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RESTRICTIONS ON MUNICIPAL BROADBAND PROVISION AND MOTHERS' LABOR MARKET OUTCOMES: EVIDENCE FROM THE COVID-19 PANDEMIC

Saket S. Hegde
Jessica Van Parys

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ABSTRACT

Prior to the COVID–19 pandemic, 17 states had legally restricted local governments from building their own broadband infrastructure and/or providing broadband internet to their communities. We use Federal Communications Commission (FCC) data to show that broadband subscribership grew slower in states with these restrictions compared to states without restrictions post-2020, particularly in counties with less affordable plans and fewer broadband providers. Then we examine how municipal restrictions affected the labor supply of married women with children following the sharp increase in telework after the pandemic. We employ the Current Population Survey (CPS) from 2018–2022 to estimate event studies and difference–in–differences models. Results show that mothers' post-pandemic employment decreased by 4.7% in restriction states relative to non-restriction states additionally, mothers in restriction states left the labor force and shifted to caregiving. Among mothers continuing to work, the share of time spent working from home decreased by 49%, while commute time increased 27%, in states with broadband restrictions versus states without. The results suggest that mothers were less likely to enter the workforce in states where remote work opportunities were limited by restrictions on broadband provision.

Saket S. Hegde
Federal Reserve Bank of Philadelphia
saket.hegde@phil.frb.org

Jessica Van Parys
Hunter College of the City University of New York
Department of Economics
and NBER
jv947@hunter.cuny.edu

Introduction

The internet is an important medium for the exchange of information. Most Americans have access to the internet, but many still lack access to a high–speed broadband connection at home. In 2017, 21.3 million Americans lacked broadband connectivity (FCC 2019). More recent surveys find that over 25% of Americans do not have home broadband internet (Pew Charitable Trusts 2021). Pew also finds that the lack of access is twice as high for those without any college education and is even higher in rural and tribal areas. Non–broadband users say they do not subscribe because service is too expensive (21%), because they use a smartphone to access the internet (23%), or because service is unavailable or speeds are too slow (7%) (Anderson 2019). Meanwhile, the benefits of broadband diffusion are significant. For example, it increases income (Whitacre, Gallardo, and Strover 2014), improves health outcomes (Van Parys and Brown 2024), and increases employment (Atasoy 2013), partly through improved labor market matching (Bhuller, Kostol, and Vigtel 2020).

Despite the benefits of broadband access, state–level regulations often block public communications initiatives and public–private broadband partnerships. As of 2020, 17 states had regulations that explicitly banned the establishment of municipal broadband (Chamberlain 2021). Most of those 17 states also previously had roadblocks making it onerous for telephone and electric cooperatives to provide broadband internet (Whitacre and Gallardo 2020; Morton 2021; Pew Charitable Trusts 2021).[1] Yet, cooperative and municipal broadband providers are prevalent in states without restrictions. Maps from the Institute for Local Self Reliance (ILSR) show that nearly 60% of U.S. counties have at least one community (municipal or cooperative) broadband provider (Parker 2024).

Cooperative and municipal broadband networks operate differently from private internet service providers (ISPs). For example, as nonprofit membership corporations, rural electric and telephone cooperatives are exempted from paying taxes under section 501(c)(12) of the Internal Revenue Code (IRC). Similarly, municipal providers are often oriented towards the local community and they lack a profit motive, which may also incentivize them to maximize broadband access and pass on significant cost savings to subscribers. Case studies find that community–owned networks offer prices that are 3–50% lower than private ISPs. Private

[1] 11 of the 17 restriction states either had significant roadblocks for co-ops to provide broadband, or they had easement ambiguities, which made it risky for co-ops to provide broadband. However, many of these ambiguities were eliminated by 2019. We explore this in the Robustness Section.

ISPs often offer low promotional rates that later rise sharply (Talbot, Hessekiel, and Kehl 2018). Therefore, restricting the entry of public or non–profit firms may have consequences for access and prices in broadband markets. In a country–wide analysis, Whitacre and Gallardo (2020) find that states' restrictions on municipal broadband provision had reduced the number of people with broadband access by approximately 4.5% between 2012 and 2018.

Two recent events brought state–level restrictions on municipal broadband to the forefront of public discourse. First, beginning in March 2020, the COVID–19 pandemic forced millions of people to stay at home for an extended period of time. The "COVID–19 shut–downs" elevated the importance of having a high–quality broadband connection for employment, education, entertainment, and social connection. The increase in remote work persisted well after the end of COVID shut-downs in July 2021 (Kaiser Family Foundation 2021). Therefore, state–level restrictions on municipal broadband provision may have been especially limiting after the COVID–19 pandemic when many Americans needed to have high–speed internet at home to find and perform work. Second, in November 2021, a bipartisan infrastructure bill was signed into law. The bill included the largest-ever federal investment in broadband. The bill included $42.45 billion for the Broadband Equity, Access and Deployment (BEAD) program, which gave money to states to fund broadband deployments in unserved and underserved areas.[2] So while some states blocked local governments from building their own broadband networks, the federal government encouraged states to suspend their restrictions to spend billions in new grant dollars (NTIA 2022). Thus, there has been an increasing tension between state–level policies that restrict municipal broadband provision and federal policies that seek to expand access.

Therefore, it is important to examine whether policies that restrict municipalities from providing broadband affect peoples' labor market outcomes. In this paper, we estimate how municipal broadband restrictions affected the labor force participation of married mothers in the aftermath of the COVID–19 pandemic. We link data on state–level broadband restrictions from BroadbandNow to county–level data on broadband subscribership from the Federal Communications Commission (FCC), and to individual–level Current Population Survey (CPS) data on labor market outcomes measured between Q1:2018 and Q4:2022. We focus on municipal restrictions in place prior to the COVID–19 pandemic. Since all municipal broadband restrictions in our data had been implemented prior to 2018, the empirical strategy exploits the exogeneity

[2] As of mid-2025, BEAD funds have still not been spent at the local level. Therefore, the BEAD program does not confound our estimates of the impacts of broadband restrictions around the COVID-19 pandemic.

of the COVID–19 shock. The COVID–19 shock normalized peoples' use of broadband to work from home, and arguably increased demand for in-home broadband.

We first use FCC data on the share of broadband subscriptions per county (measured every 6 months) to show that restriction states had lower broadband adoption and ISP competition leading up to the pandemic. We also estimate event studies and difference–in–differences models that show broadband subscriptions grew between 4.4% (at 100Mbps+) and 8.4% (at 25Mbps+) slower in states with municipal restrictions after the pandemic compared to states without restrictions. We then show that the relative decreases in broadband subscribership observed after the pandemic in restriction states were likely driven by mechanisms such as reduced ISP competition, fewer plans, and higher prices.

In our labor supply results, we focus on married women with children because (i) their labor force participation is responsive to new technology (Greenwood, Seshadri, and Yorukoglu 2005; Nieto 2023), and (ii) they have a preference for working-from-home, on average (Angrisani, Burke, and Perez-Arce 2023). Prior work has found that even during periods without widespread telework (i.e., before COVID), broadband access had direct and substantial effects on maternal labor supply (Dettling 2017; Viollaz and Winkler 2020).[3] We also examine the effects of municipal restrictions on women without children and married men with children.

Trends in maternal labor force recovery after the COVID pandemic also motivate our focus on this group. Initially, the labor supply of women with children was disproportionately negatively impacted during the early stages of the pandemic (Albanesi and Kim 2021; Couch, Fairlie, and Xu 2022). However, these effects diminished over time since female employment prior to the pandemic was more concentrated in jobs that could be done remotely (Cowan 2023). Recent work by the Hamilton Project at the Brookings Institution shows that prime-age women with children played an outsized role in the overall post-pandemic rebound in labor force participation. During the post-COVID recovery, prime-age female labor force participation reached a record high, driven by a rebound in labor force participation for married women with children under five (Bauer and Yu Wang 2023). As of mid-2023, full days worked at home continued to account for 28% of paid workdays among Americans 20-64 years old (Barrero, Bloom, and Davis 2023), with married mothers still more likely to report that they teleworked once a week compared with other workers (Bauer and Yu Wang 2023).

[3] We discuss this research and the role of telework in the section titled, "Telework and Maternal Labor Force Participation."

We use individual-level CPS data to estimate event studies and difference-in-differences models to show how labor market outcomes differed for married mothers living in states with municipal restrictions compared to their counterparts in states without restrictions before and after the COVID–19 pandemic. Our results show that the trends in maternal labor force participation diverged sharply across states with and without municipal broadband restrictions by Q3-2021. In states with restrictions, mothers reduced their labor force participation (LFP) and employment by 2.68 percentage points and 3.1 percentage points respectively (or 3.6% and 4.8%), compared with mothers in states without broadband restrictions.[4] For comparison, Jones and Wilcher (2024) find that paid family leave reduces maternal labor force detachment by more than 5 percentage points in the year of birth. Hence our estimated effects of broadband restrictions on maternal labor force detachment post-pandemic are about half as large as the estimated effects from paid family leave.[5] In contrast, broadband restrictions had no effects on the LFP or employment of women without children or married men with children – groups with lower labor supply elasticity (Hansen, Sabia, and Schaller 2024; Heggeness, Suri et al. 2021). Among mothers who continued to work, those living in restriction states experienced a 49% decrease in the share of time spent working from home and a 27% increase in commute time. We conclude that state-level restrictions on municipal broadband had important economic implications for families in states with restrictions after the COVID-19 pandemic.

We offer two contributions to the literature. First, we show that states with restrictions on municipal broadband experienced slower growth in broadband adoption after the COVID-19 pandemic compared to states without such restrictions. We also find that growth in broadband adoption was especially slow in counties in restriction states with non-competitive ISP markets and/or less affordable broadband plans leading up to the pandemic. Our results are consistent with Carvalho, Hagerman, and Whitacre (2022), who show that low broadband adoption rates prevented some counties from recovering their COVID-19-related employment losses. We build on their work by showing *why* adoption rates were lower in certain counties. Second, we are the first to show that states without municipal restrictions on broadband experienced larger increases in maternal labor force attachment. Our results suggest that these "non-restriction states" had more affordable high-speed plans, which helped mothers balance work and caregiving in the post-COVID era. Given the recent discourse around return-to-office policies and the labor-force-detachment of working mothers (Lutz

[4]Throughout the paper, we refer to married women ages 25-54 who were living with their children ages 0-17 years old as "married mothers" or simply "mothers."

[5]We use the estimate for maternal labor force participation of -2.68 percentage points taken from Table 8, panel A, column 1. Our estimated effect of -2.68 pp is approximately 54% as large as the -5 pp paid family leave effect ($\frac{2.68}{5} = 0.536$).

2025), our paper shows how policies that reduce workplace flexibility can have consequences for working mothers.

State–Level Restrictions on Municipal Broadband

We rely on the 2020 annual report on the state of municipal broadband from BroadbandNow (Chamberlain 2021) – which details restrictions in 2019 – to assess the degree to which state policies restrict the provision of municipal broadband.[6] BroadbandNow describes state-level regulatory barriers that categorically ban the establishment and/or operation of broadband infrastructure by local government or non-profits. Outright bans mean that counties, towns, cities, municipal governments, and sometimes electric and telephone cooperatives cannot provide broadband to the general public. The 17 states that have explicit bans on municipal broadband during our sample period (2018-2022) are Alabama, Florida, Louisiana, Montana, Michigan, Missouri, Minnesota, Nebraska, Nevada, North Carolina, Pennsylvania, South Carolina, Tennessee, Texas, Utah, Virginia, and Wisconsin (Chamberlain 2021). For the rest of the paper, we refer to such policies as "municipal broadband restrictions." In other research, such restrictions are sometimes called "preemption barriers," as in state law "pre-emptively blocks" a local government from providing broadband. The reasoning behind such restrictions is explained in the text of the proposed 2021 CONNECT Act in the House Commerce Committee of the U.S. Congress (Republicans, Energy and Commerce Committee 2021). The bill would create a nationwide ban on establishing and operating municipal networks. The bill's sponsors say the ban would promote competition by limiting government-run broadband networks throughout the country and by encouraging private investment. The spirit of bills that restrict municipal broadband suggest that sponsors and supporters believe local governments are inefficient at providing broadband, and that public funds should not be used in an effort to subsidize such an endeavor.

Three other states (Iowa, Oregon, and Wyoming) had implemented other roadblocks, such as requiring the municipal broadband service to be self-sustaining, price matching with incumbent internet service providers, and/or phantom cost requirements (Chamberlain 2021). We code these states as control states in our analysis.[7] To better understand this minority of 3 states with other roadblocks but not explicit bans, we

[6] For more current information on restrictions, we direct readers to the most recent BroadbandNow report on municipal broadband (Cooper 2024).

[7] Results are qualitatively similar if we code them as treatment states or drop them.

elaborate on the state of Iowa below.

In Iowa, there are no statutes that specifically ban municipalities from offering broadband services to residents, but state law requires that all new public utilities must be approved by voter referendum of 51%. If the referendum fails, the municipality cannot hold another referendum vote on the same proposal or a similar proposal for at least four years. If a municipality wishes to use bonds to finance a public broadband network, the measure needs to obtain 60% approval in a referendum. Municipalities are also prevented from using general fund revenues to support a broadband network, and they must complete a detailed annual audit, subject to open meeting requirements, meaning that aspects of the audit which might contain commercially sensitive information must be made public. Two Iowa bills that passed in 2004 erected most of the barriers discussed above (Chamberlain 2021). Despite this, some cities in Iowa have been able to establish municipal broadband. An example is the city of Waterloo, Iowa (City of Waterloo 2023). Waterloo voters approved a $20 million General Obligation Bond in a special election in September 2022 to fund the network. Construction of the network started in 2023 and the city said it would take 3 years to complete.

The population covered by community (municipal or cooperative) broadband is not insignificant – approximately 20% of the population in non-restriction states lives in a county with municipal broadband in 2023. Previous studies have noted that states with municipal restrictions also tend to impose restrictions on cooperative broadband (Whitacre and Gallardo 2020; Pew 2019; Trostle et al. 2019; Morton 2021). Among these, we were able to identify at least 8 states that also restricted cooperatives from participating in the broadband market as of 2018 - these include Alabama, Louisiana, Michigan, Minnesota, North Carolina, Pennsylvania, South Carolina and Virginia (Pew Charitable Trusts 2021). Updated maps from ILSR suggest that nearly 60% of U.S. counties have access to broadband via either municipal broadband or telephone and electric cooperatives (Parker 2024).

Cooperatives have also played a vital role in state broadband expansion alongside municipal governments once municipal restrictions are removed – even if there were never restrictions on cooperatives. As an example, we describe the evolution of broadband legislation and the interplay between co-ops and municipalities in providing broadband in the state of Arkansas.

The (Arkansas) Telecommunications Regulatory Reform Act of 1997 prohibited any government entity in the state from offering basic exchange services (Gonzalez 2012). In legal circles, "basic exchange" means "telephone services." Therefore, no town in Arkansas could create its own telephone company that offered the traditional concept of telephone service, as defined in statute. In 2011, Arkansas expanded this prohibition to include data, broadband, video, and wireless. Arkansas towns were prohibited from offering broadband services to the public (unless they owned community electric utilities or cable television systems) (Gonzalez 2012). As of 2018, this prohibition remained in place in Arkansas. FCC Form 477 for that year shows that Arkansas ranked second from last among all U.S. states for broadband access (FCC 2021; Arkansas State Broadband Manager 2021).[8]

In 2018, the Arkansas Republican Woman's Legislative Caucus sought to overturn state barriers to municipal broadband by introducing Senate Bill SB150 (Gonzalez 2019). However, when ultimately passed in February 2019, SB150 said government entities are only able to deploy broadband in unserved areas and only if they apply for and received grant funding. Community-centric providers often have needs that are too large for foundation grants (Vo and Miller 2025). Initial deployment costs can be high, especially in unserved areas. Often, broadband expansion is funded by tax revenue, bonds, or in some cases, the members of a cooperative pitch in to fund the expansion themselves. Since SB150 did not allow grant dollars to be supplemented with revenue from areas considered served, it effectively left the ban in place.

In February 2021, Arkansas passed SB74. SB74 allows government entities "to acquire, construct, furnish, or equip facilities for the provision of voice services, data services, broadband services, video services, or wireless telecommunications services" so long as they "partner, contract, or otherwise affiliate with an entity that is experienced in the operation of the facilities," as well as conduct due diligence, and provide ten-days' notice and hold a public hearing (Marcattilio 2021).

Notably, laws prior to 2021 did *not* prevent cooperatives from participating in Arkansas prior to that. However, the expansion of broadband by electric co-ops was likely accelerated by the 2021 repeal of municipal restrictions (Bode 2024). Since electric utilities in Arkansas were already authorized to deploy broadband (Arkansas General Assembly 2007), many local government entities chose to partner with them as the 2021 reform allowed government entities in Arkansas to provide broadband if they "partner with experienced operators." Some examples include the partnership between the city of Cabot and First Electric Cooperative (Zhone 2024). Similarly, the Ozarks Electric Cooperative has expanded residential broadband access after initially partnering with local school districts (Cash 2024a).

[8] The measure we used to determine state-level broadband access was the percentage of population with access to 25/3 Mbps (download/upload) speeds.

Many cities in Arkansas also operated municipally owned utilities providing electric, water, and wastewater services for many decades before the reform. The 2021 legislation enabled these municipally owned utilities to begin providing broadband; many leveraged existing community ties and infrastructure to expand broadband *without* coop support. For example, the cities of Conway and Clarksville have recently expanded broadband through municipally owned utilities (Marcattilio 2021) and did so independently of cooperatives.

Following SB74, the state of Arkansas saw rapid growth in community broadband. As of November 2024, 17 cooperative broadband providers in Arkansas said they had constructed 40,000 miles of fiber and connected 1.4 million Arkansans with broadband capability, representing over one-third of the state's population (Cash 2024b). 80% of this increase was self-funded (by members of the cooperatives themselves). Only 20% of the expansion was grant-funded.

Using the example of Arkansas, we see how barriers to municipal broadband developed (1997-2011) and were eventually removed (2018-2021). This trajectory is typical in many states, although the removal of barriers remains less common. As detailed above, 17 states still had barriers as of 2022 (end of our sample period), while only 3 states rolled them back between 2018-2022. These states include Arkansas (2021), California (2018), and Washington (2021).[9]

Telework and Maternal Labor Force Participation

Research on the COVID-19 pandemic has shown that the pandemic had a disproportionate impact on the labor force participation of married women (Alon et al. 2020; Faberman, Mueller, and Şahin 2022; Schroeter, Lalive, and Karunanethy 2023; Cheng et al. 2020). For example, Garcia and Cowan (2022) find that school closures during the 2020–21 academic year led to a decrease in the probability of women being at work. Importantly, they find workers in jobs with high telework potential did not experience reductions in work hours when schools were closed. Hansen, Sabia, and Schaller (2024) study the effect of K–12 reopenings on the employment outcomes for married women with school–age children. They find K–12 reopenings were associated with a 3.3 percentage point increase in employment and a 0.76 increase in (con-

[9] Arkansas removed barriers in February 2021. A bill removing all restrictions on public broadband in Washington was signed into law by Governor Jay Inslee in May 2021. California removed its roadblocks (before COVID) in 2018. Although our results are not sensitive to including or excluding California, we drop the state as it removed roadblocks to municipal broadband in the year our sample period begins (2018).

ditional) weekly work hours among married women with school–aged children. Hansen, Sabia, and Schaller (2024) estimate reopenings led to a 3.9 percentage point reduction in remote work among married women with children, with larger reductions among college–educated mothers. This suggests that broadband access and remote work during the COVID–19 pandemic helped women balance the competing demands of family and career.

Research prior to the COVID–19 pandemic has also shown that access to broadband increases the labor force participation of married women. Dettling (2017) finds that broadband use increases labor force participation for married women in the U.S. by 4.1 percentage points but has no impacts on single women or men. She finds that the increase in participation is explained by women using the internet for telework and time–saving tasks related to home production. Outside the U.S. context, Viollaz and Winkler (2020) also find that a 1 percentage point increase in internet adoption in Jordan between 2010–2016 increased female labor force participation by about 0.7 percentage points.

We contribute to the existing literature on how broadband affects maternal labor supply by focusing on the importance of state–level municipal broadband restrictions during a period when work–from–home increased in a plausibly exogenous manner for a broad swath of workers. We focus on municipal restrictions because they create barriers to broadband access despite other state–level policies aimed at increasing broadband access (Whitacre and Gallardo 2020). Municipal restrictions have also been shown to affect broadband access more than other state–level policies such as funding subsidies to private ISPs or opening a broadband office (Whitacre and Gallardo 2020).

Data

We utilize data from six sources. First, we determine whether states had broadband restrictions in place as of January 2018 (and through 2022) using reports from BroadbandNow. Second, we analyze broadband subscribership, including the role of broadband prices and ISP competition using data from the FCC Form 477. Third, to estimate labor supply effects, we employ individual labor force outcomes from the monthly Current Population Survey (CPS) between January 2018 and December 2022. Fourth, we complement the labor-force-related outcomes in the CPS with data on time spent working from home from the American Time Use Survey (ATUS) and the CPS Internet Use Supplement. Fifth, to construct control variables for our analysis, we collect data from the Centers for Disease Control (CDC) on COVID-19 cases and COVID-19 deaths, and finally, we employ data from the school closures database compiled by Parolin and Lee (2022).

Restrictions on Municipal Broadband

Data on state-level broadband policies come from BroadbandNow (Chamberlain 2021; Cooper 2024). We focus on whether states had outright bans or severe roadblocks on the provision of municipal broadband at the start of our sample period. These regulations include outright bans on the establishment or operation of municipal broadband infrastructure or bureaucratic obstacles making it practically infeasible to create a citywide network. Between 2018 and 2022, 17 states met this criteria. Figure 1 shows a map of the states with and without municipal restrictions in place as of 2018. The states with broadband restrictions are generally spread out geographically. With the exception of New England, we find that all regions of the country feature states with and without restrictions on municipal broadband.

Figure 2 shows the years in which states first implemented restrictions on municipal broadband. Most restrictions went into place between 1999 and 2005, although two states passed restrictions between 2006-2011. We find that states rarely revoke their restrictions on municipal broadband once they are put into place. Indeed, only Arkansas, California and Washington reversed their policy on municipal restrictions between January 2018 and December 2022. For this reason, we drop Arkansas, California and Washington from our analysis. Finally, we focus our analysis on the continental U.S. and drop Alaska and Hawaii.

Broadband Adoption/Subscribership and the Number of Broadband Providers

We use the county-level data on Internet Access Services from FCC Form 477 to measure broadband adoption (i.e., subscribership) before and after the COVID-19 pandemic. The FCC collects broadband subscribership data through its bi-annual Form 477 data collection (Federal Communications Commission 2025a). There are 10 data points, one in June and one in December for each of the 5 years in our sample period (2018-2022). Our primary variables of interest are the number of connections at or exceeding 25Mbps download per 1,000 households in a county, and at or exceeding 100Mbps download per 1,000 households in a county. We focus on the 25Mbps and 100Mbps threshold because 25+/3+ [10] was the FCC standard since

[10] 25+/3+ refers to download speeds of 25Mbps or higher and upload speeds of 3Mbps or higher

2015, and 100+/20+ Mbps is the current standard (in effect since 2024). Further, upload speeds of 4Mbps are often required for common modalities of telework such as video conferencing especially when multiple household members are streaming video or on calls at the same time (Microsoft 2024; University of Chicago, Data Science Institute 2022).

