large reforms and rich administrative data from the state of Vermont. I validate my findings on the effects of subsidy design on equilibrium prices in Section 5 in another context, using data from Georgia.

In Section 6, I then estimate a structural model of the child care market, using Georgia rather than Vermont. Although Vermont's setting and data are sufficient for the preceding empirical analysis, as a small, rural state, it is less suitable for demand estimation, providing only a few markets where households face large choice sets. With a population of roughly 11 million, however, Georgia offers many large markets, such as metropolitan Atlanta and several mid-size cities, in which households choose among many providers at a range of travel distances, prices, and qualities.

Vermont. I use administrative data obtained from the Vermont Department for Children and Families through freedom of information requests. These records contain detailed information on child care providers, subsidy recipients and weekly data on subsidy payments for each provider-recipient pair. I use data from the years 2022 and 2023. At the provider-level, the data includes provider names, addresses, licensed capacities, quality ratings and prices

[12]Many states also provide additional subsidies, for which statutory incidence is on the provider. This includes quality bonus subsidies as well as subsidy payments that establish a minimum guaranteed price on subsidized transactions.

as well as the type of care provided. For subsidy recipients, the data includes family size and household income. Descriptive statistics on subsidy recipients and providers are reported in Appendices B.1 and B.2, respectively.

In July 2022, the Department for Children and Families transitioned to a new information system. While a reduction in child care payments occurred in the month of July, there are otherwise no issues in linking the data from before and after the transition.

The price data comes from prices reported to the Department for Children and Families for the facilitation of child care assistance payments. The price data is therefore not always up to date, as providers only have to renew their data every three years. There are, however, two details that mitigate any problems this would cause. Firstly, the transition to the new information system required all providers to provide their latest information, meaning price data was up to date immediately prior to the July 2022 reform evaluated in Sections 4 and 5. Secondly, as subsidy payments depend on prices, providers below the maximum reimbursement rate have a strong incentive to update their prices. Since Section 4 uses pre-reform prices and Section 5 primarily exploits cross-sectional variation in prices at or below the maximum reimbursement rate, this feature of the data is of little concern.

Georgia. For analyses involving Georgia, I use administrative data supplied by the Georgia Department of Early Care and Learning (DECAL). This data includes provider-level characteristics from 2017–2026, including provider names, addresses, licensed capacities, quality ratings and prices.

A component of the data that is key for estimating the model in Section 6 is child care attendance for both subsidized and unsubsidized children. To measure attendance, I obtained administrative data on all inspections of child care centers in Georgia from 2017–2026 from DECAL via freedom of information requests. This data details the number of children of each age present in each room of the child care center during an inspection as well as the number of staff present in each room. On average, centers are inspected twice per year, providing year-to-year measures of the demand for child care.

The model estimation is supported by micro-moments calculated using data from the 2012, 2019 and 2024 editions of the National Survey of Early Care and Education (NSECE), a nationally representative survey of child care supply and demand in the US. The NSECE is particularly valuable, as it provides data on the education and income of families as well as their choices, including whether they purchase child care, what form of child care they choose, how much they pay and how far they travel.

4 How Subsidy Design Affects Consumption

In this section, I present three empirical findings. First, that subsidy recipients respond to a lower price level by increasing the consumption of child care on the extensive margin. Second, that subsidy recipients respond to a lower marginal price by consuming higher-quality child care. In other words, when price differences between cheaper and more expensive providers are reduced, recipients shift towards higher-quality options. Third, I show that subsidy recipients' choice of quality can also be affected by a quality-dependent subsidy. These effects are estimated using reforms to Vermont's child care subsidy program implemented in 2022 and 2023.

4.1 The July 2022 Reforms

In 2016, under the Obama administration, the US Department of Health and Human Services issued a final rule implementing the 2014 reauthorization of federal child care funding. Among its many provisions, the rule resolved an ambiguity in prior federal guidance by prohibiting states from tying child care subsidy recipients' copayments to the price of care.[13] As of today, the federal government therefore requires child care subsidy recipients to be made insensitive to price for any care priced below the voucher amount.

Among the roughly nine states affected by this change, Vermont voiced particularly strong opposition. In comments on the proposed rule, the state wrote that it "most vigorously opposes" the change and would be "compelled to up-end almost 20 years of intentional policy and program development" [Vermont Department for Children and Families, 2016]. The fact that the reform was federally mandated makes it a well-suited setting to study, as it was not adopted in response to developments in Vermont's child care market.

Vermont implemented the change on July 31, 2022. Before the reform, subsidy recipients paid a percentage of the price of care up to a threshold. Afterwards, they paid a flat fee up to a threshold. Under both systems, recipients paid in full for any costs exceeding the threshold. As shown in Appendix Figures C1 and C2, the exact percentage paid prior to the reform and the exact amount of the flat fee were dependent on family income and family size, with co-payments ranging from 0% to 90% before the reform and \$0 to \$200 afterwards. The extent to which families were made less sensitive to price therefore depended on their income and size.

Vermont also implemented two other reforms in July 2022. First, the thresholds above

[13] Note, the "copayment" refers only to payments made for costs incurred within the amount of the voucher. If the price of care exceeds the voucher amount, the family must typically pay the difference in full. This additional payment is not considered part of the copayment.

which recipients became fully sensitive to price differences were raised. Additionally, Vermont replaced per-child copayments with a single copayment per family. A family with two children in care therefore paid the same copayment as an otherwise identical family with one child, rather than twice as much. This was also in response to the 2016 federal rule, which lowered the affordability benchmark from 10% of family income to 7% and more explicitly called on states to justify co-payments above that level.

4.2 The Effect of Price on Utilization

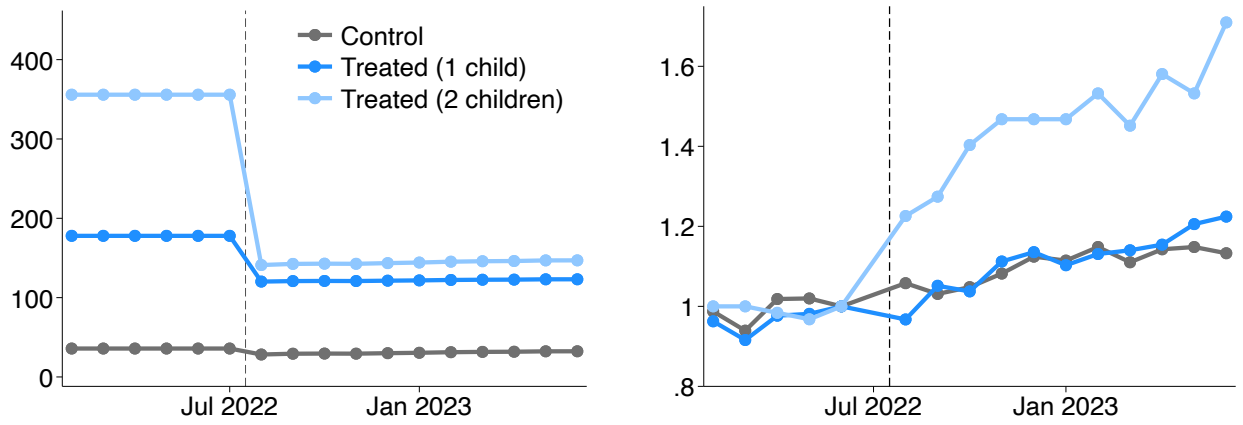
I exploit the large reduction in co-payments for families with multiple children in care to estimate how child care utilization responds to the price level.

Define treated families as those who would have faced a 60–90% co-payment prior to the reform and a $75–125 co-payment post-reform and separate them into families with one child aged 0–5 in child care and families with two children aged 0–5 in care.[14] This definition of the treated group includes the families who faced the largest change in prices while retaining a sufficiently large sample size. For a three-person household, this includes households with monthly incomes of roughly $3,800–5,300 and higher income brackets for larger households.[15] While both groups experienced a drop in the average price faced in the market, families with two children faced a considerably larger drop in the price of care. Families with incomes below a certain threshold paid a 0% co-payment prior to the reform and a $0 co-payment afterwards. The only reform affecting these families, regardless of the number of children they had, was the increase in the voucher amount (i.e., the price threshold above which they become sensitive to price). These families serve as a natural control group, the identifying assumption being that their child care consumption would have followed similar trends in the absence of the reform. For a three-person household, the control group includes households with incomes of approximately $0–1,800, with a larger upper bound applying to larger households. The baseline sample also excludes families in receipt of subsidies for less than a year, as such families are presumably less likely to make consumption decisions based on the subsidy given the long-term and inertial nature of child care. Including these families has little effect on the results, as shown in Appendix C.3.

Panel (a) of Figure 1 shows the weekly cost to a family, after subsidies, of enrolling all

[14]Families are defined as having two children aged 0–5 in care for the entire observation period if they did in any month from February 2022 to June 2023. A potential concern is that the number of children in care is endogenous if families with multiple children respond in the number of children they place in care. Appendix C.2 uses household size rather than number of children, as household size is not endogenous (at least in the short run).

[15]Public assistance benefits, such as TANF cash assistance, SNAP, Medicaid and SSI, are generally excluded from the income used to determine subsidy eligibility and amount. Military pay for individuals on active duty is also excluded. Unemployment insurance benefits are counted as income.



(a) Mean weekly subsidized price per family

(b) Number of subsidized families

Figure 1: Effect of the per-family copayment reform on participation

Notes: Panel (a) shows the mean subsidized preschool price per family averaged across all providers in the market. For the control group, the average is taken for families with different numbers of children in care, weighted by their share. For the pre-reform period, June 2022 prices are used. For the post-reform period, up-to-date monthly prices are used. Panel (b) reports the number of subsidized families for whom subsidy payments for a child aged 0–5 were made each month by group. July 2022 is dropped due to under-reporting. Appendix C.5 presents the same plot including July 2022.

its children aged 0–5 in full-time child care, averaged across all providers in the market. The reform had little effect on costs for the control group. Among treated families, those with one child experienced a reduction in cost, while those with two children experienced a larger reduction. Panel (b) shows how these drops translate into greater utilization of child care for the treated population with 2 children. The baseline observation window is set to February 2022 to June 2023 due to an increase in the voucher amount occurring in February 2022 and another increase in voucher amounts for certain providers occurring in July 2023, as evaluated in Section 4.4. Appendix C.4 presents robustness to other observation windows.

Performing statistical inference on this effect is complicated by the fact that families who do not consume the subsidy are not observed. I therefore use monthly aggregate counts to estimate the following time-series specification,

$$\ln\left(\frac{n_t^{\text{treated,k}}}{n_t^{\text{control}}}\right) = \beta_0 + \beta_1 \,\text{post}_t + \epsilon_t,$$

where $n_t^{\text{treated,k}}$ and n_t^{control} denote the number of families for whom subsidy payments were recorded in month t for the treated group with k children and the control group, respectively. The indicator $post_t$ equals one after the reform. β_1 captures the post-reform change in participation in the treated group relative to the control group.

For families with two children, the estimated coefficient is 0.294 (s.e. 0.030), corre-

sponding to a 34% increase in participation relative to the control group (further results are reported in Appendix C.6). Combined with a relative decline in the average subsidized price of 0.69 log points, this implies a participation elasticity of approximately -0.43. This is comparable to estimates in the literature of -0.25 to -0.6 [Blau and Robins, 1988; Ribar, 1995; Blau and Hagy, 1998; Gathmann and Sass, 2018]. The estimated effect on the treated population with one child is much smaller. This may be explained by the reductions in subsidized prices for this group being considerably smaller at the bottom of the price distribution than at the mean, as reported in Appendix C.7.

4.3 The Effect of Marginal Price on Choice of Quality

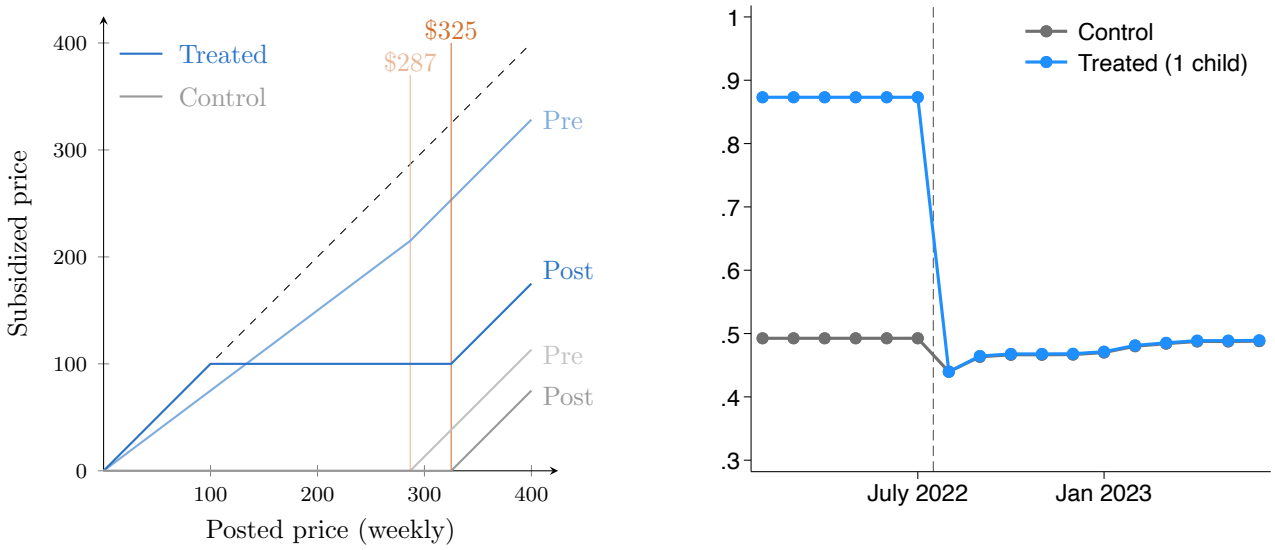
The same reform can be used to estimate how making subsidy recipients less sensitive to price affects their choice of provider. Panel (a) of Figure 2 shows how the subsidized price depends on the posted price for treated families with one child and control families for full-time care for one child at a center with a five-star rating. Price schedules for other types of providers are shown in Appendix Figure C3. While the treated face a range of subsidized prices depending on their income, the figure only plots the price faced by a family at the midpoint of the treated income range, 75% of the posted price up to the voucher amount pre-reform and $100 up to the voucher amount post-reform. Treated families with more than one child are excluded from the analysis due to the large compositional changes documented in Section 4.2.

One way to measure how the reform affected sensitivity to price is to calculate the average marginal price a subsidy recipient would have faced over all providers. Panel (b) of Figure 2 plots this, showing that a representative subsidy recipient in the treated group faced a large drop in the average marginal price faced in the market. The average marginal price faced by the control group drops slightly due to the parallel increase in the voucher amount. Due to the sizable effect on the number of families with multiple children in care, I impose an additional restriction on the baseline treated group, including only families with one child in care.

I estimate the effect of the reform on the characteristics of the chosen provider with the event-study specification

$$Y_{it} = \alpha + \gamma \,\text{Treated}_i + \sum_{\tau \neq -1} \lambda_\tau \,\mathbb{1}[t = \tau] + \sum_{\tau \neq -1} \beta_\tau \left(\text{Treated}_i \times \mathbb{1}[t = \tau]\right) + \varepsilon_{it},$$

where Y_{it} is an outcome for family i in month t, Treated_i indicates membership in the treated group, and t indexes months relative to the reform.

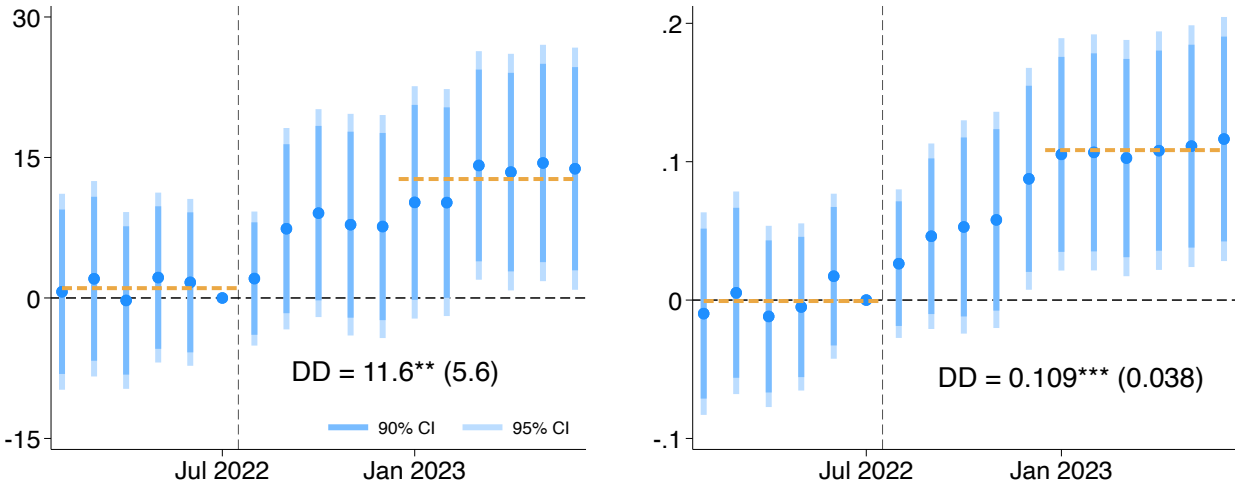


(a) Subsidized price schedule (5-star centers)

(b) Average marginal price

Figure 2: The 2022 Vermont child care subsidy reform

Notes: While the treated group includes recipients who faced co-payments of 60–90% pre-reform and \$75–125 post-reform depending on income, these plots show values for those at the midpoint, with co-payments of 75% pre-reform and \$100 post-reform. Panel (a) plots the subsidized preschool price faced by treated and control subsidy recipients when attending a child care center with a five star quality rating. Price schedules for other types of providers are shown in Appendix Figure C3. The vertical lines at \$287 and \$325 represent the voucher amount. Panel (b) reports the average marginal price faced in the market $\bar{m} = \int m \, dF(p)$, where $F(p)$ is the distribution of prices weighted by licensed capacity. As prices were not updated regularly prior to the reform, panel (b) assumes prices from June 2022 applied to the whole pre-reform period. Following the reform, prices were updated regularly.



(a) Pre-reform price

(b) Attending a 5-star center

Figure 3: Effect of the reform on choice of quality

Notes: Event-study estimates of the difference between treated and control families around the July 2022 reform. Panel (a) uses the pre-reform price of the chosen provider as the outcome. Panel (b) uses the share attending a 5-star center as the outcome. The number of children is 2185 in panel (a) and 2251 in panel (b). Standard errors are clustered at the family level. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Figure 3 plots the quality of providers chosen by treated consumers relative to control consumers in the months around the reform. One proxy for quality is the posted price charged by a center. Since prices may also be affected by the reform, Panel (a) plots the *pre-reform* price of the chosen provider. On average, the reform causes treated families to choose providers with an \$11.6 higher pre-reform price. This is a 4.8% increase relative to the baseline mean of \$240.6. Appendix C.8 also shows that the reform causes treated families to choose providers with a \$21.5 higher *post-reform* price.

An alternative measure of quality is the rating assigned to the chosen provider under STARS, Vermont's child care quality rating system. Over 99% of providers in Vermont have a rating, which can range from one to five stars. Higher ratings reflect points earned for staff qualifications, educational and administrative practices, engagement with families, national accreditation, and performance on external assessments.

Panel (b) of Figure 3 plots the proportion of subsidy recipients choosing a five-star rated child care center relative to the control group. This proportion increases by 10.9 percentage points from a pre-treatment mean of 21.9%.

