INTRODUCTION

The COVID-19 pandemic has wrought a huge human and economic toll worldwide, and there remains a pressing need for evidence on what policy tools have been effective at preventing case rates from overwhelming healthcare resources. In this paper, we examine whether a policy of providing free, in-home Internet with unlimited data to lower-income households leads to fewer cases of COVID-19 amongst those households.

Starting in 2020, governments worldwide implemented a range of non-pharmaceutical interventions (NPIs) to attempt to limit COVID-19 transmission. Many of these policies took the form of direct restrictions on individual behavior—for instance, mask mandates, curfews, lockdowns, movement limitations, and stay-at-home orders—and have been demonstrated to be effective at promoting social distancing and reducing cases. Conversely, providing free, in-home WiFi to low-income individuals can be conceptualized not as a direct restriction on behavior, but as a change in incentives. By making safety information easier to access, and by making in-home leisure, employment, and education more attractive relative to substitute activities outside the home, unlimited in-home WiFi—especially for the least-connected households—may shift incentives in a way that helps reduce transmission risk. Compared to direct restrictions, incentive-shifting strategies as a policy tool have received limited research attention but may be important, as many of consumers' pandemic responses have been driven not by lockdowns but by their own voluntary choices (Goolsbee & Syverson, 2021).

We analyze a pilot program initiated in Toronto, Canada during the COVID-19 pandemic that saw free WiFi extended to four large clusters of low-income apartment blocks. The program was an ambitious undertaking requiring collaboration between the municipal government, private telecommunication companies, and apartment building owners and managers that saw free, in-home digital access provided to thousands of low-income families. We combine information on the dates of the rollout of free WiFi in each of the four apartment clusters with COVID-19 case information at the neighborhood level. The research design is a difference-in-differences analysis, where control neighborhoods are selected based on a propensity score approach.
using demographic and housing stock variables of neighborhoods as input features, and lagged cases and vaccinations are allowed to affect current cases. Importantly for identification, because Toronto comprises a single geographic decision-making unit for COVID-19 policy, all neighborhoods are subject to the same COVID-19 government regulations at the same times.

We find that free WiFi leads to 0.22 fewer cases per 1000 residents per week in the neighborhoods in which it is implemented, an approximately 14.4% decrease. The negative coefficient is not driven by negative weights in the two-way fixed effect estimator (De Chaisemartin & d’Haultfoeuille, 2020) and is robust to different sample selection rules as well as an alternative estimation approach using synthetic controls (Abadie, 2021). We further show that reductions in cases are concentrated amongst individuals younger than 60, which is in line with expectations about which age groups are most likely to take advantage of increased Internet access. Finally, using vaccination data, cellphone mobility data, and WiFi usage statistics, we provide suggestive evidence that the main benefit of WiFi is to provide access to online, in-home leisure activities including social media and video streaming that can substitute for leisure activities outside the home.

To the best of our knowledge, this paper provides the first causal evidence on how Internet access benefited lower-income households during the COVID-19 pandemic. There is substantial evidence that the economic and health effects of COVID-19 have fallen disproportionately on lower-income households (Adams-Prassl et al., 2020; Alstadsæter et al., 2020; Brodeur et al., 2021), and Chiou and Tucker (2020) find that a lack of Internet access may have kept lower-income households from being able to social distance in the first months of the pandemic. We show that Internet access can help lower-income groups avoid exposure to COVID-19 by providing in-home alternative leisure activities, a mechanism which echoes Goldfarb and Prince (2008), who show that lower-income individuals spend more leisure time online than higher-income individuals (conditional on having Internet access.) This paper’s finding that changing household incentives can explain social distancing is consistent with Goolsbee and Syverson (2021), who find that most of the drop in visits to businesses at the start of the pandemic is due to voluntary changes in consumer behavior. Chernozhukov et al. (2021) find that private behavioral responses played a large role in reducing case growth rates in the US early in the pandemic.

