The Benefits from Bundling Demand in K-12 Broadband Procurement[*]

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Abstract

We study a new market design for K-12 school broadband procurement that switched from school-specific bidding to a system that bundled schools into groups. Using an event study approach, we estimate the program reduced internet prices by 37% per Mbps per month while increasing bandwidth by 500%. These benefits occurred by mitigating exposure risk in broadband procurement – the risk that providers win too few contracts to cover fixed infrastructure costs. Using a bounds approach, we show robustness of our estimates and document that participants saved at least as much as their federal subsidies and experienced substantial welfare gains.

JEL: D44, H42, L86, L96.

Keywords: broadband internet, exposure problem, bundling, welfare.

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1 Introduction

In many auction settings, particularly in large-scale procurements, cost complementarities or returns to scale play an important role in determining outcomes because of the *exposure problem* (Milgrom, 2004). The exposure problem arises when there are complementarities or returns to scale across items, but each item is auctioned separately. In such cases, bidders cannot guarantee to win all complimentary items, so they are "exposed" to the risk of winning only a subset of items, leading to conservative bids, inefficiency, and lower revenue. A leading example of exposure is the FCC spectrum auction because carriers require adjacent frequencies and geographic regions for effective service.

Governments increasingly rely on large-scale procurements for private provision of public services (e.g., infrastructure, healthcare, and school lunches) that may be subject to exposure concerns. Given that public procurement represents 12% of GDP in OECD countries, improving its efficiency could generate substantial economic benefits. While auction theory has proposed *package bidding* (or bundling) as a solution, its computational and strategic complexity (Rothkopf et al., 1998), significant administrative burdens (Katok and Roth, 2004) and difficulty in maintaining transparency (Ausubel and Milgrom, 2002) limit practical implementation.[1] Moreover, there is limited direct empirical evidence on the benefits of design changes, such as bundling, on procurement outcomes.

In this paper, we provide direct evidence using novel data on the outcomes following a design change to K-12 broadband internet procurement, a setting with clear exposure concerns. In the U.S., the federal government subsidizes K-12 schools' internet contracts through a program called *E-Rate*. To receive the subsidy, schools must use competitive bidding to choose internet service providers (ISP), and each school typically organizes its auction individually. In 2014, the New Jersey Department of Education initiated a program that bundled willing schools into four regional groups, requiring providers to bid for entire regions collectively (pure bundles). This design is less complex than package bidding solutions used in other contexts (e.g., Fox and Bajari, 2013; Kim et al., 2014). We evaluate the price and bandwidth benefits of this design change.

Our empirical strategy compares contracts in 2014, before the program began, to contracts in 2015, the first year after the program began. In particular, we use a difference-in-differences (DiD) approach and estimate the average treatment effect of bundling on internet prices and bandwidth for the participating schools. Under the standard DiD assumptions, we estimate the causal effect of the program to be a \$10.32 per megabits per second (Mbps) per month price decrease (from a base of \$26.78) and a 978.22 Mbps increase (from a base of 267.59 Mbps) in the internet speed or bandwidth. Thus, a streamlined pure bundling that offers fewer but strategically chosen package combinations

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[1] There is a long literature that examines the trade-off between optimal and simple mechanisms. For recent advances in characterizing simple, practical mechanisms, see, for example, Li (2017) and Pycia and Troyan (2023).

improves prices and speed, providing new empirical evidence of the benefits of practical market design solutions in public procurement.

Two mechanisms rationalize the sharp price decline: mitigation of the exposure problem and increased competition for larger (bundled) contracts. First, ISPs face large fixed costs to build broadband infrastructure, which is built out in a network structure with hubs and spokes. Telecommunication markets have the feature that contiguous service areas are cheaper for providers to serve than dispersed ones (Ausubel et al., 1997; Beresteanu, 2005; Elliott et al., Forthcoming). Under decentralized procurement, ISPs risk winning contracts for only one school in a geographic area, which may lead them not to place as low bids as they would if they could bid on a bundle of schools in the same area. Second, ISPs have existing networks that only partially overlap. By grouping schools, the program may have induced additional competition for the bundle than for individual schools, which can also lead to lower prices.

To formalize the intuition behind the first mechanism (exposure), we present a stylized model with a simulation exercise demonstrating that pure bundling can reduce procurement prices. We then separately analyze the effect of bundling on two distinct internet services offered by the Consortium, which differ substantially in their fixed costs and infrastructure requirements. The first is a basic single-connection service that utilizes existing infrastructure, typically suitable for individual schools or small districts. The second, the most commonly demanded service, is a complex dedicated enterprise solution requiring significant infrastructure investment and regional hubs. Given these characteristics, the basic service faces minimal exposure risk, while the enterprise solution is particularly vulnerable to exposure. Our empirical results are consistent with this distinction: We find no significant price effects for the basic service but observe a substantial decrease of approximately \$12.70 per Mbps for the enterprise solution.

For the second mechanism (competition), the coefficient estimate on the number of ISPs is too small to explain the observed price reductions. While this latter observation is suggestive, as the number of ISPs may be endogenous and could vary systematically between the two internet services, our findings indicate that mitigating exposure risk through bundling, rather than increased competition, is the primary mechanism that generates the observed benefits.

We also evaluate the effect of participating in the program on schools' expenditures and welfare. Our estimates suggest substantial yearly cost savings to participating schools, ranging from \$2.43 million to \$11.25 million (in total) for participants who purchased Category D service, depending on assumptions about their bandwidth choices in the absence of the program. In comparison, participating Category D schools received a total of \$2.70 million in E-rate subsidies the year before joining the program.

Next, we determine how these savings translate into changes in schools' welfare. Typically, quantifying welfare change requires estimating schools' demand. Instead, we follow the "robust bounds approach" by Kang and Vasserman (2022) and determine the lower and upper bound for the *change in welfare* from 2014 to 2015. Under the assumption that the demand functions are log-concave, the change in welfare due to bundling is positive and potentially large, suggesting widespread benefits for schools.

Therefore, bundling demand delivers substantial savings to schools and the government. The federal government has identified fast and affordable Internet as a policy goal (United States Congress, 2021), and practically, the FCC has been concerned about the ballooning costs of supporting schools' broadband needs. One of the three goals of the 2014 E-rate Modernization Order (Federal Communications Commission, 2014) is promoting practices enabling schools and libraries to get the most out of the subsidies provided. Our results are important because they identify demand bundling as a potentially effective way to achieve this dual goal.

Participation in the program is voluntary, and only a subset of all New Jersey schools participate. We may be concerned about the assumptions required to estimate treatment effects with the DiD approach. To address this concern, we explore how sensitive our estimates are to the assumption of parallel trends. Specifically, we use the insights of Manski and Pepper (2018) and determine the treatment effects as a function of the degree of violation in the parallel trends assumption. Our results are robust to violations in parallel trends. In particular, we find that the treatment group trend must be about 1.45 times steeper than the control group trend to erase our treatment effect on the price (in the sense of not rejecting a zero difference at the 95% confidence level). The treatment effect on bandwidth disappears if the treatment group has twice as steep a trend as the control group. Furthermore, as we explain later, there are economic reasons to expect the trend to be steeper for the control group than for the treatment group, suggesting that our findings are robust to selection concerns.

We also perform additional robustness checks. First, some schools may not be appropriate as controls if they were locked into multi-year contracts in 2014. Second, some schools may be located near participating schools and thus have access to lower prices from winning providers. We find similar results after removing both types of schools from the control group. Third, while bundling could violate SUTVA through infrastructure spillovers affecting both participants and non-participants, these effects likely take several years to manifest and are not reflected in our data.

Additionally, to understand the program's potential total impact, we also examine potential effects on control schools by computing the difference in price between their current contract and the winning contract in their geographic region. Out of 64 schools that we can examine, 60 would have lower prices if they had participated–their average price would have decreased by \$10.4/Mbps, similar to our treated schools.

Related Literature

Our main contributions are to the empirical auction design literature that studies the benefits and proper design of auctions. Many papers (e.g., Bajari et al., 2009; Roberts and Sweeting, 2013; Covert and Sweeney, 2023; Ding et al., Forthcoming) have confirmed theoretical insights by empirically documenting the benefits of auctions. Our paper is the first to document the effect of pure bundling using field data. This result is important because others, using structural empirical methods, have shown that complementarities may play an important role in auction outcomes and that bundling or package bidding can significantly improve the efficiency of auctions, for example, see Cantillon and Pesendorfer (2010); Caplice and Sheffi (2010) for auctions of transportation services, Fox and Bajari (2013); Xiao and Yuan (2022) for demand complementarities in spectrum auctions, Kim et al. (2014); Agarwal et al. (2023) for procurements of school lunches, and Gentry et al. (2023) for the economy of scale in procurements of highway construction services.[2]

Our results will likely inform policymakers in other private-public settings, where essential services are provided by the private sector but paid for by public funds. Examples include healthcare, social security, and transportation. In recent years, policymakers across various sectors have employed market design tools to improve the outcomes of such public-private partnerships (Congressional Budget Office, 2017, 2020). As Decarolis et al. (2020) points out, the prima facie objective of using private markets in combination with public provision of such services is to "leverage the benefits of competition to provide high-quality services at low cost to both consumers and the government." Our results highlight that when such partnerships are structured to take advantage of demand aggregation and bundling, they can effectively enhance service delivery while minimizing costs. These findings underscore the broader applicability of our results, suggesting that carefully crafted procurement strategies can optimize outcomes not just in isolated cases but across a range of essential services where public funds are involved.