One potential threat to identification is that voucher amounts are higher for providers with higher quality ratings. By increasing the share of the voucher received by treated families, the 2022 reform also strengthened this quality incentive. To assess whether this explains the change in quality choices, I examine attendance at providers charging at or

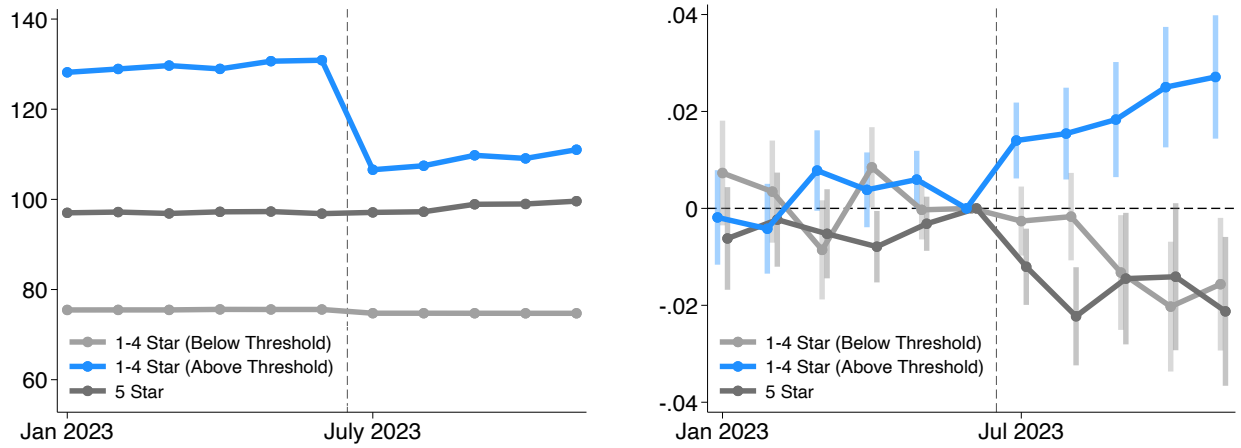
below their respective voucher amounts. After the reform, families faced the same out-of-pocket price at all such providers, so the quality incentive did not affect their relative prices. Appendix Figure C10 shows that treated families became less likely to attend providers that were lower-quality and below the voucher amount and more likely to attend providers that were higher-quality and below the voucher amount, suggesting that the quality incentive alone does not explain the results. Additionally, in July 2023, the voucher amounts for all centers with a 1–4 star rating were raised to the 5 star voucher amount. Appendix Figure C11 shows almost no change in the estimated effects when July–November 2023 are included in the observation window.

Another potential threat to identification is that changes in quality choices may be due to changes in the composition of the treated and control groups. However, balance tests show no significant changes in income or household size over the observation window (see Appendix C.10). Level plots also show effects are driven by the treated group rather than the control group (see Appendix C.11). Robustness to the definition of the treated group is discussed in Appendix C.12. The estimated effects are also somewhat smaller when including individuals in receipt of subsidies for less than a year, consistent with their financial incentives being less affected by the subsidy in the long run (see Appendix C.13).

4.4 Quality-Dependent Subsidies and Choice of Quality

As mentioned above, Vermont implemented another reform one year after the implementation of the reform discussed in the prior sections. Prior to July 2023, voucher amounts were larger for higher-quality providers. This reform increased the voucher amount for all child care centers up to the amount received by five-star centers. Panel (a) of Figure 4 shows the effect of removing this quality incentive on the mean subsidized price for different types of providers. For five-star centers, there was no change. For other centers pricing above the voucher amount, a larger voucher reduced the out-of-pocket price subsidized families paid. For other centers priced below the voucher amount, an increase in the voucher amount has no effect on the out-of-pocket price paid by a subsidized family.

To estimate the effects of this reform on subsidized families' choice of quality, I estimate single-group event studies including in the estimation sample all subsidized families with incomes that would have made them eligible for the subsidy during the full observation window. I use an observation window of January 2023 to November 2023, due to the implementation of another large reform in December 2023. The sample also excludes families receiving the subsidy for less than one year (see Appendix C.14 for robustness to this restriction). The



(a) Mean subsidized price (weekly)

(b) Share attending various providers

Figure 4: Removing quality incentives causes families to switch providers

Notes: Panel (a) plots the mean weekly subsidized price for the different types of child care centers. Panel (b) plots the percentage of families attending various providers, with 95% confidence intervals.

event study estimates the equation

$$Y_{it} = \alpha + \sum_{\tau \neq -1} \beta_\tau \, \mathbb{1}[t = \tau] + \varepsilon_{it},$$

where Y_{it} is an indicator for whether family i attends a given type of provider in month t, and τ indexes months relative to the July 2023 reform.

Panel (b) plots monthly estimates for β_τ for the three outcomes, estimating that, after five months, the share of families attending 1- to 4-star centers priced above the voucher threshold increased by 2.7 percentage points (s.e. 0.6) from a pre-reform mean of 16.9%. Over the same time period, the share of families attending other centers fell.[16] Pooled estimates of the effects on the share of attendance for each group are reported in Appendix C.14.

5 Effects on Equilibrium Prices

This section explores the extent to which reducing subsidy recipients' exposure to price differences affects equilibrium prices. I first estimate the effect of the July 2022 reform in Vermont on prices using a difference-in-differences approach. To complement this analysis, I then use bunching methods to isolate the effect of exposure to price differences on prices. This strategy exploits the fact that subsidy recipients are completely insulated from price

[16]Note, the changes in attendance do not sum to zero as home-based care is not included.

differences below a threshold price and fully exposed to differences above. Providers therefore face much more inelastic demand below the threshold than above, leaving many providers to bunch at the threshold price. This bunching can be used to estimate the effect of the marginal price faced by subsidy recipients on equilibrium prices.

5.1 Price Effects of the July 2022 Reform

The July 2022 reform consisted of moving from percentage copayments to flat copayments, per-child copayments to per-family copayments, an increase in the voucher amount and an increase in the income eligibility threshold.[17] This section estimates the joint effect of these reforms on providers' posted prices.

Figure 5 plots the mean weekly price of preschool care for two groups: centers with pre-reform prices below the voucher threshold and centers with pre-reform prices at or above the threshold. I report these separately because of the distinct effects that the reform would be expected to have on these two groups. Those below the threshold served subsidy recipients who went from mostly being at least somewhat responsive to prices to being completely insensitive to price up to the threshold. Those at or above the threshold, however, served subsidy recipients who were always exposed to price differences both before and after the reform. Heterogeneity in the effects for these two groups may also be driven by the proportion of their customers who are subsidized. While I do not have data on total attendance, I do know each center's licensed capacity. Prior to the reform, subsidized children made up 31% of the licensed preschool capacity for centers priced below the threshold but only 18% for those priced above.

I estimate a price increase of \$20.2 (s.e. \$1.7) between 1 July 2022 and 1 August 2022 for providers below the threshold—an increase of 9.7%. Over the same month, providers at or above the threshold increased their prices by only \$3.9 (s.e. \$1.5). While these single-group price increases are my preferred estimates, I also present difference-in-differences estimates comparing providers below and above the threshold in Appendix D.1.

There are a few caveats to bear in mind when interpreting these results. First, as mentioned previously, this is the joint effect of all four reforms on prices. That being said, the absence of a large effect on the prices of providers above the threshold suggests the primary driver of these price effects is the reduction in the marginal price faced by subsidy recipients. Second, the presence of the threshold itself censors the effects to some extent. If marginal prices were set to zero and the threshold price were much higher, the price response would presumably also be much larger. Third, administrative records of providers' prices may

[17] The analyses in Section 4 are restricted to families with incomes below the pre-reform income eligibility threshold.

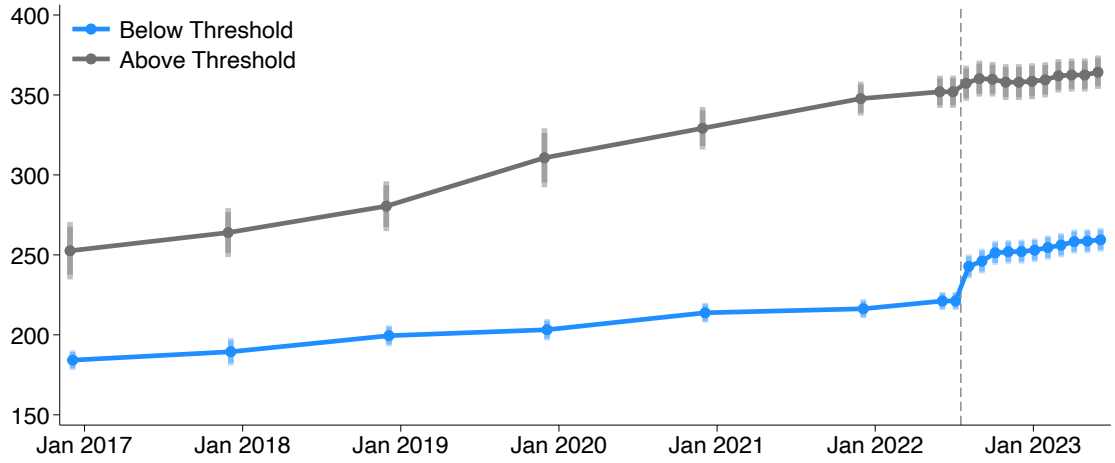


Figure 5: Mean weekly price by group

Notes: Mean full-time weekly preschool prices (nominal dollars) of child care centers. Treated centers are those whose July 2022 price was below the new voucher threshold. Control centers are those at or above it.

not always be up-to-date. In particular, providers below the threshold have an incentive to log any price updates immediately as it directly affects the size of their subsidy payments. Providers above the threshold, however, do not have an incentive to update their prices. A closer look at the data alleviates this concern. In each year from December 2017 to December 2021, at least 49% of centers report a new price. Additionally, 64% of centers that logged a price increase in July–August 2022 had also reported a new price sometime between December 2020 and July 2022 (97.9% between December 2017 and July 2022). This suggests the estimated effects represent actual price changes. This is further supported by the bunching at kink points in the subsidized price schedule observed in the following section.

5.2 Bunching

In this section, I use bunching methods to estimate the equilibrium price response to the marginal price faced by subsidy recipients. This strategy exploits that subsidy recipients are completely insulated from price differences below a threshold price and fully exposed to differences above. Providers therefore face much more inelastic demand below the threshold than above, leaving many providers to price exactly at the threshold. The resulting bunching can then be used to estimate the effect of marginal price on equilibrium prices.

Firm pricing under a kinked subsidy. Typically, bunching methods have been used to estimate individuals' earnings responses to income taxation [Saez, 2010; Kleven, 2016]. I apply the general framework from this literature to estimate firms' price responses to a

kinked demand-side subsidy.[18]

Consider a population of firms producing differentiated products. For now, assume each firm is a monopolist in its local market. Firm j has constant marginal cost c_j. In each market, a constant share σ of consumers are eligible for a subsidy, and pay the subsidized price $p_s(p_j)$ when firm j posts price p_j. Because the firm posts a single price to both the subsidized and unsubsidized groups, it faces demand

$$q(p_j) = \sigma\, q_s\big(p_s(p_j)\big) + (1-\sigma)\, q_u(p_j),$$

where q_s and q_u denote demand from subsidized and unsubsidized consumers, respectively, which I assume are common across firms, and $\varepsilon(p)$ is the resulting price elasticity.

There is heterogeneity in marginal costs, captured by a density $f(c)$. Firm j chooses its price to maximize profit, which yields the optimal price p_j^* satisfying,

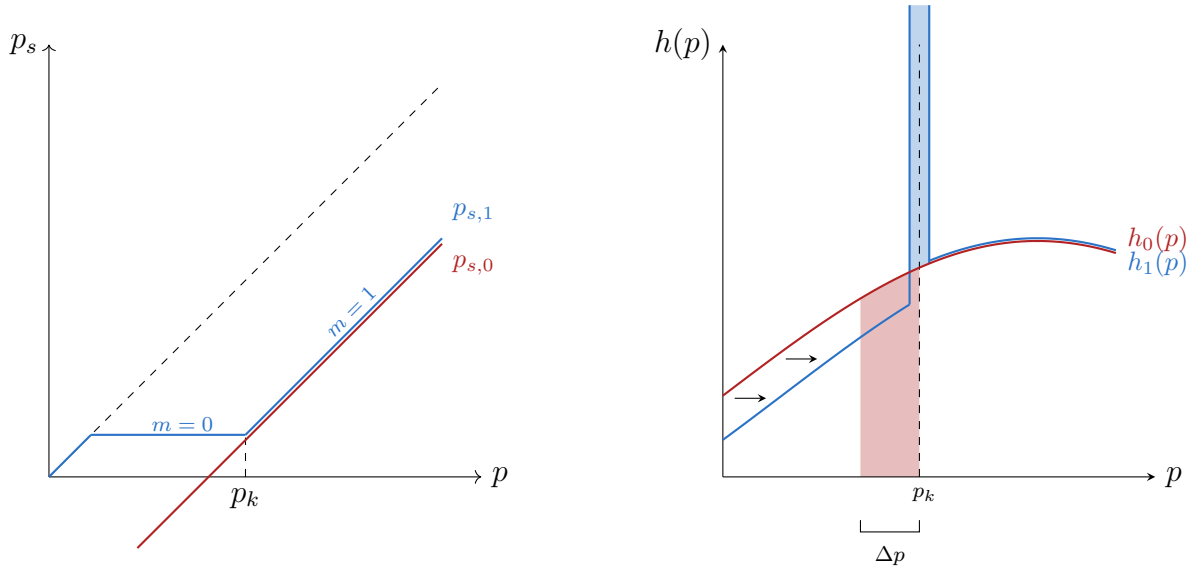
$$p_j^* = \frac{\varepsilon(p_j^*)}{1+\varepsilon(p_j^*)}\, c_j.$$

Consider a baseline linear subsidized price schedule $p_s(p) = mp - a$, so that the marginal price is m. A smooth density $f(c)$ then implies a smooth distribution of baseline prices, which I denote $h_0(p)$. Now suppose the subsidy is increased for prices below a threshold p_k such that the slope of the subsidized price schedule falls from m to $m - \Delta m$ and $p_s(p)$ remains continuous at the threshold (equivalently, the marginal subsidy rate rises from $1-m$ to $1-m+\Delta m$). Subsidized consumers become less price sensitive just below the threshold than just above. Hence, the subsidy causes firm j's price elasticity to vary discretely at the threshold from $\varepsilon_L(p)$ below to $\varepsilon_H(p)$ above.

Under the baseline subsidy, firms are distributed smoothly around the threshold price. Upon the introduction of the kink, the marginal bunching firm increases its price from $p_k - \Delta p_k$ to an interior solution at p_k. All firms initially located in $(p_k - \Delta p_k, p_k]$ choose p_k when facing the kinked subsidy schedule. The density function below $p_k - \Delta p_k$ shifts to the right as those firms choose a new interior solution below p_k. Firms above the threshold are unaffected by the reform.

In reality, responses to the kink may be heterogeneous, for example because firms differ in the share of their consumers who are eligible for the subsidy or in their demand curves. The bunching estimator then approximately identifies the average price response of the marginal bunching firms across firm types.

[18] In methodologically related work, Allcott et al. [2024] estimate bunching responses in the price of used electric vehicles due to a notch caused by a tax credit.



(a) Subsidized price schedule

(b) Bunching illustration

Figure 6: Bunching identification of the price response to the subsidy reform

Bunching estimation. I begin by focusing on the subsidy schedule in place in Vermont from July 2023 to November 2023. Under this schedule, all child care centers face the same kink, which is at $349/week for full-time infant care. Below this threshold, subsidy recipients face a marginal price of zero. Above it, they face a marginal price of one. A simplified version of this price schedule is presented in Panel (a) of Figure 6 as the blue curve.[19] Panel (a) also shows a counterfactual subsidized price schedule, under which the kink is removed and instead the price schedule above the kink extends its slope to prices below the kink.

Bunching arises solely from the discontinuity in the marginal price. For a large kink, however, the estimated price response reflects the change in firms' optimal prices under the two schedules, including the differences in price levels that necessarily accompany the change in slope far from the threshold. The implication of this is that a large bunching estimate will be an underestimate of the effect of marginal price alone. Instead it is the joint effect of a change in the marginal price faced by subsidy recipients and an increase in the price level faced by subsidy recipients.

Panel (b) shows the difference in the cross-sectional distribution of firms' prices. Under the smooth pre-reform schedule, prices are smoothly distributed around p_k, given by $h_0(p)$. After the reform, firms face a discrete drop in their price elasticity as they cross p_k, causing bunching in the actual price distribution $h_1(p)$. Equating the blue bunching mass to the

[19] While the same slopes apply to all subsidized consumers attending child care centers, the levels of the price schedule depend on household income and size. This does not affect the bunching analysis, which relies on the change in slope of the price schedule.

mass below the kink identifies the distance Δp, the price response of the marginal bunching firm.

To estimate the bunching mass, I estimate a counterfactual price distribution, $h_0(p)$, in three steps. I group prices into \$1 bins, indexed by i with bin midpoint p_i, and let n_i denote the number of centers in bin i. First, I fit a polynomial of order $d = 7$ to the bin counts by OLS, excluding the bins around the kink,

$$n_i = \sum_{j=0}^{d} \beta_j p_i^j + \varepsilon_i.$$

Second, because providers tend to set prices at reference points, I add indicators for multiples of \$5, \$10 and \$25 as well as bins \$5 from a multiple of \$25 (where there is often a missing mass). I then estimate a second model in which these indicators are interacted with the predicted counts from the first step,

$$n_i = \sum_{j=0}^{d} \beta_j p_i^j + \sum_{r \in \mathcal{R}} \eta_r \, \hat{n}_i^{(1)} \, \mathbb{1}\left\{ \tfrac{p_i}{r} \in \mathbb{N} \right\} + \varepsilon_i,$$

where $\mathcal{R}$ is the set of indicators and $\hat{n}_i^{(1)}$ are the fitted values from the first step. Third, following Chetty et al. [2011], I iterate on the counterfactual so that the mass added below the kink equals the estimated bunching mass. Specifically, I scale up the counts below the excluded region by a common factor, re-estimate the polynomial, and repeat until the estimated excess mass in the excluded region equals the density added to its left. The estimated bunching mass B is the difference between the observed and counterfactual counts in the excluded region, which is \$349–\$350 in the main specification.

To estimate the marginal buncher's price response, I then solve for $\Delta p_{\text{marg. buncher}}$ such that

$$\int_{p_k - \Delta p_{\text{marg. buncher}}}^{p_k} h_0^{nr}(p)\, dp = B,$$

where $h_0^{nr}(p)$ is a counterfactual distribution estimated without reference points.[20] A distribution without reference points is used for this step due to such estimation methods being sensitive to the location of the kink relative to the reference points. I report results using $h_0(p)$ rather than $h_0^{nr}(p)$ for this step in Appendix D.2. That is, I sum the counterfactual counts in the bins immediately below the kink, moving leftward, until they equal B.

[20] $h_0^{nr}(p)$ sets the density added below the kink equal to the excess bunching mass estimated under $h_0(p)$.