FCC measures of potential broadband access or availability lack consistency during our sample period and overstate access. This includes the older 477 data, which the FCC chair notes overstates access (Rosenworcel, Jessica 2023) and the new broadband maps, which received millions of challenges (FCC 2024; ILSR 2023). In contrast, county-level subscriptions provide a more accurate measure of actual broadband use. For example, the FCC says in their Section 706 Report for 2024 (FCC 2024) that "if the cost of broadband service is higher than millions of people can afford, service cannot be said to be available." Further, they state "low adoption rates in areas where broadband is technically deployed and available, for example, may evidence that other factors are in play that make it effectively unavailable for some portion of the population." Another advantage of the subscriptions/adoption data is that it is consistently coded during our sample period (June 2018-December 2022). Previous work also finds that the effect of broadband on economic growth in the United States is driven by adoption rather than access - this helps validate the ordinal measures we employ (Whitacre, Gallardo, and Strover 2014).

Despite this, the FCC subscriptions data have some drawbacks. Granular data on the exact number of subscriptions in each county are considered confidential data at the FCC, and are not available to the public.[11] Instead of the exact number of subscriptions, the FCC provides external researchers with the number of connections per 1,000 households in each county in terms of ordinal tier values where 0 represents 0 connections, 1 represents between 1 and 200 connections, 2 represents between 200 and 400 connections, 3 represents between 400 and 600 connections, 4 represents between 600 and 800 connections, and 5 represents above 800 connections (Federal Communications Commission 2025a). Another drawback is that the FCC does not consider upload speeds when assigning ordinal tier values. For example, the tier values for 100Mbps+ are determined solely by the download speed of the connections.

Therefore, we supplement the subscribership data with measures of the percentage of the county-level population with access to different numbers of residential ISPs, using the biannual (June, December) FCC Area Tables between June 2018 and the last period available, June 2021. Unlike adoption, this is a different

[11] The granular data are available internally to FCC researchers – see LoPiccalo (2022) for example.

measure that captures the percentage of the county-level population with *access* to different numbers of residential ISPs through the FCC Area Tables. For the percentage of county-level population with access to different numbers of residential ISPs, we use "acfo" for 10+/1+ speeds and only "cfo" for 25+/3+ speeds or higher.[12] The use of acfo follows the FCC's own definition for fixed wired or wireline connected broadband; hence we exclude satellite and fixed wireless. We additionally exclude ADSL from 25+/3+ speeds or higher because the theoretical maximum download speed for the ADSL2+ standard is 24Mbps, and in practice, ADSL speeds are lower ITU (2003). As an illustrative example for a given county and biannual period, FCC Area Tables report that 10% of a county's population had access to 0 residential broadband providers, 40% had access to 1 provider, 20% had access to 2 providers and the remaining 30% of the county population had access to 3 or more providers.[13] Note that these data are available at different speeds. In contrast to the subscribership/adoption data, competition measures incorporate upload speeds as well; for example, the percentage of the county population covered at 25+/3+ speeds. We use this data to show (1) how states with municipal restrictions differ from states without restrictions in terms of the percent of the population with access to different numbers of ISPs, and (2) how ISP competition mediates the effects of municipal restrictions on broadband subscribership.

Tables 1 and 2 include county-level summary statistics for the pre-COVID period (2018–19) by whether the state had broadband restrictions (column 2) or not (column 1). Before the pandemic, counties in restriction states had lower levels of adoption (as measured by 60% or higher subscribership) across all 3 speed tiers. These differences are significant beyond the 5% level for 100Mbps+ (row 1) and close to the 10% level for 10Mbps+ (row 3, $p = 0.11$). Counties in restriction states also had a higher percentage of the population with 0 providers at every speed tier (100/10 Mbps+, 25/3 Mbps+ and 10/1 Mbps+) during the pre-period (June 2018 to December 2019), as seen in rows 4 through 6. These differences are statistically significant beyond the 1% level. Finally, counties in restriction states have a lower percentage of the population with access to 2 or more providers where the differences are significant beyond the 5% level for 25/3 Mbps+ (second from last row) and 10/1 Mbps+ (last row). These results indicate that pre-COVID levels of broadband penetration, as well as ISP competition, were lower in restriction states.

[12] a=ADSL, c=Cable, f=fiber, o=other

[13] These data are available under Area Summary at https://broadband477map.fcc.gov/#/data-download.

Labor Market Outcomes

Data on labor market outcomes and respondent characteristics come from the monthly Current Population Survey (CPS) (January 2018– December 2022). The CPS provides information on labor force participation, (conditional) hours worked, and employment. For employment, we consider whether a person is employed (Employed, Any) and whether an employed person is at work the previous week (Employed, At Work).[14] Whether an individual is at work is defined as doing any work for pay or profit or working at least fifteen hours without pay in a family business or farm in the previous week. This excludes individuals who are employed but currently absent from work due to illness, childcare problems, or family/personal obligations. For hours worked, we examine effects on the number of hours the individual worked last week. These work hours are conditional on being employed.

To examine the channels that affect labor force attachment, we also include measures of non- or under-employment related to childcare. We use CPS variables to identify respondents who report that they were "taking care of house and family" while not in the labor force (NILF) or working part time last week due to "child care problems." These variables have been used in previous COVID-19-related research to show that women with school-aged children took on a disproportionate share of childcare responsibilities while schools were closed (i.e., the "COVID motherhood penalty") (Garcia and Cowan 2022; Hansen, Sabia, and Schaller 2024).

The CPS records the demographic characteristics of respondents, such as age, gender, education, race, marital status, number of children, age of children, metro status, state-of-residence, and survey month. We characterize the respondent's racial/ethnic group as either White Non-Hispanic, Black Non-Hispanic, other Non-Hispanic, or Hispanic. We characterize the respondent's educational attainment as either less than a high school degree, high school degree, some college, bachelor's degree, or advanced degree.

Our empirical approach compares labor market outcomes for people living in states with municipal broadband restrictions to people living in states without restrictions before and after the COVID-19 pandemic. Table 3 reports summary statistics for our CPS sample in restriction and non-restriction states during the pre-pandemic period (2018-19). Our sample includes prime-age adults (25-54 years old) who were not

[14] During the early stages of the pandemic, some workers who were not at work during the entire reference week were misclassified as employed but absent from work by the BLS. Analyses of the underlying data suggest that this group included some workers affected by the pandemic who should have been classified as unemployed on temporary layoff (U.S. Bureau of Labor Statistics 2022). For this reason, we use both "employed" and "employed at work" as our outcomes.

employed in public administration or education.[15] Respondents in both types of states had similar levels of labor force attachment in the pre-pandemic period. Prime-age respondents in states without restrictions were more likely to have bachelor's or advanced degrees. They were also less likely to be Black or Hispanic, more likely to be White, and more likely to live in a central city. Therefore, we control for education and individual-level demographic characteristics in our difference-in-differences design.

The final row in Table 3 reports the mean (and standard deviation) for the proportion of teleworkable jobs in restriction and non-restriction states. We classify jobs as teleworkable based on occupation using the methodology developed by Dingel and Neiman (2020). During the pre-pandemic period (when telework was less common), we find no differences between restriction and non-restriction states in the proportion of prime-age individuals in teleworkable jobs.

Much of our analysis focuses on married women with children. This group of workers might have benefited the most from using broadband to work from home during the pandemic. Therefore, Appendix Table A1 shows summary statistics for married women with children from 2018-19 in states with and without broadband restrictions. As with our full sample, we focus on married women with children aged 25-54 years old (prime-age) from the CPS. Children are coded as present if a respondent has at least one child in the household between the ages of 0 and 17. In states without municipal restrictions, married women with children have slightly higher labor force attachment prior to the pandemic. They are also more likely to go to college, hold an advanced degree, less likely to be Black or Hispanic, and more likely to be White. As in the full sample of prime-age workers, married women with children are no more likely to be in teleworkable occupations across restriction or non-restriction states.

Time-Use and Remote Work

Data on working from home comes from the American Time Use Survey (ATUS). The ATUS is a 24-hour time diary, where respondents report the activities they were doing between 4:00 am of the first day and 4:00 am of the following day. Respondents are randomly sampled from individuals who completed the Current Population Survey (CPS). They respond to the ATUS between two and five months after their

[15]Consistent with previous work, we exclude workers in public administration and education from our analysis (Althoff et al. 2022). Employment in government and education was hit hard due to the early and direct effects of the pandemic (Dylan Maloney 2019) (e.g., from school closures). We exclude workers with ind1990 codes 900,932 (public administration) and 842,850,851,852,860,862 (education).

final CPS interview. We restrict our sample to prime-age workers (25-54 years old) and exclude time-use on weekends and holidays.

We follow Cowan, Jones, and Swigert (2024) and aggregate ATUS activity codes into 10 main categories. Our primary outcome is work-from-home (WFH), defined as work activity performed at one's residence. Additionally, we examine total time spent working, share of work time from home, and time spent commuting. Similar to the CPS, for the ATUS, we follow Cowan (2024) and exclude all of 2020 and January-July 2021 to avoid the confounding effects of the COVID-19 pandemic and associated policies on time use.

Control Variables Related to the COVID-19 Pandemic

The COVID-19 pandemic affected labor supply and labor demand in non-random ways across states (Gupta, Simon, and Wing 2020). We include several control variables in our analysis to account for these differences. All of our models control for school closures, COVID-19 cases per capita, and COVID-19 deaths per-capita. These measures vary by county-quarter. We also include COVID-19 controls to assuage concerns that our results might be driven by state differences in stay-at-home orders and restaurant closures. Stay-at-home orders were highly correlated with COVID cases, deaths, and school closures – and we argue our measures better control for such orders given issues such as non-compliance and differences in the duration of such orders.

We employ the strategy used by Garcia and Cowan (2022) for measuring school closures. They use the school closures database from Parolin and Lee (2022). These data are aggregated and anonymized mobile phone records from Safegraph, which track year-over-year changes in the number of visitors to each school or childcare facility relative to the pre-pandemic baseline in 2019. We consider institutions closed if there is at least a 50 percent year-over-year decline in the number of in-person visits. We use the share of closed institutions in each county in each quarter between 2020 and 2022 as the measure of the extent of school closures in the analysis; thus, the variable is a continuous measure between zero and one. The variable equals zero for all counties and quarters before January 2020 and after May 2022 (the last month with available data).

The cumulative COVID-19 case rate and the cumulative COVID-19 death rate come from the CDC (CDC 2022), and the rates are measured in each county-quarter between 2020 and 2022. Prior to Q1:2020, these measures equal 0 in all counties and quarters. We use these rates to control for changes in the severity of the pandemic, and to proxy for differences in labor demand and supply, across counties over time. For instance, research shows that COVID-19 cases per capita affected the number of unemployment claims filed across states in the early pandemic period (Lozano Rojas et al. 2020). In addition, Goda and Soltas (2023) show that workers with severe COVID-19 infections reduced their labor supply one year later. Coombs et al. (2022) show that the early withdrawal of enhanced unemployment benefits in 22 states temporarily increased weekly employment among Unemployment Insurance (UI) beneficiaries. However, enhanced unemployment benefits ended in all states by September 2021. Therefore, the post-period in much of our analysis begins in August 2021, after enhanced unemployment benefits expired.

The CPS obscures county identifiers (57% of our observations) so as to not identify persons in smaller counties. Therefore, including county fixed effects will drop observations for respondents living in small counties. To overcome this challenge, similar to Hansen, Sabia, and Schaller (2024) we match geographic areas as follows – if a county identifier is available we assign it as an area identifier for the CPS (or ATUS) individual. If a county is unavailable but the metro area FIPS is identified (typically covering multiple counties) we use that instead as the area identifier.[16] Finally, if neither county nor metro area is identified, we combine individuals into "unincorporated geographic areas" within each state. We then include these area fixed effects in order to control for area-level initial broadband availability and any fixed characteristics in areas over time.

Our COVID-19 controls – cumulative case and death rates are similarly aggregated. If the CPS identifies an individual's county, we match it with cumulative COVID case and death rates for that county and quarter. For state-wide averages, we calculate within-state averages across counties other than the larger counties for which the CPS reports the county identifier, rather than overall statewide averages of these data, since this better represents cases and deaths for counties for which the county identifiers are not available.

Methods

[16] Definitions of metropolitan areas along with their codes change over time. However, consistent coding is used between May 2014 and April 2025.

Broadband Adoption

For the effects of municipal broadband restrictions on broadband adoption, we estimate results using a biannual county-panel constructed from FCC Form 477 County Data on Internet Access Services between June 2018 and December 2022. Our model compares broadband adoption for counties in states with restrictions on municipal broadband to adoption in counties in states without restrictions before and after the COVID-19 pandemic. We estimate our main effects on broadband subscriptions using a standard difference-in-differences estimator:

$$y_{ist} \quad = \quad \beta(Muni_s \times Post_t) + \Omega X_{it} + \delta BB_i + \lambda_t + \epsilon_{ist} \tag{1}$$

First, we estimate Equation 1 as an ordered logistic model where y_{ist} is an ordinal (0, 1 or 2) biannual value that we consolidate from the ordinal FCC measure for the number of connections from the FCC County Broadband Access (corresponding to 0 to 40%, 40 to 60% and over 60% subscribership, respectively) at various speeds in a county $i = 1, \ldots, N$ in state s at time t, where t indicates a biannual time period between June 2018 and December 2022 (10 periods total). Second, we estimate linear probability models that use binary outcomes for broadband subscribership of 20%+ at 100Mbps+ speeds, 60%+ subscribership at 25Mbps+, and 60%+ subscribership at 10Mbps+ speeds.[17] $Muni_s$ is a state-level binary variable that equals one for counties in states that had restrictions on municipal broadband as of Q1:2018 – since the panel is biannual (June/December), this corresponds with June 2018. $Muni_s$ is interacted with an indicator for the post-pandemic period ($Post_t$) that equals 1 for each biannual period after June 2020. β represents the effect of restrictions on broadband subscribership following the onset of the pandemic. Year$\times$6-month fixed effects (λ_t) help account for unobservables that vary every half-year like economic conditions and seasonality. To avoid the confounding effects of cross-county differences in initial broadband availability, we include county-fixed effects in our linear probability models. However, using county-fixed effects is not possible in our ordered logistic models, so we include initial broadband adoption in the county for 2017, as a control instead.[18] Specifically, we use the December 2017 measure for broadband adoption in that

[17] From Table1, 100Mbps+ subscribership is comparatively low – for the median county, adoption is 0 to 20% during the pre-period. Hence we use the 20% threshold for 100Mbps+ and the higher 60% threshold for other speeds.

[18] Because we have a large number of counties, but only use a small number of time periods (FCC data are biannual leading to only

county at the same speed as a control. For example, when estimating effects on 100Mbps+ speeds, we use the December 2017 measure of broadband adoption at 100Mbps+ for that county as a control in lieu of county fixed-effects. Finally, we control for a suite of time-varying county characteristics known to affect broadband adoption including total population, population density, % population with a bachelor's degree or higher, lagged % employed and median income, and % population below the poverty line, all of which are described in Table 2.[19]

We also present the results of event studies estimated as linear probability models over the sample period (2018–2022) to rigorously estimate changes over time. Our event-study model is defined as follows:

$$y_{ist} = \sum_{k=-4, k\neq -1}^{5} \beta_k (Muni_s \times I_k) + \Omega X_{it} + \delta_i + \lambda_t + \epsilon_{ist} \tag{2}$$

In Equation 2, y_{ist} is now a binary outcome that indicates whether broadband adoption (as measured by actual connections per total number of households) is 20% or more, 60% or more, or 60% or more at download speeds of 100Mbps+, 25Mbps+, or 10Mbps+ respectively in a county $i = 1, \ldots, N$ in state s at time t, where t indicates a biannual time period between June 2018 and December 2022 (10 periods total). $Muni_s$ is a state-level binary variable that equals one for counties in states that had restrictions on municipal broadband as of Q1:2018. We interact the binary $Muni_s$ variable with binary variables indicating biannual periods from June 2020 (when the COVID-19 pandemic begins). We omit the interaction between $Muni_s$ and December 2019 as the period just before the base period. We estimate Equation 2 separately for download speeds of 100Mbps+, 25Mbps+, and 10Mbps+. The coefficients of interest, β_k, show the effects of municipal restrictions on broadband adoption for different speeds before and after the COVID-19 pandemic. X_{it} and λ_t are defined in the same way as in Equation 1. To avoid the confounding effects of cross-county differences in initial broadband availability, we include county-fixed effects (δ_i).

10 observations for each county between 2018 and 2022), we do not include county-fixed effects in our ordered logistic models. This helps us avoid the incidental parameters problem that is well known in non-linear panel data models.

[19]We employ the 5-year ACS estimates to adequately cover all counties including those with smaller populations. The ACS also has measures of broadband usage. Employing the 5-year ACS measure on broadband (ACS5), we do not find significant impacts of municipal restrictions on broadband adoption. This could be because effects are dampened by averaging the overlapping broadband measures over 5 years and because ACS survey collection was disrupted by the COVID pandemic. Further, ACS respondents are not queried about broadband speeds. This makes the ACS5 measures prone to errors such as changing broadband standards over a 5-year survey period. In contrast, our FCC and BLS measures on broadband adoption are consistently coded, they cover most counties, and they are higher in frequency (i.e., FCC adoption measures are biannual; ATUS telework measures are monthly), allowing us to identify sharp changes in usage after COVID.

Labor Market Outcomes

Next, we estimate the effects of municipal broadband restrictions on labor force outcomes using a repeated (quarterly) cross-section of data constructed from the CPS between Q1:2018 and Q4:2022. We compare labor force outcomes for people living in states with restrictions on municipal broadband to labor force outcomes for people living in states without restrictions before and after the COVID-19 pandemic. We focus our analysis on married women with children, a group of people whose labor force participation may have been particularly sensitive to broadband availability during the COVID-19 pandemic. For comparison, we also show results for two other groups: (i) women without children and (ii) married men with children.

We estimate an event-study model defined as follows:

$$y_{irst} = \sum_{k=-8,k\neq -1}^{11} \beta_k(Muni_s \times I_k) + \mathbf{X}_{it}\Omega + \mathbf{C_{ct}\Phi} + \delta_s + \lambda_{txr} + \zeta B_{st} + \epsilon_{irst} \tag{3}$$

We estimate Equation 3 as a linear probability model where y_{irst} is an indicator variable for outcomes such as labor force participation and employment for individuals $i = 1, \ldots, N$ in state s from region r at time t, where t indicates a quarter between Q1:2018 and Q4:2022 (20 quarters total). $Muni_s$ is a state-level binary variable that equals one for people living in states that had restrictions on municipal broadband as of Q1:2018. We interact the binary $Muni_s$ variable with binary variables indicating quarters from Q1:2020 (when the COVID-19 pandemic begins). We omit the interaction between $Muni_s$ and Q4:2019 as the period just before the base period. Individual-level control variables (X_{it}) include age, race (Black Non-Hispanic, other Non-Hispanic, and Hispanic), education (less than high school, some college, bachelor's degree, advanced degree), metro area residency, MSA or CBSA size, number of children in the household, and an indicator for at least one child under age 13. The δ_s are state fixed effects, and C_{ct} includes the following county-quarter-level controls: school closures,[20] cumulative COVID-19 cases per 1 million residents, and cumulative COVID-19 deaths per 1 million residents.[21] $\lambda_{t\times r}$ are time-by-region fixed effects and B_{st} is a

[20] The data set we use for school closures does not have estimates for counties not found in the CPS; it only provides estimates for larger counties (Parolin and Lee 2022). Thus, in the case of school closures, we are forced to use statewide averages if the CPS excludes the county. Helpfully, all 3 of our COVID-19-related measures are highly correlated, so including accurate controls for cumulative COVID case and death rates should control for a large proportion of the labor supply effects coming from school closures.

[21] Our COVID controls - cumulative case and death rates are similarly aggregated. If the CPS identifies an individual's county, we

Bartik shift-share variable, both of which we use to control for time-varying demand for labor (explained below). We estimate Equation 3 separately for married women with children, women without children, and married men with children. We cluster the standard errors at the state-level. The coefficients of interest, β_k, show the effects of municipal restrictions on labor force attachment for these three groups before and after the COVID-19 pandemic.

Finally, our main specification for labor market outcomes is Equation 4, where we summarize the results from Equation 3 with a standard difference-in-differences estimator:

$$y_{isrt} = \beta(Muni_s \times Post_t) + \mathbf{X_{it}\Omega} + \mathbf{C_{ct}\Phi} + \delta_s + \lambda_{t\times r} + \zeta B_{st} + \epsilon_{isrt} \quad (4)$$

Equation 4 contains the same control variables as in Equation 3. The difference is that we only include one interaction between the binary variable for states with municipal restrictions on broadband ($Muni_s$) and an indicator for the post-pandemic period ($Post_t$). In Equation 4, $Post_t = 0$ from Q1:2018-Q4:2019 and $Post_t = 1$ from Q3:2021 (beginning August 2021) to Q4:2022. Data from January 2020 to July 2021 are excluded to avoid the confounding effects of the early pandemic. Data from the second quarter of 2020 (April to June 2020) and more generally for the entire year of 2020 are also known to have significant noise. This is because the pandemic impacted CPS data collection and led to a large dip in response rates for all measures during that period.[22] Although we include the full period between 2018 to 2022 in our event studies, while plotting raw trends and using the most restrictive specification in our robustness Table in Figure A13, we believe that significant noise and higher than usual sampling error during the entire year of 2020 provides further justification to exclude this period when estimating our main results. Moreover, our event study estimates showed that broadband restrictions became statistically significantly important for married mothers' labor supply starting in Q3:2021, after extended unemployment benefits expired. Finally, we estimate Equation 4 for our three subgroups: (i) married women with children, (ii) women without children, and (iii) married men with children.

match it with cumulative COVID case and death rates for that county and quarter. For state-wide averages, we calculate within-state averages across counties other than the larger counties for which the CPS reports the county identifier, rather than overall statewide averages of these data, since this better represents cases and deaths for counties for which the county identifiers are not available.

[22]See Table1 in https://www.bls.gov/osmr/research-papers/2020/pdf/st200030.pdf and the CPS response rate chart in https://www.bls.gov/cps/methods/response_rates.htm

Identifying Assumptions and Limitations

Our identifying assumption is that the labor force outcomes for mothers in states with municipal restrictions would have evolved comparably over time to mothers' outcomes in states without restrictions, if not for the restrictions. We cannot test this assumption directly, but we can compare differences in trends in labor force participation across these states prior to COVID-19. To evaluate differences in trends in the pre-COVID-19 period, we plot our dynamic difference-in-differences estimates. Our estimates for $[\beta_{-8}, \ldots, \beta_{-1}]$ show a lack of differential pre-trends in labor force outcomes for married women with children. To verify the lack of differential pre-trends, we test whether these parameters are jointly statistically significant.

One advantage of the COVID-19-era analysis is that nearly all of the states with restrictions on municipal broadband implemented those restrictions prior to the COVID-19 pandemic (we drop the exception states – Arkansas, California and Washington). Moreover, it was less common to work from home prior to the pandemic. The U.S. Government Accountability Office (GAO) used the ATUS to estimate that the percentage of workers who teleworked for any portion of an average workday increased from 24 percent to 38 percent from 2019 to 2021 (U.S. Government Accountability Office 2023). Using the ACS instead, GAO found that the percentage of workers who primarily teleworked in the prior week more than tripled from 5.7 percent in 2019 to 17.9 percent in 2021.