Lastly, we contribute to the policy literature on Internet access by understanding ways to use insights from market design to improve access and affordability of technological resources for K-12 students. Goolsbee and Guryan (2006) show that the E-rate program significantly improved California schools' internet access in the late 1990s. Recent federal initiatives continue to prioritize school connectivity, including the Emergency Connectivity Fund and Digital Equity Act Programs in the 2021 Infrastructure Investment and Jobs Act. However, their effectiveness remains to be evaluated. Our contribution is to document that bundling can complement these programs and improve internet access.

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[2] There is related work that documents the exposure problem and the benefits of package bidding using laboratory experiments (e.g., Kagel et al., 2010; Goeree and Lindsay, 2020).

2 Model

In this section, we present a model of procurement with economies of scale. The goal is to capture the mechanism through which bundling can lead to lower payments. We begin by outlining the basic setup, then describe decentralized procurement, followed by centralized procurement with pure bundling. The main result is that the total payments under pure bundling can be lower than the sum of payments under decentralized bundling when the economies of scale are sufficiently large.

Environment

Suppose there are two schools, and they are indexed by $s \in S = \{1, 2\}$. Initially, these schools conduct auctions for the Internet separately. Suppose there are $N \geq 2$ ISPs who are interested in servicing both schools. Let $c_{is} \in [\underline{c}, \overline{c}]$ denote ISP i's cost of providing internet service to school s. We assume that these costs are independently and identically distributed (across ISPs and schools) as $F_c(\cdot)$ with density $f_c(\cdot) > 0$.

Furthermore, suppose that if ISP i services both schools, then its total cost is

$$\varphi(c_{i1}, c_{i2}) = (c_{i1} + c_{i2}) - \Gamma, \tag{1}$$

where $0 \leq \Gamma \leq 2\underline{c}$ captures cost savings from serving both schools together. While Γ could depend on the observed characteristics of the schools, including the terrain, for parsimony, we assume that it is deterministic and common for all bidders. When it is clear from the context, for notational ease, we use φ_i to denote the total cost $\varphi(c_{i1}, c_{i2})$, and let $F^*(\cdot)$ be its distribution, which is given by an appropriate convolution of $F_c(\cdot)$, with support $[\underline{\varphi} = \varphi(\underline{c}, \underline{c}), \overline{\varphi} = \varphi(\overline{c}, \overline{c})]$.

Decentralized Procurement

Each school chooses the winning ISP as the one that submits the lowest bid among all the bids received by the school. For simplicity, we assume that all N ISPs submit bids for both schools simultaneously. ISP i, with costs (c_{i1}, c_{i2}), chooses bids (b_{i1}, b_{i2}) to maximize its expected profit:

$$\max_{(b_1,b_2)} \Big\{ \sum_{s=1}^{2} (b_s - c_{is}) \times \Pr(\text{win school } s \text{ and lose } s' \neq s) \\ + (b_1 + b_2 - \varphi_i) \times \Pr(\text{win both schools}) \Big\}. \tag{2}$$

Bidders cannot condition their bid for a school on the outcome of the other school, and because schools decide the winner independently, $\Pr(\text{win both schools}) = \prod_s \Pr(\text{win school s})$. This feature of decentralized bidding is the source of the exposure effect. Then, we can rewrite the objective function to make clear the benefit of serving both schools as follows:

$$\max_{(b_1,b_2)} \sum_{s=1}^{2} \left((b_s - c_{is}) + \frac{\Gamma}{2} \times \Pr(\text{win school } s' \neq s) \right) \times \Pr(\text{win school } s). \tag{3}$$

We focus on a symmetric bidding strategy $\beta : [\underline{c}, \overline{c}] \times [\underline{c}, \overline{c}] \rightarrow \mathbb{R}_+$. Under the assumption that $(N-1)$ bidders use the strategy $\beta(\cdot)$, an ISP i with costs $(c_{is}, c_{is'})$ chooses bids (b_s, b'_s) that maximizes its expected profit, i.e.,

$$\max_{(b_s,b'_s)} \sum_{s=1}^{2} \Big((b_s - c_{is}) + \frac{\Gamma}{2} \times (1 - F_c(\beta^{-1}(b_{s'})))^{(N-1)} \Big) \times (1 - F_c(\beta^{-1}(b_s)))^{(N-1)}, \tag{4}$$

where with a slight abuse of notation, we use $\beta(c_{is})$ to denote the bid submitted by an ISP for school s with cost c_{is} after suppressing the dependence of the bids on i's cost of serving school $s', c_{is'}$. The optimal bidding function in the decentralized system, which we denote by β^{pre} can be written as follows:

$$\beta^{\text{pre}}(c_{is}, c_{is'}) = \underbrace{c_{is}}_{\text{stand alone cost}} + \underbrace{\int_{c_{is}}^{\overline{c}} \left(\frac{1 - F_c(t)}{1 - F_c(c_{is})} \right)^{(N-1)} dt}_{\text{stand alone markup}} - \underbrace{\Gamma \times (1 - F_c(c_{is'}))^{(N-1)}}_{\text{expected cost savings}}. \tag{5}$$

Thus, even under decentralized bidding, the expected cost savings of serving both schools are reflected in equilibrium bids. Since there is a cost complementarity, an ISP will pass on the expected savings to increase its chance of winning. The bidding nests, as a special case, the bidding strategy in a standard procurement when $\Gamma = 0$. Adding (5) for both schools gives the total bid under decentralized procurement.

Pure Bundling

We now consider a centralized procurement model in which the two schools are bundled, and ISPs compete for both. They cannot bid for only one school. Thus, this system can be best understood as "pure bundling" because ISPs cannot bid for only one school. Under pure bundling, ISP i with costs (c_{i1}, c_{i2}) (and total cost $\varphi_i = \varphi(c_{i1}, c_{i2})$) chooses a total bid b to maximize its expected profit:

$$\max_{b} \Big\{ (b - \varphi_i) \times \Pr(\text{win}) \Big\}. \tag{6}$$

This maximization problem is a standard procurement problem for a single item where the cost is $\varphi_i \sim F^*(\cdot)$. Therefore, the equilibrium bidding strategy under centralized procurement is

$$\beta^{\text{post}}(\varphi_i) = \underbrace{\varphi_i}_{\text{total cost}} + \underbrace{\int_{\varphi_i}^{\overline{\varphi}} \left(\frac{1 - F^*(t)}{1 - F^*(\varphi_i)} \right)^{(N-1)} dt}_{\text{total markup}} . \tag{7}$$

This bidding strategy represents the equilibrium behavior of ISPs in the centralized procurement scenario. Each ISP with costs (c_{i1}, c_{i2}) and total cost φ_i chooses bid using Equation (7).

Analysis: The Benefits from Bundling

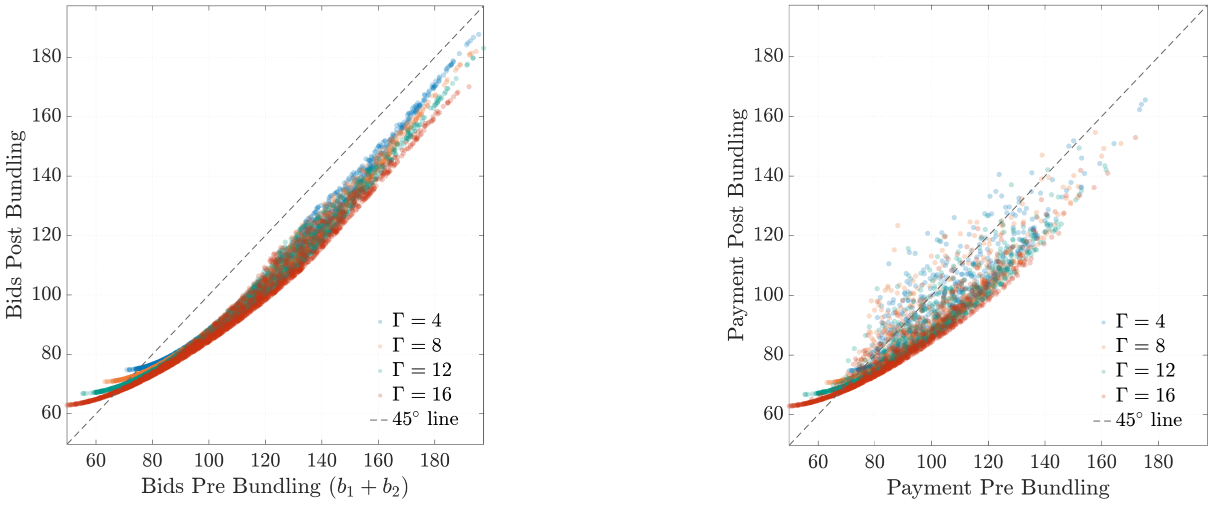
While, in general, bids under pure bundling may be smaller or larger than the total payments in the decentralized setting, establishing sufficient conditions under which it is always lower requires characterizing the relationship between the order statistics of individual costs and their sum, accounting for both the complementarity parameter and the number of bidders. See, for example, Krishna and Rosenthal (1996); Chakraborty (1999); Avery and Hendershott (2000) and Subramaniam and Venkatesh (2009), and the references therein for more on the topic. Instead, we use numerical simulations to demonstrate cases (different numbers of bidders and different complementarity parameters) where bundling reduces total payments by allowing bidders to internalize complementarities in their bidding strategies. While more limited in scope, this approach provides the necessary economic intuition for how bundling can mitigate exposure risk, which motivates our empirical exercise.