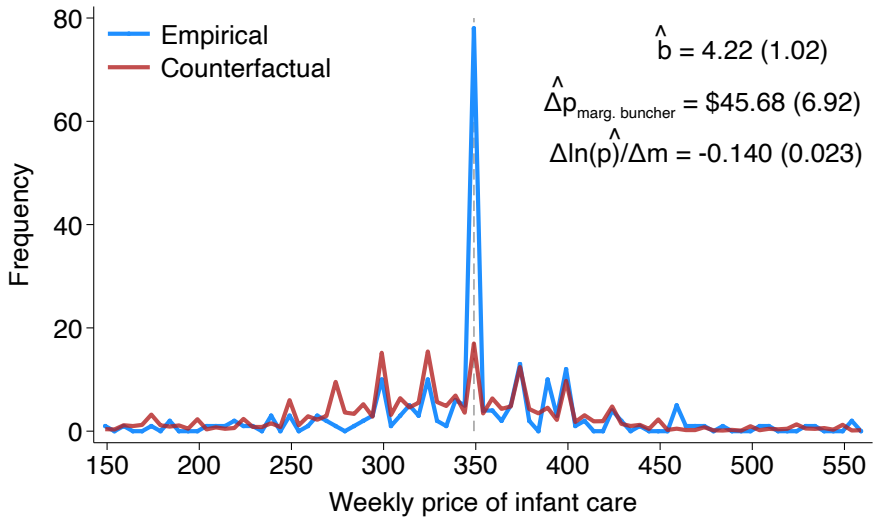


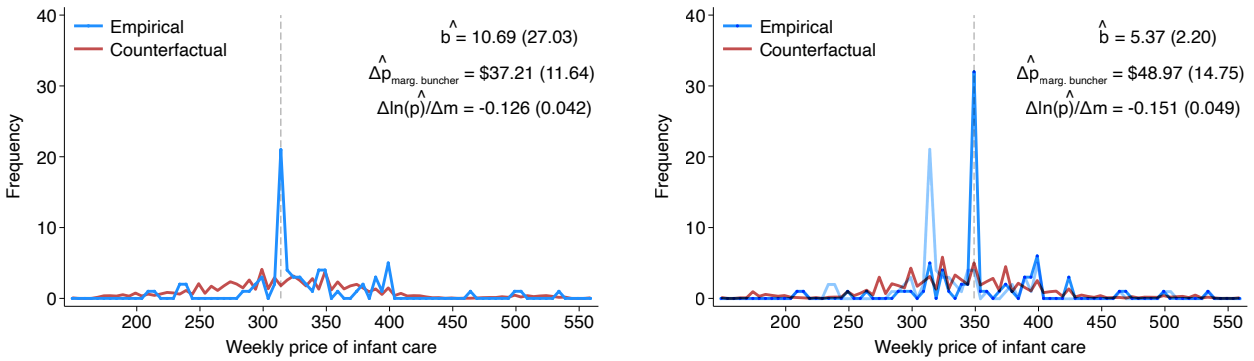
Figure 7: Bunching in the distribution of child care prices, 1 November 2023

Notes: Distribution of posted prices for full-time infant care among all Vermont child care centers on 1 November 2023. A bin width of $5 is used for the plot and a bin width of $1 is used for the estimates. The dashed line is at the kink price of $349. Bootstrapped standard errors based on 100 replications are reported in parentheses.

Results. Figure 7 plots the observed distribution of the weekly price of full-time care for infants in blue. The estimated counterfactual distribution $h_0(p)$ is in red. For visual clarity, the plot applies a bin width of $5, but for precision the estimates apply a bin width of $1. A large mass can clearly be seen bunching in the [$349, $354) bin. When a bin width of $1 is applied to the plot, it can be seen that the vast majority of this bunching occurs at exactly $349 and not $350 (see Appendix D.3). The observed mass at $349 and $350 is 4.22 times what would be expected under the counterfactual distribution. The marginal buncher's estimated price response to a reform moving from the counterfactual subsidy schedule to the actual subsidy schedule is $45.68 (s.e. 6.92). This can also be interpreted as a semi-elasticity: lowering the marginal price faced by subsidy recipients from 1 to 0 causes equilibrium prices to rise by 0.140 in logs (s.e. 0.023), or 15.0%.

While Figure 7 presents the price distribution for infant care (ages 0–1), the same exercise can be performed for toddler care or preschool, defined as ages 2 and 3–5, respectively. Appendix D.4 reports estimates for these age groups.

Bunching moves with the kink, and quickly. Figure 8 shows the effects of the July 2023 reform on the prices charged by child care centers with a 4-star rating. The reform moved the threshold from $314 to $349 for these centers. As can be seen, the previous bunching mass disappears entirely and a new bunching mass appears at the new kink. Furthermore, the estimated semi-elasticity is relatively stable across estimates.



(a) 1 June 2023 (b) 1 November 2023

Figure 8: Bunching moves with the kink (4-star centers)

Notes: Price distributions of 4-star child care centers before and after the kink moved. Panel (b) overlays the empirical distribution from panel (a) in faded blue.

Interpretation and robustness. An alternative explanation for the observed bunching is that it could reflect the threshold's roles as a focal point for price coordination rather than the subsidy schedule's effect on demand. Government-set thresholds have been shown to facilitate such coordination in other contexts Knittel and Stango [2003]. Appendix D.5 shows little to no bunching prior to the July 2022 reform, which fully shielded subsidy recipients from price differences below the kink. This suggests that the threshold's role as a focal point alone cannot explain the observed bunching.

The interpretation of the estimated price response also depends on the size of the kink. For a small kink, the estimated price response is solely due to the change in marginal price. For a large kink, however, while the bunching itself arises solely from the discontinuity at the threshold, the estimated response reflects both the change in marginal price as well as the change in price levels as providers move away from the threshold. The estimated semi-elasticity therefore reflects the joint effect of the change in marginal price between the counterfactual policy and the actual policy as well as the increase in the subsidized price level between where the marginal buncher would locate under the counterfactual policy and the actual policy. If an increase in the subsidized price level reduces demand, the estimated semi-elasticity may be biased towards zero.

A large kink may also affect firm entry and exit. The bunching estimator assumes firms priced at the kink would otherwise have priced below it. Some of the excess mass could instead come from firms that would not have entered the market under the counterfactual policy, which would bias the estimated price response. Two features of the setting make this an unlikely concern. First, the bunching response appears within a few months of a change in the kink, which is likely too short for substantial entry or exit. Second, the estimates are

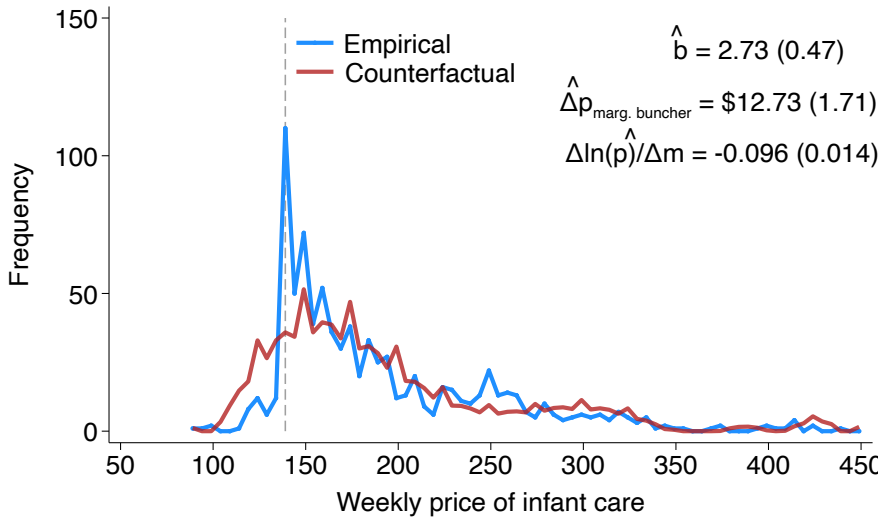


Figure 9: Bunching in Georgia, May 2018

Notes: Price distributions of child care centers in Georgia, May 2018, with estimated bunching at the kink. Distribution of posted prices for full-time infant care among child care centers in Atlanta, Georgia as of May 2018. A bin width of $5 is used for the plot and a bin width of $1 is used for the estimates. The dashed line is at the kink price of $139. Bootstrapped standard errors based on 100 replications are reported in parentheses.

similar when using shorter and longer post-reform periods, suggesting the effects of entry and exit are small.

Such results are also not limited to Vermont. Figure 9 shows bunching in Atlanta, Georgia in 2018. The child care market in Atlanta is very different in nature to the child care market in Vermont, with much lower prices and different policies. The kink price is much lower, at only $139 per week and the proportion of consumers receiving subsidies is lower. I estimate a semi-elasticity of -0.096 (s.e. 0.014). The smaller estimate relative to Vermont is consistent with subsidies having a weaker effect when a smaller share of the population receives them.

Bunching under competition. The analysis so far treats each provider as a local monopolist. While bunching designs generally assume each agent's choice is unaffected by the choices of others, this assumption may be hard to justify for firms setting prices.[21] Below, I highlight three implications of competition for bunching estimation, one concerning the interpretation of the estimates and the other two their validity. I then conduct three validity checks, the results of which suggest that any bias is small.

The first implication of competition is that it can either amplify or dampen providers' price responses to the subsidy schedule. With competition, the marginal buncher responds

[21]The assumption may also fail in other settings where bunching methods are applied. A worker's choice of earnings, for example, may depend on the earnings of other workers.

both to the subsidy schedule and to its competitors' prices. When prices are strategic complements, competitors priced above the kink are not directly affected by the subsidy schedule below, muting the marginal buncher's price response, whereas competitors priced below the kink may also raise their prices, amplifying the response. The price response may therefore be larger when more of the marginal buncher's competitors are priced below the kink. This may also imply that the size of the response depends on where the kink sits in the price distribution. Absent competition, and with homogeneous responses, the marginal buncher's price response is the same regardless of the kink's location. With competition, a kink higher in the price distribution may increase the share of competitors below the kink and thus generate a larger response. This dependence may be weaker, however, if providers compete primarily with similarly priced providers, so that moving the kink also changes the marginal buncher's set of competitors. This dependence on the competitive environment does not itself bias the estimator, but means that the estimated price response is specific to the competitive environment faced by the marginal buncher.

Second, competition can reduce bunching in a way that causes the estimator to understate the marginal buncher's price response. The estimator assumes a provider priced exactly at the kink under the counterfactual schedule remains there under the actual subsidy schedule. Under competition, however, increases in competitors' prices may induce this provider, and others priced below it, to set prices above the kink. While standard bunching theory assumes all providers with counterfactual prices in $[p_k - \Delta p_k, p_k]$ bunch at the kink, under competition the bunching interval may instead be $[p_k - \Delta p_k, p_k - \delta_k]$, with providers whose counterfactual prices lie in $(p_k - \delta_k, p_k]$ locating above the kink. The estimator interprets the smaller bunching mass as a smaller price response, thereby underestimating Δp_k.

Third, competition can bias the estimated counterfactual density. The counterfactual is estimated from the observed distribution on either side of the excluded region, with a correction for the mass that shifts into the kink from below. This relies on providers above the kink being unaffected by the change in the schedule below it, so that their observed prices coincide with their counterfactual prices. Under competition, providers above the kink may instead charge more than they would under the counterfactual schedule, because some of their competitors below the kink have raised their prices. The fitted counterfactual density at the kink may then be too high or too low, biasing the bunching mass in either direction. The resulting bias in the estimated price response is nonetheless likely to be small. Setting aside reference points, the counterfactual density is approximately flat around the kink, so a modest rightward shift in the distribution above the kink should result in little change of the fitted density at the kink.

The first strategy I use to address these two sources of bias exploits variation in the

intensity of local competition experienced by providers. Appendix D.6 reports bunching estimates separately for child care centers with zero, one to two, and three or more competing centers within 5 kilometers. If competition materially affected the estimates, they should vary with the number of nearby competitors. Instead, the estimated semi-elasticity remains remarkably stable across the estimates. These results suggest that competition does not substantially bias the bunching estimates.

The second and third approaches use the equilibrium model of the child care market estimated in Section 6 using data from Georgia. In Appendix F.2, I use the model to generate equilibrium price distributions under the actual subsidy schedule and under the counterfactual schedule that removes the kink by extending the policy above it to prices below. I then construct a third price distribution in which centers choose their optimal prices under the actual schedule while keeping competitors' prices fixed at their counterfactual levels. I find whether or not providers' taking into account competitors' price responses has very little effect on the extent of bunching, suggesting little bias due to competition.

Appendix F.2 then shows the third approach, which generates price distributions using the model and compares bunching estimates from these price distributions to direct calculations of the average response of the marginal bunchers. The two estimates are remarkably close, suggesting that bunching methods are well suited to estimating the price response.

Additionally, Appendix F.2 shows how the estimated semi-elasticity varies for kinks at different locations throughout the price distribution. No clear relationship between the estimated semi-elasticity and the location of the kink emerges. Overall, the presence of competition appears to have minimal effects on the bunching estimates.

6 Model

The empirical results show that subsidy design has large effects on both families' consumption choices and providers' prices. To compare welfare across budget-neutral reforms while accounting for both responses, I estimate an equilibrium model of the child care market. The model follows recent work on structural models of child care markets [Bodéré, 2026].

6.1 Demand

The key determinants of demand are the out-of-pocket price, provider quality, travel time and parents' propensity to participate in the labor market, which I proxy using mothers' college education, a characteristic that is more likely to be predetermined. Modeling this, household

i chooses among child care providers j in year t. Household i's utility from provider j is

$$u_{ijt} = \gamma \mathbb{1}\{\text{college}_i\} - \sigma \nu_i - \alpha p_{jt}(y_i) - \lambda d_{ij} + \boldsymbol{x}'_{jt}\boldsymbol{\beta} + \xi_t + \xi_j + \Delta\xi_{jt} + \varepsilon_{ijt}, \tag{1}$$

where the first two terms capture households' overall valuation of center-based care relative to the outside option. The term $\gamma \mathbb{1}\{\text{college}_i\}$ allows college-educated households to differ in this valuation and $\nu_i \sim N(0,1)$ is a household-specific draw, common to all providers, that adds unobserved variation in households' taste for care with scale $\sigma \geq 0$. Next, $p_{jt}(y_i)$ is the out-of-pocket price faced by a household with income y_i and d_{ij} is the travel time to the provider, with α and λ the disutility of each. Provider attributes such as quality rating and opening times are denoted $\boldsymbol{x}_{jt}$, year and provider fixed effects are denoted ξ_t and ξ_j, and a provider-year demand shock is denoted $\Delta\xi_{jt}$. ε_{ijt} is an idiosyncratic taste shock. Utility for the outside options, parental, relative and home-based care, are normalized to zero. The subsidy policy determines the out-of-pocket price. For a household that receives a subsidy, $p_{jt}(y_i)$ is the posted price net of the subsidy, as given by the subsidy schedule, whereas households that do not receive a subsidy pay the posted price. Formally, a household is eligible for the subsidy if its income y_i is below the eligibility cutoff $\bar{y}_t$. Letting $f_{jt}(y_i)$ denote the weekly family fee implied by the subsidy schedule and R_{jt} the maximum reimbursement rate that applies to provider j, the out-of-pocket price is

$$p_{jt}(y_i) = \begin{cases} p_{jt} & \text{if } y_i \geq \bar{y}_t \text{ or } p_{jt} < f_{jt}(y_i), \\ f_{jt}(y_i) & \text{if } y_i < \bar{y}_t \text{ and } f_{jt}(y_i) \leq p_{jt} < R_{jt}, \\ f_{jt}(y_i) + p_{jt} - R_{jt} & \text{if } y_i < \bar{y}_t \text{ and } p_{jt} \geq \max\{f_{jt}(y_i), R_{jt}\}. \end{cases} \tag{2}$$

An eligible household therefore pays the posted price if it is below the family fee, pays only the family fee if the posted price is below the maximum reimbursement rate, and pays the family fee plus any amount by which the posted price exceeds the maximum reimbursement rate otherwise. In the middle region the household's out-of-pocket price does not respond to the posted price. The family fee is a percentage of annual household income that rises across income brackets. The schedule varies by year and county. Equation (2) is the schedule used in Georgia. In the counterfactuals in Section 7, I also consider alternative schedules.

6.2 Supply

Providers set prices under multi-product Bertrand competition. Firm f operates a set of providers $\mathcal{J}_{ft}$ and jointly sets the posted prices of all of its providers to maximize profit,

$$\max_{\boldsymbol{p}_{ft}} \sum_{j \in \mathcal{J}_{ft}} (p_{jt} - mc_{jt})\, s_{jt}(\boldsymbol{p}_t), \tag{3}$$

where mc_{jt} is the marginal cost of provider j and $s_{jt}(\boldsymbol{p}_t)$ is its market share, which depends on the vector of all posted prices $\boldsymbol{p}_t$ through the demand system above and through the subsidy schedule. A provider posts a single price for subsidized and unsubsidized families.

The model assumes providers are not capacity constrained. I discuss this assumption further in Appendix E.1, where I show that in any given year roughly a quarter of providers operate at or very close to capacity. However, I also show that these constraints are primarily driven by personnel rather than space and that providers that are constrained in one year on average increase their capacity by 3.27 children in the following year. I therefore view the context as one in which centers face capacity constraints in the short run, but where these centers can adjust their capacity in the medium run.

6.3 Identification and Estimation

I estimate the model using data from Georgia in 2017–2026. Markets are predominantly modeled on commuting zones (see Appendix E.2). Households are modeled as families with toddlers (defined in the Georgia child care market as ages 1–2). I use the 2020 census to estimate market sizes and generate block-level residential locations for each household. To generate these locations, I use the distribution of block-level residential locations for all households with children aged 0–5 and assume households with toddlers face the same distribution over the full estimation period. I model households' education and income using block-group level estimates from the American Community Survey and assume these to be constant over time, after adjusting for inflation. The providers are child care centers, for which I observe locations, posted prices and attendance each year.

I estimate the demand parameters using generalized method of moments to match market shares [Berry et al., 1995]. I also match subsidized attendance shares and micro-moments on distance between home and the provider as well as participation by education obtained from the 2012, 2019 and 2024 waves of the National Survey of Early Care and Education. I also match the extensive-margin consumption response to the Vermont reform documented in Section 4.2. Using the model, I compute the change in the probability that subsidy-eligible families use child care when their out-of-pocket price moves from the pre-reform schedule,

in which families pay on average 75 percent of the posted price, to the post-reform flat fee. I set the flat fee so that the average out-of-pocket price falls by the 0.69 log points observed in Vermont and I match the model's log change in participation to the estimated response of 0.294 (s.e. 0.030).

I instrument for prices using competitors' characteristics, following Berry et al. [1995], as well as variation in subsidy policy. Specifically, I use a distance- and capacity-weighted measure of the number of nearby high-quality providers and the subsidy's maximum reimbursement rate, which, as shown in Section 5, causes providers to raise their prices. The Kleibergen–Paap F-statistic for these instruments is 46.41.

Table 1: Demand estimates

Nonlinear parameters		Estimate	Std. error
Price	α	0.0126	(0.0127)
College household	γ	4.24	(4.29)
Travel time (minutes)	λ	0.179	(0.0248)
SD of unobserved taste for care	σ	4.04	(3.41)

Notes: The sample includes 19,969 center-year observations from the 2017–18 school year to the 2025–26 school year.

6.4 Parameter Estimates and Model Fit

Table 1 reports the demand estimates. The coefficient on college education is positive, indicating that college-educated households are more likely to use center-based care, and the coefficient on travel time is negative, so households prefer providers that are closer. The median own-price elasticity of demand is -2.28.

In Section 7, I use the model to estimate how counterfactual subsidy policies affect welfare, where welfare includes consumer surplus, producer surplus and the social benefits of changes in child care consumption, both at the extensive margin (whether families use child care) and the intensive margin (the quality they choose). These estimates are only reliable if the model correctly predicts how consumption and prices respond to changes in subsidy design. I target the extensive margin response when estimating the model, using the response estimated in Vermont in Section 4.2, and the model reproduces it closely. Appendix E.3 reports the fit to this and the other targeted moments. The responses I do not target, and therefore need to check, are how changes in the marginal price affect consumers' choice of quality and firms' prices.