Since it was less common to work from home before the pandemic, it is plausible that municipal broadband restrictions had small and constant labor market effects before 2020. The fact that we fail to find differential pre-trends in the labor force outcomes of mothers across states with and without municipal restrictions prior to the COVID-19 pandemic seems to support this hypothesis. As we show later, labor market impacts of municipal broadband policy increased after the pandemic due to the increased importance of telework.

We control for several potential confounders in our analysis. First, we control for confounders related to the COVID-19 pandemic itself. For example, it is well known that state and local governments took different approaches to in-person K-12 schooling during the pandemic. If states with restrictions on municipal broadband also closed in-person schools for longer periods of time, then our results for mothers could be driven by differences in school closures rather than differences in access to broadband. Therefore, we control for school closures. In addition, we control for COVID-19 cases per capita and COVID-19 death rates per capita at the county-quarter-level in an effort to proxy for the severity of the pandemic. The rationale is that COVID-19 case/death rates could have affected labor supply/demand differently across counties and/or they could have been correlated with state policies to address the pandemic.

Second, we use two strategies to control for differences in labor demand across states during our sample period. First, we include include time-by-region fixed effects to control for time-varying demand for labor by region over time.[23] This approach helps mitigate the concern that differences in women's labor force attachment across states reflects regional differences in underlying economic conditions rather than the effects of state restrictions on municipal broadband. It also means we compare states from the same region and the same quarter – for example, we compare Alabama and Tennessee (restriction states) with Kentucky and Mississippi in the same quarter. Second, we use a Bartik shift-share variable (B_{st}) (Bartik 1991). In the literature, it is common to include shift-share variables as controls when estimating models for labor supply, especially when policy variation is at the state-level (Ne'eman and Maestas 2023; Chodorow-Reich et al. 2012). This helps to mitigate the concern that our estimated labor supply effects are driven by state differences in pre-existing shares of workers in industries with teleworkable jobs. We follow Ne'eman and Maestas (2023) and construct the shift-share variable at the state-level using data from the ACS. We measure each state's initial industry share in 2015[24] and interact that share with the national employment level for that industry over time, t. The variable is rescaled (divided by 1,000) to produce the estimates in the Appendix tables.

Finally, we note a key limitation in our analysis. Although we show that prime-age workers in restriction states are less likely to use the internet at home and mothers are especially less likely to use the internet for calls (using data from the CPS Internet Use Supplement), we do not know the menu of broadband plans or prices that the CPS respondents face. Therefore, the major limitation of our study is that we cannot prove directly that the mothers who opted out of the labor force in restriction states post-COVID-19 did so because they lacked access to a high-speed broadband connection at home. Instead, in the results that follow, we provide suggestive evidence that lower broadband adoption in restriction states– driven by fewer provider choices and more expensive plans – may have affected maternal labor supply decisions.

[23] The CPS identifies the region (census division) where the respondent's housing unit is located. Collections of states are categorized into regions. For example, the East North Central Division includes Illinois, Indiana, Michigan, Ohio and Wisconsin. There are 9 Census divisions in the U.S.

[24] Our results are not sensitive to using a different year for calculating initial industry shares.

Results

We first present difference-in-differences (DD) and event-study results demonstrating how municipal broadband restrictions affected broadband adoption post-COVID, and we highlight the role of (higher) prices and (less) competition. Next we confirm that municipal restrictions reduced internet usage by showing difference-in-differences results using the CPS Internet Use Supplement. Notably, married women with children see a substantial decrease in using the internet for calls – a good predictor of high-speed internet use for a common modality of telework. Then we examine how municipal broadband restrictions affected maternal labor supply, and we compare pre-trends in maternal labor supply across states with and without restrictions. We further estimate the effects of municipal broadband restrictions on telework and caregiving – highlighting their role as potential channels. In robustness tests, we show that our results for both broadband subscriptions and maternal labor supply are robust to several alternative specifications. Finally, we explore heterogeneity in the effects of municipal broadband restrictions on post-COVID maternal labor supply by education, race, and by the age of children in the home.

Broadband Adoption

Table 4 presents the impact of state-level municipal broadband restrictions on broadband subscribership as estimated in Equation 1.[25] Recall that the dependent variable is an ordinal tier (0, 1 or 2) biannual value for FCC County Broadband adoption (2018-2022) reflecting connections per total housing units in a county where 0 is 0 to 40%, 1 is 40 to 60%, and 2 is over 60%. The ordered logistic estimate in column 1 implies that the post-COVID effect of restrictions was a 0.45 increase in the log-odds of being in a lower ordinal tier of broadband subscribership; for example, it increased the probability of going from over 60% adoption to between 40 to 60% adoption at 100Mbps or higher. Figure 3 plots the marginal effects on the ordinal tier value of connections based on the ordered logit estimates from column 1. There is a decrease in the relative odds at the ordinal values 1 and 2, implying a lower probability of 100Mbps+ subscribership that is 40% or higher in the county. In column 2, we employ a linear probability model using a county-level binary outcome that equals 1 if county's 100Mbps subscriptions is 20% or higher. The estimate in column 2 suggests that municipal restrictions decreased the probability of at least 20% 100Mbps subscribership by 2.7 percentage

[25] Appendix Table A3 presents complete estimates from column 1 of the table correct to 4 decimal places.

points.

In columns 3 and 4 of Table 4 we see similar effects for the number of county broadband connections at 25Mbps download speeds. Column 3 has a negatively signed estimate using the ordinal tier value from 0 to 2 for 25Mbps+ penetration. The marginal effects from the ordered logit model in column 4 are plotted in Figure 4. There is a decrease in the probability of the county having 60% or more households subscribed to 25Mbps+ speeds. This is confirmed using the linear probability model estimate in column 4 (negative 2.1 percentage points). Although we do not see a significant estimate employing the ordinal tier value for 10Mbps+ in column 5, we find a significant effect using the LPM at a threshold of 60%+ subscriptions. This estimate in column 6 shows that even for speeds of 10Mbps or more, restrictions decreased the probability of at least 60% of the households in a county being connected to broadband by 2.5 percentage points. Scaling the estimates by the means of the dependent variables shows they are meaningful in magnitude. The LPM estimates in columns 2, 4, and 6 of Table 4 suggest that municipal broadband restrictions decreased the probability of 20% or more broadband adoption by 4.4% for 100Mbps+, of 60% or more broadband adoption by 8.4% for 25Mbps+, and 60% or more broadband adoption by 6.1% for 10Mbps+ in the COVID era.[26]

Figure 5 presents an event study using a window that begins in June 2018, 2 years before the first COVID-19-pandemic observation in June 2020, and extends to December 2022 – about two and half years after. Figure 5 plots OLS coefficients of the dynamic effects for states with municipal broadband restrictions compared to states without restrictions over time on county broadband subscribership of 20% or more at 100Mbps+ speeds. Specifically, the treatment variable for state-level municipal broadband restrictions[27] is interacted with dummy variables for each of the 10 semi-annual time periods between June 2018 and December 2022. Estimated effects of municipal restrictions are insignificant for the 2 periods immediately before the pandemic, covering December 2018 and June 2019. This suggests that broadband subscribership rates prior to the pandemic were not trending differently over time according to state-level broadband policy prior to the pandemic. Instead, the coefficient on municipal restrictions becomes significant only about a year into the pandemic (December 2020) with the effect rising and peaking at the end of the sample period (December 2022) at negative 7.57 percentage points. This provides evidence that the estimates in Table 4 are not biased by pre-pandemic trends in broadband adoption. Instead, it suggests that municipal restrictions may have had a causal impact on broadband subscribership following the onset of COVID-19 and the

[26]From column 2, $\frac{-0.027}{0.61} * 100 = 4.4\%$. From column 4, $\frac{-0.021}{0.25} * 100 = 8.0\%$ From column 6, $\frac{-0.025}{0.41} * 100 = 5.9\%$

[27]Recall, the treatment variable is a binary variable that equals one for counties in states that had municipal restrictions as of Q1:2018

accompanying growth of telework.

Figures 6 and 7 replicate this event study for 25Mbps+ and 10Mbps+ speeds, respectively, with similar results. Although less precise, the estimates follow a similar trend. By December 2022, there is a 4.79 percentage point lower chance that a county in a restriction state will be 60% or more covered by 25Mbps+ speeds vs. a county in a non-restriction state. For 10Mbps+ speeds, by December 2022, the estimate is slightly smaller (about 2.32 percentage points) and not significant – indicating that at lower speeds, access may have begun to converge across states by 2022. This is consistent with the convergence in maternal labor supply between restriction and non-restriction states in 2022 that we see in Figure 10.

To investigate the channels that drive the post-COVID divergence in broadband penetration across restriction and non-restriction states, we plot data on the number of broadband plans in each state and the affordability of the average plan from the FCC Urban Rate Survey. The FCC only provides data at the state-level over the sample period (Federal Communications Commission 2025b). For plan choice (Figure 8), we restrict the data to plans with minimum 25/3 (download/upload) speeds, unlimited usage, and costing $80 or less in total monthly charges for each state-year observation. For affordability (Figure 9), we also restrict to plans with unlimited usage at 25+/3+ speeds and calculate the percent of monthly median household income spent on total monthly charges for the average plan. From the figures, it is clear that broadband consumers face far fewer choices (Figure 8) and lower affordability (Figure 9) in restriction states. This result is consistent with previous work that finds private ISPs often offer low promotional rates that later rise sharply (Talbot, Hessekiel, and Kehl 2018).

To rigorously evaluate the role of consumer choice and price as channels, we interact the number of broadband ISPs available (2 or more) with our independent variable $(Muni_s \times Post_t)$ in our difference-in-differences model. Table 5 utilizes a county and time-varying measure of the percentage of county population (0-100%) covered by 2 or more broadband providers from the FCC Area Tables (2018-21). The FCC does not provide this measure after 2021. The independent variable in row 1 is a dummy that equals 1 for states with restrictions for biannual periods beginning in June 2020. The estimate on the interaction between the independent variable and the percentage of the county covered by 2 or more providers is in row 3. The coefficient on the interaction term in row 3 is positive and significant in columns 2 and 3. This suggests that counties in states with restrictions *and* competitive ISP markets have smaller relative differences in subscribership compared with counties in states without restrictions. Specifically, counties with 0-1 ISPs in restriction states are 9.3 percentage points less likely to have subscribership levels above 60% at 25Mbps (row 1, column 2) compared to counties with 0-1 ISPs in non-restriction states. However, a 10% increase in 2+ provider coverage reduces the difference in the probability of 60%+ subscribership between restriction and non-restriction states to -6.7 percentage points ($-9.3 + 2.6$). In other words, the probability of high levels of subscribership after COVID is lowest in counties in restriction states with non-competitive broadband markets.

For information on county-level broadband prices, we turn to data from BroadbandNow. BroadbandNow provides county-level measures of the lowest monthly prices in a report titled, "United States County Broadband Statistics for 2020." Unlike the FCC, which provides average prices at the state-level (during our sample period), BroadbandNow provides the lowest price at the county-level (Tanberk 2020). They define lowest price as the average of all lowest priced broadband speed plans available in the county at 25/3 speeds. This data is only available for 2020. In Table 6 we interact the independent variable with a dummy that equals one if the county's lowest price for broadband is over 2% of the the county's median household income.[28] Note that because BroadbandNow only provides prices for 2020, the dummy for affordability is absorbed as a county-fixed effect. In all three columns, we see that the effects of municipal restrictions on broadband subscriptions are operating through counties in restriction states where plans are priced higher (row 2). In particular, in restriction-state counties where the lowest priced plan costs over 2% of a household's budget, the probability of at least 60% broadband subscribership at 25Mbps or 10Mbps is between 5.1 and 6.2 percentage points lower, respectively, compared with restriction-state counties where the lowest priced plan is 2% or less of the median household's budget.

For information on broadband service quality, we use a measure of data caps from the FCC Urban Rate Survey (Federal Communications Commission 2025b). During the analysis pre-period (2018–19), plans in restriction states were 11 percentage points more likely to be subject to a data cap. Further, among plans subject to monthly data caps, the usage allowance was 106GB lower in restriction states.[29] Assuming 1 GB of use per hour, this difference would allow subscribers in non-restriction states to make over 100 hours of additional video calls every month. There are many other aspects to service quality that we wish we

[28]Results are robust to other affordability metrics – such as a $75 per month threshold while controlling for median household income in the county. Households spent 4.3% of their monthly income on all other utilities in 2025 including electricity, gas, heating and water (Lending Tree 2025), a figure that is historically high itself. FCC guidance suggests two percent of monthly household income as a preliminary standard. See https://docs.fcc.gov/public/attachments/FCC-16-38A1.pdf

[29]These state-level differences are significant beyond the 1% level. The Urban Rate Survey does not provide data at the county-level during our sample period.

could have explored. However, the FCC acknowledges "..there are additional areas and measurements other than latency and consistency of service that are likely relevant to the universal service goal of availability. These include other metrics such as service outages and access to inside wiring. At present, we do not have comprehensive sources of data, but intend to revisit this issue in future inquiries" (FCC 2024).

Taken together, our results in this section suggest that broadband markets in restriction states had less competition leading up to the pandemic (Table 1 and Figure 8). We then show that the relative decreases in broadband subscribership observed after the pandemic in restriction states were likely driven by mechanisms such as reduced competition, lack of consumer choice, and less affordable prices (Figure 9, Table 5, and Table 6).

Internet Use from the CPS

Next we examine whether municipal broadband restrictions affect how people use internet, by using the CPS Internet Supplement. The CPS Internet Use data are only collected for a single month (November) every other year. We use data from November 2017 and 2019 as our pre-period, and from November 2021 and 2023 as our post-period. Note that this time period overlaps with, but is not the same as, the sample period that we employ for our labor supply outcomes (2018-2022). Internet Supplement data are also much smaller in sample size (<10% the size of our main CPS sample) – especially for outcomes such as the use of internet for email, phone and job search – so we should not expect to precisely estimate effects. Finally, the CPS Internet Use measures that we employ are generally broad; for example, the CPS simply asks whether someone accesses the internet from home or not. So someone answering "yes" to this question could be using dial-up internet or a cellular data plan.

We present our difference-in-differences results on CPS Internet use in Table 7. In panel A, we pool the sample for all prime-age (25-54 year-old) individuals. Prime-age workers in restriction states are 2.5% ($= \frac{-0.02}{0.81} * 100\%$) less likely to access the internet at home (column 1). Despite the broader internet use question in the Internet Use Supplement (and previous caveats), the negative 2.5% estimate in column 1 is directionally consistent with the 5.9% estimate in column 6 of Table 4. CPS respondents in restriction states are also less likely to access the internet outside the home (column 2) and they are less likely to use email (column 3), although these estimates are smaller in magnitude. This suggests that during the post-COVID period, CPS respondents in restriction states not only experienced a relative decrease in a broad measure of internet use, but they may also have experienced modest reductions in internet use even for ubiquitous, low-bandwidth activities such as email.

Looking at our subgroups of interest (panels B through D), we see that all 3 subgroups experience a reduction in home internet use; however, we are only able to estimate this with precision for women without children (column 1, panel B). However, estimates for outcomes in columns 1 through 3 are not statistically different across the 3 subgroups.

Column 4 of Table 7 uses a dummy for whether or not the respondent uses the internet for calls. Because of how the CPS asks this question, we believe this outcome is a good predictor of high-speed internet access especially for telework. The CPS asks, "In the past six months, (have/has) (you/NAME) participated in video or voice calls or conferencing over the Internet, such as with SKYPE or FaceTime? (Do you/Does NAME) participate in video or voice calls or conferencing?" Higher internet speed thresholds are often required for common modalities of telework such as video conferencing. From column 4 of Table 7, we see that married women with children (panel B) experience a large relative 5.1% ($= \frac{-0.034}{0.67} * 100\%$) decrease in using the internet for telephone calls during the post-pandemic period. This scaled estimate is similar in magnitude to the 4.7% decrease in employment that we estimate married women experienced in restriction states during the post-pandemic period (see Table 8, panel A, column 2). The effects for other groups (in column 4) are not precisely estimated.

Given that there is a large literature on the role of the internet in reducing market frictions and improving labor market matching (Bhuller, Kostol, and Vigtel 2020), we also examine the use of the internet to search for jobs. In column 4 of Table 7, we see that despite a lack of statistical significance, all workers may have experienced a modest decrease in the probability of using the internet to search for jobs (panel A). This effect is driven by married men with children (panel D). The scaled estimate implies that married fathers may have experienced an 11% decline in the probability of searching for a job using the internet ($p = 0.18$). Given the small sample size and lack of statistical significance, it is important to treat this estimate with caution. However, a decrease in job searching using the internet could help explain the modest 1.0% decrease in employment that we observe for married men with children (see panel C, column 3 of Table 8).

Labor Force Outcomes

Figure 10 plots the dynamic difference-in-differences estimates for three maternal labor force outcomes: the probability of being in the labor force (LFP), the probability of being employed at all, and the probability of being employed, at work. We find that married mothers are less likely to be in the labor force, they are less likely to be employed, and they are less likely to be employed at work in states with restrictions on municipal broadband compared with states without restrictions, post-2020. The results are most significant for married mothers in states with municipal restrictions starting in Q3-2021 and extending through Q4-2022.[30] This divergence in maternal labor force participation across states with and without broadband restrictions coincides with the expiration of extended federal unemployment benefits (Coombs et al. 2022). In other words, married mothers' ability to work from home may have become especially consequential for their labor force participation as unemployment insurance benefits expired.[31][32]

Table 8 summarizes the effects of municipal broadband restrictions on labor force outcomes for married women with children (panel A), women without children (panel B), and married men with children (panel C).[33] We find that broadband restrictions only reduce labor force participation among married women with children. The probability that married mothers are in the labor force is 0.027 lower in states with restrictions on municipal broadband compared with states without restrictions during the pandemic (column 1). Additionally, the probability that married mothers are employed-at-work is 0.031 lower in states with municipal restrictions compared with states without restrictions during the pandemic (column 2). There is also a reduction in the probability that married mothers are employed (at all) in states with restrictions during the pandemic (column 3), but there is no effect on the number of conditional hours worked (column 4).[34] Our results suggest that restrictions on municipal broadband primarily affected the extensive margin

[30] This divergence in maternal labor force participation rates across restriction and non-restriction states can also be seen in the raw trends for the same labor force outcomes in Figure A1. Labor force participation and employment decrease in states with restrictions (dotted line) starting in Q3-2021 while they increase in states without restrictions (solid line).

[31] Related to the timing of the effect, we rule out alternative explanations such as changes in sample sizes or school re-openings (which we control for). We also find similar results when we only examine mothers with children aged 3 and older (results available upon request).

[32] The event studies for women without children and married men with children are plotted in Appendix Figures A2 and A3 respectively.

[33] Appendix Tables A5 through A8 present complete regression estimates for all three sub-groups. Appendix Table A5 reports the estimates for labor force participation, Table A6 reports the estimates for "employed at work," Table A7 reports the estimates for employment overall, and Table A8 reports the estimates for hours worked last week conditional on working at all. The COVID rate, COVID death rate, and Bartik Shift-Share variables are rescaled by 1,000 to produce the numerically meaningful estimates in these tables.

[34] The number of hours worked last week is conditional on working at all, the unconditional hours are 0 for individuals who are not employed.

of employment for married mothers, rather than the intensive margin. panel B shows no relationship between restrictions on municipal broadband and labor supply for women without children. Married men with children see modest effects on employment and hours, although the latter is not precisely estimated (panel C).

Scaling the estimates by the means of the dependent variables shows that married women with children in states with municipal restrictions were 4% ($= \frac{-0.0268}{0.67} * 100\%$) less likely to be in the labor force and 4.7% ($= \frac{-0.0305}{0.655} * 100\%$) less likely to be employed after the COVID-19 pandemic compared with their counterparts in states without broadband restrictions. Married fathers in restriction states were about 1.0% ($= \frac{-0.009}{0.93} * 100\%$) less likely to be employed after the COVID-19 pandemic.

Labor Supply Channels: Caregiving and Telework

Table 9 shows that much of the decrease in maternal labor supply in states with restrictions after COVID-19 comes from married women with children shifting from employment to childcare (panel A). Married women with children in states with restrictions on municipal broadband are 7.3% ($= \frac{0.023}{0.314} * 100\%$) more likely to work part-time or not-at-all due to "family reasons" (column 2). They are 8.5% ($= \frac{0.0246}{0.289} * 100\%$) more likely to "not be in the labor force (NILF)" because they are "taking care of house or family" (column 3). However, we find no difference in the probability of having "child care problems" for married mothers in states with broadband restrictions compared with states without restrictions during COVID-19 (column 4). Similar to Table 8, we find no significant effects of restrictions on municipal broadband during COVID for these outcome variables for women without children or married men with children (panels B and C, respectively).

We also consider whether an individual works part-time ($<$ 35 hours per week across all jobs) as an additional measure of the intensive margin of labor supply.[35] These estimates are reported in column 1 of Table 9. In column 1, we see that the effects for switching from full-time to part-time work are small and not precisely estimated for all 3 groups.

The effects of municipal broadband restrictions on work-related time use appear in Table 10. All estimates are measured in minutes spent per workday except for column 2, which is a share between 0 and 1

[35] Our definition is based on the definition of "usual full-time work" in the CPS published by the Bureau of Labor Statistics : https://www.bls.gov/cps/definitions.htm#fullparttime.

(conditional on working a positive amount of time). The sample in Table 10 includes employed people only. In restriction states, working mothers spend 46 fewer minutes per day ($\frac{45.9}{72} \times 100\% = 64\%$) working from home compared with working mothers in non-restriction states. They also experience an 11.2 percentage point ($\frac{0.1115}{0.226} \times 100\% = 49\%$) relative decrease in the share of work time spent at home, and a 7 minute ($\frac{6.66}{25} \times 100\% = 27\%$) relative increase in commute-time. These results indicate that even among mothers that continue to work, a lack of broadband access leads to a substantial decrease in work-from-home (about 191 hours per year)[36] and a large increase in commute time. The estimated effects in Table 10 are smaller and lack statistical significance for women without children and married fathers.

Heterogeneity by Family Characteristics

Next, we present results showing how restrictions on municipal broadband affect maternal labor supply by family characteristics. Comparing labor supply effects for mothers across different types of marital status we find that the absence of a spouse magnified the effect of municipal broadband restrictions on mothers' labor supply (effects are 3 times larger compared with if spouse/partner is present). This is reported in Table A9.[37]

Table A10 shows results by education level for married women with children. Previous research has shown that college graduates were more likely to work remotely during the pandemic (Goldin 2022). We may therefore expect our effects to be concentrated among this group. However, our estimates for college-educated and non-college educated mothers are not statistically significantly different from one another. We think there are several reasons for this. A large share of women without a college degree also worked in teleworkable jobs as of 2022 (Cowan 2023) and would therefore be similarly affected by restrictions on broadband. Further, an increasing share of consumer spending is coming from high-income workers – who tend to have a higher education.[38] The withdrawal of some of this consumer spending – from college-educated educated mothers dropping out of the labor force may have effects for mothers with lower levels of education working in consumer service establishments such as hotels, restaurants, coffee shops, bars, and

[36] We assume 250 work days per year.

[37] Some may find the large coefficient estimate on Spouse Absent in row 3 of the table surprising. This reflects a large and persistent gap in the labor supply of married women with a spouse present versus mothers when the spouse is absent. See https://www.bls.gov/news.release/archives/famee_04192023.pdf

[38] See figure 3 from https://www.federalreserve.gov/econres/notes/feds-notes/a-better-way-of-understanding-the-u-s-consumer-decomposing-retail-spending-by-household-income-20241011.html

hair salons. Previous research has found that the location decisions of highly educated workers specializing in business services affects consumer spending, and therefore the work hours of consumer service workers, who tend to be less educated (Althoff et al. 2022).