The remainder of the paper is structured as follows: we provide background information on the WiFi pilot program in Section 2 and describe the data used in Section 3. Modeling and identification is covered in Section 4. Section 5 describes the main results and robustness checks, Section 6 uses vaccination and mobility data to explore the mechanism, Section 7 provides back-of-the-envelope calculations for the benefits of the WiFi program, and Section 8 concludes.

2 FREE WIFI PILOT PROGRAM

On April 28, 2020, the City of Toronto announced that in order to help vulnerable populations stay connected during COVID-19, they would partner with private telecommunications firms to provide 25 large residential apartment buildings in low-income neighborhoods with free in-home WiFi. Free WiFi was ultimately implemented in four clusters of high-rise apartments, consisting of 18 buildings in total, with the first installation completed in July 2020 and the rest completed by late March 2021.

The apartment clusters were chosen from a large candidate pool of low-income apartment blocks in early 2020. The rationale behind why the four specific clusters were picked was based on technical and administrative requirements, and they were not chosen based on COVID-19 trends or anticipated efficacy in reducing COVID-19 transmission. In particular, buildings were required to have at least 40% or more households earning less than half of the median adjusted household after tax income (LIM-AT), to have a significant level of need for Internet access based on consultation with the Social Development Finance and Administration Community Cluster Coordination team, and to have geographic representation across Toronto. Public housing units owned by the Toronto Community Housing Corporation were not considered as candidates for the program, since those residents would have qualified for low-cost Internet from a private supplier. Not all selected apartment clusters had WiFi installed, as a number of building owners refused to participate, or building characteristics were deemed unsuitable for installation of the equipment.

Figure 1 maps which neighborhoods contain apartment buildings that received free WiFi, and how many buildings in each neighborhood received the WiFi. In the City of Toronto data, neighborhoods are well-defined geographic units; we discuss their features in Section 3. In Table A.1 in Appendix A, we provide the date the WiFi is fully installed in each apartment cluster, and each apartment cluster’s share of the total population in that neighborhood. The WiFi was brought online cluster-wide as opposed to apartment-by-apartment, since blind spots in some apartments were covered by equipment broadcasting WiFi signals from other apartments in the same cluster.

Residents were made aware of the new Internet option with a door-to-door campaign in the apartment buildings that received WiFi, primarily through the distribution of flyers. Users could expect a download speed of up to 15 Mb/s with no data caps in place, although the actual experienced speed would depend on the number of concurrent users. The free WiFi option...
was not optimized for streaming video, and while these services were not blocked, residents could expect lower quality (480p or less) while streaming video.⁶

3 | DATA

Deployment data for the free WiFi pilot project was provided by the City of Toronto Digital Canopy team. We observe the addresses of apartment buildings where WiFi was installed and the date it was installed for each building, as well as the addresses of apartments that were considered but did not have WiFi hooked up, henceforth referred to as candidate apartments. The addresses of specific apartments that received the WiFi are confidential, but in Figure 1, we show the aggregate number of apartment buildings in which WiFi was deployed by Spring of 2021 for each neighborhood.

The main outcome variable is weekly cases of COVID-19 per 1000 residents in each Toronto neighborhood. We source daily individual cases from Toronto Public Health and sum within each neighborhood-week, then divide by each neighborhood’s population and multiply by 1000.⁷ Case data begins in early 2020, and is gathered through the end of September 2021.

We also source information on weekly vaccinations at the neighborhood level from the City of Toronto.⁸ Data is provided as the weekly cumulative share of the population in each neighborhood that have two vaccinations, which we first convert to the raw numbers of newly vaccinated individuals each week by differencing successive weeks and multiplying by neighborhood populations, and then convert to weekly new completed vaccinations per 1000 residents.

We gather demographic information at the neighborhood level using the Statistics Canada 2016 Census of Population. There are 140 neighborhoods in Toronto, with an average population of 19,500 people each. The neighborhood-level demographic information are used in a propensity score matching model to create a set of matched control neighborhoods that are similar to the neighborhoods containing apartments that receive WiFi.