Table 2 reports the results of these validity checks. I first use the effect of the marginal

Table 2: Model validation of consumer and firm responses to the marginal price

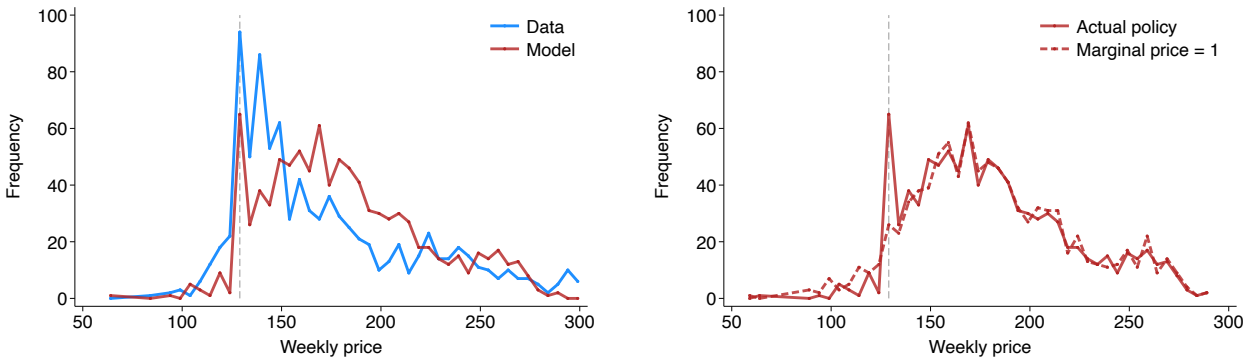
Effect of marginal price on...	Data	Model
Price chosen by consumers	−0.127	−0.113
	(0.061)	
Providers' posted price	−0.067	−0.083
	(0.011)	

Notes: Semi-elasticities with respect to the marginal price faced by subsidy recipients. The first row is the percent change in the pre-reform price of the chosen provider per unit change in the average marginal price. The data estimate is a difference-in-differences from the July 2022 reform in Vermont. The model estimate compares a 75% coinsurance schedule with a flat fee schedule in Georgia, holding provider prices fixed. The second row is the response of the weekly price of full-time toddler care to the marginal price, from bunching at the kink in Atlanta, Georgia, as of May 2018. The model estimate uses equilibrium prices with marginal costs predicted from a regression that excludes bunching providers. Standard errors are in parentheses. Standard errors are clustered by family in the first row and bootstrapped with 100 replications in the second.

price on the price of care chosen by consumers, estimated in Section 4.2, to compute a semi-elasticity of −0.127. Assuming a linear response, this means that moving from a subsidy that fully shields consumers from price differences ($m = 0$) to one that fully exposes them ($m = 1$) reduces the price they choose by 12.7 percent. To separate this from the equilibrium price response, I measure the effect on the pre-reform price of the chosen provider. I then replicate the reform in the model, which is primarily estimated on data from Georgia. The only Vermont input is the participation response, and I do not target the choice of provider. Even though consumers in Vermont may respond differently, the model's semi-elasticity of −0.113 is remarkably similar.

Next, I test whether the model produces bunching that matches the data. Simply taking the raw marginal cost estimates and solving for equilibrium would mechanically reproduce bunching. So I estimate a marginal cost function using quality, amenities, rents and wages, excluding providers that bunch at the kink. I exclude these providers for two reasons. First, including them may lead the model to predict similar marginal costs, possibly reproducing the bunching to some extent. Second, since many providers at the kink are not at an interior solution to the first-order conditions of equation (3), I cannot, in fact, reliably back out their marginal costs. Using the predicted marginal costs, I then solve for the equilibrium. Panel (a) of Figure 10 compares the resulting price distribution with the data for Atlanta in 2018–19. Table 2 reports estimated semi-elasticities of providers' posted prices with respect to the marginal price of −0.067 in the data and −0.083 in the model. As a further check, panel (b) of Figure 10 removes the kink by extending the subsidy schedule that applies above it to prices below. In the model, bunching disappears completely.

The model captures the three responses that shape how subsidy design affects welfare. It



(a) Data and model

(b) Removing bunching

Figure 10: Model-implied bunching, Atlanta, 2018–19

Notes: Distributions of posted prices in Atlanta, 2018–19. Panel (a) shows the data and the equilibrium prices from the model, estimated with marginal costs predicted from a regression that excludes bunching providers. Panel (b) compares the model's current-policy distribution with the price distribution under a counterfactual policy in which the price schedule above the kink is extended below the kink.

matches the extensive margin response I target, and it reproduces, without being targeted, how recipients' choice of provider and providers' choice of price respond to subsidy design. With these responses captured, I use the model to compare budget-neutral subsidy designs.

7 Policy Counterfactuals

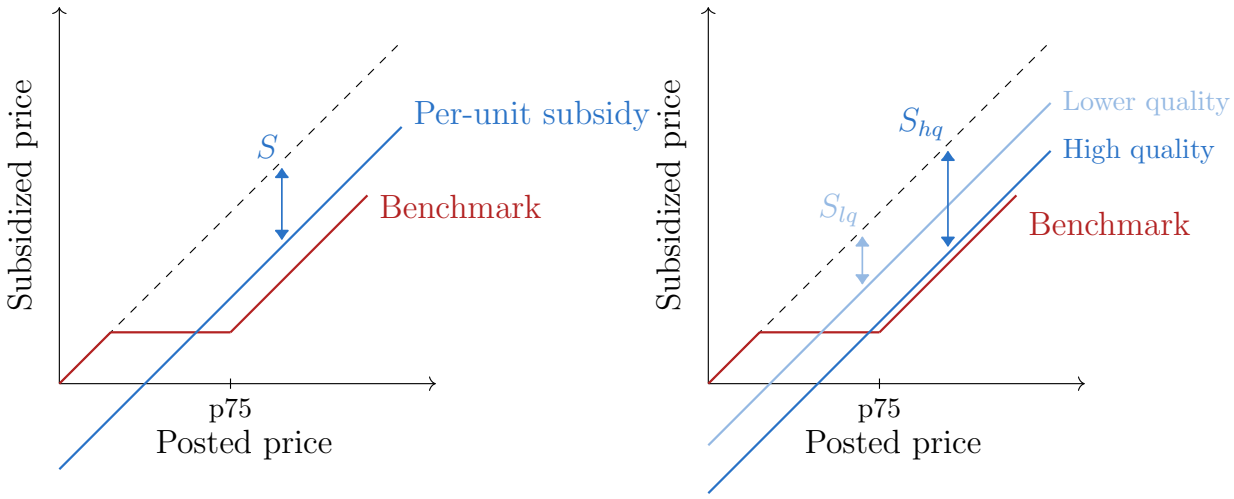
I use the estimated model to compare budget-neutral subsidy designs. I define the change in social welfare in a subsidized child care market as

$$\Delta W = \Delta CS_{sub} + \Delta CS_{unsub} + \alpha\,\Delta PS - \lambda\,\Delta G + \beta\,\Delta X_{hq}, \tag{4}$$

where ΔCS_{sub} and ΔCS_{unsub} are the changes in consumer surplus of subsidized and unsubsidized families, ΔPS is the change in producer surplus (weighted by α), ΔG is the change in government spending (valued at the cost of public funds λ), and $\beta\,\Delta X_{hq}$ is the value of the change in high-quality consumption. Each reform I consider holds government spending fixed, so that $\Delta G = 0$.

7.1 Price-Dependent Subsidy vs. Per-Unit Subsidy

In this section, I consider a benchmark policy based on guidelines and rules implemented by the US federal government. Under such a policy, subsidy recipients face a threshold price at the 75th percentile of prices. This is a recommendation of the federal government, although most states set their threshold below the 75th percentile. Below the threshold, subsidy



(a) Reform 1: per-unit subsidy

(b) Reform 2: quality-dependent subsidy

Figure 11: Subsidized price schedules: benchmark and counterfactual reforms

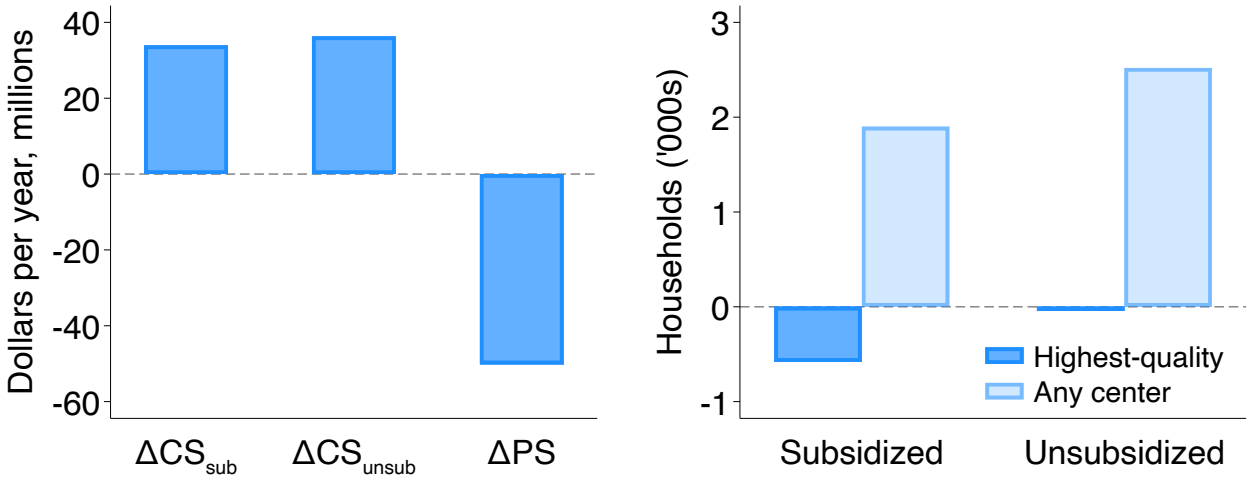
Notes: The figure shows the price paid by a subsidized family as a function of the posted price. The dashed line is the 45 degree line, corresponding to no subsidy. The benchmark (red) makes recipients insensitive to posted prices up to the 75th percentile of prices. Panel (a) shows a per-unit subsidy of S (blue). Panel (b) shows a quality-dependent subsidy, which is larger for high-quality (S_{hq}) than for lower-quality (S_{lq}) providers.

recipients are shielded from price differences. Above the threshold, they are not. I set the level of the subsidized price below the threshold at 7% of a household's income, the maximum recommended by the federal government. I also set the income eligibility threshold at 85% of the median income, the maximum for which federal funds can be used. Many states set this as their threshold. I then compare this benchmark subsidy, which depends on the price of the purchase, to a simple per-unit subsidy, which does not depend on price and is equal to a fixed amount S per unit of care. I set the amount S such that government spending on each policy is the same in equilibrium. Under the per-unit subsidy, recipients are fully exposed to price differences. The two policies are shown in Panel (a) of Figure 11.

The model estimates that moving from the benchmark to the per-unit subsidy would lower prices by 16 percent, but would also lower the share of subsidized families choosing high-quality care by 8 percent.

Figure 12 reports the components of the change in welfare (Panel (a)) and the change in consumption of all households (Panel (b)).

Whether the benchmark is more efficient than the per-unit subsidy depends on the value β of high-quality care. Suppose producer surplus receives full weight ($\alpha = 1$) and the only consumption that matters is high-quality care by subsidized children. The benchmark is then more efficient if β exceeds \$34,587 per child-year. Valuing other consumption, such as



(a) Welfare components

(b) Consumption, all households

Figure 12: Price-dependent to per-unit subsidy

Notes: Panel (a) reports the changes in the components of social welfare in equation (4) from replacing the federal benchmark with a per-unit subsidy that leaves government spending unchanged. Panel (b) reports the change in consumption of all households.

unsubsidized or lower-quality care, or placing less weight on provider profits, improves the relative efficiency of the per-unit subsidy. For example, if the fiscal externality of increasing the number of children in care is greater than zero, the required social value of high-quality care for subsidized children must be higher. If each child in other care generates a social value of $5,000 per child-year, the benchmark is more efficient only if high-quality care for subsidized children is worth more than $77,608. Appendix F.1 reports this threshold for values of other care between $0 and $15,000 per child-year. Alternatively, if zero weight is placed on provider profits and other care is valued at $0, the benchmark is more efficient only if β exceeds $120,929 per child-year.

7.2 Price-Dependent Subsidy vs. Quality-Dependent Subsidy

While price-dependent subsidies may encourage higher-quality consumption, if quality is directly and reliably observable, one could also introduce larger subsidies for higher-quality care. Empirically, Section 4.4 showed this to affect the quality of consumption in Vermont.

I now use the model to compare the benchmark policy with a subsidy that depends on provider quality and is independent of price conditional on quality. The subsidy consists of a larger subsidy S_{hq} for high-quality providers and a lower subsidy S_{lq} for lower-quality providers. The two subsidies are portrayed in Panel (b) of Figure 11. I choose S_{hq} and S_{lq} so that government spending and the attendance of subsidized children at high-quality providers are both unchanged, meaning $\Delta G = 0$ and $\Delta X_{hq,sub} = 0$. Figure 13 reports

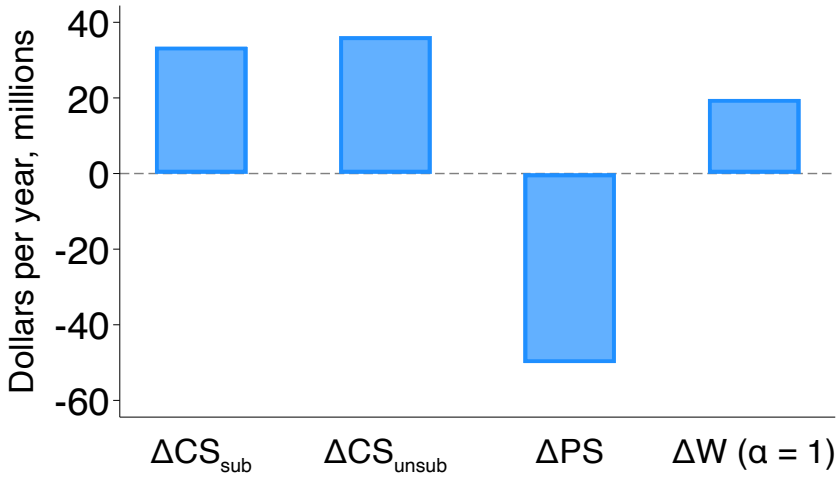


Figure 13: Price-dependent to quality-dependent subsidy

Notes: Changes in the components of social welfare in equation (4) from replacing the federal benchmark with a quality-dependent subsidy that leaves government spending and subsidized attendance at high-quality providers unchanged.

the resulting welfare components. The results indicate that the quality-dependent subsidy improves welfare relative to the benchmark policy. This result replicates when the threshold price is moved above or below the 75th percentile. These findings suggest that when quality is observable, policymakers should use quality-dependent subsidies to encourage higher-quality consumption rather than subsidies that make recipients insensitive to price.

8 Conclusion

The extent to which child care subsidies expose recipients to price differences varies widely across and within countries. This paper considers whether shielding recipients from price differences is an effective tool for encouraging the consumption of higher-quality care and the extent to which it results in higher prices. Using reforms to Vermont's subsidy program, I find that reducing recipients' exposure to price differences shifts them toward more expensive, higher-quality providers. Using bunching at kinks in the subsidy schedule, I find providers respond to recipients' reduced price sensitivity by raising prices, with estimated increases of 15 percent in Vermont. Because these higher prices are paid by unsubsidized families as well, the effects of subsidy design extend beyond the families the subsidy is intended to help.

To weigh these forces against each other, I estimate a structural model of the Georgia child care market. Relative to a budget-neutral per-unit subsidy, a benchmark subsidy based on federal guidelines moves more subsidized children into high-quality care, but also raises prices and provider profits, lowers consumer surplus for both subsidized and unsubsidized

families, and reduces overall participation. Whether the benchmark is nevertheless more efficient depends on the social value of high-quality care, and on whether lower-quality and unsubsidized care also generate social benefits. Where quality is observable, tying subsidies directly to quality can steer recipients toward better care without insulating them from price differences. Across a wide range of specifications quality-dependent subsidies are more efficient than price-dependent subsidies.

A few limitations suggest directions for future work. The model takes provider quality as given and does not allow for entry or exit. Allowing these margins to respond would provide a more complete assessment of the degree to which subsidy recipients should be exposed to price differences. It would also allow for comparisons with supply-side interventions, such as payments that reward providers for supplying high-quality care, as studied by Bodéré [2026], and regulations imposing minimum staffing and qualification requirements. The effectiveness of such policies depends on both the extent to which they induce quality improvements as well as the extent to which such improvements reach disadvantaged children.

Quantifying the social benefits of care and how they vary across providers and children also lies beyond the scope of this paper. While this analysis treats such values as parameters, further evidence on their magnitude and variation would help determine the optimal subsidy design.

More broadly, the question of how much to shield consumers from price differences involves similar trade-offs in other contexts where third parties cover part of a buyer's expenses. These include other subsidy programs as well as insurance and employer reimbursements. Evaluating these arrangements requires accounting for both the choices they encourage and the price effects they have in equilibrium.

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Appendix

A Conceptual Framework

A.1 Proof of proposition 1.

Proof. Consider a targeted consumer subsidy $S(p) = a + sp$ with $s \in [0, 1]$.

Subsidized consumers face the price $p_c = p - S(p)$. A monopolist's profit maximization problem is:

$$\max_p \quad \pi(p) = p\, q(p) - C\big(q(p)\big), \tag{A1}$$

where $q(p) = q_u(p) + q_s(p_c)$ is the sum of unsubsidized and subsidized demand and $C\big(q(p)\big)$ is the monopolist's cost function.

The first order condition is

$$\frac{\partial \pi}{\partial p} = q(p) + \Big(p - C'\big(q(p)\big)\Big)\Big[q_u'(p) + (1-s)\, q_s'(p_c)\Big] = 0. \tag{A2}$$

Since $q_u' < 0$, $q_s' < 0$ and $s \in [0, 1]$, the term in square brackets satisfies

$$q_u'(p) + (1-s)\, q_s'(p_c) < 0.$$

And as $q(p) > 0$, any interior optimum must satisfy $p^* > C'\big(q(p^*)\big)$.

Since a enters the problem only through p_c, the first order condition can be written as a function of (p, p_c, s):

$$\Phi(p, p_c, s) \equiv q_u(p) + q_s(p_c) + \Big(p - C'\big(q_u(p) + q_s(p_c)\big)\Big)\Big[q_u'(p) + (1-s)\, q_s'(p_c)\Big]. \tag{A3}$$

Differentiating Φ with respect to p gives

$$\begin{aligned} \Phi_p &= q_u' + \big(1 - C''(q)\, q_u'\big)\big[q_u' + (1-s)\, q_s'\big] + \big(p - C'(q)\big)\, q_u'' \\ &= \Big[2q_u' - C''(q)\,(q_u')^2 + \big(p - C'(q)\big)\, q_u''\Big] + \big(1 - C''(q)\, q_u'\big)(1-s)\, q_s'. \end{aligned} \tag{A4}$$

Assume that, for every fixed level of sales to subsidized consumers $Q \geq 0$, the monopolist's profit $p\,[q_u(p) + Q] - C\big(q_u(p) + Q\big)$ is strictly concave in p on the relevant range of prices. Its second derivative is the first term in square brackets, evaluated at $Q = q_s(p_c)$, so the first term is negative. And since marginal cost is non-decreasing, the second term is non-positive. Then $\Phi_p < 0$ for all $s \in [0, 1]$.

Fix a target price for subsidized consumers, $\bar{p}_c$, and consider rates $s \in [0, 1]$ for which

some specific component $a(s)$ delivers this target, that is, for which the monopolist's optimal price $p^*(s)$ is an interior optimum satisfying $p^*(s) - S\big(p^*(s)\big) = \bar{p}_c$. Then $p^*(s)$ satisfies

$$\Phi\big(p^*(s), \bar{p}_c, s\big) = 0,$$

and since Φ is strictly decreasing in p, $p^*(s)$ is the unique such price.