Table A11 shows results by race (white vs. non-white) for married women with children. Here we find that restrictions on municipal broadband affect women of different races similarly; both white (panel A) and non-white (panel B) married mothers reduce their labor force participation in states with restrictions compared with states without restrictions during the COVID-19 pandemic. In other words, our estimated effects by race are not statistically significantly different. Finally, we divide the sample of married mothers into groups based on the age of the children in Table A12. Our results are largest in magnitude, and most precisely estimated, for married mothers whose youngest child is under age 11 (panel A) (vs. 12 or older in panel B); however, the estimates are not statistically significantly different.

Robustness Checks

There may be concerns that our labor supply results are sensitive to dropping the early portion of the COVID period. Recall that our main specification in Table 8 drops CPS sample months between January 2020 and July 2021 to avoid measuring the direct effects of the pandemic. Further, cross-county differences in initial broadband penetration may drive our labor supply results. To assuage these concerns, we present labor supply results with our most restrictive specification in Table A13. This specification includes geographic area fixed effects (as described in the Data Subsection on Control Variables Related to the COVID-19 Pandemic) instead of state fixed effects. Additionally, the sample includes all months for years between 2018 and 2022. The results in Table A13 are qualitatively similar to those in Table 8, although slightly smaller in magnitude.[39]

We also estimate a robustness test where we specify the "control group" of states as those that did not have restrictions on municipal broadband *and* they had municipal broadband initiatives. This alternative analysis might show that the impacts of municipal restrictions are even larger than we previously estimated in our primary specification, since the control group in our primary specification included 6 non-restriction states without any municipal broadband initiatives. The Institute for Local Self-Reliance (ILSR) provided

[39] Differences in initial county-level broadband penetration may also be coming from differences in state municipal broadband policy. Since including area fixed-effects also absorbs some of this variation, our results in A13 could be considered a lower (conservative) bound.

us with a list of municipal broadband networks – data are available only for 2023. According to the ILSR list, the six non-restriction states that have no municipal broadband initiatives include Arizona, Delaware, Mississippi, New Mexico, North Dakota, and West Virginia. We begin by dropping these 6 former non-restriction (i.e. control states from our sample). This leaves us with the same 17 treatment, i.e. restriction states as before, but our control states now only include 23 non-restriction states that had municipal broadband initiatives. We then replicate the analysis using the main difference-in-differences specification for Table 4 but use our restricted sample. Table A2 shows the results from this specification. Similar to Table 4, we use the same outcomes and estimate both an ordered logit and a LPM model.

Comparing results between Table A2 and Table 4, we see that dropping non-restriction states without municipal broadband initiatives makes our results stronger. Estimates in columns 2–6 are larger in magnitude. In general, we also see that the effects are more precisely estimated. From columns 2, 4, and 6 in Table A2, we see that restrictions reduced the probability of achieving high broadband subscribership thresholds by between 2.6 percentage points and 4.3 percentage points when we consider only non-restriction states with municipal broadband initiatives as control states. Note that although the average effects are larger (after dropping the 6 states), they are not statistically significantly different. This robustness test suggests that states with actual municipal broadband initiatives, rather than those without bans on such initiatives, drove the relative difference in broadband subscribership between restriction and non-restriction states.

Next, we perform a similar analysis for our labor outcomes, dropping the 6 non-restriction states without municipal broadband initiatives. The results are presented in Table A14. Compared with Table 8, the effects are slightly larger but not statistically significantly different.

We also consider whether our results are driven by states with municipal restrictions alone or by states with a combination of municipal and co-op restrictions on broadband provision. Eleven out of seventeen states with municipal restrictions had some type of roadblock to cooperative broadband provision in 2017.[40] However, most of those roadblocks entailed "easement ambiguities." Easement ambiguity means the installation of any broadband infrastructure – even a single cable – may exceed the scope of the existing easement agreement and lead to claims of damages for trespassing from private landowners. Between 2019 and 2022,

[40] These include Nebraska and Missouri (explicit bans), North Carolina (ban on use of state funds), Virginia (onerous cost imputations). Tennessee and South Carolina (limited electrical cooperatives to providing broadband only within their authorized service territory or allowed a narrow buffer). The remaining states lacked explicit easement authority and included Alabama, Texas, Michigan, Minnesota and Pennsylvania.

roughly 20 states clarified their easement issues, thus allowing electric utilities to install broadband infrastructure without securing easements from individual property owners (Lide and Magee 2022). Aside from the easement ambiguity, 5 states had other significant roadblocks to co-op broadband provision in 2018. Those states were Mississippi, Missouri, Nebraska, North Carolina, and Virginia. Therefore, in Table A4, we show how broadband subscribership differed in the COVID-era between states with municipal restrictions only and states with municipal and co-op restrictions as of 2018 (MO, NC, NE, VA).[41] We find no statistically significant differences across the two types of states. The presence of municipal restrictions alone appears to be the limiting factor to higher broadband subscribership; however, we acknowledge that the history of easement ambiguities in many of these states complicates the analysis.

Next, we explore whether our labor force participation results hold up for all workers in teleworkable jobs (instead of only married mothers). We replicate the results in Table 8, but for teleworkable jobs only. We use the methodology from Dingel and Neiman (2020) to categorize occupations into teleworkable or not. This gives us the result in Table A15.[42] We see that results for married women with children in teleworkable jobs (panel A) are less precisely estimated than our main results, potentially due to the smaller sample size, but they are not statistically different than our main results. Interestingly, in panels B and C, we now see comparable decreases in labor supply for women without children and custodial fathers in teleworkable jobs, although only the result for the former group is precisely estimated (panel B). The results in Table A15 suggest that labor supply for all 3 groups may have been affected by municipal broadband restrictions if the people were in teleworkable jobs. This is consistent with previous work (Carvalho, Hagerman, and Whitacre 2022) showing that low broadband adoption rates prevented counties from recovering their COVID-19 employment losses.

Finally, we also rule out identification concerns due to drastic and differential changes in demographic characteristics across restriction vs. non-restriction states around COVID-19. For example, non-restriction states do not gain relatively large numbers of college-educated or young persons over the sample period.[43]

[41] We drop MS from this analysis since it is the only state with co-op only restrictions.
[42] Note that the table retains workers who are unemployed or not in labor force, since we cannot determine their occupation and therefore cannot use the Dingel and Neiman (2020) methodology to determine if their occupation is teleworkable.
[43] Results available upon request.

Discussion

This paper shows how restrictions on municipal broadband affected the labor force outcomes of married women with children before and after the COVID-19 pandemic. In multiple surveys, Americans cite high prices and limited access as barriers to using broadband. Municipal broadband has the potential to reduce prices and increase access; for example, Talbot, Hessekiel, and Kehl (2018) show that municipal broadband networks charge lower prices than private ISPs. Using FCC broadband subscribership data, we show that broadband subscribership grew faster post-2020 in states that did not restrict local governments and cooperatives from competing in broadband markets. We further show that subscribership growth was particularly low in states with municipal restrictions *and* non-competitive ISP markets and/or with less affordable plans. Our paper's primary hypothesis, therefore, was that reduced access to high-speed internet at home reduced peoples' ability to work from home during and after the pandemic (in restriction states). Our analysis focuses on married women with children because their labor supply may have been especially sensitive to remote work options around the pandemic. For comparison, we also consider the effects of broadband restrictions on women without children and married men with children.

Our difference-in-differences estimates reveal that municipal broadband restrictions reduced labor force participation (LFP) and employment among married women with children. Though maternal labor supply was recovering in both restriction and non-restriction states during the post-pandemic period, it recovered more quickly in states without restrictions on broadband internet. In particular, LFP among women with children in states with broadband restrictions begins its relative decline around Q3-2021. This time period coincided with the expiration of federal extended unemployment benefits. As such, LFP among mothers began increasing in the country overall in 2021, but LFP increased more in states without restrictions on municipal broadband. We posit this divergence across states occurred because mothers had more ability to work remotely in states without municipal broadband restrictions. In contrast, we find that municipal restrictions did not affect labor force outcomes for women without children or married men with children, unless they worked in teleworkable occupations.

This paper highlights the importance of broadband access in the modern economy. Fewer barriers to entry in broadband markets (i.e., removing municipal/cooperative restrictions) could lead to greater competition among ISPs. More ISP competition may improve broadband take-up by making plans more affordable and higher quality. In turn, increased broadband adoption by households may foster a flexible work environment that can be conducive to mothers and other marginally attached workers.

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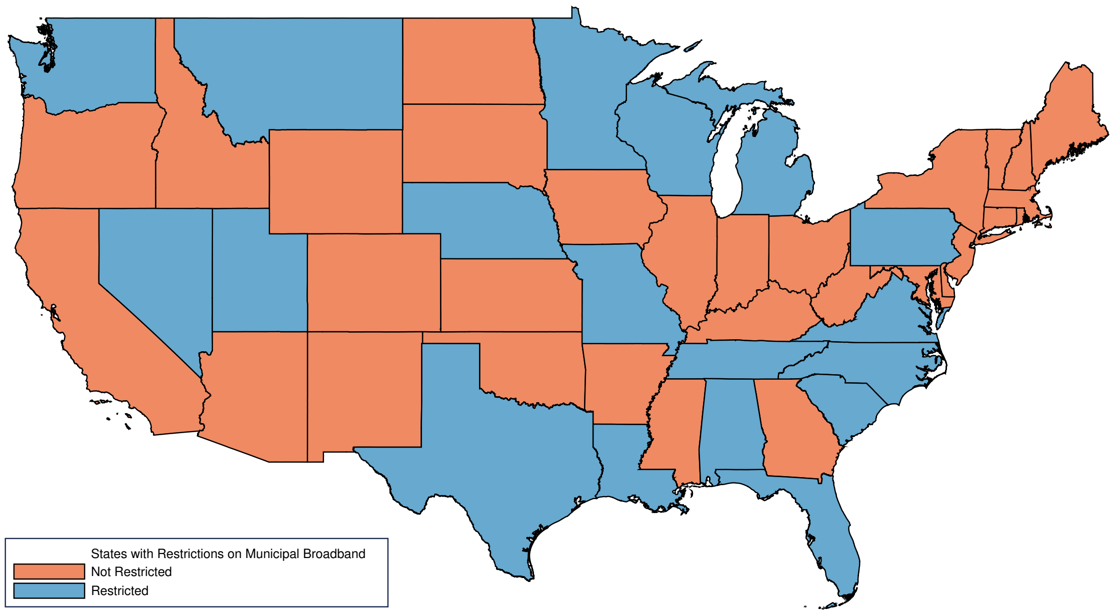
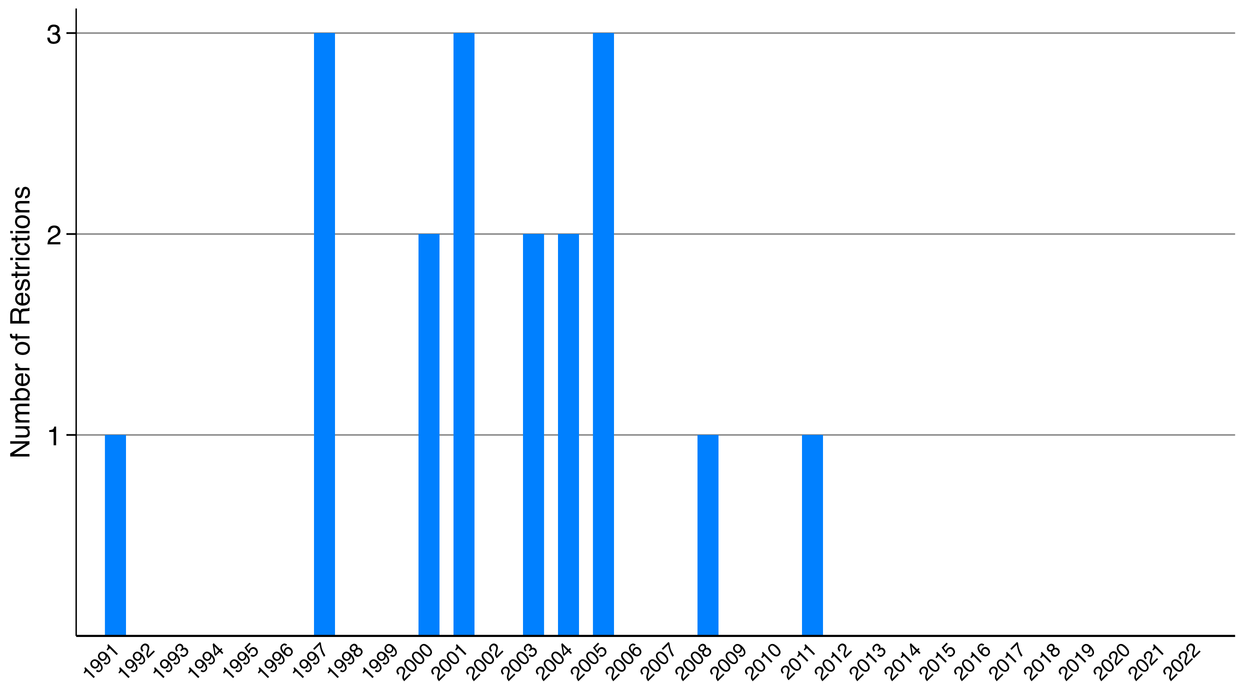
Figures

Figure 1: States with Restrictions on Municipal Broadband (2018)



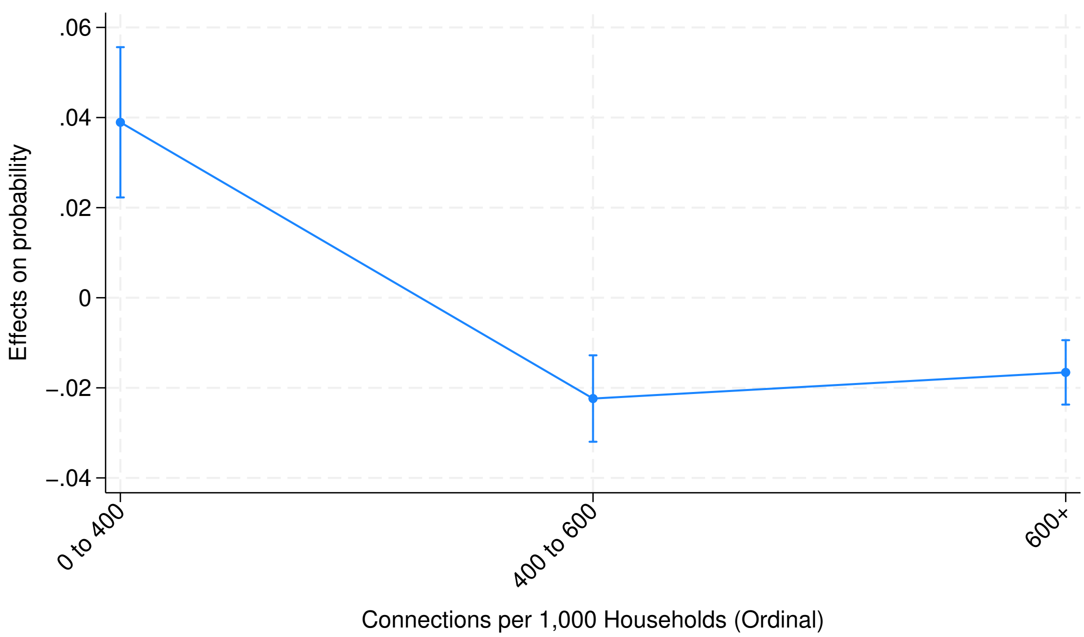
Source: Pew State Broadband Policy Explorer

Figure 2: Number of Municipal Restrictions Passed in State Legislatures



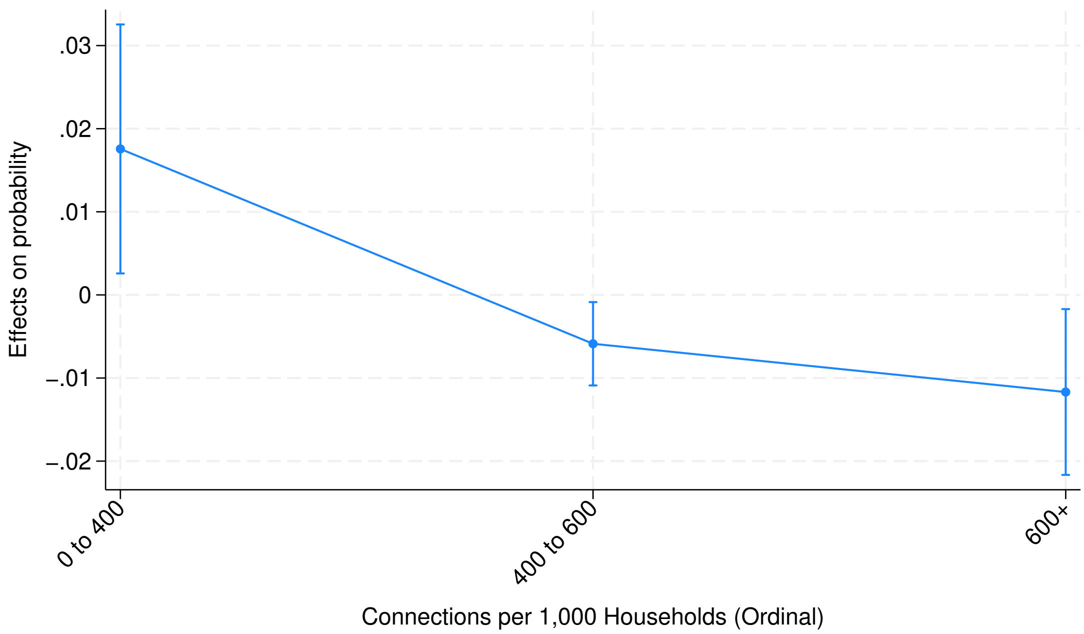
Source: Pew State Broadband Policy Explorer, 1999–2022

Figure 3: Impact of Restrictions on FCC County Broadband Access (100Mbps or Higher)



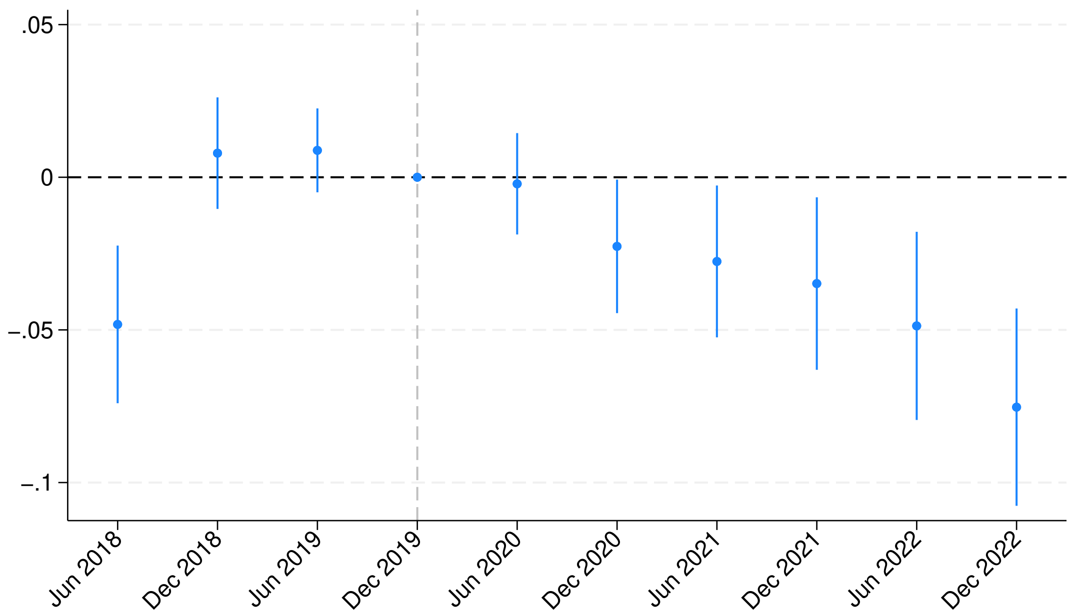
Notes. Figure plots the marginal effect of restrictions on the probability of the number of connections per 1,000 households. The dependent variable is an ordinal (0, 1 or 2) biannual value for FCC County Broadband Access reflecting connections per 1,000 housing units in a county with 25Mbps download or higher, where 0 is 0 to 400, 1 is 400 to 600, and 2 is 600+. Model controls include time-varying log of county population, log density,population with a bachelor's degree or higher, lagged log employment, lagged log of household median income, population below poverty income, 2017 broadband access, year-month and state fixed effects. Robust standard errors are clustered at the county level. Vertical bars are 95% confidence intervals. Source: FCC Form 477 County Data on Internet Access Services and American Community Survey 5-Year Data (2018-2022).

Figure 4: Impact of Restrictions on FCC County Broadband Access (25Mbps or Higher)



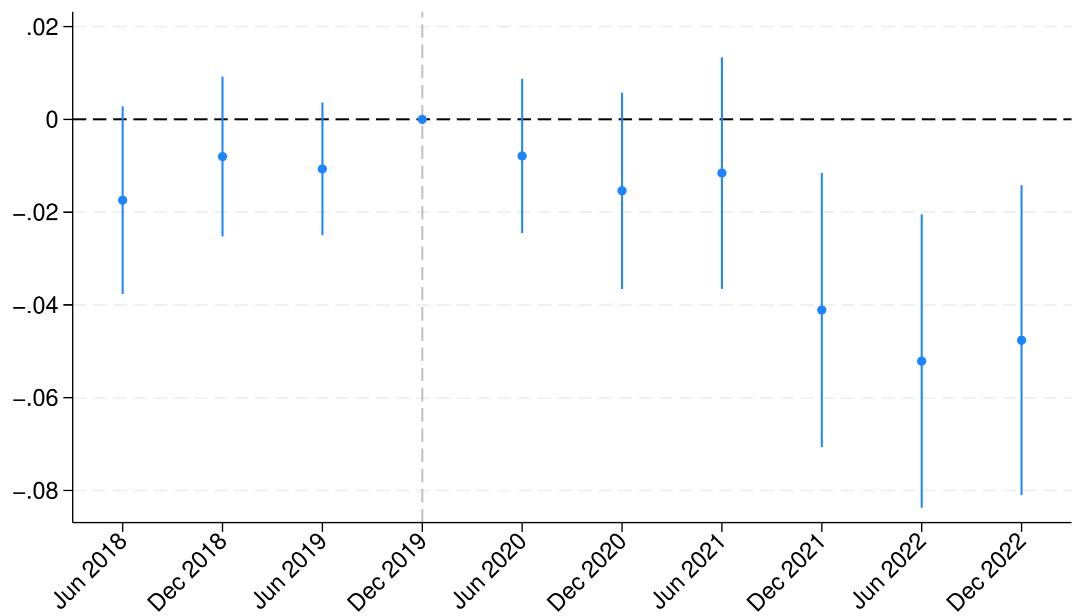
Notes. Figure plots the marginal effect of restrictions on the probability of the number of connections per 1,000 households. The dependent variable is an ordinal (0, 1 or 2) biannual value for FCC County Broadband Access reflecting connections per 1,000 housing units in a county with 25Mbps download or higher, where 0 is 0 to 400, 1 is 400 to 600, and 2 is 600+. Model controls include time-varying log of county population, log density,population with a bachelor's degree or higher, lagged log employment, lagged log of household median income, population below poverty income, 2017 broadband access, year-month and state fixed effects. Robust standard errors are clustered at the county level. Vertical bars are 95% confidence intervals. Source: FCC Form 477 County Data on Internet Access Services and American Community Survey 5-Year Data (2018-2022).