In particular, we consider procurements with $N \in \{2, 3, 5, 10\}$ bidders competing to provide broadband services to schools $s \in S = \{1, 2\}$. We assume that the costs for serving schools are Exponential random variables with a mean of 40, truncated on both sides at $[10, 100]$, and that these costs are independent across bidders and schools. Furthermore, we consider several values of cost-saving factor $\Gamma \in \{4, 6, 12, 16\}$. For each combination of N and Γ, we simulate $1,000$ auctions in decentralized and bundled settings.

Figure 1 contains visual results from our simulations for $N = 2$ ISPs. Figure 1(a) plots of all the total bids, and Figure 1(b) plots total payments before and after the bundling. The plots show that scatter points are mostly below the 45-degree lines, meaning that bundling can lead to lower total bids and total payments, and this decrease in bids increases with the level of economy of scale.

As the number of bidders increases, the competition leads to an even larger decrease in bids, all else equal. Table 1 presents the average total bids and payments for different bidders and economy of scale parameters. The averages are taken over the simulations and the bidders for each simulation. Notably, the average bids decrease with N and Γ.

Figure 1: Bids with and without Bundling



(a) All Bids (b) Final Payments

Note: The figure compares the bids before and after the bundling. Subfigure (a) shows the scatterplot of total bids before bundling (x-axis) and the total bids post bundling (y-axis) for fixed $N = 2$ bidders and different values of Γ, and (b) shows the corresponding total payments before bundling (x-axis) and post bundling (y-axis).

Table 1: Effect of Bundling on Average Bids and Payments

	All Total Bids				Final Payments			
$N\backslash\Gamma$	4	8	12	16	4	8	12	16
2	112.14	98.07	87.65	82.22	96.55	72.47	51.94	38.13
3	108.28	94.75	86.08	79.18	92.69	68.54	49.89	35.35
5	104.54	91.04	81.80	77.28	89.26	64.50	46.24	32.34
10	99.64	88.41	80.18	75.51	83.97	62.44	43.49	29.84

Note: The table presents the average total bids submitted by bidders across and the average final payments, for $(N, \Gamma) \in \{2, 3, 5, 10\} \times \{4, 8, 12, 16\}$. These bids and payments are averaged across bidders and $1,000$ simulations.

3 Institutional Details

Our setting involves the procurement of high-speed broadband internet by public and private school districts and libraries (henceforth, schools) in the U.S. state of New Jersey.[3] N.J.'s Digital Readiness for Learning and Assessment Project (DRLAP) was launched in 2013 by the state's Department of Education to help K-12 schools better incorporate technology into their classrooms. The broadband component of the program, known as DRLAP-Broadband, was designed to help schools work together to improve their internet access and network infrastructure to bridge the technology gap across schools and ensure internet access necessary to utilize new digital resources.

Typically and exclusively before 2014, schools in New Jersey organized the procurement of internet and other telecommunications services individually. In 2014, New Jersey

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[3] "Broadband" is a generic term for high-capacity transmissions, like fiber optic or coaxial wires.

began centralizing the procurement process to reduce costs and increase access to high-speed internet. The design change was meant to meet the need for federal internet subsidies. Below, we describe how subsidies work and the particular intervention in New Jersey.

3.1 The E-rate Program

In the U.S., K-12 schools can apply for subsidies for their internet expenses through a federally funded program called *E-rate*, which is administered by the FCC and funded by the *Universal Service Fund* under the *Telecommunications Act of 1996*. The subsidy ranges from 20% to 90% of a school's telecommunications expenditures, depending on the poverty level of its students and rural status. The *E-rate* program was designed to help eligible schools obtain internet. In particular, the FCC set a goal of 1 Mbps per student to support digital learning in every classroom. The total subsidy cap in 2023 was $4.5 billion. In 2023, 74% of school districts met this goal, compared to 8% in 2015.

The typical procurement and subsidy process is decentralized. A school determines the amount of internet it needs (e.g., unlimited internet with 1,000 Mbps download speed) and submits a request for competitive bids to the Universal Service Administrative Company (USAC) by filing FCC Form 470. USAC posts these requests on its website, and interested ISPs submit bids. After reviewing the bids, the school selects the most cost-effective ISP and files FCC Form 471 with details of the chosen ISP, following which either the school or the chosen ISP can apply to USAC for reimbursement. All eligible schools that conduct a fair and open competitive bidding process get the subsidies.[4]

3.2 Demand Aggregation Intervention in New Jersey

In 2014, as part of the New Jersey DRLAP, the state Department of Education planned a coordinated procurement process for broadband internet services called the Internet Cooperative Purchasing Initiative (ICPI, or the Consortium). Schools that choose to participate in the Consortium were asked to submit letters of intent and service order forms for networking and internet access services as part of a **consolidated Request For Proposals (RFP)** by spring 2014.[5] By June, 392 schools out of roughly 800 volunteered to be listed in the RFP, including 20 public charter schools and 25 private or non-public schools, the rest of which were multi-building school districts. Although the RFP included many services, we focus on dedicated broadband internet services. We describe the Internet services in more detail below because the details are important for our empirical analysis.

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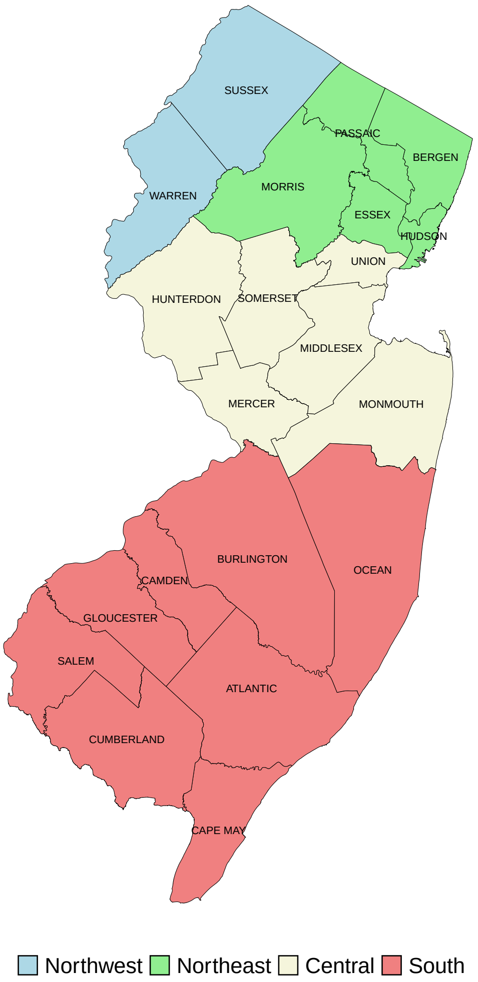
[4] See `https://e-ratecentral.com/Resources/Educational-Information/The-E-Rate-Process`.

[5] A copy of the RFP can be accessed at `https://charliemurry.github.io/ESCNJ_RFP_2014.pdf`.

Figure 2: Map of New Jersey with Demand Bundling



Note: This figure is a schematic map of the state of New Jersey in the U.S. It contains county boundaries, and each color denotes one of the four bundling regions.

After collecting demand information through the RFP, New Jersey conducted a reverse auction for the bundled school broadband services. The ISPs had to bid for all participating schools in a region so that the bundling can be best thought of as *pure bundling* and, in that regard, differed from *combinatorial auctions* as ISPs could not just bid for a subset of schools. The bids were weighted by qualitative factors such as the ISP's ability to provide service coverage that complied with the technical specifications, the deployment plan, company experience, and service support. Some competing ISPs were household residential providers, and others were large commercial backbone providers. Schools also completed a survey about their service level and price as of May 2014, i.e., prior to the introduction of the bundling policy.

The ISP that won the procurement rights for a region guaranteed to deliver the internet to *all* participating schools. The regions are illustrated in Figure 2. The state took care of the E-rate paperwork for the participants so that the winning ISP would be the official E-rate provider for these schools. Importantly, schools did not have to accept the terms after the procurement process ended, and ISPs knew this before they bid. However, the RFP also stated that other (non-participating) schools might be interested after the winners were selected. We observed both schools that were interested and backed out and schools that signed with a winning ISP in 2015, even though they were not officially part of the RFP.

Two products: Category A and Category D In practice, schools procured two types of internet services, the so-called Category A and Category D. Category A internet service is similar to residential retail internet service. A connection is made from the residential building to the ISP network, and the internet service (or payload) is provided through that connection (or transport). This type of product is most appropriate for single-location schools or small school districts with only a few locations because each location would need a separate connection, and no additional services were provided.[6]

Category D is a business-enterprise internet broadband solution. Under the Consortium RFP, the winning ISP was to provide dedicated internet to schools in the Consortium through a regional data hub, which requires high fixed costs. The schools, or school districts, would be connected to the hub through dedicated transport. Large school districts have a private wide area network (WAN) with a connection point that connects to the regional hub. Also, the ISP guarantees internet connectivity ("up-time") for this product, a typical business enterprise service. The typical purchaser of this product is a medium to large school district that has an existing WAN (or plans to deploy a WAN).[7,8]

Both products were offered at the same regional aggregation, but the winner in each category could be different. For example, Comcast may have won the Category A contract for the Southern region, but Verizon could win the Category D contract and thus provide a regional hub.