Step 1: Prices. Now take any two rates $0 \leq s < s' \leq 1$, each with a specific component delivering $\bar{p}_c$ at an interior optimum. At any p with $p > C'\big(q_u(p) + q_s(\bar{p}_c)\big)$, the two first order conditions differ only in the coefficient on q_s':

$$\Phi(p, \bar{p}_c, s') - \Phi(p, \bar{p}_c, s) = -(s' - s)\big(p - C'(q)\big)\, q_s'(\bar{p}_c) > 0, \tag{A5}$$

since $s' > s$, $p > C'(q)$ and $q_s' < 0$.

Suppose, for contradiction, that $p^*(s') \leq p^*(s)$. Since Φ is decreasing in p when p_c is held fixed,

$$\Phi\big(p^*(s'), \bar{p}_c, s\big) \geq \Phi\big(p^*(s), \bar{p}_c, s\big) = 0.$$

Since $p^*(s')$ is an interior optimum, $p^*(s') > C'(q)$, and so

$$\Phi\big(p^*(s'), \bar{p}_c, s'\big) > \Phi\big(p^*(s'), \bar{p}_c, s\big) \geq 0,$$

contradicting $\Phi\big(p^*(s'), \bar{p}_c, s'\big) = 0$. Hence

$$p^*(s') > p^*(s).$$

Step 2: Government spending. Turning to government spending, note that the per unit subsidy is exactly the wedge between the producer and consumer price:

$$S\big(p^*(s)\big) = a(s) + s\, p^*(s) = p^*(s) - \bar{p}_c.$$

Hence government spending is

$$\begin{aligned} g(s) &= S\big(p^*(s)\big)\, q_s(\bar{p}_c) = \big(p^*(s) - \bar{p}_c\big)\, q_s(\bar{p}_c), \\ g(s') - g(s) &= q_s(\bar{p}_c)\, \big(p^*(s') - p^*(s)\big) > 0. \end{aligned}$$

To summarize, in a market with a monopolist, if a linear targeted consumer subsidy $S(p) = a + sp$ with $s \in [0, 1]$ is adjusted such that the after-subsidy price faced by subsidized consumers is held fixed, then a higher ad valorem component s results in a higher price for

the unsubsidized and higher government spending.

Step 3: Profit. Turning to the monopolist's profit, note that when p_c is held fixed at $\bar{p}_c$, profit depends on the subsidy only through the price p. Define

$$V(p) \equiv p\left[q_u(p) + q_s(\bar{p}_c)\right] - C\big(q_u(p) + q_s(\bar{p}_c)\big), \tag{A6}$$

so that profit under rate s is $V\big(p^*(s)\big)$. Differentiating gives

$$\begin{aligned} V'(p) &= q_u(p) + q_s(\bar{p}_c) + \big(p - C'(q)\big)\, q_u'(p) = \Phi(p, \bar{p}_c, 1), \\ V''(p) &= \Phi_p(p, \bar{p}_c, 1) < 0, \end{aligned}$$

where the inequality follows from $\Phi_p < 0$ for all $s \in [0,1]$, shown above. Since $\Phi\big(p^*(s'), \bar{p}_c, s'\big) = 0$,

$$\begin{aligned} V'\big(p^*(s')\big) &= \Phi\big(p^*(s'), \bar{p}_c, 1\big) - \Phi\big(p^*(s'), \bar{p}_c, s'\big) \\ &= -(1 - s')\big(p^*(s') - C'(q)\big)\, q_s'(\bar{p}_c) \geq 0. \end{aligned}$$

Since V' is strictly decreasing, $V'(p) > 0$ for all $p < p^*(s')$. As $p^*(s) < p^*(s')$,

$$\pi^*(s') - \pi^*(s) = V\big(p^*(s')\big) - V\big(p^*(s)\big) = \int_{p^*(s)}^{p^*(s')} V'(p)\, dp > 0.$$

Hence a higher ad valorem component s results in higher profit for the monopolist.

Step 4: Social surplus. Sales to subsidized consumers are $q_s(\bar{p}_c)$ under both rates, so neither their value nor the external benefit e changes. By Step 1, moving from rate s' to rate s raises sales to unsubsidized consumers from $x_u' = q_u\big(p^*(s')\big)$ to $x_u = q_u\big(p^*(s)\big)$, and output from X' to X by the same amount. The resulting change in social surplus, the value of the additional consumption less its production cost, is

$$\Delta W = \int_{x_u'}^{x_u} P_u(z)\, dz - \int_{X'}^{X} C'(z)\, dz,$$

where P_u is the inverse demand of unsubsidized consumers. Each additional unit is valued at no less than $p^*(s)$, since inverse demand is decreasing, and costs no more than $C'(X)$, since marginal cost is non-decreasing. Hence

$$\Delta W \geq \big(p^*(s) - C'(X)\big)(x_u - x_u') > 0,$$

as $p^*(s) > C'(X)$ at an interior optimum. A higher ad valorem component s therefore results in lower social surplus.

□

A.2 Proof of proposition 2.

Proof. For each rate s and each subsidized price $\bar{p}_c$ considered, let $p^*(\bar{p}_c, s)$ denote the unique solution of $\Phi(p, \bar{p}_c, s) = 0$, i.e. the price the monopolist charges when the specific component is chosen to deliver $\bar{p}_c$ under rate s, and let

$$g(\bar{p}_c, s) \equiv \left(p^*(\bar{p}_c, s) - \bar{p}_c\right) q_s(\bar{p}_c)$$

denote the corresponding government spending. Steps 1 and 2 of the proof of Proposition 1 show that $g(\bar{p}_c, s') > g(\bar{p}_c, s)$ whenever $s < s'$.

Let $S(p) = a + sp$ and $S'(p) = a' + s'p$ with $0 \leq s < s' \leq 1$ result in the same government spending $G > 0$, each at an interior optimum, and let (p, p_c) and (p', p'_c) denote the outcomes under S and S'. Consider an interval of subsidized prices containing p_c and p'_c, each of which can be delivered under rate s in the sense of the proof of Proposition 1, so that $g(\cdot, s)$ is defined on it. The additional assumption is that $g(\cdot, s)$ is strictly decreasing on this interval: under a given rate, delivering a lower subsidized price requires higher government spending.

Step 1: Subsidized price. Suppose, for contradiction, that $p'_c \leq p_c$. Then

$$G = g(p'_c, s') > g(p'_c, s) \geq g(p_c, s) = G,$$

where the strict inequality is Steps 1 and 2 of the proof of Proposition 1 applied at $\bar{p}_c = p'_c$, and the weak inequality follows from $g(\cdot, s)$ being decreasing. This is a contradiction, so $p'_c > p_c$.

Step 2: Unsubsidized price. At any outcome the subsidy paid on each unit equals the wedge between the two prices, $S(p) = p - p_c$, so government spending is $G = (p - p_c)\, q_s(p_c)$, or

$$p = p_c + \frac{G}{q_s(p_c)}.$$

Since both subsidies result in the same spending G,

$$p' - p = (p'_c - p_c) + G\left[\frac{1}{q_s(p'_c)} - \frac{1}{q_s(p_c)}\right] > 0,$$

as $p'_c > p_c$ and q_s is strictly decreasing. Since both prices are higher under S', both groups purchase strictly less.

Step 3: Social surplus. Suppose, in addition, that $p_c + e \geq C'(X)$, where $X = q_u(p) + q_s(p_c)$ is output under S; that is, the subsidized price falls short of marginal cost by no more than the external benefit of subsidized consumption. By Step 2, moving from S' to S raises sales to unsubsidized consumers from $x'_u = q_u(p')$ to $x_u = q_u(p)$ and to subsidized consumers from $x'_s = q_s(p'_c)$ to $x_s = q_s(p_c)$, and output from X' to X. The resulting change in social surplus, the value of the additional consumption less its production cost, is

$$\Delta W = \int_{x'_u}^{x_u} P_u(z)\,dz + \int_{x'_s}^{x_s} \big(P_s(z) + e\big)\,dz - \int_{X'}^{X} C'(z)\,dz,$$

where P_u and P_s denote inverse demand. Each additional unit is valued at no less than its price under S, plus e for subsidized consumers, as inverse demand is decreasing, and costs no more than $C'(X)$, as marginal cost is non-decreasing. Hence

$$\Delta W \geq \big(p - C'(X)\big)(x_u - x'_u) + \big(p_c + e - C'(X)\big)(x_s - x'_s) > 0,$$

since $p > C'(X)$ at an interior optimum, $p_c + e \geq C'(X)$ by assumption, and $x_u > x'_u$. The subsidy with the lower ad valorem component therefore yields strictly higher social surplus. □

B Data

B.1 Subsidy Recipients in Vermont

Table B1: Characteristics of subsidy recipients before the reform

	Control	Treated	Difference
Household			
Monthly income (Aug 2022 $)	616	4,965	4,349*** (65)
Family size	3.23	3.33	0.11 (0.10)
Subsidized children aged 0–5	1.52	1.00	−0.52*** (0.03)
Subsidy spell			
Spell length (months): 25th percentile	25	25	
Spell length (months): median	38	40	
Child age group (%)			
Infant (0–1)	17.6	14.2	−3.3 (2.4)
Toddler (2)	20.4	14.9	−5.4** (2.3)
Preschool (3–5)	62.1	70.8	8.7*** (3.2)
Provider attended (%)			
Home-based provider	18.1	25.0	6.9** (3.1)
Center: 1 star	4.9	5.7	0.8 (1.8)
Center: 2 star	0.5	0.8	0.3 (0.6)
Center: 3 star	10.6	16.6	6.0** (2.7)
Center: 4 star	25.9	30.2	4.3 (3.3)
Center: 5 star	40.0	21.8	−18.3*** (3.2)
Provider price			
Posted full-time weekly preschool price ($)	243.2	237.4	−5.7 (4.8)
Posted price missing (%)	0.5	0.8	0.2 (0.6)
Child-months	4,805	1,186	
Children	1,131	280	
Families	935	280	

Notes: Child-months from the pre-reform months of the sample underlying Panel (b) of Figure 3. Subsidized children aged 0–5 is the family's maximum number of subsidized children aged 0–5 in any month from February 2022 to June 2023. The star rating is the 2021 STARS rating of the attended provider (2022 if missing). The last column reports the difference between the means, with standard errors clustered by family in parentheses; *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

B.2 Providers in Vermont

Tables B2 and B3 describe the licensed child care centers and the family child care homes in Vermont that were in operation in both December 2021 and December 2022.

Table B2: Characteristics of child care centers before the reform

	All	Infant or toddler	Preschool
Quality rating (%)			
1 star	8.6	9.4	8.0
2 star	1.0	1.7	0.8
3 star	10.6	17.0	10.8
4 star	37.4	37.4	37.8
5 star	42.3	34.5	42.6
Rating missing	0.0	0.0	0.0
Licensed capacity (children)			
Infant	5.2	10.9	5.0
Toddler	5.2	10.8	5.1
Preschool	22.6	21.3	23.3
Total (all ages)	36.9	48.1	37.4
Posted full-time weekly price ($)			
With subsidy recipients (%)	60.5	92.8	60.1
Infant price	303.7	303.2	303.6
Toddler price	292.4	294.0	291.9
Preschool price	273.7	279.5	273.0
Public or Head Start (%)	4.5	1.3	4.4
All prices missing (%)	0.8	0.4	0.8
No subsidy recipients (%)	39.5	7.2	39.9
Public or Head Start (%)	30.5	2.1	31.0
All prices missing (%)	32.7	3.0	33.5
Providers	489	235	474

Notes: Vermont-licensed child care centers appearing in both the December 2021 and the December 2022 licensing data with licensed capacity for infants, toddlers or preschool-age children. The infant or toddler column covers providers licensed for infants or toddlers and the Preschool column providers licensed for preschool-age children. Many providers are licensed for several age groups and so appear in both columns. Providers licensed only for school-age children and out-of-state providers are excluded. The rating is the 2021 STARS rating (2022 if missing).

Table B3: Characteristics of family child care homes before the reform

	All	Infant or toddler	Preschool
Quality rating (%)			
1 star	36.5	36.6	36.5
2 star	6.5	6.6	6.5
3 star	28.7	28.8	28.7
4 star	21.9	22.0	21.9
5 star	5.8	5.8	5.8
Rating missing	0.0	0.0	0.0
Licensed capacity (children)			
Infant	2.0	2.0	2.0
Toddler	2.1	2.1	2.1
Preschool	2.1	2.1	2.1
Total (all ages)	10.1	10.1	10.1
Posted full-time weekly price ($)			
With subsidy recipients (%)	72.3	72.2	72.3
Infant price	202.0	202.0	202.0
Toddler price	195.5	195.5	195.5
Preschool price	191.7	191.7	191.7
All prices missing (%)	0.3	0.0	0.3
No subsidy recipients (%)	27.7	27.8	27.7
All prices missing (%)	11.3	11.4	11.3
Providers	397	396	397

Notes: Same as Table B2, for licensed and registered family child care homes, without the public or Head Start rows.

C Effects on Consumption

C.1 Vermont Child Care Subsidy

Prior to the July 2022 reform studied in Section 4, the share of the child care price covered by Vermont's child care subsidy program was a percentage of the cost of care up to the voucher amount. Figure C1 shows how the copayment share depended on income and family size. After the reform, the household's contribution was instead set as a flat weekly dollar amount, independent of the price of care, as shown in Figure C2.

Child Care Financial Assistance Sliding Fee Scale

Effective October 25, 2020

Child Care Financial Assistance Based on Gross Monthly Income & Family Size

% of Financial Assistance Paid by State	Family of 3 or less	Family of 4	Family of 5	Family of 6 or More
100%	$1,810	$2,183	$2,557	$2,930
99%	$1,901	$2,293	$2,685	$3,077
98%	$1,991	$2,402	$2,813	$3,223
97%	$2,082	$2,511	$2,940	$3,370
96%	$2,172	$2,620	$3,068	$3,516
95%	$2,263	$2,729	$3,196	$3,663
90%	$2,376	$2,866	$3,356	$3,846
85%	$2,489	$3,002	$3,516	$4,029
80%	$2,602	$3,139	$3,675	$4,212
75%	$2,715	$3,275	$3,835	$4,395
70%	$2,896	$3,493	$4,091	$4,688
65%	$3,077	$3,712	$4,346	$4,981
60%	$3,258	$3,930	$4,602	$5,274
55%	$3,439	$4,149	$4,857	$5,567
50%	$3,620	$4,367	$5,113	$5,860
45%	$3,846	$4,640	$5,433	$6,226
40%	$4,073	$4,913	$5,752	$6,593
35%	$4,299	$5,186	$6,072	$6,959
30%	$4,525	$5,459	$6,392	$7,325
25%	$4,751	$5,731	$6,711	$7,691
20%	$4,978	$6,004	$7,031	$8,058
15%	$5,204	$6,277	$7,350	$8,424
10%	$5,430	$6,550	$7,670	$8,790

VERMONT
DEPARTMENT FOR CHILDREN AND FAMILIES
CHILD DEVELOPMENT DIVISION

Agency of Human Services

Figure C1: Vermont Child Care Subsidy Copayments, Effective October 25, 2020

Notes: This table gives the percentage of the child care benefit paid by the state as a function of gross monthly income and family size from October 25, 2020 until July 30, 2022. The household pays the complementary share up to the benefit amount and the full difference above it.

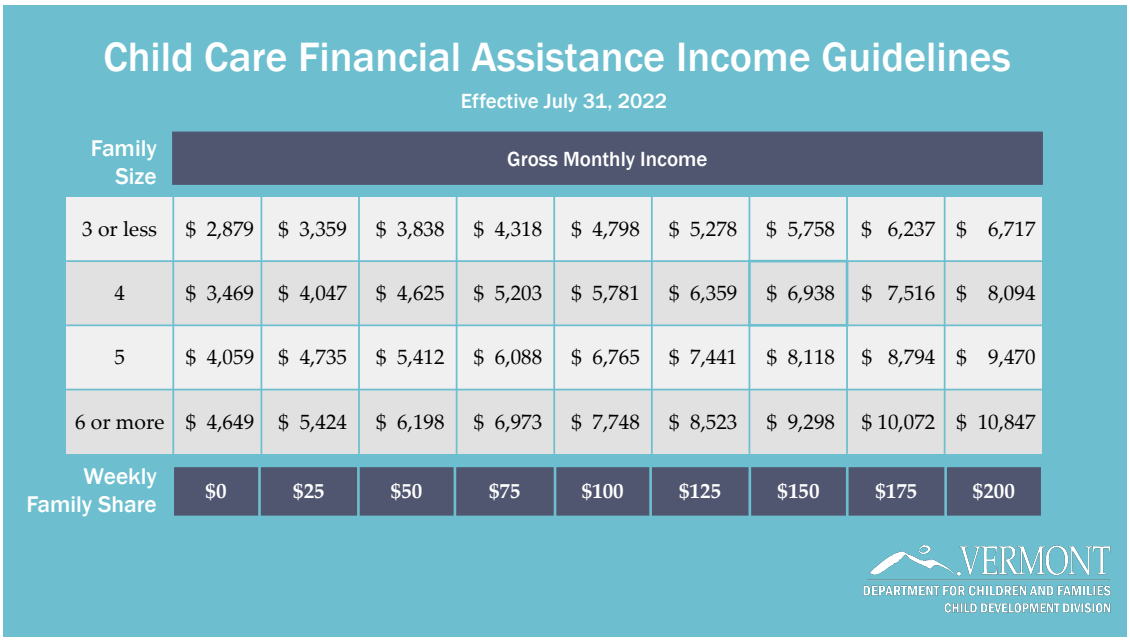
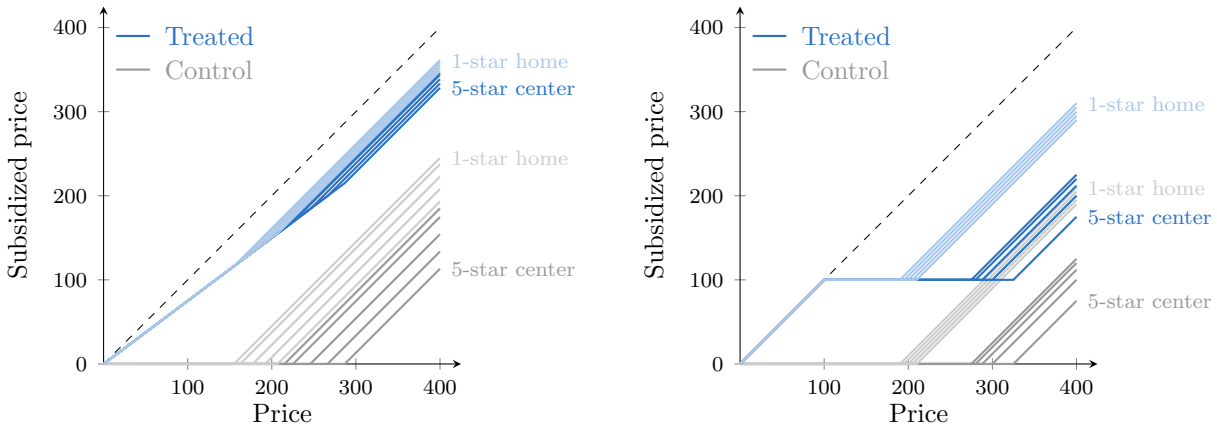


Figure C2: Vermont Child Care Subsidy Copayments, Effective July 31, 2022

Notes: This table shows the copayments paid by families from July 31, 2022. Copayments were no longer a percentage of the cost of care, but rather a fixed amount depending on family income and size.