Figure 5: Event Study Estimates of Municipal Broadband Restrictions on FCC County Broadband Access (100Mbps or Higher)



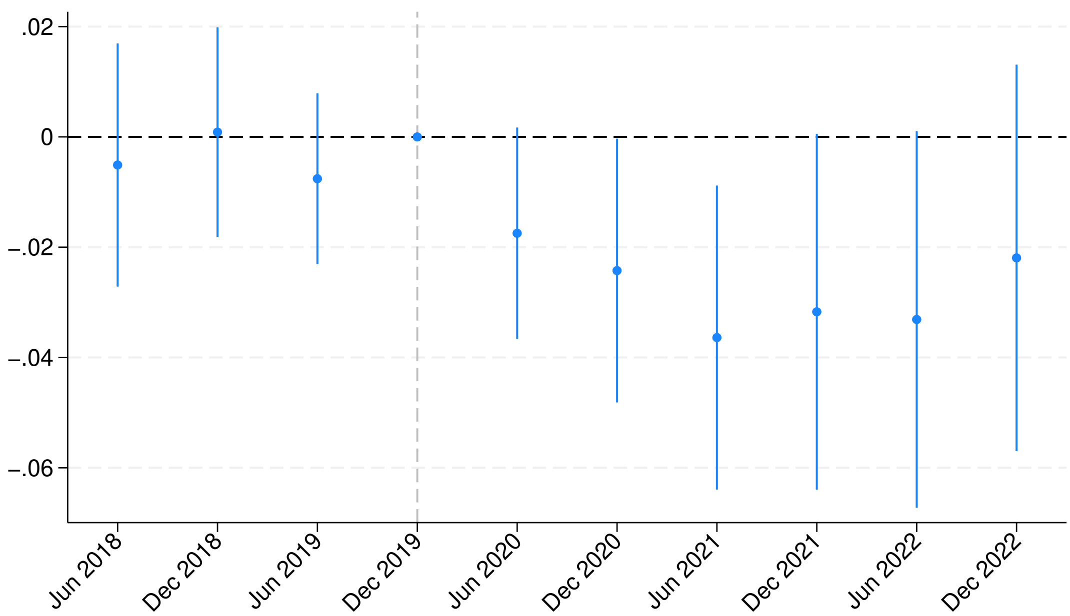
Notes. The dependent variable is a dummy that equals 1 if FCC County Broadband Access is 20% or higher. The figure plots OLS coefficients of the dynamic effects for states with municipal broadband restrictions compared with states without restrictions over time on broadband access. Model controls include time-varying log of county population, log density, population with a bachelor's degree or higher, lagged log employment, lagged log of household median income, population below poverty income, year-month and county fixed effects. Robust standard errors are clustered at the county level. Vertical bars are 95% confidence intervals. Source: FCC Form 477 County Data on Internet Access Services and American Community Survey 5-Year Data (2018-2022).

Figure 6: Event Study Estimates of Municipal Broadband Restrictions on FCC County Broadband Access (25Mbps or Higher)



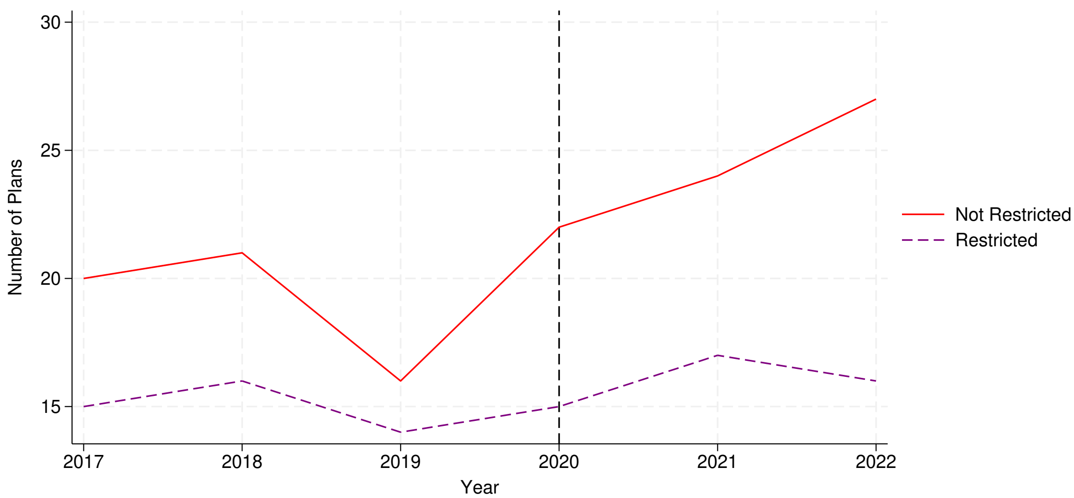
Notes. The dependent variable is a dummy that equals 1 if FCC County Broadband Access is 60% or higher. The figure plots OLS coefficients of the dynamic effects for states with municipal broadband restrictions compared with states without restrictions over time on broadband access. Model controls include time-varying log of county population, log density, population with a bachelor's degree or higher, lagged log employment, lagged log of household median income, population below poverty income, year-month and county fixed effects. Robust standard errors are clustered at the county level. Vertical bars are 95% confidence intervals. Source: FCC Form 477 County Data on Internet Access Services and American Community Survey 5-Year Data (2018-2022).

Figure 7: Event Study Estimates of Municipal Broadband Restrictions on FCC County Broadband Access (10Mbps or Higher)



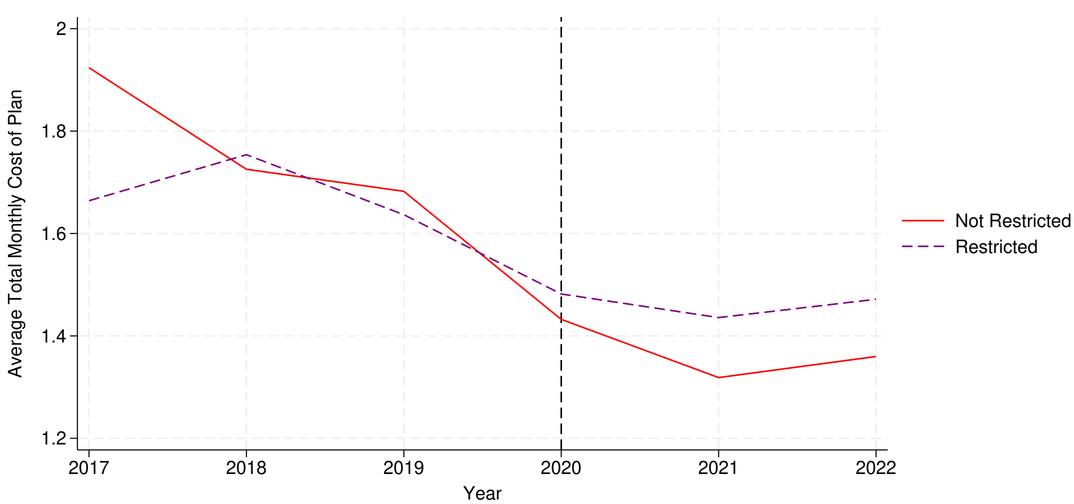
Notes. The dependent variable is a dummy that equals 1 if FCC County Broadband Access is 60% or higher. The figure plots OLS coefficients of the dynamic effects for states with municipal broadband restrictions compared with states without restrictions over time on broadband access. Model controls include time-varying log of county population, log density, population with a bachelor's degree or higher, lagged log employment, lagged log of household median income, population below poverty income, year-month and county fixed effects. Robust standard errors are clustered at the county level. Vertical bars are 95% confidence intervals. Source: FCC Form 477 County Data on Internet Access Services and American Community Survey 5-Year Data (2018-2022).

Figure 8: Number of Plans in each State



Notes. Figure plots the raw trends for the count of number of plans with at least 25/3 Mbps Speeds for Fiber to the Home, Cable and DSL broadband, unlimited usage and costing $80 or less in total monthly charges for each state-year. Source: FCC Urban Rate Survey for Fixed Broadband Service (2019-2023, reflecting data from 2018-2022).

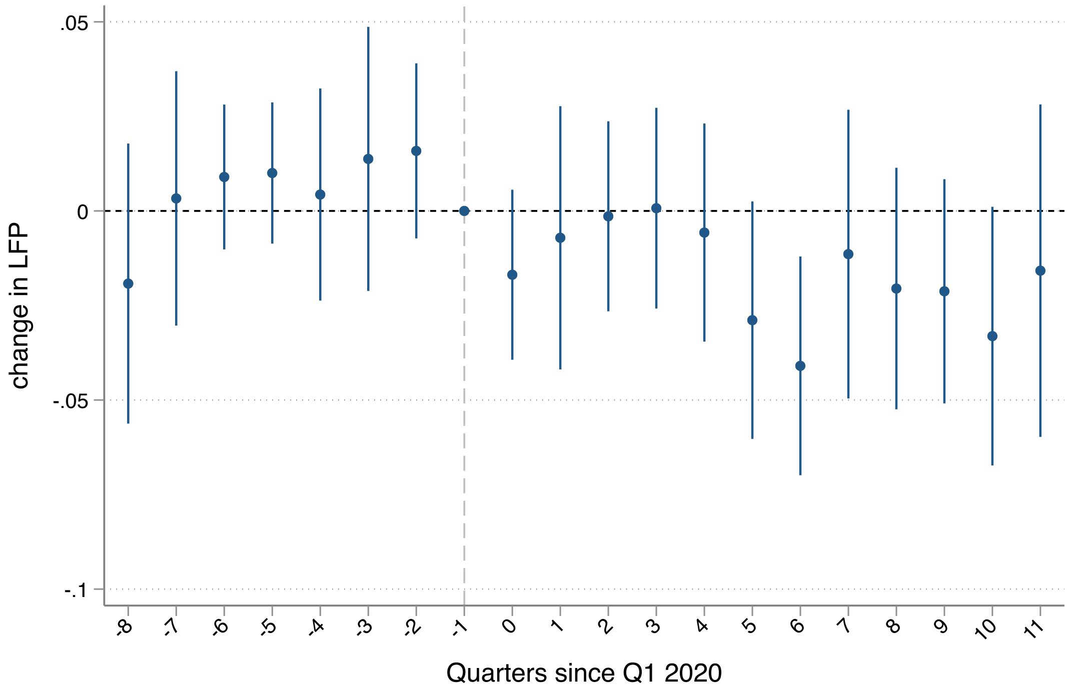
Figure 9: Average Plan Affordability as Percentage of Monthly Income



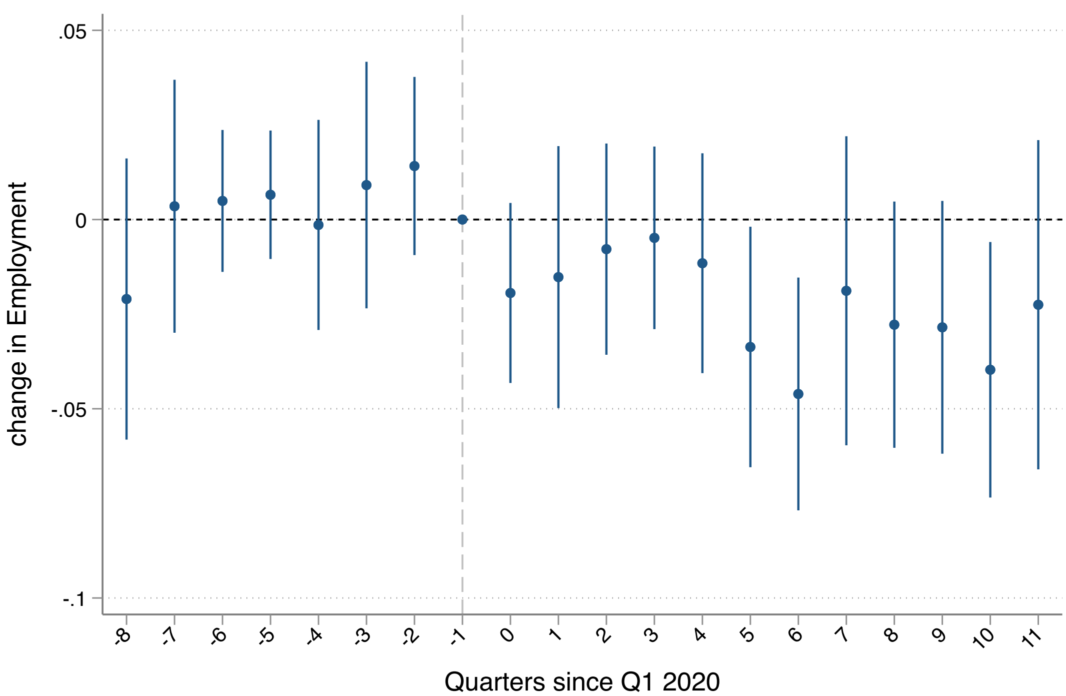
Notes. Figure plots the raw trends for percent of monthly household income spent on total monthly charges on average for plans with at least 25/3 Mbps Speeds for Fiber to the Home, Cable or DSL broadband with unlimited usage for each year. Estimates are weighted by sample weights. Source: FCC Urban Rate Survey for Fixed Broadband Service (2018-2023, reflecting data from 2017-2022).

Figure 10: Event Study Estimates of Municipal Broadband Restrictions on Labor Force Outcomes for Married Women with Children

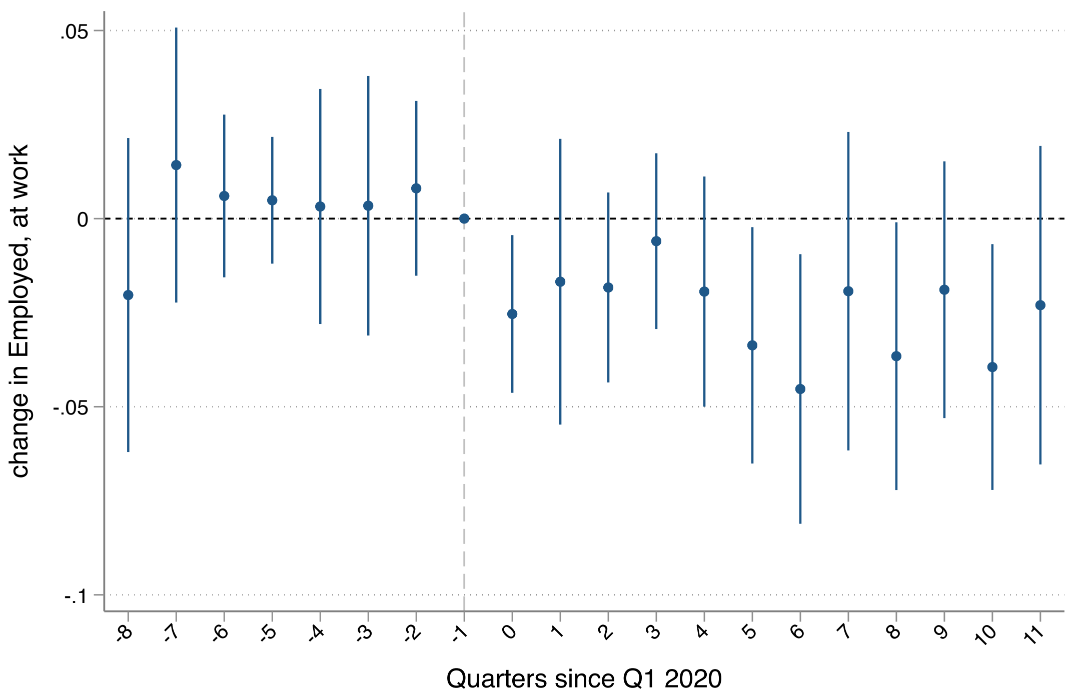
(a) Labor Force Participation



(b) Employment



(c) Employed, At Work



Notes: The dependent variables are a dummy for being in the labor force (Panel a), being employed (Panel b) and being employed, at work (Panel c) for prime-age married women with children. All models include state fixed effects, region-by-quarter fixed effects and Bartik shift-share variables. Individual control variables include age, race (Black Non-Hispanic, other Non-Hispanic, and Hispanic), education (less than high school, some college, bachelor's degree, advanced degree), metro area residency, MSA or CBSA size, marital status, number of children in the household and an indicator for at least one child under 13. County COVID-19 controls include a continuous measure of cumulative cases and deaths per capita (per 1M residents) along with a control for school closures. Observations are weighted using CPS sample weights. Robust standard errors are clustered at the state-level. 95% confidence intervals are shown.

Source:Monthly Current Population Survey, 2018–2022.

Tables

Table 1:
County-Level Summary Statistics, FCC Measures of Broadband Subscribership and Competition (2018-19)

	Non-Restriction mean/sd	Restriction mean/sd	Difference b/t
% with 60%+ households at 100MBps	3.72	2.67	1.05**
	(18.94)	(16.12)	(3.20)
% with 60%+ households at 25 MBps	12.53	12.46	0.06
	(33.10)	(33.03)	(0.10)
% with 60%+ households at 10 MBps	25.19	23.91	1.27
	(43.41)	(42.66)	(1.58)
% 0 Providers 100Mbps	30.83	35.15	-4.32***
	(27.40)	(30.08)	(-8.03)
% 0 Providers 25Mbps	22.48	26.45	-3.97***
	(21.14)	(24.17)	(-9.36)
% 0 Providers 10Mbps	14.34	16.93	-2.59***
	(15.31)	(17.99)	(-8.29)
% 2+ Providers 100Mbps	18.65	18.35	0.30
	(24.56)	(24.55)	(0.66)
% 2+ Providers 25Mbps	28.72	27.06	1.66**
	(27.46)	(27.86)	(3.21)
% 2+ Providers 10Mbps	45.10	42.07	3.03***
	(28.93)	(29.12)	(5.59)
N	5,828	5,616	11,444

Notes. The first two columns display the means with standard deviations in parentheses for a select set of variables for states without and with restrictions on municipal broadband, respectively. The last column shows the difference in the variables by state restriction status with t-statistics in parentheses. All broadband estimates are biannual county percentages (0-100%) between June 2018 and December 2019. In rows 1 to 3, we consider a county covered by broadband for a given speed if the FCC County-Level Internet Access Services Speed Tier value indicates that over 60% of households are actually connected at that speed. The remaining rows indicate the population percentage of county covered by the number of fixed residential broadband providers if broadband is available at that speed – we use "acfo' for 10+/1+ speeds and only "cfo" for 25+/3+ speeds. a=ADSL, c=Cable, f=fiber, o=other. Source: County-Level Internet Access Services Speed Tier Data from FCC Form 477 (June 2018-December 2019), FCC Area Tables (June 2018- December 2019). Means and (Standard Deviations)

Table 2:
County-Level Summary Statistics, ACS5 Measures of Socio-Demographic Covariates (2018-19)

	Non-Restriction	Restriction	Difference
County Population	91534.58	98127.53	-6592.95
	(262889.01)	(255344.57)	(-1.36)
Pop/Sq Mile	312.70	233.76	78.94*
	(2458.47)	(736.92)	(2.31)
Median Age	41.15	41.51	-0.37***
	(4.84)	(5.59)	(-3.74)
Percent Female	50.02	49.89	0.13**
	(2.02)	(2.54)	(3.08)
Percent Black	8.01	10.59	-2.58***
	(14.24)	(14.76)	(-9.52)
Percent Hispanic	7.42	10.40	-2.98***
	(10.57)	(15.66)	(-11.95)
Percent College or More	15.21	14.64	0.57***
	(6.66)	(6.12)	(4.78)
Percent Employed	44.63	43.81	0.82***
	(6.49)	(6.12)	(6.98)
Median Income	52952.43	51819.67	1132.75***
	(14055.37)	(13111.06)	(4.45)
Percent below Poverty Income	14.67	14.66	0.01
	(6.31)	(5.50)	(0.07)
N	2,914	2,808	5,722

Notes. The first two columns display the means with standard deviations in parentheses for a select set of variables for states without and with restrictions on municipal broadband, respectively. The last column shows the difference in the variables by state restriction status with t-statistics in parentheses. All demographic estimates are annual county percentages (0-100%). Source: County-Level American Community Survey 5-Year Data (2018-19). Means and (Standard Deviations).

Table 3: Summary Statistics, CPS Labor Force and Socio-Demographic Measures for All Prime-Age Workers 2018–2019

	Non-Restriction	Restriction	Difference
Labor Force Participation	0.805 (0.396)	0.801 (0.399)	0.004*** (20.07)
Employed, Any	0.777 (0.416)	0.776 (0.417)	0.001 (2.42)
Employed, At Work	0.756 (0.429)	0.755 (0.430)	0.001 (0.96)
Hours Worked Last Week	40.832 (11.774)	40.864 (11.692)	-0.032 (1.11)
Not in Labor Force, Family Reasons	0.089 (0.285)	0.095 (0.294)	-0.006*** (90.82)
Part-Time/Not Working, Family Reasons	0.097 (0.296)	0.103 (0.304)	-0.006*** (68.58)
Part-Time, Childcare Reasons	0.008 (0.087)	0.007 (0.083)	0.001*** (11.61)
Less than High-School	0.074 (0.262)	0.085 (0.279)	-0.011*** (337.59)
High-School	0.630 (0.483)	0.669 (0.471)	-0.039*** (1317.88)
Some College	0.261 (0.439)	0.283 (0.450)	-0.022*** (453.50)
College degree	0.251 (0.434)	0.232 (0.422)	0.019*** (402.47)
Advanced Degree	0.119 (0.323)	0.099 (0.299)	0.020 *** (789.85)
Age	39.054 (8.805)	39.101 (8.754)	-0.047** (5.60)
Black	0.131 (0.338)	0.151 (0.358)	-0.020*** (600.56)
White	0.618 (0.486)	0.584 (0.493)	0.034*** (968.00)
Hispanic	0.159 (0.366)	0.196 (0.397)	-0.037*** (1812.17)
Other Race	0.091 (0.288)	0.070 (0.255)	0.021*** (1271.83)
Number of Own Children	1.079 (1.254)	1.098 (1.271)	-0.019*** (42.30)
In Central City	0.286 (0.452)	0.275 (0.447)	0.011*** (110.43)
Teleworkable	0.123 (0.328)	0.122 (0.327)	0.001 (0.57)
N	445,268	354,090	799,358

Notes: The table shows summary statistics for all prime-age workers (25-54 years old) in states with (column 2) and without (column 1) restrictions on municipal broadband, measured prior to the COVID-19 pandemic in 2018-2019. Means with standard deviations reported in parentheses. Observations are weighted using CPS individual sample weights. Hours are estimated conditional on employment. In column 3, the difference in variables is reported along with F-statistics in parentheses.
Source: CPS Monthly Estimates 2018–2019

Table 4:
Impact of Restrictions on FCC County Broadband Subscribership by Download Speeds

	(1) 100Mbps 3 Categories Ologit	(2) 100Mbps 20% or More LPM	(3) 25Mbps 3 Categories Ologit	(4) 25Mbps 60% or More LPM	(5) 10Mbps 3 Categories Ologit	(6) 10Mbps 60% or More LPM
Restrictions x Post	-0.455***	-0.027**	-0.207**	-0.021*	-0.023	-0.025**
	(0.102)	(0.012)	(0.086)	(0.012)	(0.083)	(0.012)
R^2	0.524	0.755	0.562	0.699	0.578	0.720
Mean	0.433	0.611	0.817	0.254	1.192	0.408
N	28,592	28,592	28,592	28,592	28,592	28,592

Notes. All outcomes are county-level biannual values for broadband penetration (actual number of connections) between June 2018 and December 2022. In columns 1 and 2, broadband penetration is defined as a minimum download speed of 100Mbps, in columns 3 and 4 it is 25Mbps, in columns 5 and 6 it is 10Mbps. The dependent variable in columns 1, 3 and 5 is an ordinal value (0, 1 or 2) for FCC County broadband penetration reflecting actual connections per housing units in a given county (corresponding to 0 to 40%, 40 to 60% and over 60% subscribership respectively) and is regressed using an ordered logistic model (Ologit). For columns 2, 4 and 6, the dependent variable is a dummy that equals 1 if FCC County broadband penetration is 20% or higher, 60% or higher and 60% or higher respectively and is regressed using a Linear Probability Model (LPM). All specifications include time-varying log of county population, log density (county population divided by land area), percent population with a bachelor's degree or higher, lagged log of employment, lagged log of household median income, percent population below poverty income, 2017 broadband penetration, year-month and state fixed effects. LPM models additionally include county fixed-effects. Standard errors are clustered at the county-level. Source: FCC Form 477 County Data on Internet Access Services and American Community Survey 5-Year Data (2018-2022).