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[6] From the Program Results Report (2015): "Schools in Broadband Group A preferred to receive their internet access as an asymmetrical service delivered on a best-effort basis directly from an ISP. Group A contained 88 schools. Generally, these schools sought low-cost options like cable modems or Verizon FiOS [akin to products typically purchased by residential customers]. Some schools purchased multiple cable modems to serve different schools [each from a different asymmetrical connection points]."

[7] From the Program Results Report (2015): "Category D represented dedicated Internet access for schools seeking high capacity, symmetrical Internet access. It was either delivered after being purchased in bulk and distributed through regional WANs (e.g., Affiniti and Lightpath) or distributed directly (e.g., Comcast). Each school requesting service listed their desired amount of Internet access in the RFP."

[8] The Consortium also solicited services for installing and managing WANs for school districts, but this product is fundamentally different, and we do not study it.

4 Data Used in the Analysis

Our main data source is the Educational Services Commission of New Jersey (2014-2015) (ESCNJ, formerly Middlesex Regional Educational Services Commission), which ran surveys to collect information on internet contracts before and after the program. We supplement these data with E-rate data from the FCC through Form 471 (Federal Communication Commission, 2014-2015a) and Fixed Broadband Deployment data (Federal Communication Commission, 2014-2015b).

4.1 Data Sources

The ESCNJ provided us with the data they collected throughout the implementation of the new consortium procurement program. The information relevant to our analysis includes details of broadband contracts for all public school districts, charter schools, and private schools (including religious schools) that file for the FCC E-rate subsidies in the state. We observe all major contract terms before and after the Consortium, such as the contract price, delivery medium (e.g., fiber, DSL), bandwidth, servicing ISP, location, and other information about the school district. We also observed whether the school participated in the Consortium. We define a participant as a school that responded affirmatively to the initial ESCNJ RFP and signed a broadband contract with the winning ISP chosen by the Consortium. In our baseline analysis, we treat all other schools as non-participants, even though some have contracts with winning ISPs.

We merge these data with information from the FCC Fixed Broadband Deployment. These data include Census block-level information about which ISPs have active subscribers.

For the data we receive from ESCNJ to be suitable for our analysis, we make some sample selection decisions. First, we drop a few observations with unusually high prices above a threshold that appears to be a large break in the data; this drops 44/1179 observations with prices greater than \$150 per Mbps per month.[9] Second, we drop schools not present in the 2014 and 2015 surveys to achieve a balanced panel.

4.2 Description of Broadband Contracts

Before 2014, schools had existing contracts with ISPs. Schools receive internet by many transport types, including fiber, coaxial (cable), and digital subscriber line (DSL). Fiber is the highest quality/bandwidth transport medium and is the only way to connect many devices at high speeds with high-quality connections. However, in New Jersey, Comcast is a major ISP (headquartered in Philadelphia). It distributes the internet through its

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[9] Based on our consultation with the program administrator, we restrict our analysis to prices below \$150 per Mbps, as prices above this threshold seem unreasonable.

existing transport network, which is composed mostly of coaxial cables, although Comcast had also begun deploying fiber. In 2014, coaxial connections maxed out at 200 Mbps, whereas fiber connections could provide up to 10,000 Mbps bandwidth per connection.[10] We display the presence of ISPs, measured by the number of contracts they sign, across the four geographic regions for 2014 and 2015 in Appendix Table A.1.

We display descriptive statistics of the contracts we observe in Table 2. We separately describe the data for 2014 and 2015 and the participation status (participant or non-participant). We display prices as dollars per Mbps per month and bandwidth as Mbps. There is substantial variation in both price and bandwidth across schools. Some of the variation is across schools in the same region of New Jersey, and some of the variation is across regions. We display descriptive statistics by geographic region in Appendix Table A.2. Prices are typically higher and bandwidth lower in the Northwest region, a rural area of New Jersey. There was also a large decrease in prices and an increase in bandwidth from 2014 to 2015. This change was part of a general trend of decreasing prices and improved transport networks nationwide during this period.[11]

In Table 2, we also display contract terms and connectivity measures broken out by those schools that participated in the Consortium and those that did not. Looking at Table 2, in 2014, participants and non-participants had similar average prices and bandwidth. Both groups experienced large price decreases from 2014 to 2015, although participants experienced a larger average decrease. Likewise, both groups experienced increases in average bandwidth, although participant increases were much larger.

We also report a measure of the connectivity of schools to existing networks. We spatially merge data from the Fixed Broadband Deployment data to school district GIS boundaries. We count how many ISPs serve at least one residential building in the school district and report that as the number of active ISPs in a school district (“Num ISPs”). Both participants and non-participants average roughly six active ISPs.[12] Finally, we display other relevant connectivity measures in Table 2. The proportion of schools receiving fiber transport increased from 0.73 to 0.83, and coaxial decreased from 0.25 to 0.17 from 2014 to 2015.

5 Effects of Demand Bundling

In this section, we analyze the effect of consortium participation on school broadband prices and bandwidth. Recall that we define a participant as a school that responded affirmatively to the initial ESCNJ request for information and signed a broadband con-

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[10] The DOCSIS 3.0 technology that achieved 200 Mbps was introduced in 2006. The DOCSIS 3.1 protocol that achieved 1,000 Mbps was introduced in 2013 but likely not widely adopted in our sample.

[11] As a frame of reference, according to the FCCs “Urban Rate Survey,” the average residential connection speed for urban areas of the U.S. in 2014 was 53Mbps.

[12] We take this measure in 2014 only, so the table reports this measure twice across both years.

Table 2: Summary Statistics

	Pre Consortium (2014)			Post Consortium (2015)		
Outcome	**Mean**	**Median**	**SD**	**Mean**	**Median**	**SD**
Price	**27.42**	**18.00**	**28.45**	**13.29**	**8.30**	**15.02**
–Participant	26.78	16.50	27.74	5.41	3.61	4.11
–Non-participant	27.66	18.54	28.74	16.18	11.39	16.47
Bandwidth	**280.72**	**100.00**	**826.54**	**675.85**	**200.00**	**1615.05**
–Participant	267.59	100.00	378.34	1354.66	1000.00	2512.68
–Non-participant	285.55	100.00	939.37	426.46	112.50	1014.24
Num ISPs	**6.05**	**5.00**	**2.74**	**6.05**	**5.00**	**2.74**
–Participant	5.96	5.00	2.67	5.96	5.00	2.67
–Non-participant	6.08	5.50	2.77	6.08	5.50	2.77
Fiber	**0.73**	**–**	**–**	**0.83**	**–**	**–**
–Participant	0.78	–	–	0.98	–	–
–Non-participant	0.70	–	–	0.78	–	–
Coaxial	**0.25**	**–**	**–**	**0.17**	**–**	**–**
–Participant	0.21	–	–	0.02	–	–
–Non-participant	0.27	–	–	0.22	–	–
Other	**0.02**	**–**	**–**	**0.00**	**–**	**–**
–Participant	0.01	–	–	0.00	–	–
–Non-participant	0.02	–	–	0.00	–	–
Category D	**0.69**	**–**	**–**	**0.69**	**–**	**–**
–Participant	0.77	–	–	0.77	–	–
–Non-participant	0.66	–	–	0.66	–	–

Note: This table shows summary statistics for our sample of schools in New Jersey, broken out by year and whether the school participated in the ESCNJ Consortium.

tract with the winning ISP chosen by the Consortium. For our baseline analysis, we consider all other schools to be control or non-participants, even though some have contracts with winning ISPs. Our data lend to an event study strategy to estimate if the Consortium induced better outcomes among participants. In particular, we use a Difference-in-differences strategy to compare the mean outcomes for participating schools to non-participating schools before and after the Consortium. Then, using the structure of services provided by the Consortium, we provide evidence that suggests that the effects are primarily because bundling demand mitigates the exposure problem, as opposed to effects through competition.

However, as we discuss below, there may be a concern that parallel trends may not hold in our setting. To assess the robustness of our findings, we follow insights from Manski and Pepper (2018) and report event study estimates under different degrees of violation of the parallel trends assumption between participants and non-participants.

5.1 Event Study

We treat ESCNJ consortium participants as a treated group and all other schools as a control group to estimate the average treatment effect on the treated for price and broadband using the DiD strategy. In particular, we estimate the following regression specification to determine the average treatment on the treated under the DiD assumptions:

$$\begin{aligned} Y_{it} &= \beta_0 + \beta_1 \times \text{Participant}_{it} + \beta^{\texttt{Trend}} \times \text{Post-consortium}_{it} \\ &\quad + \beta^{\texttt{DiD}} \times (\text{Participant}_{it} \times \text{Post-consortium}_{it}) + X^{\top}\gamma + \omega_{it}, \end{aligned} \tag{8}$$

where Y_{it} is the outcome variable (price per Mbps or broadband) for school i in year $t \in \{2014, 2015\}$, $\text{Participant}_{it} \in \{0, 1\}$ is a binary variable equal to one if i is a participant and zero otherwise, $\text{Post-consortium}_{it} \in \{0, 1\}$ is also a binary variable that is equal to one to denote the year when the Consortium was available, and X is a vector of controls. Therefore, in this "two-by-two" setting, $\text{Post-consortium}_{it}$ is zero for all schools in $t = 2014$.