Figure C3 shows the subsidized price schedules faced by the treated and control groups for each type of provider. While providers can be divided into ten categories, just three (3-star, 4-star and 5-star centers) accounted for 73% of licensed child care spots and 76% of subsidized attendance prior to the reform. See Figure C10 for robustness of the baseline estimates to the presence of the quality incentive.



(a) Pre-reform

(b) Post-reform

Figure C3: Subsidized price schedule by type of care

Notes: Each line corresponds to one star rating of licensed centers (darker lines) or registered homes (lighter lines), with higher star ratings receiving a larger subsidy. While subsidy recipients included in the treated group paid co-payments of 60–90% pre-reform and \$75–100 post-reform, the plots show the subsidized price schedules for those at the midpoint, who paid 75% pre-reform and \$100 post-reform.

C.2 Number of Subsidized Families by Household Size

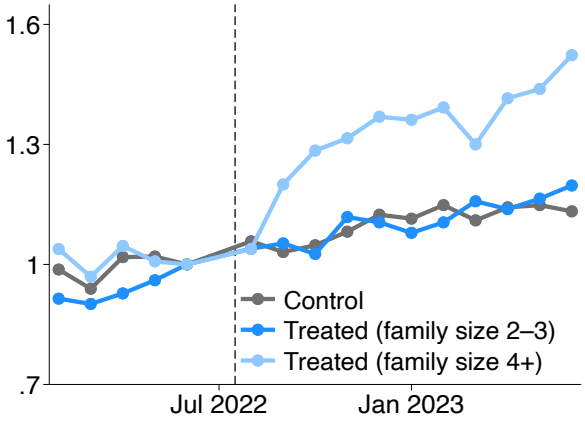


Figure C4: Number of subsidized families by household size

Notes: Same as Panel (b) of Figure 1, but groups are defined by household size rather than by the number of children aged 0–5 in care.

C.3 Including Families Receiving Subsidies for Less Than a Year

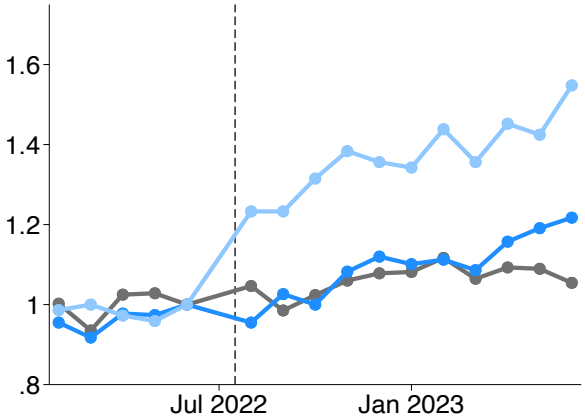


Figure C5: Number of subsidized families, any receipt duration

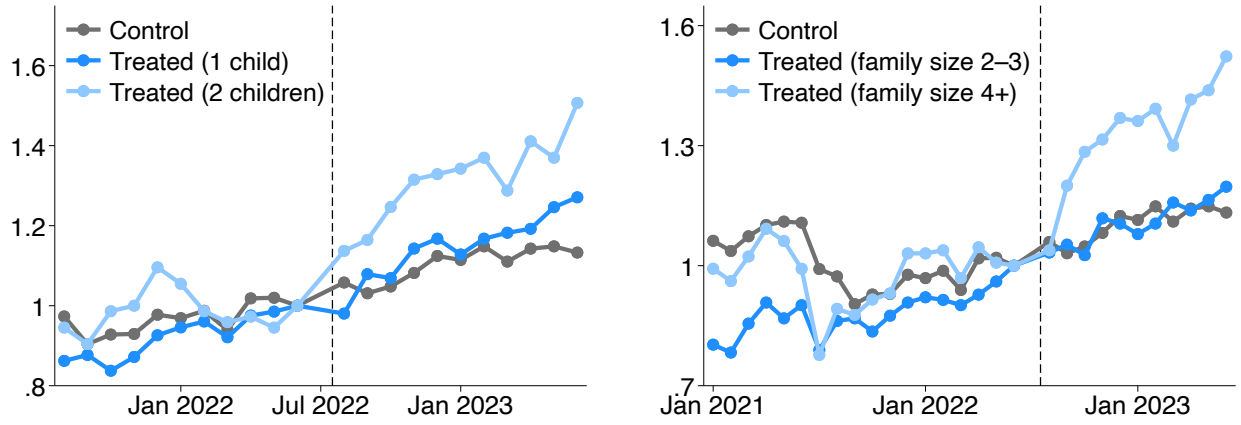
Notes: Same as Panel (b) of Figure 1, but the sample also includes families in receipt of subsidies for less than a year.

C.4 Robustness to the Observation Window

This section explores robustness to a longer pre-reform observation window. The current window of February 2022 to June 2023 was selected due to an increase in the voucher amount occurring in February 2022 and an increase in the voucher amount for all providers other than centers with a five star rating occurring in July 2023.

Panel (a) of Figure C6 shows the baseline results, extending the pre-reform period by another six months. Noticeably, the post-reform treated results change somewhat. This is due to the definition of one child and two children being the maximum number of children a family has in care in one month during the observation period. This definition moves some families with a child that aged out of preschool care from the one child group to the two child group. I therefore interpret these results with caution and avoid extending the pre-reform period further with this specification.

Panel (b) plots the number of subsidized families by family size, the definition of which is unaffected by changes to the observation window, and extends the window to January 2021.



(a) By number of kids (Aug 2021 to Jun 2023) (b) By household size (Jan 2021 to Jun 2023)

Figure C6: Number of subsidized families with a longer observation window

Notes: Same as Panel (b) of Figure 1 and Figure C4, but with extended pre-reform windows.

C.5 Number of Subsidized Families Including July 2022

The baseline specification for Panel (b) of Figure 1 drops July 2022 due to under-reporting in that month. This was due to the Department for Children and Families launching a new information system for the administration of child care subsidies. This caused a delay in payments to some providers, artificially deflating the number of families for whom payments were recorded in that month.

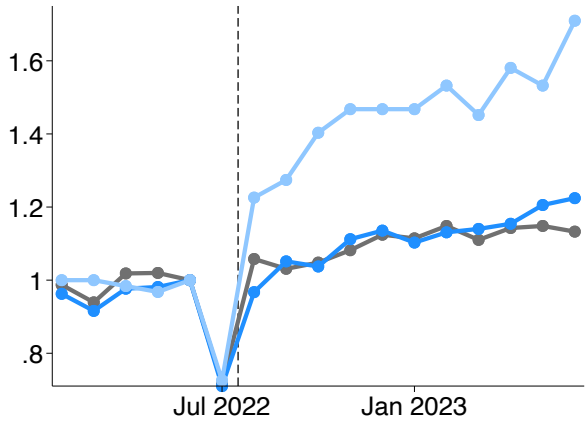


Figure C7: Number of subsidized families, including July 2022

Notes: Same as Panel (b) of Figure 1, but including July 2022.

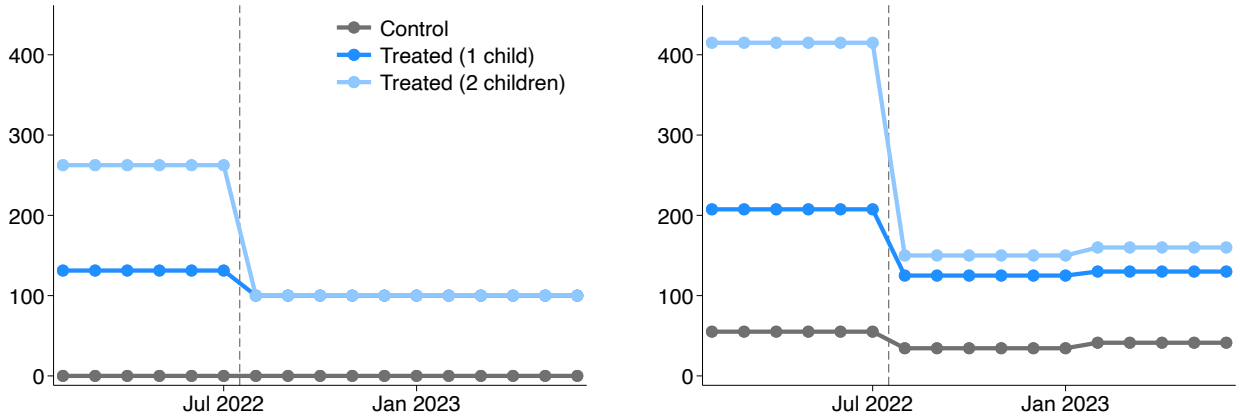
C.6 Effects on Number of Subsidized Families

Table C1: Effect of the reform on the number of subsidized families

	Number of children		Household size	
	1	2	2–3	4+
Panel A: February 2022 – June 2023				
post	0.035	0.282	0.057	0.164
	(0.017)	(0.030)	(0.017)	(0.027)
Months	16	16	16	16
Panel B: August 2021 – June 2023				
post	0.085	0.156	0.069	0.176
	(0.020)	(0.028)	(0.013)	(0.030)
Months	22	22	22	22

Notes: This table reports the coefficient on post_t underlying Panel (b) of Figure 1 (columns 1–2, groups defined by the number of children aged 0–5 in care) and Figure C4 (columns 3–4, groups defined by household size). Newey–West standard errors using one lag are reported in parentheses. Panel A uses the sample from February 2022 to June 2023 and Panel B extends the pre-period by six months.

C.7 Percentiles of the Subsidized Price



(a) 25th percentile of weekly subsidized price (b) 75th percentile of weekly subsidized price

Figure C8: Effect of the per-family copayment reform on subsidized prices

Notes: Same as Panel (a) of Figure 1, but reporting the 25th percentile and the 75th percentile of the subsidized price faced across providers in the market.

C.8 Post-reform Price

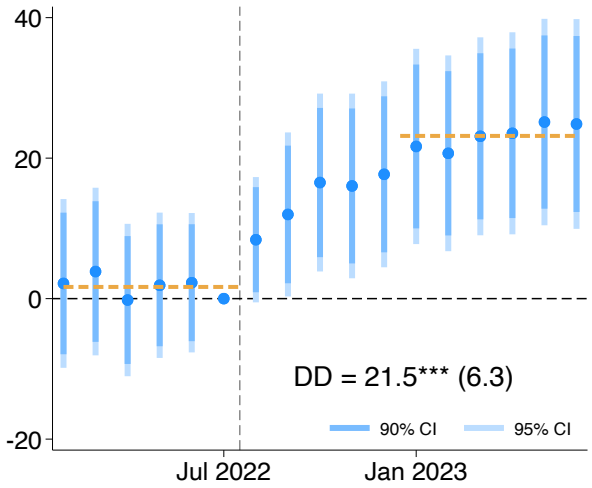


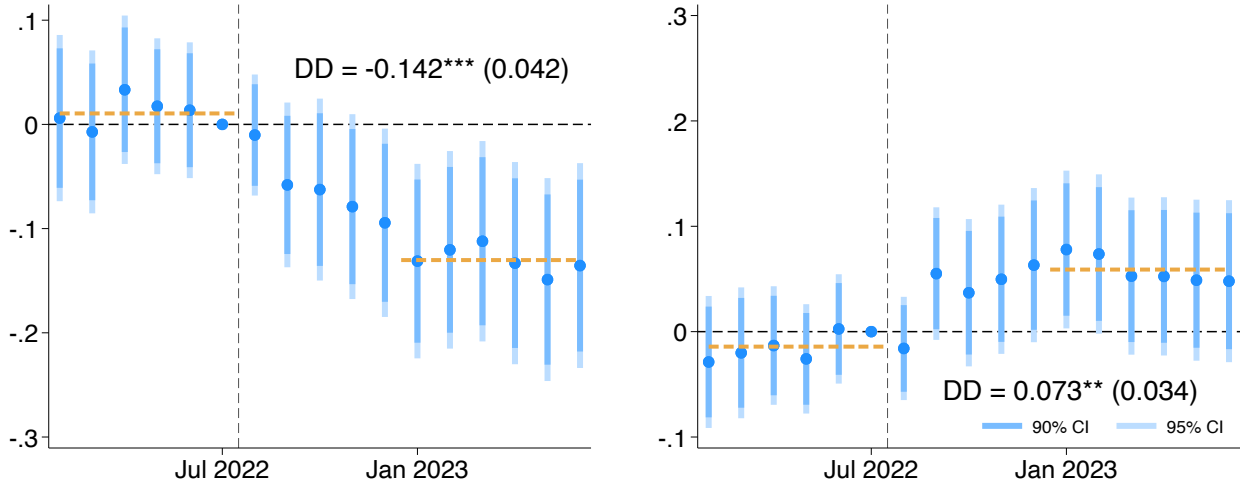
Figure C9: Effect of the reform on post-reform price of chosen provider

Notes: Event-study estimates of the difference between treated and control families around the July 2022 reform. The outcome is the post-reform price of the chosen provider. The number of children is 2217. Standard errors are clustered at the family level. 90% and 95% confidence intervals are shown. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

C.9 Robustness: the Quality Incentive

The shift towards higher-quality care documented in Section 4 could in principle reflect the presence of larger voucher amounts for higher-quality care as shown in Figure C3. Prior to the reform, if a 4-star center and 5-star center both had the same posted price and that price were above the 5-star voucher amount (i.e., the 5-star kink price), the family would have had to pay less to attend the 5-star center. Specifically, the price difference would have been 10–40% of the difference in the 4 and 5-star voucher amounts. After the reform, however, the price difference would have been equal to the full difference in voucher amounts. The reform therefore increased the financial incentive for families to attend higher-quality providers.

However, since this incentive is achieved through higher voucher amounts, it has no effect on the subsidized prices of providers pricing at or below the voucher amount. Figure C10 shows the reform reduced the proportion of subsidy recipients choosing providers priced at or below the voucher amount that were not 5-star centers. At the same time, it increased the proportion of recipients choosing providers priced at or below the voucher amount that were 5-star centers. Since post-reform, families faced the same price to attend any provider priced at or below the voucher amount, the reduction in the attendance of the lower-quality providers and increase in the attendance of higher-quality providers cannot be explained by the presence of the quality incentives.

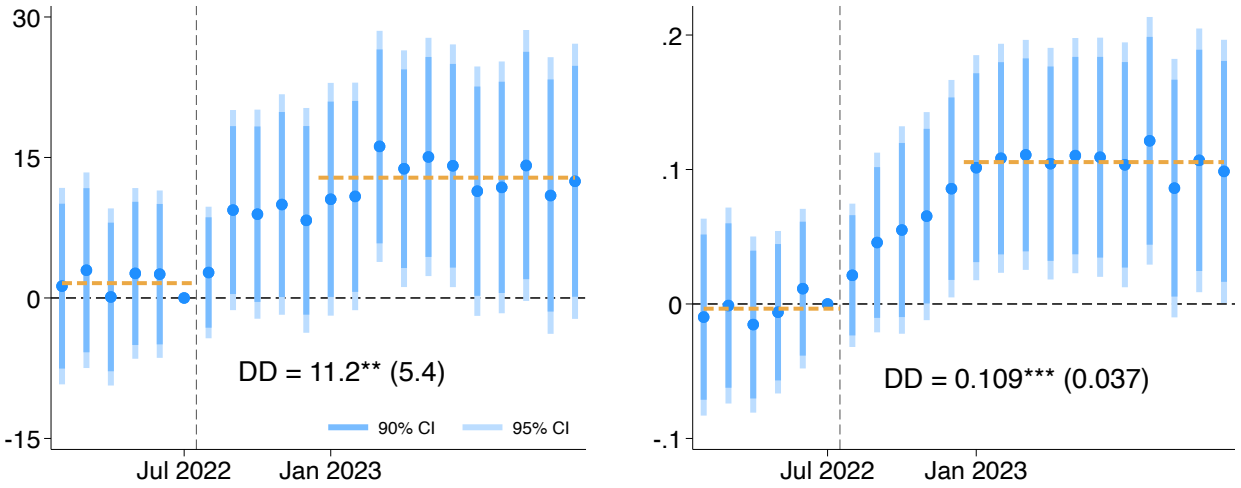


(a) Attending home care or a 1- to 4-star center

(b) Attending a 5-star center

Figure C10: Effect of the reform on quality of care chosen below the voucher threshold

Notes: Event-study estimates of the difference between treated and control families around the July 2022 reform. The outcome in panel (a) is a binary indicator for attending a provider that is priced at or below the voucher amount and is not a 5-star center. The outcome in panel (b) is a binary indicator for attending a provider that is priced at or below the voucher amount and is a 5-star center. The number of children is 2185. Standard errors are clustered at the family level. 90% and 95% confidence intervals are shown. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.



(a) Pre-reform price

(b) Attending a 5-star center

Figure C11: Effect of the reform on choice of quality (Feb 2022 to Nov 2023)

Notes: Same as Figure 3, but the observation window is extended to include July–November 2023, following the July 2023 increase in voucher amounts for centers with a 1–4 star rating. Panel (a) uses the pre-reform price of the chosen provider as the outcome. Panel (b) uses the share attending a 5-star center as the outcome. The number of children is 2538 in panel (a) and 2634 in panel (b). Standard errors are clustered at the family level. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

C.10 Balance Tests

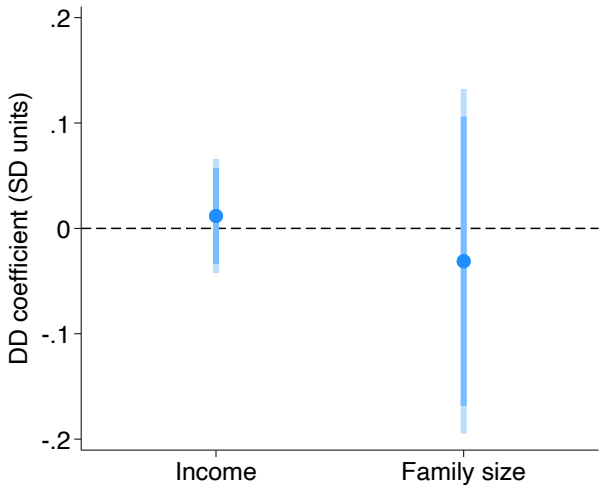
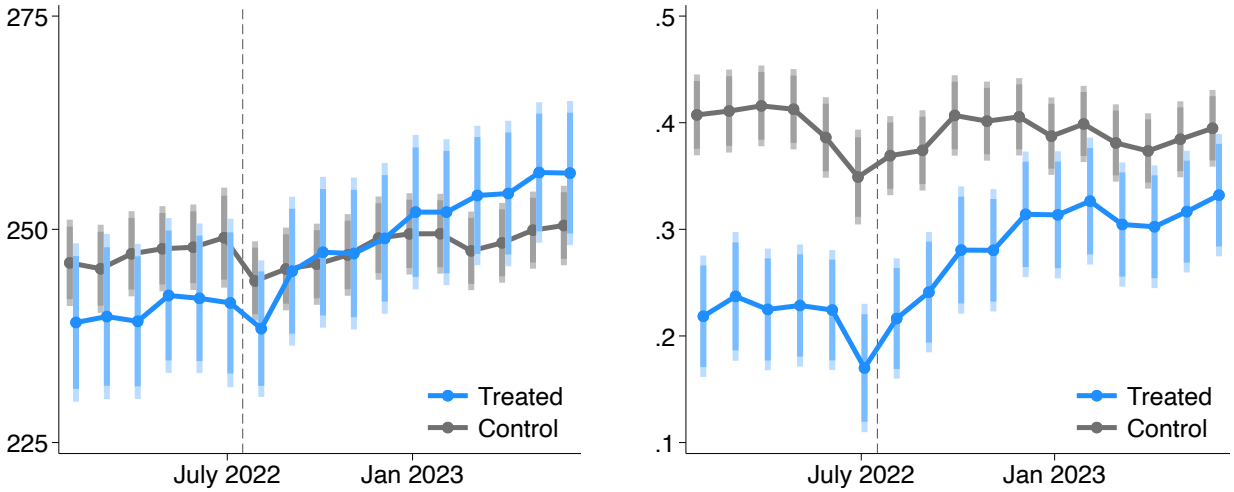