Table 5:
The Impact of Restrictions on County Broadband Subscribership - The Role of Competition

	(1)	(2)	(3)
	100Mbps	25Mbps	10Mbps
Restrictions	0.0123	-0.0928***	-0.0480***
	(0.0135)	(0.0118)	(0.0135)
% Pop. Coverage by 2+ providers	0.0025***	-0.0006*	0.0016***
	(0.0004)	(0.0003)	(0.0004)
Restrictions x 2+ Coverage	-0.0007***	0.0026***	0.0006**
	(0.0002)	(0.0003)	(0.0003)
R^2	0.826	0.759	0.792
N	20,030	20,030	20,030

Notes. All outcomes are county-level biannual values for broadband penetration (actual number of connections) between June 2018 and December 2022. In columns 1, 2 and 3 respectively, broadband penetration is defined as a minimum download speed of 100Mbps, 25Mbps or 10Mbps. The dependent variable is a dummy that equals 1 if FCC County broadband penetration is 20% or higher, 60% or higher and 60% or higher respectively and is regressed using a Linear Probability Model (LPM). The independent variable in row 1 is a dummy that equals 1 for states with restrictions after 2020. Row 2 provides the coefficient estimates on the variable for the percentage of county population (0-100%) covered by 2 or more providers. In row 3, the independent variable (for restrictions) is interacted with the percentage of population covered by 2 or more providers. All specifications include time-varying log of county population, log density, percent population with a bachelor's degree or higher, lagged log of county employment, lagged log of household median income, percent population below poverty income, county-fixed effects, year-month and state fixed effects. Standard errors are clustered at the county-level. Source: FCC Form 477 County Data on Internet Access Services and American Community Survey 5-Year Data (2018-2022). Percentage of population covered by 2 or more providers comes from FCC Area Tables (2018-21).

Table 6:
Do Higher Prices Serve as a Channel for the Impact of Restrictions on County Broadband Subscribership?

	(1) 100Mbps	(2) 25Mbps	(3) 10Mbps
Restrictions	-0.022*	-0.014	-0.016
	(0.012)	(0.012)	(0.013)
Restrictions x Over 2 percent of Budget on Broadband	-0.035**	-0.051***	-0.062***
	(0.018)	(0.017)	(0.020)
R^2	0.755	0.699	0.721
N	28,592	28,592	28,592

Notes. All outcomes are county-level biannual values for broadband penetration (actual number of connections) between June 2018 and December 2022. In columns 1, 2 and 3 respectively, broadband penetration is defined as a minimum download speed of 100Mbps, 25Mbps or 10Mbps. The dependent variable is a dummy that equals 1 if FCC County broadband penetration is 20% or higher, 60% or higher and 60% or higher respectively and is regressed using a Linear Probability Model (LPM). The independent variable in row 1 is a dummy that equals 1 for states with restrictions on municipal broadband after 2020 and zero otherwise. In row 2, the same independent variable is interacted with an indicator that equals one if the county lowest price for broadband constitutes over 2% of the county's median monthly household income. All specifications include time-varying log of county population, log population density, percent population with a bachelor's degree or higher, lagged log of county employment, percent population below poverty income, county-fixed effects, year-month and state fixed effects. Standard errors are clustered at the county-level. Source: FCC Form 477 County Data on Internet Access Services and American Community Survey 5-Year Data (2018-2022). County-level measures of lowest monthly prices from BroadbandNow's United States County Broadband Statistics for 2020. Lowest price is the average of all lowest priced broadband speed plans available in the county. Note that because BroadbandNow only provides prices for 2020, the dummy for prices is absorbed as a county-fixed effect.

Table 7:
Impact of Restrictions on CPS Internet Use by Subgroup

	(1) Home Internet	(2) Outside Home	(3) Email	(4) Phone	(5) Job Search
Panel A: All Prime-Age Individuals					
Restrictions $\times$Post	-0.0201*	-0.0168**	-0.0116*	0.0152	-0.0089
	(0.0108)	(0.0082)	(0.0058)	(0.0128)	(0.0092)
$Mean$	0.812	0.736	0.932	0.607	0.235
R^2	0.043	0.073	0.071	0.123	0.029
N	122,576	122,576	55,870	55,870	55,870
Panel B: Married Women with Children					
Restrictions $\times$Post	-0.0076	0.0031	0.0093	-0.0340*	0.0195
	(0.0149)	(0.0106)	(0.0109)	(0.0199)	(0.0173)
$Mean$	0.841	0.754	0.953	0.675	0.206
R^2	0.036	0.075	0.096	0.116	0.025
N	21,574	21,574	8,487	8,487	8,487
Panel C: Women without Children					
Restrictions $\times$Post	-0.0244**	-0.0232*	-0.0012	0.0293	-0.0030
	(0.0118)	(0.0127)	(0.0121)	(0.0239)	(0.0133)
$Mean$	0.803	0.724	0.937	0.615	0.249
R^2	0.044	0.081	0.074	0.135	0.040
N	30,185	30,185	13,964	13,964	13,964
Panel D: Married Men with Children					
Restrictions $\times$Post	-0.0120	-0.0080	-0.0190	0.0098	-0.0218
	(0.0135)	(0.0197)	(0.0133)	(0.0196)	(0.0153)
$Mean$	0.844	0.785	0.939	0.635	0.200
R^2	0.041	0.072	0.098	0.124	0.027
N	21,722	21,722	9,179	9,179	9,179

Notes. This table reports estimates from difference-in-differences models that compare workers in states with restrictions on municipal broadband to workers in states without restrictions before and after the COVID-19 pandemic. The outcomes in columns 1 through 5 are respectively - a dummy for whether or not a person accesses the internet from home, a dummy for whether or not a person accesses the internet outside of the home, whether or not a respondent uses the internet for email, whether or not a person used the internet for telephone calls and whether or not a person has used the internet to search for jobs. The treatment variable is a binary variable for persons (aged 25 to 54) living in states that had municipal restrictions as of Q1:2018. CPS data are from the Computer and Internet Use Supplement for the month of November in 2017, 2019 (pre-COVID) and 2021, 2023 (post-COVID). All models include state fixed effects and region-by-quarter fixed effects. Individual control variables include age, race (Black Non-Hispanic, other Non-Hispanic, and Hispanic), education (less than high school, some college, bachelor's degree, advanced degree), metro area residency, MSA or CBSA size, marital status, number of children in the household and an indicator for at least one child under 13. Area COVID-19 controls include a continuous measure of cumulative cases and deaths per capita (per 1M residents) along with a control for school closures. Observations are weighted using Computer and Internet Use Supplement Weights or Computer and internet use supplement primary respondent weights as appropriate. Robust standard errors are clustered at the state-level. Significance at 1%, 5%, and 10% levels are indicated by ***, **, and *, respectively.

Table 8: Effects of State-Level Restrictions on Municipal Broadband on Labor Force Outcomes by Sub-Group

	(1) Labor Force Participation	(2) Employed, At Work	(3) Employed, Any	(4) Hours Last Week
Panel A: Married Women with Children				
Restrictions $\times$Post	-0.0268**	-0.0310**	-0.0305**	0.1789
	(0.0121)	(0.0132)	(0.0125)	(0.2067)
$Mean$	0.670	0.627	0.655	36.552
R^2	0.078	0.074	0.080	0.027
N	222,009	222,009	222,009	139,138
Panel B: Women without Children				
Restrictions $\times$Post	0.0041	0.0036	0.0041	-0.1340
	(0.0062)	(0.0063)	(0.0064)	(0.2083)
$Mean$	0.752	0.707	0.728	39.330
R^2	0.069	0.063	0.068	0.021
N	309,196	309,196	309,196	218,528
Panel C: Married Men with Children				
Restrictions $\times$Post	-0.0028	-0.0082	-0.0093*	-0.2065
	(0.0041)	(0.0050)	(0.0046)	(0.1561)
$Mean$	0.947	0.908	0.930	44.035
R^2	0.016	0.017	0.020	0.021
N	224,011	224,011	224,011	203,372

Notes: This table reports estimates from difference-in-differences models that compare workers in states with restrictions on municipal broadband to workers in states without restrictions before and after the COVID-19 pandemic. The outcome in column 4 is hours worked last week conditional on working at all. The treatment variable is a binary variable for persons (aged 25 to 54) living in states that had municipal restrictions as of Q1:2018. CPS data are for all months in 2018,19 (pre-COVID) and August 2021 to December 2022 (post-COVID). All models include state fixed effects, region-by-quarter fixed effects and Bartik shift-share variables. Individual control variables include age, race (Black Non-Hispanic, other Non-Hispanic, and Hispanic), education (less than high school, some college, bachelor's degree, advanced degree), metro area residency, MSA or CBSA size, marital status, number of children in the household and an indicator for at least one child under 13. Area COVID-19 controls include a continuous measure of cumulative cases and deaths per capita (per 1M residents) along with a control for school closures. Observations are weighted using CPS sample weights. Robust standard errors are clustered at the state-level. Significance at 1%, 5%, and 10% levels are indicated by ***, **, and *, respectively.

Table 9:
Effects of Municipal Restrictions on Labor Force Status (Family Reasons)

	(1) Part-Time	(2) Part-Time or Not Working, Family Reasons	(3) NILF, Taking Care of House or Family	(4) Part-Time, Child-Care Problems
Panel A: Married Women with Children				
Restrictions $\times$Post	-0.0089	0.0233**	0.0246**	-0.0010
	(0.0078)	(0.0115)	(0.0113)	(0.0027)
$Mean$	0.286	0.314	0.289	0.024
R^2	0.029	0.086	0.086	0.008
N	139,138	222,009	222,009	222,009
Panel B: Women without Children				
Restrictions $\times$Post	0.0013	-0.0068	-0.0066	-0.0002
	(0.0069)	(0.0044)	(0.0041)	(0.0005)
$Mean$	0.190	0.087	0.085	0.002
R^2	0.017	0.056	0.056	0.002
N	218,528	309,196	309,196	309,196
Panel C: Married Men with Children				
Restrictions $\times$Post	0.0066	0.0028	0.0025	0.0004
	(0.0044)	(0.0030)	(0.0027)	(0.0006)
$Mean$	0.089	0.020	0.017	0.003
R^2	0.009	0.005	0.005	0.002
N	203,372	224,011	224,011	224,011

Notes: "Part-Time" is defined as working less than 35 hours per week. "Part-Time or Not Working, Family Reasons" is an indicator for any the following: (i) Main reason not looking for work during last four weeks - "can't arrange childcare," (ii) Major activity (NILF) - "taking care of house or family," (iii) Reason for working part time last week - "childcare problems," or (iv) Reason for absence from work - "childcare problems." The treatment variable is a binary variable for persons (aged 25 to 54) living in states that had municipal restrictions as of Q1:2018. CPS data are for all months in 2018,19 (pre-COVID) and August 2021 to December 2022 (post-COVID). All models include state fixed effects, region-by-quarter fixed effects and Bartik shift-share variables. Individual control variables include age, race (Black Non-Hispanic, other Non-Hispanic, and Hispanic), education (less than high school, some college, bachelor's degree, advanced degree), metro area residency, MSA or CBSA size, marital status, number of children in the household and an indicator for at least one child under 13. Area COVID-19 controls include a continuous measure of cumulative cases and deaths per capita (per 1M residents) along with a control for school closures. Observations are weighted using CPS sample weights. Robust standard errors are clustered at the state-level. Significance at 1%, 5%, and 10% levels are indicated by ***, **, and *, respectively.

Table 10: Estimated Effects of Municipal Restrictions on Work-Related Time Use

	(1) WFH	(2) Share of Work from Home	(3) Commuting
Panel A: Married Women with Children			
Restrictions $\times$Post	-45.8648**	-0.1115**	6.6612*
	(20.2221)	(0.0533)	(3.9203)
$Mean$	72.148	0.226	25.484
R^2	0.208	0.222	0.134
N	1,408	1,182	1,408
Panel B: Women without Children			
Restrictions $\times$Post	16.5709	0.0573	5.0953
	(23.3363)	(0.0403)	(4.2571)
$Mean$	84.357	0.207	30.088
R^2	0.231	0.256	0.164
N	1,301	1,124	1,301
Panel C: Married Men with Children			
Restrictions $\times$Post	-26.9908	-0.0015	-6.3635
	(20.6934)	(0.0410)	(5.7215)
$Mean$	72.446	0.168	40.527
R^2	0.246	0.267	0.135
N	1,624	1,481	1,624

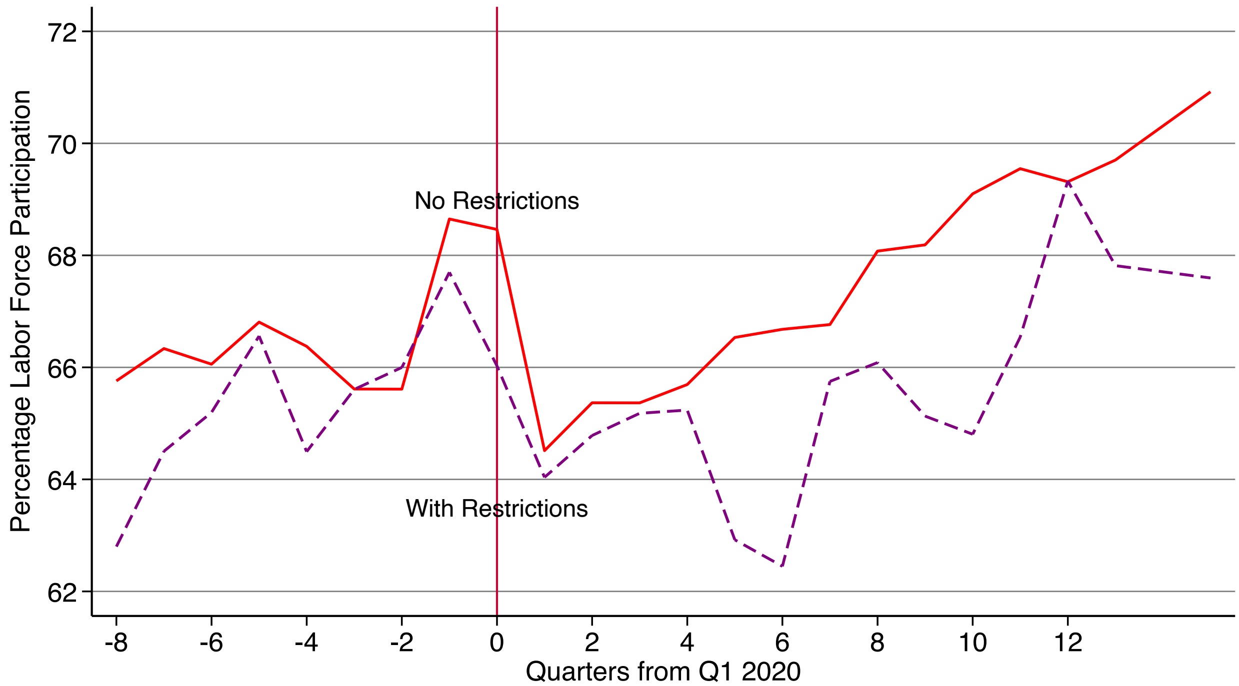
Notes: All outcomes are measured in minutes spent per workday, except for column 2 which is a share between 0 and 1. Column 1 is time spent working from home, column 2 is the share of work time from home (conditional on a positive amount of work), and column 3 is commute time. The sample includes employed people only. The treatment variable is a dummy for persons (aged 25 to 54) living in states with municipal restrictions as of Q1:2018. ATUS data are restricted to employed persons only for all months in 2017-19 (pre-COVID) and August 2021-December 2022 (post-COVID). All models include state fixed effects, region-by-quarter fixed effects and Bartik shift-share variables. Individual control variables include age, race (Black Non-Hispanic, other Non-Hispanic, and Hispanic), education (less than high school, some college, bachelor's degree, advanced degree), metro area residency, MSA or CBSA size, marital status, number of children in the household and an indicator for at least one child under 13. Area COVID-19 controls include a continuous measure of cumulative cases and deaths per capita (per 1M residents) along with a control for school closures. Observations are weighted using CPS sample weights. Robust standard errors are clustered at the state-level. Significance at 1%, 5%, and 10% levels are indicated by ***, **, and *, respectively.

Appendix Figures

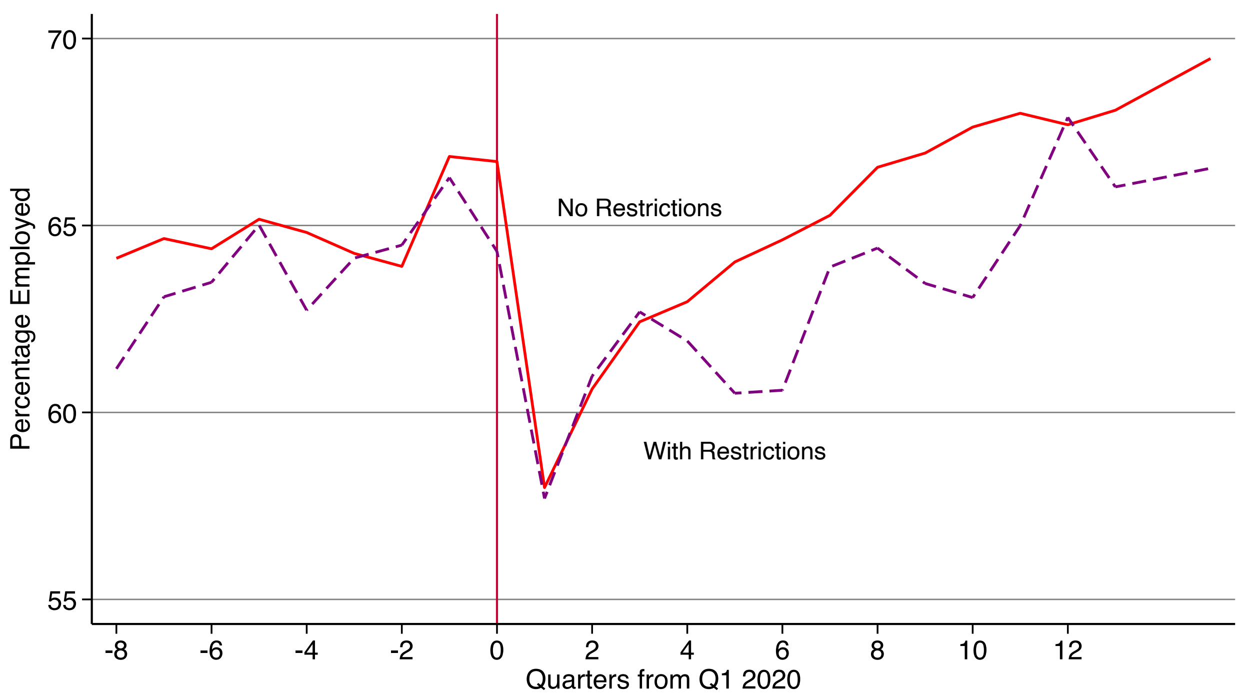
Appendix Tables

Figure A1: Trends in Labor Force Outcomes for Married Women with Children by State Municipal Broadband Restrictions, Unadjusted Data

(a) Labor Force Participation



(b) Employment

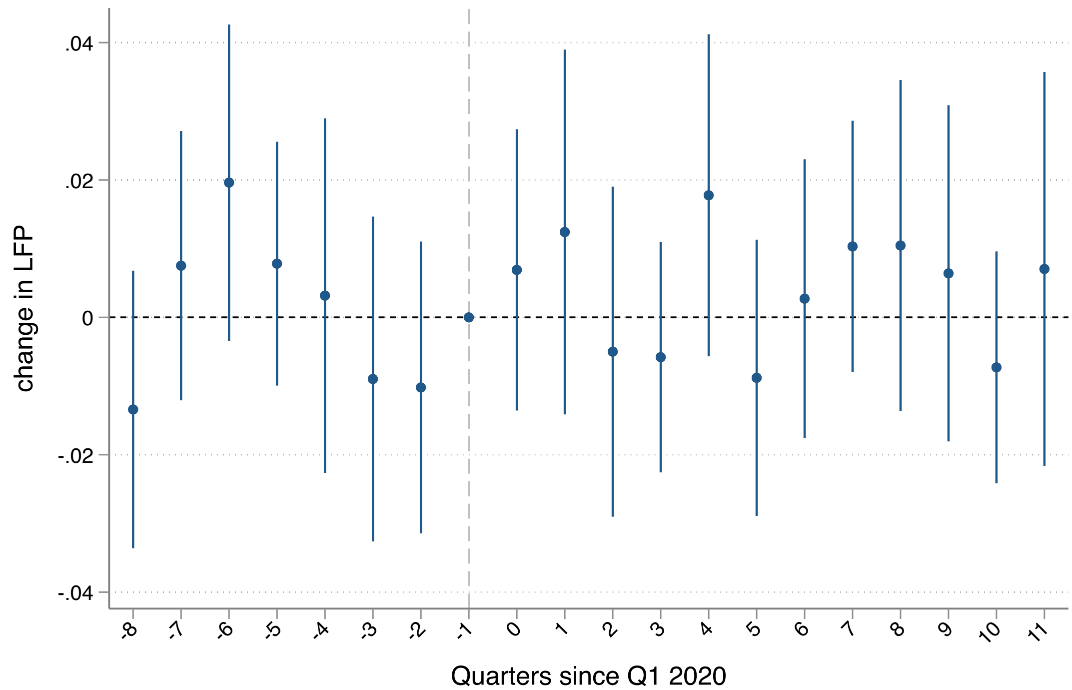


(c) Employed, At Work

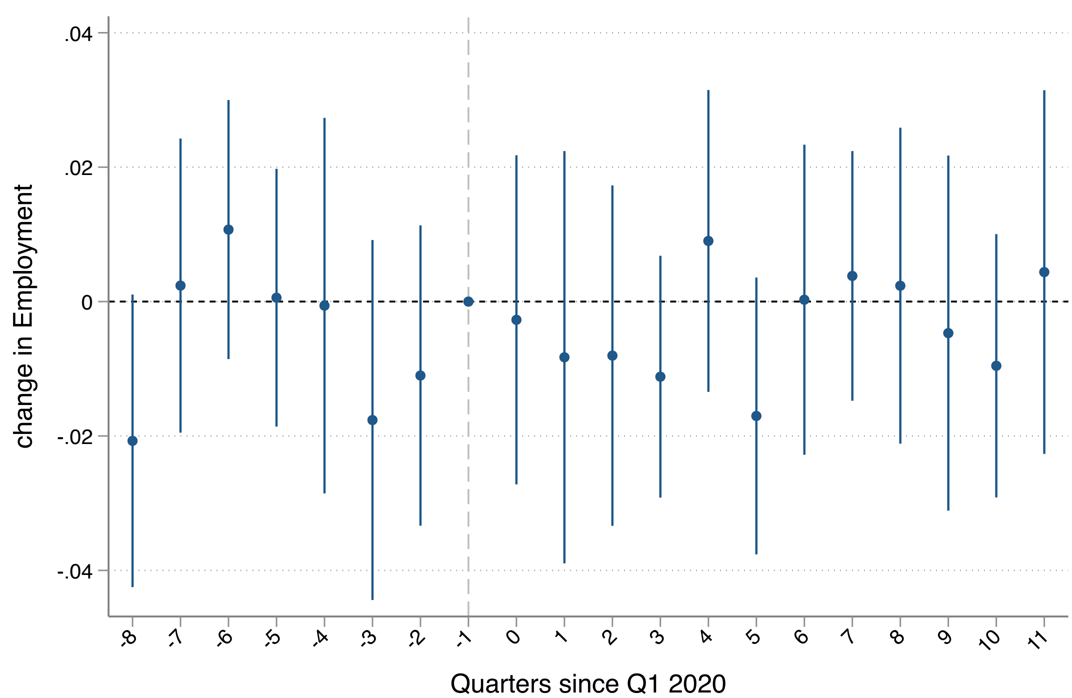
Notes: The variables are the percentage of married women with children in the labor force (Panel a), employed (Panel b) and employed, at work (Panel c) plotted by states with and without restrictions. Observations are weighted using CPS sample weights. Source:Monthly Current Population Survey, 2018–2022.