Lastly, we use cellphone mobility data from the Patterns dataset provided by SafeGraph as an auxiliary outcome variable.⁹ For a set of approximately 60,000 Toronto businesses, we observe the number of visitors from each Aggregate Dissemination Area (ADA) in Toronto to that business every week, where visitors correspond to mobile devices in the SafeGraph panel. We describe more how we use this data in the mechanism check in Section 6. Details about which cellphones are part of the SafeGraph panel and potential biases in the cellphone data are available in Goolsbee and Syverson (2021) and Chiou and Tucker (2020), and exactly how the home ADA of a mobile device is computed is available in Chiou and Tucker (2020).
3.1 Matched sample and visual evidence

Our main analysis compares weekly COVID-19 cases and vaccinations for the four neighborhoods that receive free WiFi with four control neighborhoods drawn from the pool of the 136 neighborhoods in Toronto that did not receive free WiFi. We use a logit propensity score matching model to select these neighborhoods, using neighborhood-level demographic and housing stock variables as the input features. Our demographics include income level, household size and composition, the age distribution, and average education, which all play key roles in the propensity to have Internet (Chiou & Tucker, 2020), as well as neighborhood density, which is correlated with movement reductions experienced during COVID-19 (Chan, 2020). Matching is done to improve causal identification, which we discuss more when we present the model in Section 4.

In Table 1, we provide average values of the demographic variables used for matching, as well as weekly average COVID-19 case and vaccine rates, for the four treated neighborhoods, the four matched control neighborhoods, the two candidate control neighborhoods, and the unmatched set of all 136 untreated Toronto neighborhoods. Treated and matched control neighborhoods tend to have lower average incomes and a higher poverty rate compared to the average untreated neighborhood in Toronto, and roughly double the number of weekly COVID-19 cases and 30% more weekly vaccinations.

Figure 2 provides visual evidence of the effect of the WiFi deployment on the weekly case rates in treated neighborhoods compared to the matched control neighborhoods. COVID-19 cases per thousand residents move similarly for treatment and control neighborhoods prior to the WiFi introduction, with a higher average value for treated neighborhoods. After the WiFi introduction there is a small but persistent relative decrease in weekly cases for treated neighborhoods.

Table 1, Figure 2, and the baseline analysis use a trimmed version of the matched sample, where each treated neighborhood is trimmed in a bandwidth of 6 months around the rollout of the WiFi in that neighborhood. Control neighborhoods are also trimmed, based on the WiFi rollout date of the treated neighborhood to which their propensity score is closest in absolute distance. In robustness checks in Section 5, we consider different sample trimming rules, including different time windows.
around the WiFi rollout and alternative controls, including synthetic controls constructed from the pool of 136 untreated neighborhoods.

4 | MODELING

4.1 | Difference-in-differences specification

We use the following specification to model weekly cases per 1000 residents in each neighborhood in the data:

$$Y_{it} = \beta X_{it} + \delta' Z_{it} + \alpha_i + \alpha_t + \epsilon_{it}.$$  \hspace{1cm} (1)

$Y_{it}$ is the number of cases per 1000 residents in neighborhood $i$ in week $t$. $\alpha_i$ and $\alpha_t$ are neighborhood and time level fixed effects. The neighborhood level variable $X_{it}$ is a Bernoulli variable that equals one once a cluster of apartment buildings in neighborhood $i$ has received the free WiFi and equals zero otherwise. Each neighborhood has at most one cluster of apartments that receives WiFi, and once it is adopted it is never removed. $\beta$ is the primary coefficient of interest, and its estimate is expected to be negative if the rollout of free WiFi reduces the spread of COVID-19.

The evolution of cases in a neighborhood may be related to past values of cases in that neighborhood as well as the number of completed vaccinations. We include up to three weeks of lagged cases per 1000 residents and lagged completed vaccinations per 1000 residents in the vector of control variables, $Z_{it}$. This specification allows the cases at the end of a 4 week rolling window to depend directly on all weekly lags of cases and vaccinations within that window and is chosen to avoid omitting important lags.\hspace{1cm} We test specifications that introduce lags one at a time in Appendix B.1.