Figure C12: Balance tests on income and household size

Notes: Difference-in-differences estimates of the effect of the July 2022 reform on income and household size, in standard deviation units. The number of children is 2251. Standard errors are clustered at the family level. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

C.11 Level Plots for Choice of Quality



(a) Pre-reform price

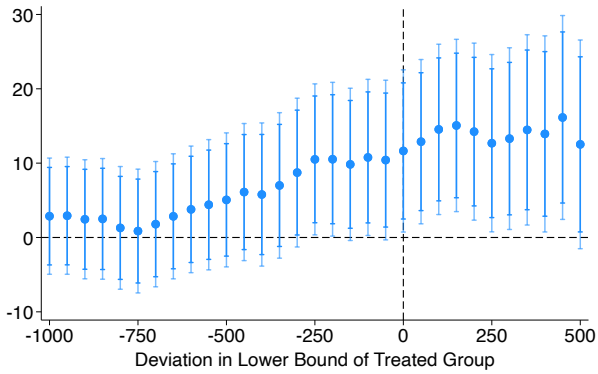
(b) Attending a 5-star center

Figure C13: Levels of the quality of chosen provider for treated and control families

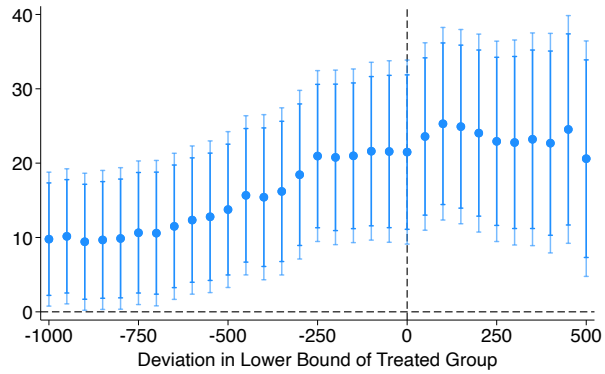
Notes: Same outcomes as Figure 3, but plotting levels for the treated and control groups instead of the difference between them. Panel (a) uses the pre-reform price of the chosen provider as the outcome. Panel (b) uses the share attending a 5-star center as the outcome. The number of children is 2185 in panel (a) and 2251 in panel (b). Standard errors are clustered at the family level. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

C.12 Definition of the Treated Group

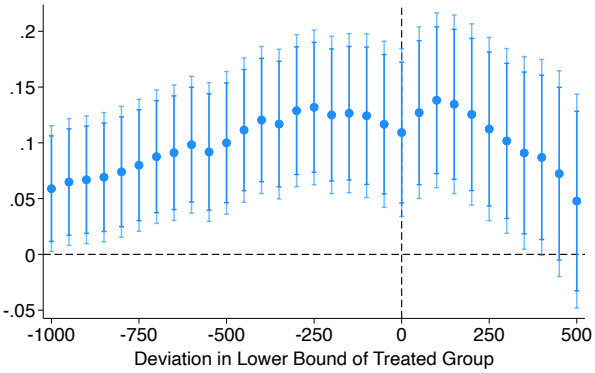
This section reports how the estimated effect of the 2022 reform on the choice of provider depends on the income range used to define the treated group. Each figure reports the pooled difference-in-differences estimate as the lower bound of the treated group's income range is moved in steps of \$50, from \$1,000 below to \$500 above its baseline value. As expected, as lower income families, who experienced a smaller change in marginal prices, are added to the treated sample the estimated effect is attenuated. Noticeably, the effect on the pre-reform price of the chosen provider eventually becomes insignificant while the effect on the post-reform price and the proportion attending 5-star centers remain statistically significant at the 10% level.



(a) Pre-reform price



(b) Post-reform price

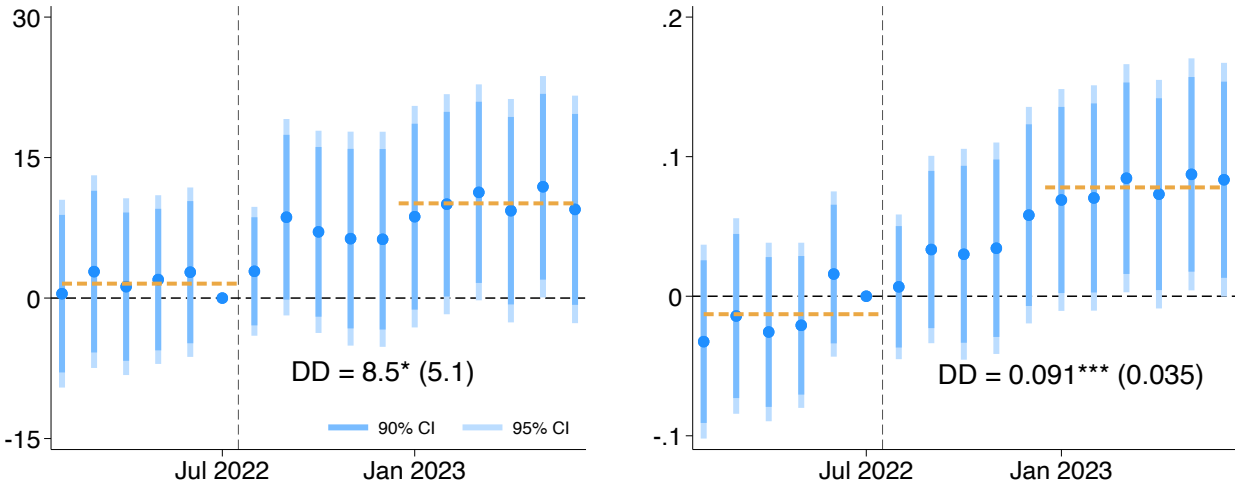


(c) Attending a 5-star center

Figure C14: Effect on chosen provider by treated definition

Notes: These plots show the pooled difference-in-differences estimates of the effect of the reform on choice of provider for different definitions of the treated group. For each estimate, the lower bound of the treated group's income range is shifted relative to the baseline lower bound by the dollar amount on the horizontal axis. Panels (a) and (b) report the pre-reform and post-reform prices of the chosen provider as the outcome. The pre-reform price is from June 2022 while the post-reform price is from September 2023. Panel (c) uses an indicator equal to 1 if a child attends a center that had a five star quality rating in the pre-reform period (or in the post-reform period for providers with no pre-reform quality rating). The baseline pre-reform and post-reform periods are used, which excludes the five months directly following reform. The control group is kept the same across specifications. Dark and light bars show 90% and 95% confidence intervals, with standard errors clustered by family. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

C.13 Robustness to Duration of Subsidy Receipt



(a) Pre-reform price

(b) Attending a 5-star center

Figure C15: Effect of the reform on choice of quality, including families in receipt of subsidies for less than a year

Notes: Same as Figure 3, but the sample also includes families in receipt of subsidies for less than a year. Panel (a) uses the pre-reform price of the chosen provider as the outcome. Panel (b) uses the share attending a 5-star center as the outcome. The number of children is 3105 in panel (a) and 3207 in panel (b). Standard errors are clustered at the family level. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

C.14 Quality-Dependent Subsidies: Estimates

Table C2: Effect of removing quality incentives on choice of quality

	1–4 star centers		5-star centers
	Below threshold	Above threshold	
post	-0.013**	0.018***	-0.013**
	(0.005)	(0.005)	(0.006)
Child-months	35081	35081	35081
Children	3491	3491	3491

Notes: This table reports the coefficient on the post-reform indicator underlying Panel (b) of Figure 4. The outcomes are indicators for attending a 1- to 4-star center priced below the voucher threshold (column 1), a 1- to 4-star center priced above the voucher threshold (column 2), and a 5-star center (column 3). Standard errors, clustered by family, are reported in parentheses. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table C3: Effect of removing quality incentives on choice of quality (any durations)

	1–4 star centers		5-star centers
	Below threshold	Above threshold	
post	-0.011**	0.017***	-0.017***
	(0.005)	(0.005)	(0.006)
Child-months	40608	40608	40608
Children	4448	4448	4448

Notes: This table reports the same estimates as Table C2, but does not exclude families in receipt of subsidies for less than a year. Standard errors, clustered by family, are reported in parentheses. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

D Effects on Equilibrium Prices

D.1 Price Effects of the July 2022 Reform

Figure D1 plots monthly difference-in-differences estimates of the effect of the July 2022 reform on the weekly price of preschool care at child care centers initially priced below the new voucher threshold, relative to centers at or above it.

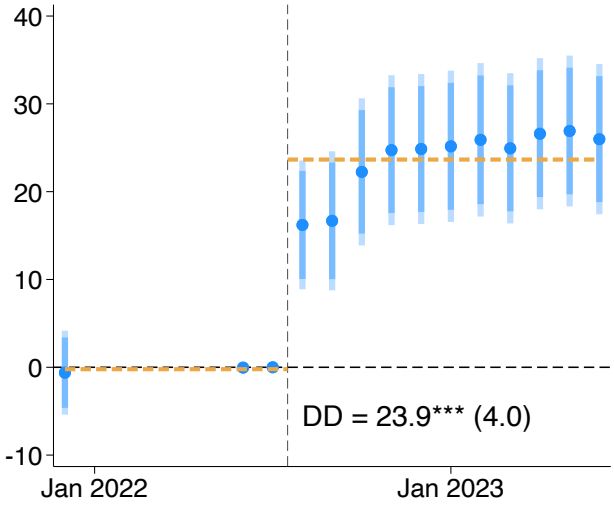


Figure D1: Effect on weekly price

Notes: Full-time weekly preschool prices (nominal dollars) of child care centers. Treated centers are those whose July 2022 price was below the new voucher threshold for their 2021 star rating; control centers are those at or above it. The figure plots monthly difference-in-differences estimates relative to July 2022, with 90% and 95% confidence intervals clustered by center, and the pooled estimate.

D.2 Price Response Using the Counterfactual with Reference Points

Figure D2 reports the same estimates as Figure 7, but the price response of the marginal buncher is calculated using the counterfactual distribution $h_0(p)$, estimated with reference points, in place of $h_0^{nr}(p)$.

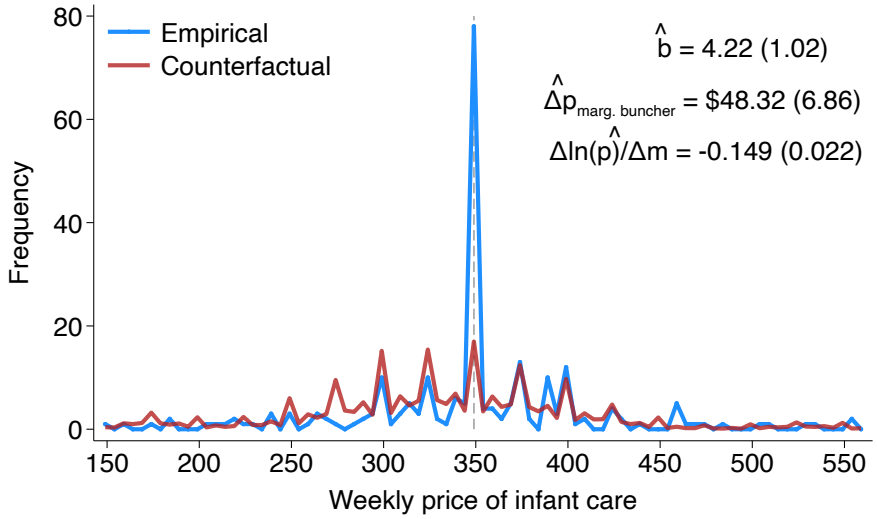


Figure D2: Bunching in the distribution of child care prices, 1 November 2023: counterfactual with reference points

Notes: Distribution of posted prices for full-time infant care among all Vermont child care centers on 1 November 2023. A bin width of $5 is used for the plot and a bin width of $1 is used for the estimates. The dashed line is at the kink price of $349. The price response of the marginal buncher and the semi-elasticity are calculated using the counterfactual distribution estimated with reference points. Bootstrapped standard errors based on 100 replications are reported in parentheses.

D.3 Bunching with $1 Bins

Figure D3 plots the price distribution and the estimated counterfactual using a bin width of $1, the bin width used for the estimates. The bunching mass is concentrated at $349, with little at $350.

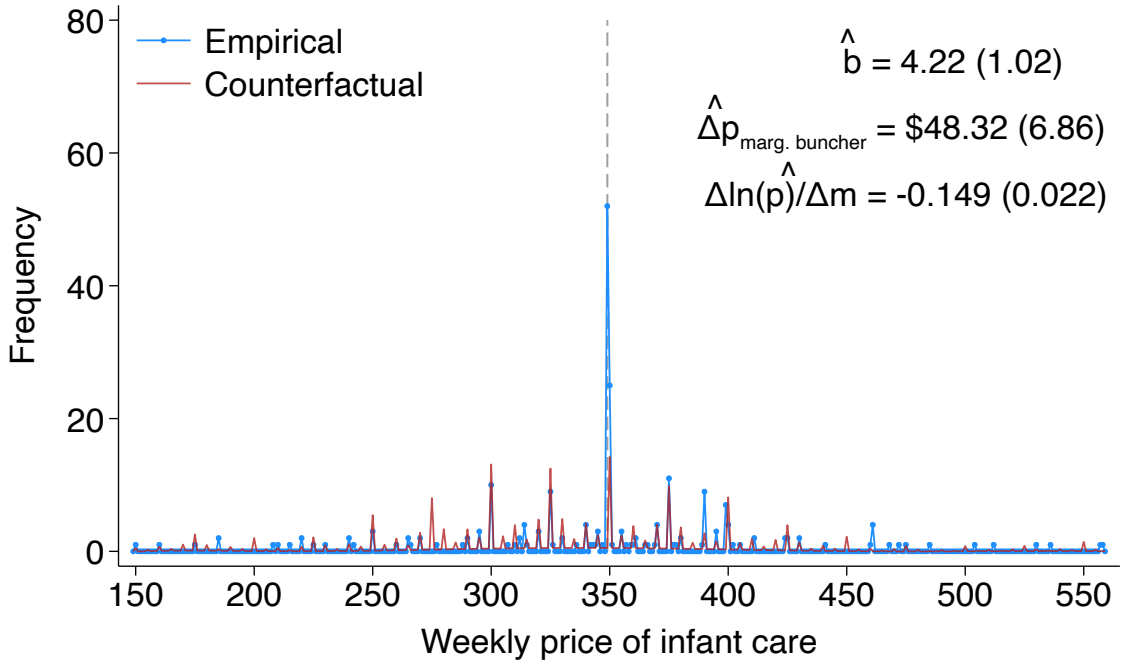


Figure D3: Bunching in the distribution of child care prices, 1 November 2023

Notes: Distribution of posted prices for full-time infant care among all Vermont child care centers on 1 November 2023. A bin width of $1 is used. The dashed line is at the kink price of $349. Bootstrapped standard errors based on 100 replications are reported in parentheses.

D.4 Bunching for Toddler and Preschool Care

Figures D4 and D5 repeat the bunching analysis for the prices of toddler and preschool care.

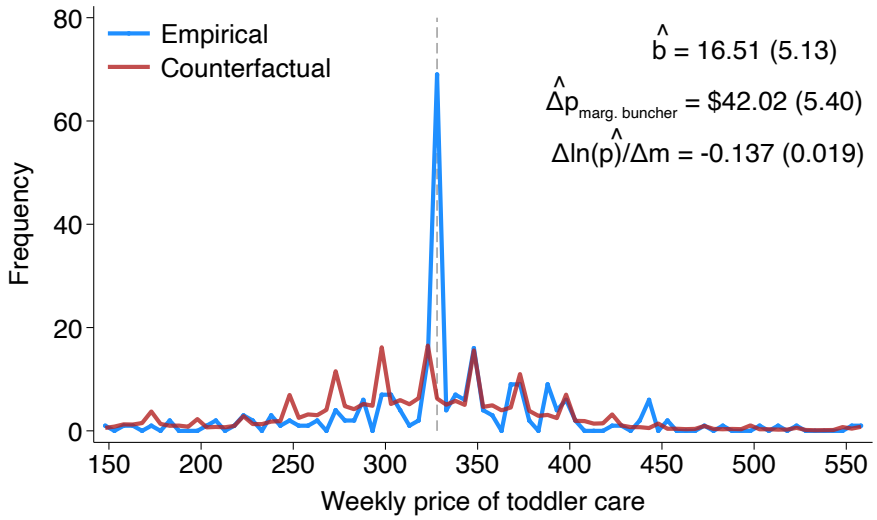


Figure D4: Bunching in the distribution of toddler prices, 1 November 2023

Notes: Distribution of posted prices for full-time toddler care among all Vermont child care centers on 1 November 2023. A bin width of $5 is used for the plot and a bin width of $1 is used for the estimates. The dashed line is at the kink price of $328. Bootstrapped standard errors based on 100 replications are reported in parentheses.

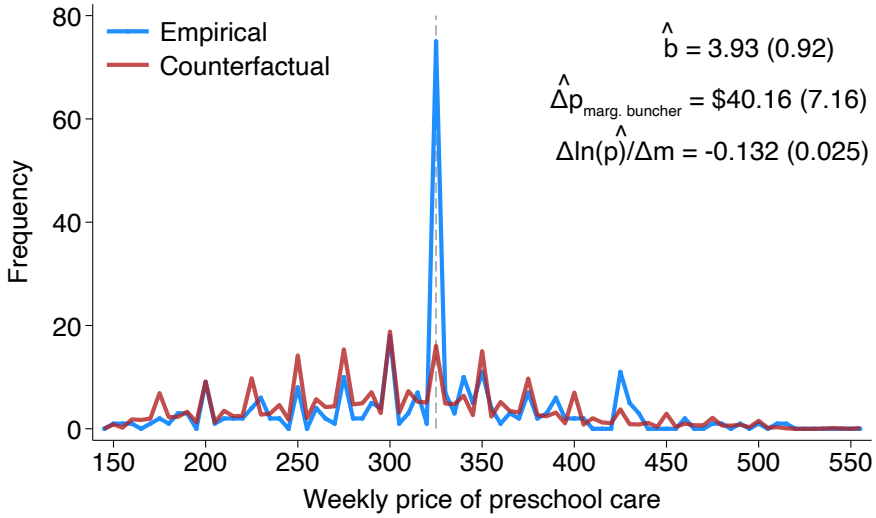
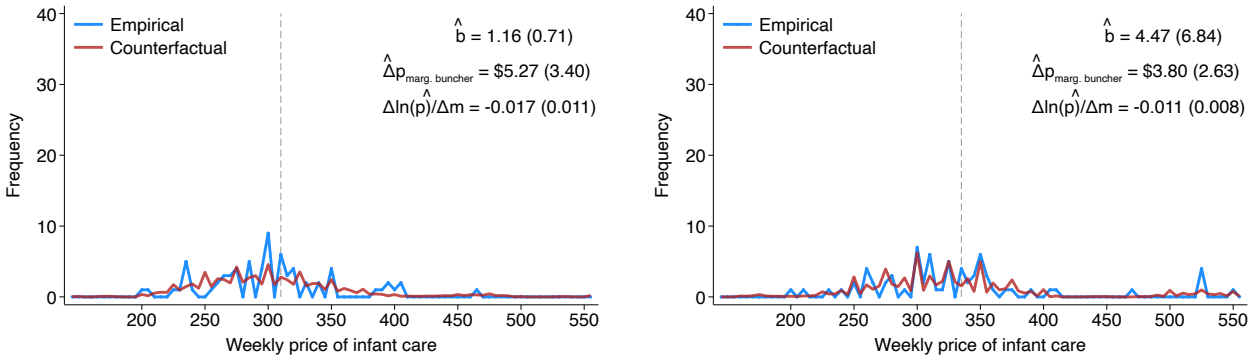


Figure D5: Bunching in the distribution of preschool prices, 1 November 2023

Notes: Distribution of posted prices for full-time preschool care among all Vermont child care centers on 1 November 2023. A bin width of $5 is used for the plot and a bin width of $1 is used for the estimates. The dashed line is at the kink price of $325. Bootstrapped standard errors based on 100 replications are reported in parentheses.