Figure A2: Event Study Estimates of Municipal Broadband Restrictions on Labor Force Outcomes for Women without Children

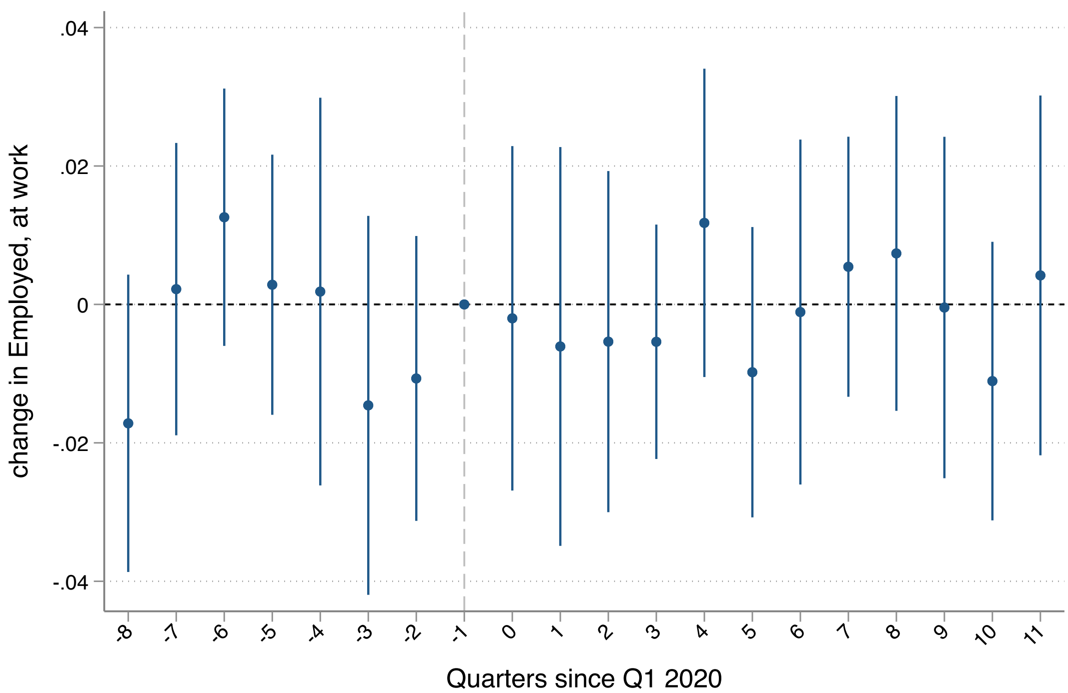
(a) Labor Force Participation



(b) Employment



(c) Employed, At Work

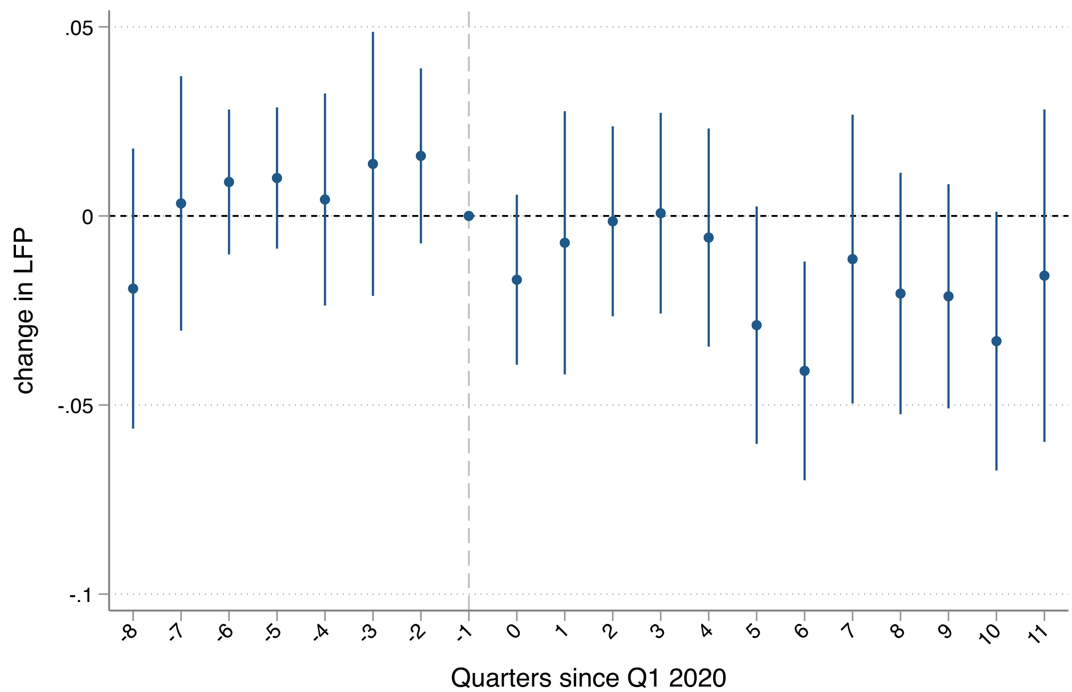


Notes: The dependent variables are a dummy for being in the labor force (Panel a), being employed (Panel b) and being employed, at work (Panel c) for prime-age women without children. All models include state fixed effects, region-by-quarter fixed effects and Bartik shift-share variables. Individual control variables include age, race (Black Non-Hispanic, other Non-Hispanic, and Hispanic), education (less than high school, some college, bachelor's degree, advanced degree), metro area residency, MSA or CBSA size, marital status, number of children in the household and an indicator for at least one child under 13. County COVID-19 controls include a continuous measure of cumulative cases and deaths per capita (per 1M residents) along with a control for school closures. Observations are weighted using CPS sample weights. Robust standard errors are clustered at the state-level. 95% confidence intervals are shown.

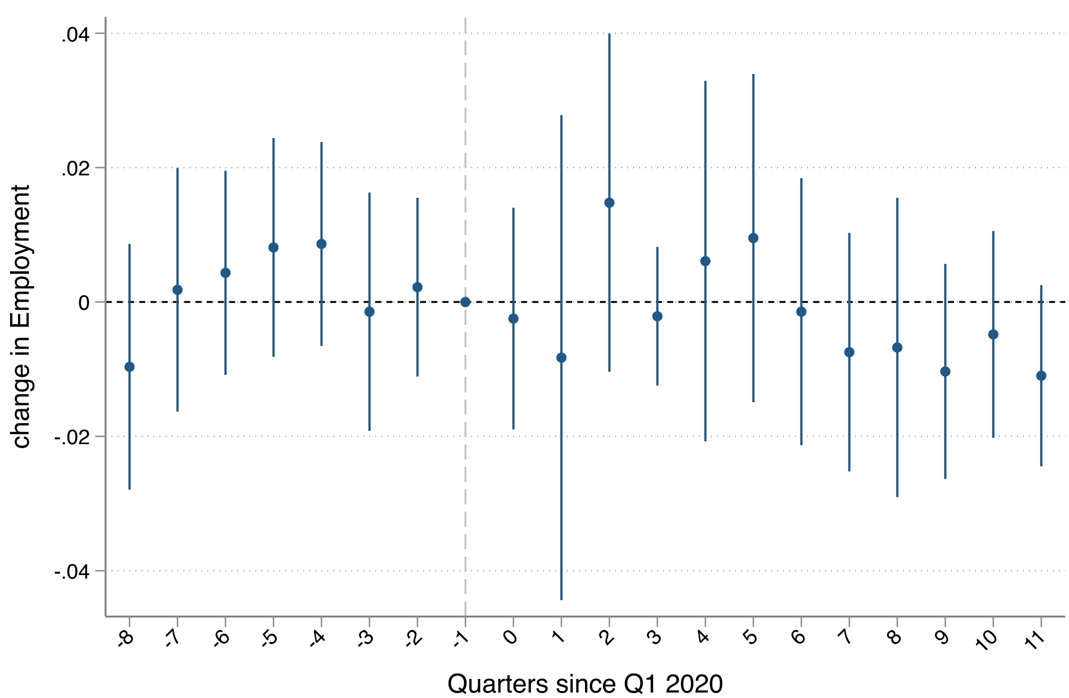
Source:Monthly Current Population Survey, 2018–2022.

Figure A3: Event Study Estimates of Municipal Broadband Restrictions on Labor Force Outcomes for Married Men with Children

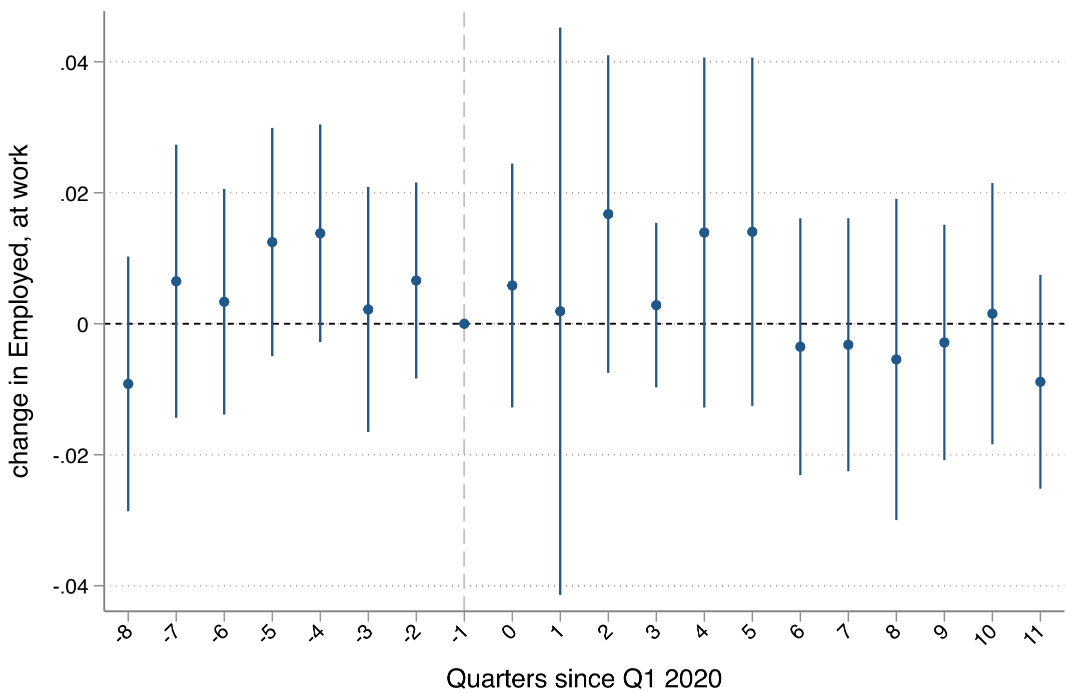
(a) Labor Force Participation



(b) Employment



(c) Employed, At Work



Notes: The dependent variables are a dummy for being in the labor force (Panel a), being employed (Panel b) and being employed, at work (Panel c) for prime-age married men with children. All models include state fixed effects, region-by-quarter fixed effects and Bartik shift-share variables. Individual control variables include age, race (Black Non-Hispanic, other Non-Hispanic, and Hispanic), education (less than high school, some college, bachelor's degree, advanced degree), metro area residency, MSA or CBSA size, marital status, number of children in the household and an indicator for at least one child under 13. County COVID-19 controls include a continuous measure of cumulative cases and deaths per capita (per 1M residents) along with a control for school closures. Observations are weighted using CPS sample weights. Robust standard errors are clustered at the state-level. 95% confidence intervals are shown.

Source:Monthly Current Population Survey, 2018–2022.

Table A1: Summary Statistics, Married Women with Children 2018–2019

	States without Restrictions	States with Restrictions
Labor Force Participation	0.664	0.654
	(0.472)	(0.476)
Employed, Any	0.648	0.638
	(0.478)	(0.481)
Employed, At Work	0.621	0.613
	(0.485)	(0.487)
Hours Worked Last Week	36.548	36.511
	(11.957)	(12.065)
Not in Labor Force, Family Reasons	0.293	0.306
	(0.455)	(0.461)
Part-Time/Not Working, Family Reasons	0.319	0.328
	(0.466)	(0.470)
Part-Time, Childcare Reasons	0.026	0.022
	(0.159)	(0.146)
Less than High-School	0.069	0.081
	(0.254)	(0.273)
High-School	0.544	0.571
	(0.498)	(0.495)
Some College	0.256	0.279
	(0.437)	(0.448)
College degree	0.301	0.292
	(0.459)	(0.455)
Advanced Degree	0.155	0.137
	(0.362)	(0.344)
Age	38.869	38.470
	(7.242)	(7.159)
Black	0.078	0.083
	(0.268)	(0.276)
White	0.631	0.610
	(0.483)	(0.488)
Hispanic	0.174	0.213
	(0.379)	(0.410)
Other Race	0.118	0.094
	(0.322)	(0.291)
Number of Own Children	2.173	2.201
	(1.048)	(1.077)
In Central City	0.228	0.227
	(0.419)	(0.419)
Teleworkable	0.141	0.139
	(0.348)	(0.346)
N	78,089	63,780

Notes: The table shows summary statistics for prime-age (25-54 years old) married women with children in states with (column 2) and without (column 1) restrictions on municipal broadband, measured prior to the COVID-19 pandemic in 2018-2019. Means with standard deviations reported in parentheses. Observations are weighted using CPS individual sample weights. Hours are estimated conditional on employment.
Source: CPS Monthly Estimates 2018–2019

Table A2:
Robustness Check 1– Impact of Municipal Restrictions (States with Municipal Broadband Initiatives) on Broadband Subscribership

	(1) 100Mbps 3 Categories Ologit	(2) 100Mbps 20% or More LPM	(3) 25Mbps 3 Categories Ologit	(4) 25Mbps 60% or More LPM	(5) 10Mbps 3 Categories Ologit	(6) 10Mbps 60% or More LPM
Restrictions x Post	-0.431***	-0.036***	-0.267***	-0.026**	-0.120	-0.045***
	(0.107)	(0.012)	(0.089)	(0.012)	(0.086)	(0.013)
R^2	0.525	0.754	0.560	0.700	0.575	0.719
N	26,256	26,256	26,256	26,256	26,256	26,256

Notes. All outcomes are county-level biannual values for broadband penetration (actual number of connections) between June 2018 and December 2022. In columns 1 and 2, broadband penetration is defined as a minimum download speed of 100Mbps, in columns 3 and 4 it is 25Mbps, in columns 5 and 6 it is 10Mbps. The dependent variable in columns 1, 3 and 5 is an ordinal value (0, 1 or 2) for FCC County broadband penetration reflecting actual connections per housing units in a given county (corresponding to 0 to 40%, 40 to 60% and over 60% subscribership respectively) and is regressed using an ordered logistic model (Ologit). For columns 2, 4 and 6, the dependent variable is a dummy that equals 1 if FCC County broadband penetration is 20% or higher, 60% or higher and 60% or higher respectively and is regressed using a Linear Probability Model (LPM). All specifications include time-varying log of county population, log population density, percent population with a bachelor's degree or higher, lagged log of county employment, lagged log of county household median income, percent population below poverty income, 2017 broadband penetration, year-month and state fixed effects. LPM models additionally include county fixed-effects. Standard errors are clustered at the county-level. Source: FCC Form 477 County Data on Internet Access Services and American Community Survey 5-Year Data (2018-2022).

Table A3:
Impact of Restrictions on FCC County Broadband Subscribership by Download Speeds – Complete Estimates

	(1) 100Mbps 3 Categories Ologit
Restrictions $\times Post$	-0.4552***
	(0.1022)
2017 BB=1	1.0293***
	(0.3386)
2017 BB=2	2.6436***
	(0.3566)
2017 BB=3	4.1916***
	(0.4010)
2017 BB=4	19.8759***
	(0.8100)
Population	0.5667
	(0.5914)
Employment	-0.0291
	(0.5868)
Density	0.6872***
	(0.0786)
Percentage College or More	0.0501***
	(0.0098)
Lagged Ln Median Income	1.3760***
	(0.4069)
Percent below Poverty Income	-0.0395***
	(0.0152)
State FE	Yes
Year-Month FE	Yes
$PseudoR^2$	0.524
Mean	0.433
N	28,592

Notes. This table includes complete regression coefficient estimates from column 1 of Table 4. The outcome is a county-level biannual value for broadband penetration (actual number of connections) between June 2018 and December 2022. In Column 1, broadband penetration is defined as a minimum download speed of 100Mbps. The dependent variable in column 1 is an ordinal value (0, 1 or 2) for FCC County broadband penetration reflecting actual connections per housing units in a given county (corresponding to 0 to 40%, 40 to 60% and over 60% coverage respectively) and is regressed using an ordered logistic model (Ologit). The specification includes time-varying log of county population, log density, percent population with a bachelor's degree or higher, lagged log employment, lagged log of household median income, percent population below poverty income, 2017 broadband penetration, year-month and state fixed effects. LPM models (not included in this table) additionally include county fixed-effects. Standard errors are clustered at the county-level. Source: FCC Form 477 County Data on Internet Access Services and American Community Survey 5-Year Data (2018-2022).

Table A4:
Effects of Municipal and/or Cooperative Restrictions on County Broadband Subscribership

	(1) 100Mbps 3 Categories Ologit	(2) 100Mbps 20% or More LPM	(3) 25Mbps 3 Categories Ologit	(4) 25Mbps 60% or More LPM	(5) 10Mbps 3 Categories Ologit	(6) 10Mbps 60% or More LPM
Muni Only x Post	-0.448***	-0.028**	-0.185*	-0.021	-0.014	-0.030**
	(0.113)	(0.013)	(0.095)	(0.013)	(0.093)	(0.014)
Muni+Coop x Post	-0.429***	-0.027	-0.308**	-0.041**	-0.007	-0.040**
	(0.141)	(0.017)	(0.123)	(0.017)	(0.120)	(0.018)
R^2	0.522	0.755	0.559	0.699	0.574	0.724
Mean	0.443	0.619	0.832	0.259	1.210	0.416
N	27,792	27,792	27,792	27,792	27,792	27,792

Notes. Note that Mississippi is excluded from this table as it is the only state with restrictions on cooperative broadband but not municipal broadband in 2018. All outcomes are county-level biannual values for broadband penetration (actual number of connections) between June 2018 and December 2022. In Columns 1 and 2, broadband penetration is defined as a minimum download speed of 100Mbps, in columns 3 and 4 it is 25Mbps, in columns 5 and 6 it is 10Mbps. The dependent variable in columns 1, 3 and 5 is an ordinal value (0, 1 or 2) for FCC County broadband penetration reflecting actual connections per housing units in a given county (corresponding to 0 to 40%, 40 to 60% and over 60% coverage respectively) and is regressed using an ordered logistic model (Ologit). For columns 2, 4 and 6, the dependent variable is a dummy that equals 1 if FCC County broadband penetration is 20% or higher, 60% or higher and 60% or higher respectively and is regressed using a Linear Probability Model (LPM). All specifications include time-varying log of county population, log density, percent population with a bachelor's degree or higher, lagged log employment, lagged log of household median income, percent population below poverty income, 2017 broadband penetration, year-month and state fixed effects. LPM models additionally include county fixed-effects. Standard errors are clustered at the county-level. Source: FCC Form 477 County Data on Internet Access Services and American Community Survey 5-Year Data (2018-2022).

Table A5:
Effects of Municipal Restrictions on Labor Force Participation

	(1) Married Women with Children	(2) Women without Children	(3) Married Men with Children
Municipal Restrictions	-0.0268**	0.0041	-0.0028
	(0.0121)	(0.0062)	(0.0041)
Age	0.0420***	0.0047***	0.0061***
	(0.0044)	(0.0016)	(0.0016)
Age-Squared	-0.0005***	-0.0001***	-0.0001***
	(0.0001)	(0.0000)	(0.0000)
High School	0.1085***	0.1850***	0.0137**
	(0.0115)	(0.0114)	(0.0068)
Some College	0.1852***	0.2877***	0.0268***
	(0.0100)	(0.0131)	(0.0068)
Bachelor's Degree	0.2050***	0.3364***	0.0526***
	(0.0116)	(0.0130)	(0.0071)
Advanced Degree	0.2699***	0.3469***	0.0543***
	(0.0123)	(0.0121)	(0.0074)
Child under 13	-0.0592***		0.0006
	(0.0072)		(0.0028)
COVID Rate (per 1,000)	0.0004**	0.0002***	-0.0000
	(0.0002)	(0.0001)	(0.0000)
COVID Death Rate (per 1,000)	-0.0092	-0.0064*	-0.0042
	(0.0072)	(0.0038)	(0.0029)
Share of School Closures	0.0576**	-0.0741**	-0.0229
	(0.0263)	(0.0308)	(0.0260)
Bartik Shift-Share	0.0161	-0.0081	-0.0079
	(0.0152)	(0.0062)	(0.0056)
State Fixed Effects	Yes	Yes	Yes
Region-Year-Quarter Fixed Effects	Yes	Yes	Yes
Other Indicators	Yes	Yes	Yes
R^2	0.078	0.069	0.016
N	222,009	309,196	224,011

Notes: This table reports estimates from difference-in-differences models that compare workers in states with restrictions on municipal broadband to workers in states without restrictions before and after the COVID-19 pandemic. The treatment variable is a binary variable for persons (aged 25 to 54) living in states that had municipal restrictions as of Q1:2018. CPS data are for all months in 2018,19 (pre-COVID) and August 2021 to December 2022 (post-COVID). Individual control variables in all models include age, age-squared, education (less than high school, some college, bachelor's degree, advanced degree) and an indicator for at least one child under 13. Other indicators include individual race (Black Non-Hispanic, other Non-Hispanic, and Hispanic), marital status, number of children in the household, metro area residency and a categorical variable (provided by the IPUMS CPS) for CBSA size. Area COVID-19 controls include a continuous measure of cumulative cases and deaths per capita (per 1M residents) along with a control for school closures. All models include state fixed effects, region-by-quarter fixed effects and Bartik shift-share variables. Observations are weighted using CPS sample weights. Robust standard errors are clustered at the state-level. Significance at 1%, 5%, and 10% levels are indicated by ***, **, and *, respectively.