The estimation results from (8) are shown in Table 3, with our preferred specifications in columns (3) and (4), which include additional control variables. Our parameter of interest is $\beta^{\texttt{DiD}}$, which, under the DiD assumptions, is the effect of consortium participation for participating schools on the outcome variable. The estimates suggest that participation in the Consortium reduced the price of the internet by \$10.32 per Mbps and increased the chosen broadband speed by 978.22 Mbps. Both estimates are statistically significant at 1% level. Thus, under the parallel trends assumption, we conclude that demand bundling caused the price to decrease and the demand for bandwidth to increase.

5.2 Exposure versus Competition

There are at least two reasons that bundling could lead to a price drop in our setting. First, as our model highlighted in Section 2, ISPs may not fully realize the returns to scale when bidding on schools separately without bundling. Bundling can correct this exposure problem, although there is a countervailing force due to the distribution of costs (see Figure 1), so it is an empirical question whether the exposure problem is severe enough to distort bid up in the unbundled setting. As shown in Figure 1, bundling corrects the exposure effect to deliver lower prices when the returns to scale are sufficiently large. Second, bundling schools may induce more bidders for the bundle than there were for each procurement before bundling. More competitors should also lead to lower winning bids (see Table 1).[13]

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[13] Bundling may also have the opposite effect on the number of bidders. If the bundle sizes are too large, smaller ISPs may be unable to serve the entire bundle.

Table 3: Estimated Effect of Demand Bundling

	Price (1)	Bandwidth (2)	Price (3)	Bandwidth (4)
Non-participant	27.661^{***} (1.183)	285.549^{***} (65.732)	45.855^{***} (14.669)	192.117 (147.717)
Participant	26.779^{***} (1.952)	267.594^{**} (108.444)	46.202^{***} (14.875)	67.578 (179.795)
Post-Consortium	-11.477^{***} (1.674)	140.909 (92.959)	-10.993^{***} (1.548)	105.610 (89.698)
Participant x Post-Consortium	-9.889^{***} (3.229)	946.159^{***} (179.337)	-10.323^{***} (3.022)	978.220^{***} (174.289)
Number of ISPs			-0.742^{***} (0.277)	
Observations	990	990	990	990

Note: This table reports difference-in-differences estimates from equation (8) under varying specifications. The analysis is structured as follows: Columns (1) and (2) present baseline estimates without control variables, and Columns (3) and (4) present estimates with a full set of control variables, including school type, ISP, and region effects.
**p<0.05; ***p<0.01.

Although the policy change was not designed to disentangle the exposure effect from the competition effect, we present suggestive evidence that correcting the exposure problem is the dominant reason for the price decline by exploiting the differences in the two internet services offered by the Consortium. As discussed in Section 3, the New Jersey ICPI procured two types of internet service products.

Category A: A connection (transport) and internet service (payload) to the ISPs existing network. This product was designed for single-location schools and small school districts.

Category D: Dedicated transport to a regional hub and internet service (payload) through this hub. This service was designed for medium to large school districts and provided enterprise-level service, including guaranteed uptime.

The severity of the exposure risk likely differs between Category A and Category D products, driving heterogeneous effects of participation on prices and bandwidth. Category A involves connecting buildings to existing infrastructure, presenting a relatively modest exposure problem despite some variation in infrastructure proximity. In contrast,

Table 4: Effect of Demand Bundling for Different Internet Products

	Category A		Category D	
	Price (5)	Bandwidth (6)	Price (7)	Bandwidth (8)
Non-participant	7.423	265.167	46.519^{***}	185.169
	(10.828)	(188.373)	(15.357)	(188.738)
Participant	0.801	290.826	48.360^{***}	16.359
	(11.403)	(204.789)	(15.673)	(238.842)
Post-Consortium	-9.359^{***}	-0.688	-11.023^{***}	149.574
	(2.510)	(70.261)	(2.007)	(129.474)
Participant x Post-Consortium	-0.403	445.061^{***}	-12.698^{***}	1111.327^{***}
	(5.963)	(160.306)	(3.656)	(238.026)
Number of ISPs	0.107		-1.110^{***}	
	(0.464)		(0.347)	
Observations	308	308	682	682

Test: H_0: $|\beta^{\texttt{DiD}}_{CatD}| - |\beta^{\texttt{DiD}}_{CatA}| = 0$ and H_1: $|\beta^{\texttt{DiD}}_{CatD}| - |\beta^{\texttt{DiD}}_{CatA}| > 0$

Price p-value: 0.03

Bandwidth p-value: 0.001

Note: This table reports difference-in-differences estimates from equation (8) for the two products separately: Category A and Category D (explained in text). All the specifications include school type, ISP, and region and service type fixed effects. The analysis is structured as follows: Columns (5) and (7) present price as dependent variable for Category A and D, respectively. Columns (6) and (8) present bandwidth as a dependent variable for Category A and D, respectively. $^{**}p<0.05$; $^{***}p<0.01$.

Category D service, which requires connecting schools to dedicated regional hubs, faces substantially higher fixed costs, leading to exposure challenges. These projects often require new transport infrastructure and hub installations, creating high fixed costs that can only be efficiently amortized across multiple schools.

Table 4 presents separate event study results for each category. Columns (5) and (6) show price and bandwidth effects for Category A, while Columns (7) and (8) show results for Category D. Consistent with our expectations about exposure problems, we find no significant effects for Category A participants. In contrast, Category D participants show effects similar to our baseline estimates. The results suggest that exposure was a major factor in generating price declines due to Consortium participation.

Next, we assess the contribution of increased competition due to the Consortium. Although we do not have a measure of competition pre-Consortium, post-Consortium data shows that schools have an average of 6 ISPs in their location that can serve residential or commercial customers (see Table 2).[14] We estimate how broadband contract prices vary with the number of ISPs that can provide service. In our event study, we control for the number of ISPs with an active presence in a school district (or in the same Census Tract as a single school in the case of private schools). As shown in Column (3) of Table 3, broadband contract prices are about \$0.74 less for an additional present ISP (or \$1.10 for the Category D result in Table 4). Recall that the main effect of the Consortium from the event study is -\$10.32 (for the baseline specifications, Table 3) or -\$12.70 (for the Category D result in Table 4). Therefore, explaining the observed price effects would require approximately 12 or 13 additional ISPs, assuming a linear relationship between competition and prices.

It is implausible that the competition effect would dominate for two reasons. First, it would require too many ISPs (more than we observe in our sample) to enter the consortium auctions. In fact, the modal number of bidders across regions and service categories who submit bids with complete coverage of schools is one. Second, the assumption of linearity for the price-competition effect likely overstates the competitive effect as the impact of additional competitors is likely diminishing; (see, for example Watt, 2024). Therefore, while increased competition may have contributed to price reductions, the magnitude and pattern of our results suggest that the resolution of exposure problems through bundling was likely the primary driver of the observed price effects.

In the remainder of our paper, we focus on the Category D results in Table 4 because the benefits of the Consortium appear tailored to the Category D product, which is also the most common product demanded by schools.

5.3 Sensitivity Analysis

5.3.1 Violations of Parallel Trends

The Consortium was marketed to all schools and school districts in New Jersey participating in *E-rate*. Their participation was voluntary, and not all schools joined the Consortium. There may be several reasons why a school did not participate. For example, some schools were on existing long-term contracts and, presumably, they could not switch. Other districts use third-party E-rate consultants and may delegate all broadband decisions to those consultants, who would lose business if their clients joined the Consortium. Other schools (perhaps large school districts) may expect no gain by pooling demand with other (smaller) schools.

As such, participants may have different baseline trends in prices and bandwidth than non-participants. First, participating schools may join the Consortium because they have poor prospects for gaining cheaper and faster internet through their current procurement

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[14] The FCC did not collect data on the number of bids for Erate before 2015.

process. This "negative" selection into the program would imply that our DiD estimate understates the true effect. Second, schools that think they have the most to gain are also in areas that would naturally have steeper gains. They could be in areas that are "catching up" in broadband availability. This "positive" selection into the program would imply that our DiD estimate overstates the true effect.

We explore how our estimates would change if the parallel trend (invariance) assumption were violated. To that end, we take an approach suggested by Manski and Pepper (2018) and determine the DiD estimate if the trend for the treatment groups were $g \in [0, 2]$ times the trend for the control groups, such that $g = 1$ denotes the baseline with parallel trends. With a slight abuse of notations, we consider violations of the parallel trends assumption of the form

$$\hat{\beta}^{\text{Robust}} = \hat{\beta}^{\texttt{DiD}} + [\hat{\beta}^{\texttt{Trend,0}} - \hat{\beta}^{\texttt{Trend,1}}] = \hat{\beta}^{\texttt{DiD}} + [\hat{\beta}^{\texttt{Trend,0}} - g \times \hat{\beta}^{\texttt{Trend,0}}], \tag{9}$$

where $\hat{\beta}^{\texttt{Trend,0}}$ is the trend for the control group, $\hat{\beta}^{\texttt{Trend,1}}$ is the (unidentifiable) trend for the participants. For the second equality we imposed that $\hat{\beta}^{\texttt{Trend,1}} = g \times \hat{\beta}^{\texttt{Trend,0}}$ with $g \geq 0$ is the degree of violation of the parallel trend. We ask, "What if the participants had a g times the trend of the control schools?" Note that this setup nests our baseline specification with parallel trend assumption ($\hat{\beta}^{\texttt{Trend,0}} = \hat{\beta}^{\texttt{Trend,1}}$) as a special case, i.e., when $g = 1$ we have $\hat{\beta}^{\text{Robust}} = \hat{\beta}^{\texttt{DiD}} = \hat{\beta}^{\texttt{ATT}}$, identifying the causal effects of the Consortium, as shown in Table 4.