D.5 Bunching Before the July 2022 Reform

Figure D6 plots the distribution of infant prices on 1 July 2022, before the July 2022 reform, for 4-star and 5-star centers. Before the reform, most subsidy recipients were exposed to price differences below the kink, at least to some degree. There is little to no bunching at the kink for either group and the implied semi-elasticities are small and statistically insignificant. The threshold's role as a focal point alone therefore cannot explain the bunching observed in the data.



(a) 4-star centers

(b) 5-star centers

Figure D6: Bunching in the distribution of infant prices before the July 2022 reform

Notes: Distribution of posted prices for full-time infant care among Vermont child care centers on 1 July 2022, by star rating. A bin width of $5 is used for the plot and a bin width of $1 is used for the estimates. The dashed lines are at the kink prices of $312 for 4-star centers and $336 for 5-star centers. Bootstrapped standard errors based on 100 replications are reported in parentheses.

D.6 Bunching by Competition

Figure D7 reports bunching estimates for providers facing varying degrees of competition.

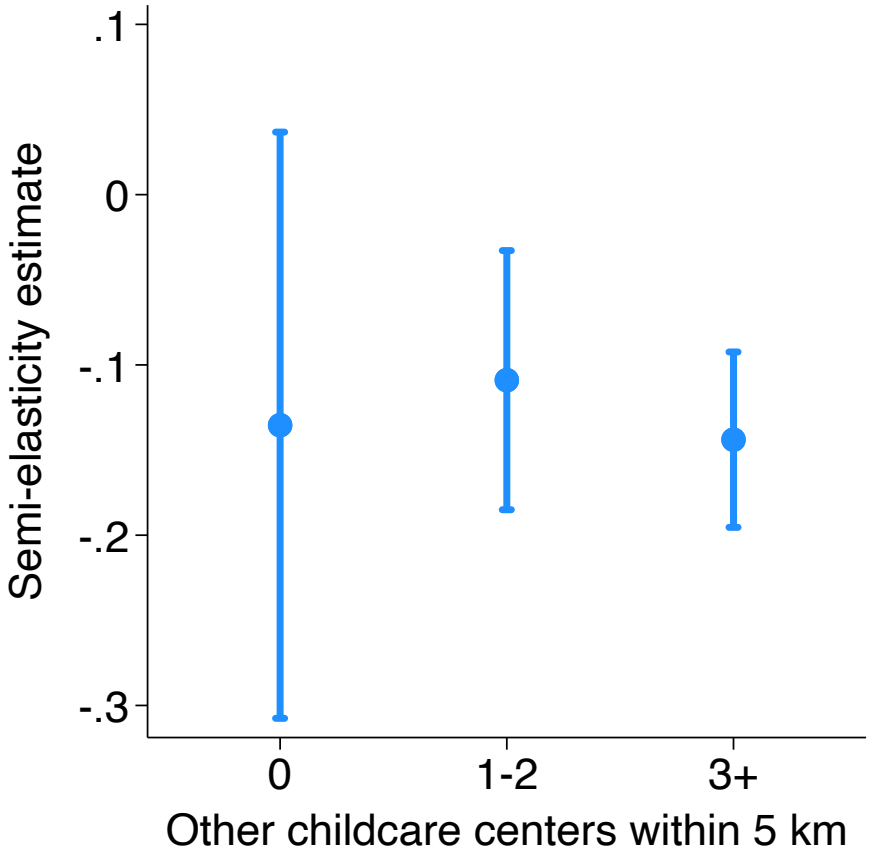


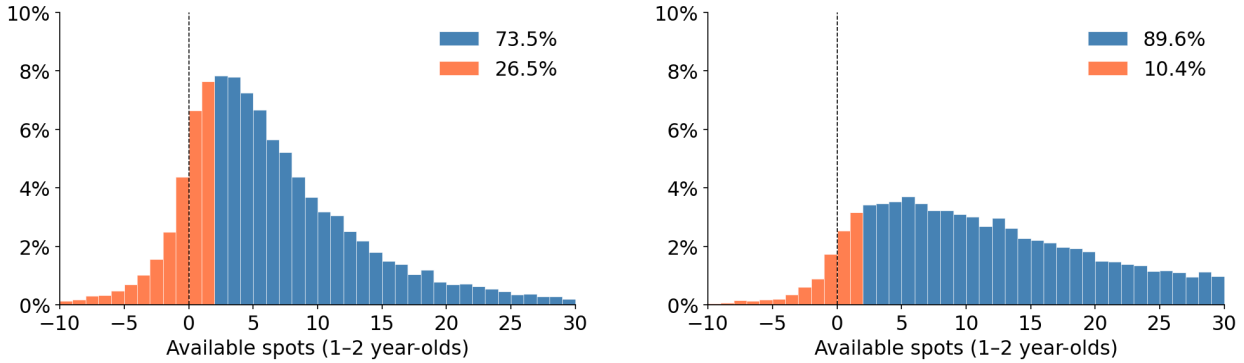
Figure D7: Robustness: bunching by competition

Notes: Bunching estimates for all child care centers on 1 November 2023, by number of competitors within 5 km.

E Model

E.1 Capacity

Figure E1 shows the distribution of unused capacity at child care centers, measured by space and staff (Panel (a)) and by space only (Panel (b)). Around 25–30% of providers operate at or near capacity in any given year. However, on average, these providers also expand their capacity in the following year. Regressing the change in a provider's space and staff-based maximum capacity on an indicator for whether the provider was constrained in the previous year, with year fixed effects and standard errors clustered by provider, gives a coefficient of 3.27 (s.e. 0.13). Figure E2 shows the distribution of unused capacity separately for lower-quality and high-quality providers, showing that lower quality providers are more likely to be constrained than higher-quality providers.

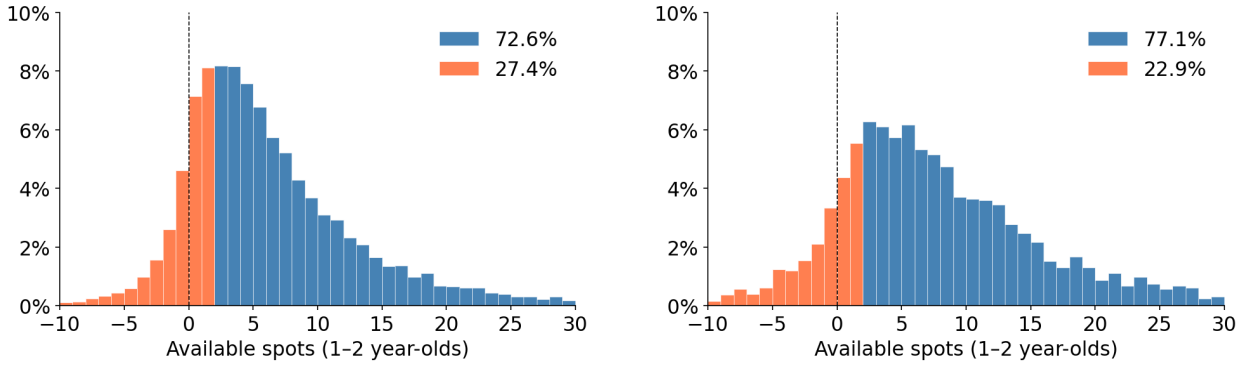


(a) Unused capacity (space and staff)

(b) Unused capacity (space only)

Figure E1: Unused capacity at child care centers

Notes: Distribution of unused capacity at Georgia child care centers.



(a) Lower quality

(b) High quality

Figure E2: Unused capacity by quality

Notes: Distribution of unused capacity (space and staff) at Georgia child care centers, by provider quality.

E.2 Markets

Markets are approximately commuting zones. Figure E3 shows the market boundaries and the child care centers used in the estimation.

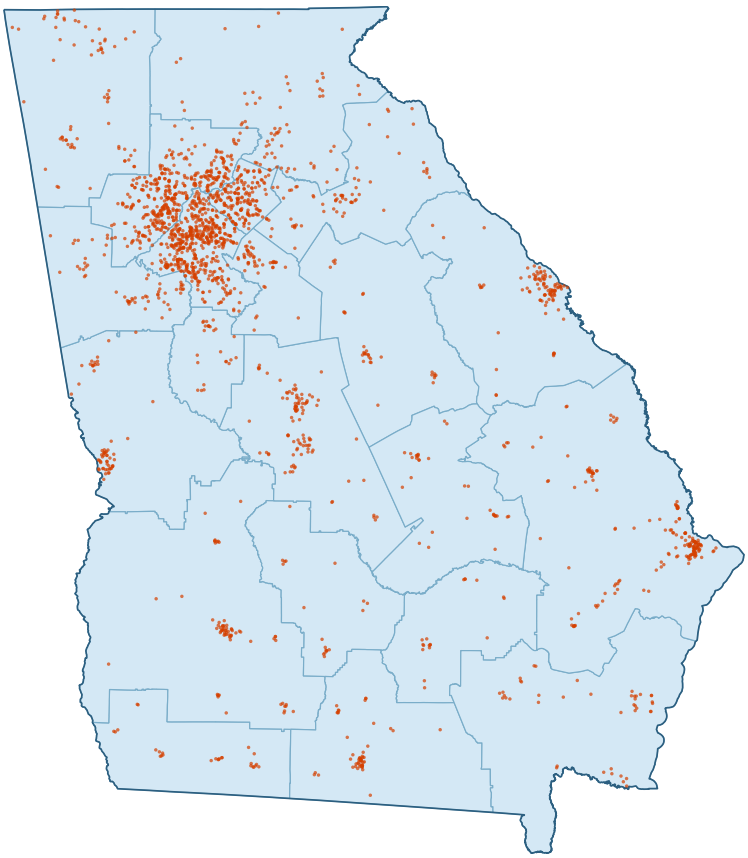


Figure E3: Centers and market boundaries

Notes: Commuting-zone market boundaries and child care centers in Georgia.

E.3 Moment Fit

Table E1 compares the targeted survey and Vermont moments with the moments implied by the model.

Table E1: Moment fit

Moment	Target	Model	Deviation
Center-care take-up (log: college / non-college)	1.404	1.340	−0.064
Distance shares (pooled across urban, suburban and rural)			
0–1 mile	0.199	0.108	−0.091
1–3 miles	0.283	0.319	+0.036
3–8 miles	0.344	0.435	+0.090
Vermont participation response (log change)	0.294	0.285	−0.009

Notes: The table compares the targeted moments with the moments implied by the estimated model.

F Policy Counterfactuals

F.1 Value of High-Quality Care

The benchmark is more efficient than the per-unit subsidy if the value β of high-quality care for subsidized children exceeds a break-even level, the value at which $\Delta W = 0$ in equation (4). Figure F1 plots this break-even value against the value placed on other child care consumption, which I define as all additional children in center care under the per-unit subsidy relative to the benchmark, other than subsidized children in high-quality care. Valuing this consumption reflects, for example, a positive fiscal externality from increasing the number of children in care. When other consumption has no value, the break-even value is \$34,587 per child-year if producer surplus receives full weight ($\alpha = 1$) and \$120,929 if it receives no weight ($\alpha = 0$). It then rises linearly with the value of other consumption.

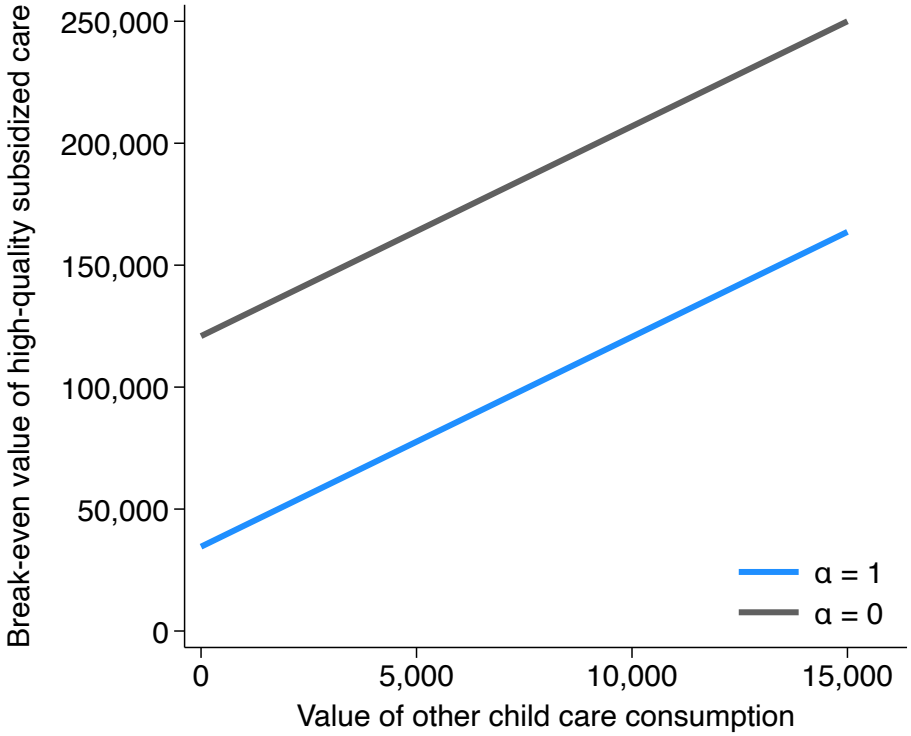
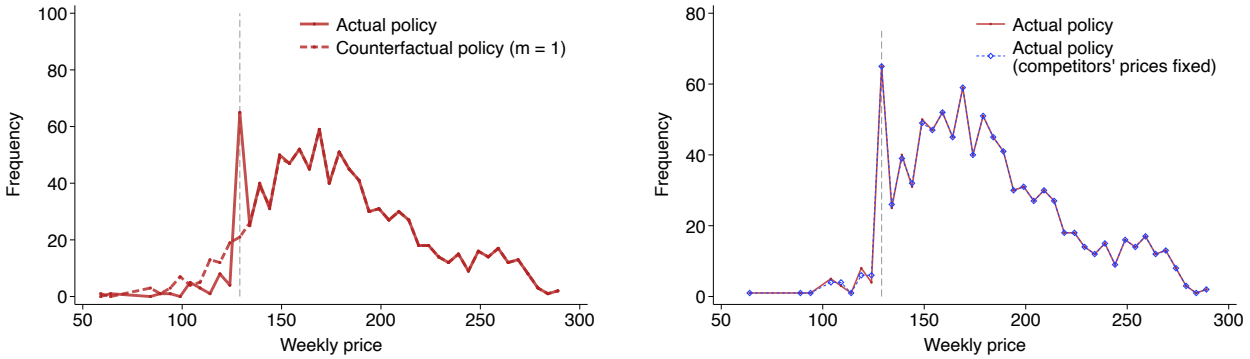


Figure F1: Break-even value of high-quality care

Notes: The figure plots the value of high-quality care for subsidized children above which the benchmark is more efficient than the per-unit subsidy, as a function of the value placed on each additional child in other center care (lower quality care or care consumed by the unsubsidized). The two lines show the break-even value when producer surplus receives full weight ($\alpha = 1$) and no weight ($\alpha = 0$). The per-unit subsidy adds 5,000 children per year in other center care relative to the benchmark, including unsubsidized children in high-quality care. Both axes are in dollars per child-year.

F.2 Using the Model to Evaluate Bunching Methods

Figure F2 uses the equilibrium model to assess the extent to which competition biases the bunching estimates. Panel (a) plots the equilibrium price distribution under the actual subsidy schedule alongside the distribution under the counterfactual schedule that removes the kink by extending the schedule above it to prices below. In panel (b), I solve for the price each center would choose under the actual, kinked schedule holding its competitors' prices fixed at their counterfactual levels. The resulting distribution is nearly identical to the price distribution under the actual policy, suggesting competitors' price responses have little effect on bunching estimates.



(a) Actual and counterfactual policy

(b) Holding competitors' prices fixed

Figure F2: Bunching with and without competitors' price responses

Notes: Panel (a) compares equilibrium prices under the actual subsidy schedule with equilibrium prices under the counterfactual schedule that removes the kink by extending the schedule above it to prices below. Panel (b) compares equilibrium prices under the actual schedule when providers' competitors are also able to adjust prices freely versus when their prices are held at their counterfactual levels.

Figure F3 semi-elasticities estimated using bunching to semi-elasticities directly calculated using the equilibrium model. This is done for kinks at different locations in the price distribution. For each kink location on the horizontal axis, I introduce a kink in the subsidy schedule at that price in the model and solve for the equilibrium. The blue line reports estimates from applying the bunching estimator to the resulting simulated price distribution. The red line instead reports estimates directly calculated using the average price response of the marginal bunchers. To identify the marginal bunching providers, the equilibrium price is solved under a subsidy with a \$5 higher kink price. The bunching providers that fail to bunch at the new kink are the marginal bunchers—those for whom the old kink was an internal solution. For these providers, the average price difference is calculated between their prices under the original kinked schedule and under a subsidy schedule that extends the schedule above the kink below it, removing the kink. The two estimates are close for most kink locations, and both are mostly between -0.05 and -0.15. Additionally, neither shows a clear

relationship between the semi-elasticity and the location of the kink. The direct estimates are noisier, as they rely on the few providers near each kink. The two diverge most at the lowest kinks, below $140 per week, where there are few providers.

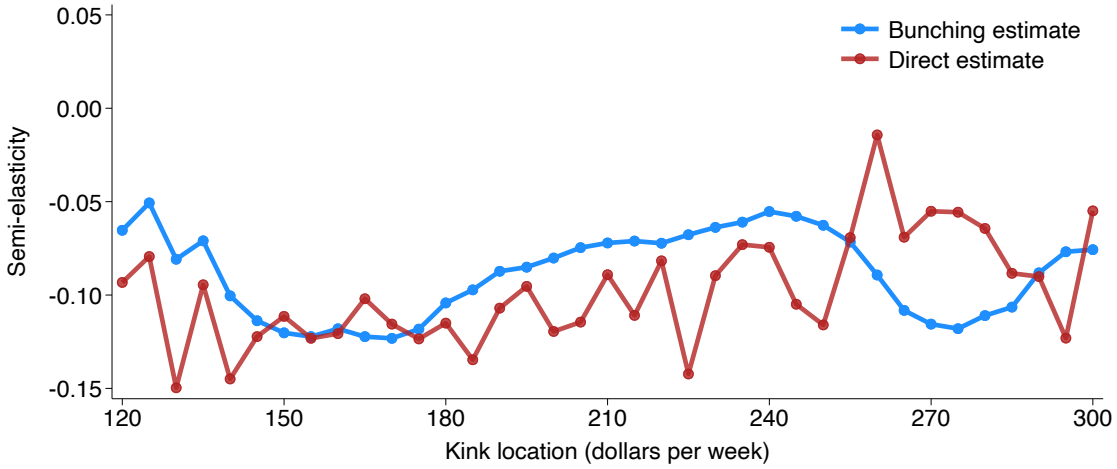


Figure F3: Semi-elasticity by kink location

Notes: The figure plots the semi-elasticity of the posted price with respect to the marginal price as a function of the location of the kink in the subsidy schedule, in dollars per week. The bunching estimate (blue) applies the bunching estimator to the price distribution simulated by the model. The direct estimate (red) calculates the average price response of the marginal bunchers directly from the model's equilibrium under counterfactual subsidy schedules.