Table A6:
Effects of Municipal Restrictions on Employed, At Work

	(1) Married Women with Children	(2) Women without Children	(3) Married Men with Children
Municipal Restrictions	-0.0310**	0.0036	-0.0082
	(0.0132)	(0.0063)	(0.0050)
Age	0.0491***	0.0044***	0.0099***
	(0.0047)	(0.0016)	(0.0019)
Age-Squared	-0.0006***	-0.0001***	-0.0001***
	(0.0001)	(0.0000)	(0.0000)
High School	0.1088***	0.1830***	0.0192**
	(0.0108)	(0.0114)	(0.0088)
Some College	0.1797***	0.2819***	0.0405***
	(0.0098)	(0.0130)	(0.0086)
Bachelor's Degree	0.2014***	0.3403***	0.0727***
	(0.0112)	(0.0132)	(0.0087)
Advanced Degree	0.2616***	0.3542***	0.0781***
	(0.0132)	(0.0120)	(0.0090)
Child under 13	-0.0628***		0.0009
	(0.0065)		(0.0029)
COVID Rate (per 1,000)	0.0004*	0.0002***	-0.0000
	(0.0002)	(0.0001)	(0.0000)
COVID Death Rate (per 1,000)	-0.0044	-0.0076*	-0.0058
	(0.0076)	(0.0044)	(0.0036)
Share of School Closures	0.0521	-0.0805**	-0.0158
	(0.0335)	(0.0299)	(0.0304)
Bartik Shift-Share	0.0083	-0.0083	-0.0074
	(0.0142)	(0.0050)	(0.0085)
State Fixed Effects	Yes	Yes	Yes
Region-Year-Quarter Fixed Effects	Yes	Yes	Yes
Other Indicators	Yes	Yes	Yes
R^2	0.074	0.063	0.017
N	222,009	309,196	224,011

Notes: This table reports estimates from difference-in-differences models that compare workers in states with restrictions on municipal broadband to workers in states without restrictions before and after the COVID-19 pandemic. The treatment variable is a binary variable for persons (aged 25 to 54) living in states that had municipal restrictions as of Q1:2018. CPS data are for all months in 2018,19 (pre-COVID) and August 2021 to December 2022 (post-COVID). Individual control variables in all models include age, age-squared, education (less than high school, some college, bachelor's degree, advanced degree) and an indicator for at least one child under 13. Other indicators include individual race (Black Non-Hispanic, other Non-Hispanic, and Hispanic), marital status, number of children in the household, metro area residency and a categorical variable (provided by the IPUMS CPS) for CBSA size. Area COVID-19 controls include a continuous measure of cumulative cases and deaths per capita (per 1M residents) along with a control for school closures. All models include state fixed effects, region-by-quarter fixed effects and Bartik shift-share variables. Observations are weighted using CPS sample weights. Robust standard errors are clustered at the state-level. Significance at 1%, 5%, and 10% levels are indicated by ***, **, and *, respectively.

Table A7:
Effects of Municipal Restrictions on Employment

	(1) Married Women with Children	(2) Women without Children	(3) Married Men with Children
Municipal Restrictions	-0.0305**	0.0041	-0.0093*
	(0.0125)	(0.0064)	(0.0046)
Age	0.0442***	0.0042**	0.0086***
	(0.0044)	(0.0016)	(0.0018)
Age-Squared	-0.0005***	-0.0001***	-0.0001***
	(0.0001)	(0.0000)	(0.0000)
High School	0.1086***	0.1856***	0.0193**
	(0.0111)	(0.0113)	(0.0086)
Some College	0.1888***	0.2900***	0.0402***
	(0.0100)	(0.0131)	(0.0088)
Bachelor's Degree	0.2131***	0.3475***	0.0700***
	(0.0115)	(0.0134)	(0.0087)
Advanced Degree	0.2797***	0.3611***	0.0744***
	(0.0126)	(0.0120)	(0.0089)
Child under 13	-0.0601***		0.0018
	(0.0071)		(0.0027)
COVID Rate (per 1,000)	0.0004**	0.0002***	-0.0000
	(0.0002)	(0.0001)	(0.0000)
COVID Death Rate (per 1,000)	-0.0082	-0.0092**	-0.0071*
	(0.0070)	(0.0042)	(0.0041)
Share of School Closures	0.0485*	-0.0764**	-0.0397
	(0.0272)	(0.0304)	(0.0326)
Bartik Shift-Share	0.0130	-0.0084	-0.0047
	(0.0158)	(0.0058)	(0.0063)
State Fixed Effects	Yes	Yes	Yes
Region-Year-Quarter Fixed Effects	Yes	Yes	Yes
Other Indicators	Yes	Yes	Yes
R^2	0.080	0.068	0.020
N	222,009	309,196	224,011

Notes: This table reports estimates from difference-in-differences models that compare workers in states with restrictions on municipal broadband to workers in states without restrictions before and after the COVID-19 pandemic. The treatment variable is a binary variable for persons (aged 25 to 54) living in states that had municipal restrictions as of Q1:2018. CPS data are for all months in 2018,19 (pre-COVID) and August 2021 to December 2022 (post-COVID). Individual control variables in all models include age, age-squared, education (less than high school, some college, bachelor's degree, advanced degree) and an indicator for at least one child under 13. Other indicators include individual race (Black Non-Hispanic, other Non-Hispanic, and Hispanic), marital status, number of children in the household, metro area residency and a categorical variable (provided by the IPUMS CPS) for CBSA size. Area COVID-19 controls include a continuous measure of cumulative cases and deaths per capita (per 1M residents) along with a control for school closures. All models include state fixed effects, region-by-quarter fixed effects and Bartik shift-share variables. Observations are weighted using CPS sample weights. Robust standard errors are clustered at the state-level. Significance at 1%, 5%, and 10% levels are indicated by ***, **, and *, respectively.

Table A8:
Effects of Municipal Restrictions on Conditional Hours Worked

	(1) Married Women with Children	(2) Women without Children	(3) Married Men with Children
Municipal Restrictions	0.1789	-0.1340	-0.2065
	(0.2067)	(0.2083)	(0.1561)
Age	0.5243***	0.4266***	0.0695
	(0.1351)	(0.0375)	(0.0768)
Age-Squared	-0.0059***	-0.0050***	-0.0006
	(0.0017)	(0.0004)	(0.0009)
High School	1.6564***	1.6862***	1.5665***
	(0.3925)	(0.2967)	(0.2703)
Some College	1.4098***	2.4808***	1.9407***
	(0.5163)	(0.3099)	(0.2977)
Bachelor's Degree	2.0257***	3.7614***	1.9901***
	(0.4924)	(0.2378)	(0.2904)
Advanced Degree	3.4444***	5.3804***	2.9950***
	(0.3661)	(0.2876)	(0.2990)
Child under 13	-1.3026***		-0.4235***
	(0.2204)		(0.1292)
COVID Rate (per 1,000)	-0.0005	0.0015	-0.0033*
	(0.0032)	(0.0021)	(0.0018)
COVID Death Rate (per 1,000)	0.0310	-0.1854	-0.0292
	(0.1799)	(0.1248)	(0.1322)
Share of School Closures	-2.4171**	-0.2506	-0.5360
	(1.0277)	(0.6041)	(0.7449)
Bartik Shift-Share	0.7151***	-0.0218	0.0368
	(0.2152)	(0.2637)	(0.1574)
State Fixed Effects	Yes	Yes	Yes
Region-Year-Quarter Fixed Effects	Yes	Yes	Yes
Other Indicators	Yes	Yes	Yes
R^2	0.027	0.021	0.021
N	139,138	218,528	203,372

Notes: This table reports estimates from difference-in-differences models that compare workers in states with restrictions on municipal broadband to workers in states without restrictions before and after the COVID-19 pandemic. The treatment variable is a binary variable for persons (aged 25 to 54) living in states that had municipal restrictions as of Q1:2018. CPS data are for all months in 2018,19 (pre-COVID) and August 2021 to December 2022 (post-COVID). Individual control variables in all models include age, age-squared, education (less than high school, some college, bachelor's degree, advanced degree) and an indicator for at least one child under 13. Other indicators include individual race (Black Non-Hispanic, other Non-Hispanic, and Hispanic), marital status, number of children in the household, metro area residency and a categorical variable (provided by the IPUMS CPS) for CBSA size. Area COVID-19 controls include a continuous measure of cumulative cases and deaths per capita (per 1M residents) along with a control for school closures. All models include state fixed effects, region-by-quarter fixed effects and Bartik shift-share variables. Observations are weighted using CPS sample weights. Robust standard errors are clustered at the state-level. Significance at 1%, 5%, and 10% levels are indicated by ***, **, and *, respectively.

Table A9: Presence/Absence of Spouse in Mediating Maternal Labor Supply Effects

	(1) Labor Force Participation	(2) Employed, At Work	(3) Employed, Any	(4) Hours Last Week
Restrictions x Spouse Present	-0.025**	-0.030**	-0.029**	0.198
	(0.012)	(0.013)	(0.012)	(0.212)
Restrictions x Spouse Absent	-0.078***	-0.070***	-0.064***	-1.033
	(0.020)	(0.018)	(0.017)	(1.063)
Spouse Absent	0.118***	0.095***	0.096***	0.739**
	(0.013)	(0.014)	(0.012)	(0.358)
$Mean$	0.670	0.627	0.655	36.552
R^2	0.078	0.074	0.080	0.027
N	222,009	222,009	222,009	139,138

Notes: Sample restricted to women with children. The outcome in column 4 is hours worked last week conditional on working at all. The treatment variable is a dummy for persons (aged 25 to 54) living in states with municipal restrictions as of Q1:2018. All models include state fixed effects, region-by-quarter fixed effects and Bartik shift-share variables. Individual control variables include age, race (Black Non-Hispanic, other Non-Hispanic, and Hispanic), metro area residency, MSA or CBSA size, marital status, number of children in the household and an indicator for at least one child under 13. Area COVID-19 controls include a continuous measure of cumulative cases and deaths per capita (per 1M residents) along with a control for school closures. Observations are weighted using CPS sample weights. Robust standard errors are clustered at the state-level. Significance at 1%, 5%, and 10% levels are indicated by ***, **, and *, respectively.

Table A10:
Effects of Municipal Restrictions on Labor Force Outcomes by Education

	(1) Labor Force Participation	(2) Employed, At Work	(3) Employed, Any	(4) Hours Last Week
Panel A: College-Educated Married Women with Children				
Restrictions $\times Post$	-0.0260**	-0.0268*	-0.0268**	0.1238
	(0.0128)	(0.0135)	(0.0131)	(0.3459)
$Mean$	0.734	0.688	0.723	37.127
R^2	0.051	0.049	0.050	0.026
N	100,219	100,219	100,219	68,976
Panel B: Non College-Educated Married Women with Children				
Restrictions $\times Post$	-0.0267*	-0.0332**	-0.0327**	0.2449
	(0.0143)	(0.0150)	(0.0148)	(0.3676)
$Mean$	0.619	0.576	0.600	35.987
R^2	0.062	0.059	0.061	0.028
N	121,790	121,790	121,790	70,162

*N*otes: This table reports estimates from difference-in-differences models that compare workers in states with restrictions on municipal broadband to workers in states without restrictions before and after the COVID-19 pandemic. The outcome in column 4 is hours worked last week conditional on working at all. The treatment variable is a binary variable for persons (aged 25 to 54) living in states that had municipal restrictions as of Q1:2018. CPS data are for all months in 2018,19 (pre-COVID) and 2020,21 (post-COVID). All models include state fixed effects, region-by-quarter fixed effects and Bartik shift-share variables. Individual control variables include age, race (Black Non-Hispanic, other Non-Hispanic, and Hispanic), education (less than high school, some college, bachelor's degree, advanced degree), metro area residency, MSA or CBSA size, marital status, number of children in the household and an indicator for at least one child under 13. Area COVID-19 controls include a continuous measure of cumulative cases and deaths per capita (per 1M residents) along with a control for school closures. The specification used for this table additionally includes area fixed effects. Observations are weighted using CPS sample weights. Robust standard errors are clustered at the state-level. Significance at 1%, 5%, and 10% levels are indicated by ***, **, and *, respectively.

Table A11:
Effects of Municipal Restrictions on Labor Force Outcomes by Race

	(1) Labor Force Participation	(2) Employed, At Work	(3) Employed, Any	(4) Hours Last Week
Panel A: White Married Women with Children				
Restrictions $\times Post$	-0.0257**	-0.0320**	-0.0300**	0.2058
	(0.0126)	(0.0137)	(0.0127)	(0.2593)
$Mean$	0.698	0.654	0.685	36.296
R^2	0.077	0.073	0.078	0.032
N	150,613	150,613	150,613	98,476
Panel B: Non-White Married Women with Children				
Restrictions $\times Post$	-0.0274*	-0.0285*	-0.0317*	0.0436
	(0.0149)	(0.0160)	(0.0158)	(0.3478)
$Mean$	0.613	0.570	0.593	37.174
R^2	0.066	0.063	0.067	0.027
N	71,396	71,396	71,396	40,662

Notes: This table reports estimates from difference-in-differences models that compare workers in states with restrictions on municipal broadband to workers in states without restrictions before and after the COVID-19 pandemic. The outcome in column 4 is hours worked last week conditional on working at all. The treatment variable is a binary variable for persons (aged 25 to 54) living in states that had municipal restrictions as of Q1:2018. CPS data are for all months in 2018,19 (pre-COVID) and 2020,21 (post-COVID). All models include state fixed effects, region-by-quarter fixed effects and Bartik shift-share variables. Individual control variables include age, race (Black Non-Hispanic, other Non-Hispanic, and Hispanic), education (less than high school, some college, bachelor's degree, advanced degree), metro area residency, MSA or CBSA size, marital status, number of children in the household and an indicator for at least one child under 13. Area COVID-19 controls include a continuous measure of cumulative cases and deaths per capita (per 1M residents) along with a control for school closures. The specification used for this table additionally includes area fixed effects. Observations are weighted using CPS sample weights. Robust standard errors are clustered at the state-level. Significance at 1%, 5%, and 10% levels are indicated by ***, **, and *, respectively.

Table A12:
Effects of Municipal Restrictions on Labor Force Outcomes by Age of the Youngest Child

	(1) Labor Force Participation	(2) Employed, At Work	(3) Employed, Any	(4) Hours Last Week
Panel A: Oldest Child Age 0–11				
Restrictions $\times Post$	-0.0264***	-0.0295***	-0.0308***	-0.0951
	(0.0074)	(0.0077)	(0.0075)	(0.2499)
$Mean$	0.655	0.604	0.640	35.966
R^2	0.085	0.077	0.087	0.032
N	110,649	110,649	110,649	66,874
Panel B: Youngest Child Age 12–17				
Restrictions $\times Post$	-0.0259	-0.0365**	-0.0335*	-0.3512
	(0.0169)	(0.0176)	(0.0179)	(0.5469)
$Mean$	0.742	0.706	0.727	37.955
R^2	0.056	0.052	0.056	0.022
N	55,642	55,642	55,642	39,281

Notes: This table reports estimates from difference-in-differences models that compare workers in states with restrictions on municipal broadband to workers in states without restrictions before and after the COVID-19 pandemic. The outcome in column 4 is hours worked last week conditional on working at all. The treatment variable is a binary variable for persons (aged 25 to 54) living in states that had municipal restrictions as of Q1:2018. CPS data are for all months in 2018,19 (pre-COVID) and 2020,21 (post-COVID). All models include state fixed effects, region-by-quarter fixed effects and Bartik shift-share variables. Individual control variables include age, race (Black Non-Hispanic, other Non-Hispanic, and Hispanic), education (less than high school, some college, bachelor's degree, advanced degree), metro area residency, MSA or CBSA size, marital status, number of children in the household and an indicator for at least one child under 13. Area COVID-19 controls include a continuous measure of cumulative cases and deaths per capita (per 1M residents) along with a control for school closures. The specification used for this table additionally includes area fixed effects. Observations are weighted using CPS sample weights. Robust standard errors are clustered at the state-level. Significance at 1%, 5%, and 10% levels are indicated by ***, **, and *, respectively.

Table A13:
Robustness Check - Effects on Labor Force Outcomes, Area Fixed Effects, All Months (2018-2022)

	(1) Labor Force Participation	(2) Employed, At Work	(3) Employed, Any	(4) Hours Last Week
Panel A: Married Women with Children				
Restrictions $\times$Post	-0.0188*	-0.0243**	-0.0216**	0.1492
	(0.0105)	(0.0105)	(0.0104)	(0.1637)
$Mean$	0.668	0.619	0.649	36.488
R^2	0.088	0.085	0.092	0.038
N	318,256	318,256	318,256	197,074
Panel B: Women without Children				
Restrictions $\times$Post	0.0054	0.0040	0.0033	-0.0611
	(0.0048)	(0.0047)	(0.0047)	(0.1822)
$Mean$	0.751	0.698	0.720	39.183
R^2	0.077	0.073	0.077	0.028
N	442,590	442,590	442,590	308,928
Panel C: Married Men with Children				
Restrictions $\times$Post	0.0026	0.0001	-0.0021	-0.0999
	(0.0043)	(0.0052)	(0.0047)	(0.0981)
$Mean$	0.945	0.899	0.923	43.810
R^2	0.027	0.033	0.036	0.031
N	320,575	320,575	320,575	288,279

*N*otes: This table reports estimates from difference-in-differences models that compare workers in states with restrictions on municipal broadband to workers in states without restrictions before and after the COVID-19 pandemic. The outcome in column 4 is hours worked last week conditional on working at all. The treatment variable is a binary variable for persons (aged 25 to 54) living in states that had municipal restrictions as of Q1:2018. CPS data are for all months in 2018,19 (pre-COVID) and 2020,21 (post-COVID). All models include state fixed effects, region-by-quarter fixed effects and Bartik shift-share variables. Individual control variables include age, race (Black Non-Hispanic, other Non-Hispanic, and Hispanic), education (less than high school, some college, bachelor's degree, advanced degree), metro area residency, MSA or CBSA size, marital status, number of children in the household and an indicator for at least one child under 13. Area COVID-19 controls include a continuous measure of cumulative cases and deaths per capita (per 1M residents) along with a control for school closures. The specification used for this table additionally includes area fixed effects. Observations are weighted using CPS sample weights. Robust standard errors are clustered at the state-level. Significance at 1%, 5%, and 10% levels are indicated by ***, **, and *, respectively.

Table A14:
Robustness Check - Effects of Municipal Restrictions (States with Municipal Broadband Initiatives) on Labor Force Outcomes

	(1) Labor Force Participation	(2) Employed, At Work	(3) Employed, Any	(4) Hours Last Week
Panel A: Married Women with Children				
Restrictions $\times Post$	-0.0290*	-0.0333*	-0.0322*	0.2215
	(0.0132)	(0.0144)	(0.0135)	(0.2086)
$Mean$	0.673	0.629	0.658	36.506
R^2	0.079	0.074	0.081	0.027
N	198,455	198,455	198,455	124,820
Panel B: Women without Children				
Restrictions $\times Post$	0.0050	0.0050	0.0062	-0.1125
	(0.0066)	(0.0067)	(0.0067)	(0.2198)
$Mean$	0.758	0.712	0.733	39.343
R^2	0.068	0.062	0.067	0.021
N	274,887	274,887	274,887	195,766
Panel C: Married Men with Children				
Restrictions $\times Post$	-0.0006	-0.0056	-0.0071	-0.2177
	(0.0042)	(0.0051)	(0.0048)	(0.1609)
$Mean$	0.948	0.909	0.932	43.991
R^2	0.015	0.017	0.020	0.021
N	200,616	200,616	200,616	182,311

Notes: This table reports estimates from difference-in-differences models that compare workers in states with restrictions on municipal broadband to workers in states without restrictions before and after the COVID-19 pandemic. The outcome in column 4 is hours worked last week conditional on working at all. The treatment variable is a binary variable for persons (aged 25 to 54) living in states that had municipal restrictions as of Q1:2018. CPS data are for all months in 2018,19 (pre-COVID) and August 2021 to December 2022 (post-COVID). All models include state fixed effects, region-by-quarter fixed effects and Bartik shift-share variables. Individual control variables include age, race (Black Non-Hispanic, other Non-Hispanic, and Hispanic), education (less than high school, some college, bachelor's degree, advanced degree), metro area residency, MSA or CBSA size, marital status, number of children in the household and an indicator for at least one child under 13. Area COVID-19 controls include a continuous measure of cumulative cases and deaths per capita (per 1M residents) along with a control for school closures. Observations are weighted using CPS sample weights. Robust standard errors are clustered at the state-level. Significance at 1%, 5%, and 10% levels are indicated by ***, **, and *, respectively.

Table A15:
Robustness Check - Effects of Municipal Restrictions (Teleworkable Jobs) on Labor Force Outcomes

	(1) Labor Force Participation	(2) Employed, At Work	(3) Employed, Any	(4) Hours Last Week
Panel A: Married Women with Children				
Restrictions $\times Post$	-0.0270	-0.0256	-0.0267	0.3493
	(0.0167)	(0.0151)	(0.0159)	(0.4326)
$Mean$	0.316	0.298	0.309	38.023
R^2	0.126	0.122	0.128	0.035
N	105,524	105,524	105,524	31,439
Panel B: Women without Children				
Restrictions $\times Post$	-0.0236***	-0.0227**	-0.0224**	0.0240
	(0.0078)	(0.0091)	(0.0086)	(0.2327)
$Mean$	0.395	0.374	0.383	40.584
R^2	0.142	0.135	0.139	0.026
N	124,499	124,499	124,499	46,537
Panel C: Married Men with Children				
Restrictions $\times Post$	-0.0142	-0.0189	-0.0216	0.0366
	(0.0127)	(0.0127)	(0.0137)	(0.3104)
$Mean$	0.826	0.798	0.816	44.727
R^2	0.125	0.116	0.127	0.029
N	66,106	66,106	66,106	52,761

Notes: This table reports estimates from difference-in-differences models that compare workers in states with restrictions on municipal broadband to workers in states without restrictions before and after the COVID-19 pandemic. The outcome in column 4 is hours worked last week conditional on working at all. The treatment variable is a binary variable for persons (aged 25 to 54) living in states that had municipal restrictions as of Q1:2018. CPS data are for all months in 2018,19 (pre-COVID) and August 2021 to December 2022 (post-COVID). All models include state fixed effects, region-by-quarter fixed effects and Bartik shift-share variables. Individual control variables include age, race (Black Non-Hispanic, other Non-Hispanic, and Hispanic), education (less than high school, some college, bachelor's degree, advanced degree), metro area residency, MSA or CBSA size, marital status, number of children in the household and an indicator for at least one child under 13. Area COVID-19 controls include a continuous measure of cumulative cases and deaths per capita (per 1M residents) along with a control for school closures. Observations are weighted using CPS sample weights. Robust standard errors are clustered at the state-level. Significance at 1%, 5%, and 10% levels are indicated by ***, **, and *, respectively.