In Figure 3, we present the estimates of $\hat{\beta}^{\text{Robust}}$ (represted as a "dot") from (9) using the estimates from the Category D product for $g \in \{0, 0.10, \cdots, 1, \cdots, 1.90, 2\}$. For instance, to estimate $\hat{\beta}^{\text{Robust}}$ for price, we use $\hat{\beta}^{\texttt{Trend,0}} = -11.023$ and $\hat{\beta}^{\texttt{DiD}} = -12.698$ (see Table 4 Column (7), coefficients Post-consortium and Particiapant × Post-consortium, respectively). We use the standard errors and covariance from Table 4 to determine the 95% confidence intervals.[15]

The red confidence interval is our baseline estimate with $g = 1$. Next, consider the price effect under the extreme case of $g = 2$. Then, we assume that the participant trend would have been twice as steep as the non-participant trend, or $2 \times -\$11.023 = -\$22.046$ for the price outcome, and the treatment effect would be \$12.698 + (\$11.043 - \$22.056) = -\$1.685. In other words, the treatment effect point estimate would almost disappear if the price trend for participating schools was twice as steep as that of non-participants in the world where they did not participate. The estimates suggest that the treatment effect would be completely erased if participant price trends were slightly more than

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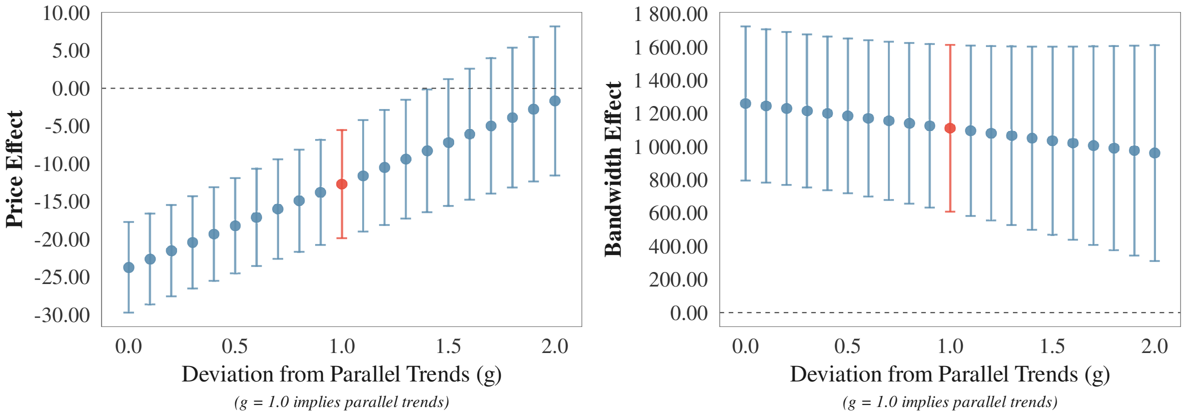
[15] Manski and Pepper (2018) have a long panel and use past data to inform the level of violation (g). Furthermore, Rambachan and Roth (2023) propose a uniformly valid inference procedure when the parallel trends assumption is violated. In our "two-by-two" setup, we do not have past data that can inform g, underpinning our choice of a fixed g. Furthermore, our confidence intervals do not consider sampling variability that may affect non-participant trends, nor do we adjust for multiple testing.

Figure 3: Estimates Accounting for Violations in Parallel Trends



Note: This figure displays robust coefficient estimates ($\hat{\beta}^{\texttt{Robust}}$) from equation (9) for both price (left panel) and broadband demand (right panel). Each panel shows the estimates and their 95% confidence intervals across for $g \in \{0, 0.10, \cdots, 1, \cdots, 1.90, 2\}$, which represents the magnitude of potential parallel trends violations.

twice as steep (decreasing) than that of non-participants. Taking into account confidence intervals, the statistical significance of the robust estimate disappears if participant price trends are approximately 1.4 times that of the non-participant price trend.

The treatment effect on bandwidth is less sensitive to the assumption of parallel trends. In other words, the treatment effect is so large and the non-participant trend so flat that it would take an extreme difference in trends to “wash out” our estimated effect. The statistical significance disappears if the trend for participants is twice as steep.

5.3.2 Robustness: Definition of Control Group

Although the treated and control groups look fairly similar in the pre-treatment period (see Table 2), we assess the robustness of the definition of the control group. Some schools may not be actively choosing a new ISP in 2014 when the Consortium was announced because they may have signed a previous multi-year contract. If so, those schools may not be the appropriate control group. As a robustness exercise, we exclude schools with the same contract (i.e., the same ISP, bandwidth, and price) in 2014 and 2015 that we previously deemed to be in the control group. This process eliminates 40 schools from the analysis. The results with this alternative sample are in Columns (9) and (11) of Table 5. The results are similar to those in Columns (7) and (8) of Table 4.

Additionally, some schools that did not participate in the Consortium may still be impacted because they can contract with the winning ISP at a lower (winning bid) price. Again, these schools may not be the appropriate control schools. For a robustness specification, we exclude those schools that did not participate in the Consortium but signed 2015 contracts with the winning ISP in their region. There were 162 such schools – the winning ISPs are popular choices even without the Consortium program. The results with this alternative sample are in Columns (10) and (12) of Table 5. Excluding those

Table 5: Robustness Checks: Alternative Sample Specifications

	Price (9)	Price (10)	Bandwidth (11)	Bandwidth (12)
Non-participant	65.051***	69.307***	−390.922	−635.482
	(22.144)	(19.921)	(1313.365)	(1567.967)
Participant	64.964***	73.290***	−525.583	−794.957
	(22.414)	(20.562)	(1330.024)	(1619.586)
Post-Consortium	−13.273***	−9.545***	161.760	122.847
	(2.273)	(2.562)	(135.044)	(201.916)
Participant x Post-Consortium	−10.709***	−13.672***	1090.105***	1170.425***
	(3.908)	(3.855)	(232.137)	(303.829)
Number of ISPs	−0.979**	−1.239***		
	(0.380)	(0.409)		
Observations	606	462	606	462

Note: This table presents robustness checks using alternative sample specifications. All the specifications include school type, ISP, and region and service type fixed effects. Columns (9) and (11) restrict the sample to active participants only. Columns (10) and (12) exclude impacted schools from the analysis. All specifications include the full set of control variables and include only Category D schools.
**p<0.05; ***p<0.01.

non-participants that contract with winners yields a similar effect of bundling than the estimate in Columns (7) and (8) of Table 4.

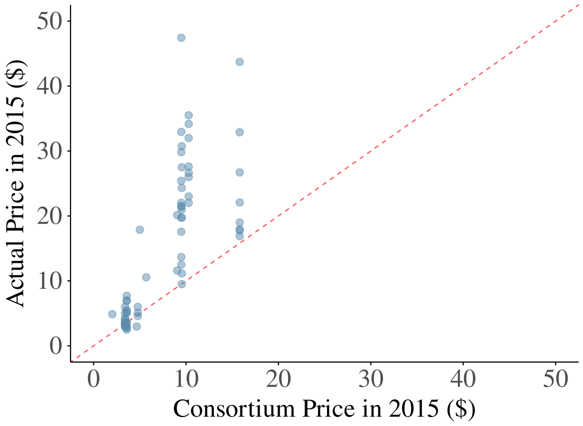
5.3.3 SUTVA and Selection

An additional identification concern involves the program's effect on untreated schools because broadband prices are determined in equilibrium, a possible violation of SUTVA. SUTVA requires the absence of spillover effects, which may be violated in our setting if a school's participation in the Consortium influences equilibrium bids across the region.

We identify two specific concerns. First, control schools may receive lower prices in 2015 if the winning Internet Service Provider (ISP) adjusts its pricing strategy based on new Consortium contracts. However, this would only attenuate our estimates of the price difference between treated and control schools, making our results conservative. Second, the program's infrastructure build-out could lower costs for the control group. We mitigate this concern by restricting our analysis to one year before and after the Consortium's establishment, as such infrastructure effects would take several years to materialize.

Our setting permits us to consider the potential impact of joining the Consortium on those who did not participate. This counterfactual analysis is a way to investigate

Figure 4: Price Difference Between 2015 Contract and Winning Bid for Control Schools



Note: Scatter plot of the 2015 actual prices and the Consortium prices for the schools' chosen bandwidths. Represents all schools that did not express interest in the program and did not sign a contract with a Consortium winner.

whether selection drives our results. Specifically, we examine the potential savings for non-participating schools had they individually joined the Consortium in 2015. We focus specifically on schools that neither expressed interest in the program nor held contracts with the winning ISP in their region, allowing us to avoid capturing potential spillover effects. We also restrict the analysis to schools purchasing a Category D-type product. We compare these schools' actual 2015 contracts with the winning ISP's price for the bandwidth mentioned in their current contracts. Sixty-four schools in our sample did not participate in the Consortium, contracted with a non-winning ISP in 2015, and purchased a bandwidth level available from their region's winning ISP.

Figure 4 compares actual 2015 prices with Consortium prices for these 64 schools. We find that 53 schools paid higher prices than they would have under the winning ISP's Consortium bid. Among these schools, the average potential savings from joining the Consortium would have been $9.62 per Mbps, comparable to the baseline estimate of a $12.70 per Mbps decline in price (Table 4). For the 11 schools that would not benefit from switching, the average difference in their price and the Consortium's is $0.54.

The similarity between potential savings for non-participants and realized savings for participants suggests that selection into treatment is not driving our main results – schools that chose not to participate appear to have forgone similar benefits to those realized by participants. This finding indicates that factors other than heterogeneous potential gains likely drove participation decisions. Future research could explore these participation frictions and the program's dynamic implications, though such analysis would require additional data from subsequent years and program waves.

6 Expenditures and Welfare

In this section, we measure the effect of the Consortium on the schools' expenditures and welfare. First, we use the DiD estimates from Category D participants to determine the savings from participating in the Consortium. Second, we use observed prices and broadband choices before and after the Consortium to determine the bounds for the change in school welfare for participating and non-participating schools. Then, using the DiD estimates for different degrees of parallel trend violation, we isolate the change in welfare from participating in the Consortium. Our main finding is that savings are large relative to the total E-rate subsidy, and the program significantly increases the welfare of the participating schools.

6.1 Expenditures

In our 2014 sample, schools that participated in the Consortium and purchased Category D services spent a total of approximately \$5.23 million on internet services, of which \$2.70 million was reimbursed by the *E-rate* program. Next, we determine the savings that accrue to the schools from the Consortium and compare them to this *E-rate* subsidy.

As shown in the last section, the Consortium lowered the prices and increased the chosen bandwidth. In particular, under the parallel trend assumption, participation in the Consortium lowered prices for the schools by an average of \$12.70 and increased bandwidth by 1,111.33 Mbps. Using these estimates, we determine two savings measures, the Paasche and Laspeyres price indices for participants, which represent lower and upper bounds on savings, depending on the assumption of the chosen bandwidth absent the new prices.

First, we determine the savings by imposing the treatment effect, $\beta^{\texttt{DiD}}$, and holding school i's demand at the 2014 level, Q_{i0}. This exercise determines the lower bound for savings because it keeps the demand fixed at the 2014 level. Second, we determine the savings by allowing the broadband demand to increase by the treatment effect of bandwidth, 1,111.33 Mbps, which gives us the upper bound. We also compute the effective E-rate subsidy for the schools to benchmark these savings. In Table 6, we display all three calculations and the measurement of the bound for savings and the total E-rate subsidy for all the participating schools aggregated for the entire year.

We find that the savings ranges between approximately \$2.47 million and \$11.24 million. These savings translate into 90% to 416% of the \$2.7 million total E-rate subsidy that FCC paid to participating schools in New Jersey. Thus, the demand bundling program can obtain similar cost savings to the schools or greater bandwidth-adjusted savings at no cost to the taxpayers.

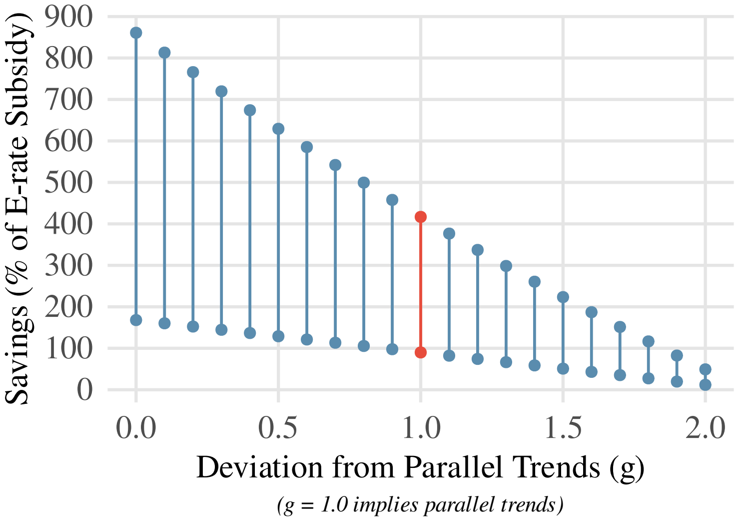
Similar to the robustness exercise in Figure 3, in Figure 5, we present the bounds on the savings, expressed as a percentage of \$2.69 million–the total E-rate subsidy paid to

Table 6: Bounds on the Expenditure Savings

	Expression	Amount
Lower Bound	$-\hat{\beta}^{\text{price}} \times Q_0 \times (1-\rho)$	\$2,427,239
Upper Bound	$-\hat{\beta}^{\text{price}}(Q_0 + \hat{\beta}^{\text{mbps}}) \times (1-\rho)$	\$11,249,680
E-rate Subsidy	$Q_0 \times P_0 \times \rho$	\$2,698,798

Note: $\hat{\beta}^{\text{price}}$ and $\hat{\beta}^{\text{mbps}}$ are the estimates in Table 4 Columns (7) and (8), P_0 is the price paid in 2014 and ρ is the E-rate subsidy rate.

Figure 5: Bounds on Savings under the Violation in Parallel Trends



Note: This figure illustrates the total cost savings achieved by participating schools in 2015, expressed as a percentage of the total E-rate subsidy (\$2.69 million). Each bar represents the upper and lower bounds for savings determined using the formulae in Table 6 (in columns marked "Expression"), with estimates based on β^{Robust} from equation (9) across a range of $g \in \{0, 0.25, \cdots, 1, \cdots, 1.75, 2\}$ of violation of parallel trends assumption.

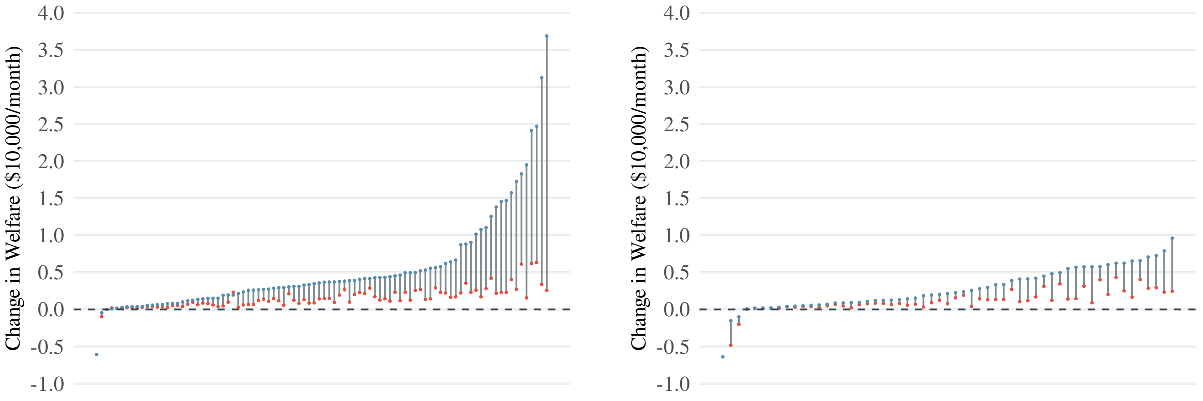
participating schools. As we can see, the bounds are strictly positive for violations of parallel trends less than a factor of 2. This evidence suggests that a policymaker deciding between implementing the current E-rate subsidy design and the ESCNJ Consortium design may prefer the latter because the latter achieves similar savings at no direct taxpayer cost.

6.2 Welfare of Schools

Measuring the total savings conflates lower prices and higher purchased bandwidth. In other words, a school's total cost could be unchanged, but it could be better off because it pays the same amount for greater bandwidth. A better measure of the Consortium's effect would be the change in welfare resulting from lower prices and greater bandwidth.

In this section, we quantify the change in welfare due to the Consortium. Let $D_i(\cdot)$:

Figure 6: Bounds for Change in School Welfare



(a) Participants

(b) Non-participants

Note: This figure shows the changes in school welfare, measured in $10,000 increments per month. Blue dots represent the upper bounds, while the lower bounds are shown in red dots, as calculated using equation (10). The left panel displays results for 117 participating schools, while the right panel shows results for 109 non-participating schools.

$\mathbb{R}_+ \to \mathbb{R}_+$ be the demand function for school i. For school s, we observe the prices and broadband choices pairs (net of the E-rate subsidies) before the Consortium, (P_{i0}, Q_{i0}), and after the Consortium (P_{i1}, Q_{i1}). We want to determine the change in welfare, ΔW_i, which is the area under the demand curve between two prices:

$$\Delta W_i = \int_{P_{i1}}^{P_{i0}} D_i(\xi) d\xi.$$

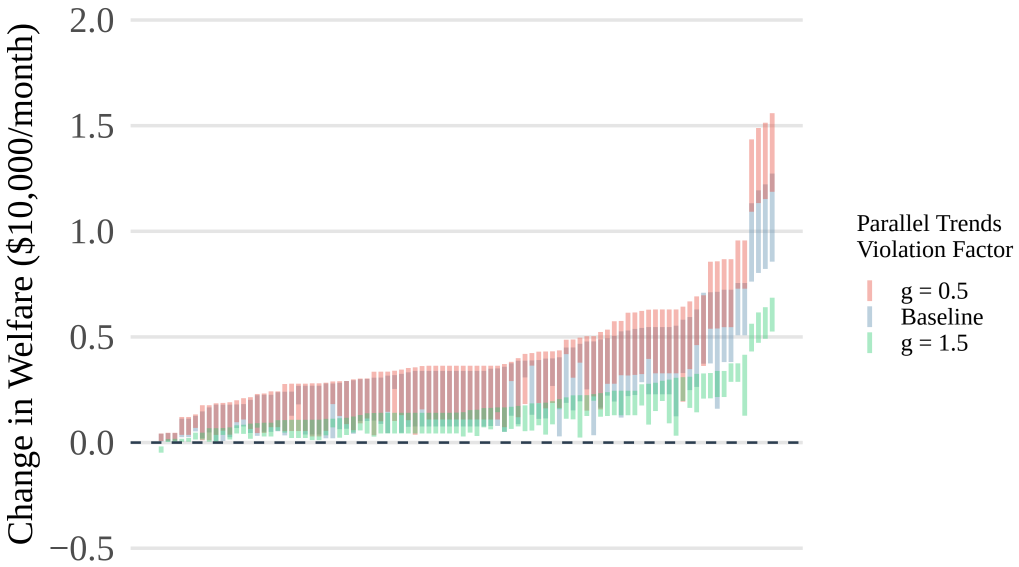
Instead of directly estimating the demand function for broadband for schools, which is difficult given our data, we rely on insights from Kang and Vasserman (2022) and instead determine the bounds for ΔW_i under the assumption that the demand functions satisfy log-concavity, i.e., $\frac{D_i'(P)}{D_i(P)}$ is decreasing in P for all schools. Kang and Vasserman (2022) show that the change in welfare can be bounded as

$$Q_{i0}(P_{i0} - P_{i1}) \leq \Delta W_i \leq \frac{(P_{i0} - P_{i1}) \times (Q_{i1} - Q_{i0})}{\log\left(\frac{Q_{i1}}{Q_{i0}}\right)}, \tag{10}$$

where this bound is sharp. Note that both the lower and upper bounds depend on the data. However, before determining these bounds, we select our sample appropriately.

Of the 516 schools we observed in both years, 135 of them participated in the Consortium. We discard schools that upgrade service, for example, from DSL to fiber, as we expect the demand function $D_i(\cdot)$ for different services to be different, and fiber availability may change across the two years. We are left with 117 participating schools and 109 non-participating schools. In Figure 6, we separately present these bounds for participat-

Figure 7: Estimated Bounds for Change in School Welfare



Note: This figure illustrates the bounds on welfare changes (in $10,000 per month) for participating schools. These bounds are calculated using equation (10), where we substitute $P_{i1} = P_{i0} + \hat{\beta}^{\text{price}}$ and $Q_{i1} = Q_{i0} + \hat{\beta}^{\text{mbps}}$. The coefficients are derived from equation (9) using three different values of g: 0.5, 1, and 1.5, where $g = 1$ denotes the baseline estimate.

ing and non-participating schools. We use observed prices for both periods to compare the welfare across the two groups. In both cases, most schools have a positive lower bound, likely because prices decreased and bandwidth increased for all schools between 2014 and 2015. In other words, comparing Figure 6(a) and (b) conflates a treatment effect with underlying common trends.

The welfare bounds presented in Figure 6 for participating schools reflect the total effect, including the effects of trends and product types, as they are computed using observed prices and quantities. To isolate the specific impact of bundling on welfare changes for the participating schools relative to the non-participating schools, we conduct a counterfactual analysis using our estimated price and quantity coefficients to determine the new $P_1 = P_0 + \beta^{\text{price}}$ and $Q_1 = Q_0 + \beta^{\text{mbps}}$ for all participating schools.

Figure 7 presents the change in welfare using coefficients defined in (9) and given in Figure 3. Specifically, we consider $g = 0.5$, $g = 1$, and $g = 1.5$, representing generous, baseline, and conservative estimates of the program's impact. In other words, this figure displays the true effect of the Consortium on participants' welfare for different violations of the parallel trends assumption. We find meaningful welfare gains for participating schools, reinforcing the positive impact of the new program.

7 Conclusion

In 2014, New Jersey implemented a program for schools to pool demand for broadband internet. Our analysis shows that schools can save substantial money by pooling their broadband purchases. In particular, we present three main findings: bundling led to lower prices and higher chosen internet speeds; total expenditure savings due to the consortium was between at least 90% of the current E-rate subsidy for participating schools, and there was a substantial increase in school welfare due to the consortium.

The success of this program has important policy implications. Rather than subsidizing individual school purchases, policymakers should consider helping schools coordinate their buying. Such programs could reduce costs to schools without requiring taxpayer funding. More broadly, this finding suggests that redesigning how public institutions purchase services may be as effective as traditional subsidies for achieving policy goals.

The program addressed two market failures: it helped internet providers mitigate exposure problem (Milgrom, 2004) and increased competition between providers. We find strong evidence that reducing bid exposure risk drove most of the savings, with effects concentrated in high fixed-cost Category D services rather than Category A services. Future research should examine how these mechanisms interact. In particular, carefully analyzing competition would require explicitly modeling entry decisions, as the market structure is likely endogenous to procurement design.

Lastly, while bundling may improve immediate procurement outcomes, awarding multiple schools to a single ISP could have unintended consequences for market structure. Specifically, ISPs who repeatedly lose bundled auctions may exit the market or reduce infrastructure investments, potentially reducing competition in the long run. Although we do not have the data beyond 2015 to analyze long-run outcomes, analyzing this tradeoff is an interesting future research agenda.

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A Appendix: Additional Data Description

As noted in the main text, there is substantial geographic variation in prices and bandwidth across New Jersey’s regions. Table A.1 provides a detailed breakdown of market, showing the number of contracts signed by each Internet Service Provider (ISP) across the four regions of New Jersey (Northeast, Central, South, and Northwest) in 2014 and 2015. The data reveal significant heterogeneity in ISP presence across regions. Comcast has a strong presence in the Southern region, while Lightpath dominates the Northeast.

The Northwest region, being predominantly rural, has notably fewer ISPs and contracts overall, with PenTeleData and NetCarrier being the primary providers.

Table A.2 further illustrates this regional variation by comparing key contract characteristics between consortium participants and non-participants across regions. Consistent with the discussion in the main text, the Northwest region exhibits systematically higher prices and lower bandwidth compared to other regions. For instance, in 2014, the average price per Mbps in the Northwest was nearly twice that of other regions, while the average bandwidth was roughly one-fifth. The technology mix also varies substantially across regions, with fiber being less prevalent in the Northwest pre-consortium. However, by 2015, participating schools across all regions show a marked shift toward fiber connectivity, accompanied by significant price reductions and bandwidth improvements.

Table A.1: Aggregated Vendor Data by Region, 2014 and 2015

	Pre-Consortium (2014)				Post-Consortium (2015)			
ISP	N.E.	Cent.	South	N.W.	N.E.	Cent.	South	N.W.
Comcast	17	50	121	10	19	59	139	10
Lightpath	77	33	1	1	99	39	4	1
XO	7	9	19	2	7	16	33	3
Verizon	19	22	15	0	23	13	8	0
Cablevision	21	11	3	1	16	13	3	0
PenTeleData	0	6	0	16	0	2	0	15
NetCarrier	4	1	0	7	4	3	1	8
Windstream	4	0	3	0	6	0	2	0
DNS	3	6	1	0	5	5	2	0
CenturyLink	0	6	1	4	0	4	0	4
Line Systems	0	1	8	0	0	0	8	0
FiberTech	0	4	5	0	0	2	7	0
Other	24	10	7	3	16	6	5	6
Total	176	159	184	44	195	162	212	47

Note. This table shows the number of school contracts by Internet Service Providers (ISPs) across four regions of New Jersey in 2014 and 2015. Regions are Northeast (N.E.), Central (Cent.), South, and Northwest (N.W.).

Table A.2: Summary Statistics by Region

		Pre Consortium (2014)				Post Consortium (2015)			
Outcome	Participant	Cent.	N.E.	N.W.	South	Cent.	N.E.	N.W.	South
Participant	Price	24.61	27.24	46.51	25.31	5.84	3.31	7.14	7.13
Non-participant	Price	18.19	28.28	49.04	29.98	12.22	15.94	32.89	15.78
Participant	Bandwidth	365.57	352.19	59.17	125.80	1419.00	2256.25	566.67	427.39
Non-participant	Bandwidth	488.68	217.12	59.67	227.97	613.23	379.63	122.03	382.50
Participant	Fiber	0.89	0.94	0.50	0.57	0.97	1.00	1.00	0.95
Non-participant	Fiber	0.75	0.74	0.59	0.67	0.77	0.87	0.66	0.75
Participant	Coaxial	0.09	0.06	0.50	0.43	0.03	0.00	0.00	0.05
Non-participant	Coaxial	0.22	0.23	0.38	0.32	0.23	0.13	0.34	0.25
Participant	Other	0.03	0.00	0.00	0.00	0.00	0.00	0.00	0.00
Non-participant	Other	0.03	0.03	0.03	0.02	0.00	0.00	0.00	0.00
Participant	Category D	0.89	0.94	0.50	0.55	0.89	0.94	0.50	0.55
Non-participant	Category D	0.66	0.67	0.59	0.66	0.66	0.67	0.59	0.66

Note. This table presents means of key contract characteristics by region and consortium participation status. Regions are Northeast (N.E.), Central (Cent.), South (South), and Northwest (N.W.). Price is measured in dollars per Mbps per month, and bandwidth is measured in Mbps. Fiber, Coaxial, and Other are indicator variables for connection type that sum to one within each region-participant-year cell. Category D is an indicator for the service type.