4.2 | Identification

Under the assumption of a homogeneous treatment effect and parallel trends, the coefficient on $X_{it}$ in Equation (1) can be consistently estimated using a two-way fixed effect (TWFE) regression. Figure 2 suggests that the propensity score matching procedure succeeded in selecting a group of control neighborhoods with parallel pre-trends. Moreover, given the large dispersal of neighborhoods throughout the city (Figure 1) and the substantial variation in treatment timing (Table A.1, Appendix A), any geographically or temporally localized violation of the parallel trends assumption would not affect all treated units. Since all neighborhoods are located within the boundaries of the City of Toronto, they are all subject to the exact same time-varying COVID-19 policy shocks and restrictions during every week of the pandemic, which will be absorbed by $\alpha_t$.\hspace{1cm}

F I G U R E 2  Graphical evidence of the effect of free WiFi. The solid lines represent the 2 months (8 weeks) moving averages of the dotted lines, which are the raw weekly averages for the treated and control groups. The data is truncated to 24 weeks before and after the WiFi adoption in each neighborhood. Control neighborhood time periods are centered based on the WiFi installation week of the treated neighborhood to which their propensity score is closest in absolute distance.
Recent work that relaxes the assumption of a homogeneous treatment effect shows that the TWFE estimator can be inconsistent, and even the incorrect sign, as it is a weighted sum of heterogeneous treatment effects with potentially negative weights (Callaway & Sant’anna, 2021; De Chaisemartin & d’Haultfoeuille, 2020; Goodman-Bacon, 2021). As recommended in De Chaisemartin and d’Haultfoeuille (2020), we check for the prevalence of negative weights on any of the heterogeneous treatment effects using the TwoWayFEWeights package in R. We find that all weights are positive, so that the standard TWFE is an appropriate estimator of the average treatment effect.

Work on the physical infrastructure for the WiFi may have paused during periods of high case rates and resumed after cases began to fall, which could present an identification issue if stoppages were tied to case rates in the specific neighborhoods that were being worked in. The City of Toronto staff responsible for the rollout confirmed that COVID-19-induced work stoppages did occur; however, the choice to delay was based on city-wide safety guidance and not on cases in specific neighborhoods. Since $\alpha_t$ absorbs the aggregate trend in cases, delaying based on city-wide trends will not impede identification of $\beta$, unless treated neighborhoods are themselves driving city-wide trends. We find that in weeks where there were at least 100 cases city-wide (90% of weeks from March 2020 through September 2021), the four treated neighborhoods together accounted for at most 7.4% of cases, with a median share of 3.8%.

5 | RESULTS

5.1 | Main findings

We present results from estimating Equation (1) in Table 2. We begin with the diff-in-diff with no covariates before moving to a specification that controls for lagged cases, and finally to the preferred specification in Column (3) that controls for lagged cases and lagged completed vaccinations. The regression error is allowed to exhibit arbitrary within-neighborhood correlation (Abadie et al., 2017; Bertrand et al., 2004), which is implemented using a wild cluster bootstrap given the small number of neighborhoods in the matched sample (Cameron & Miller, 2015).

Estimates from the full specification in Column (3) of Table 2 indicate that the WiFi rollout reduces COVID-19 cases by 0.22 per thousand residents per week in a treated neighborhood. From Table 1, there are 1.53 cases per 1000 residents per week on average in treated neighborhoods during the sample period; the introduction of WiFi thus leads to a 14.4% percent decrease in cases. In comparison, the introduction of mask mandates in Canadian provinces was estimated to lead to a 22% reduction in COVID-19 cases (Karaivanov, Lu, et al., 2021). To check the validity of the findings and provide nuance to this number, we conduct several robustness checks in Section 5.4; depending on the specification the percent reduction in cases is estimated between 7.2% and 22.7%.

| Dependent variable | Cases per 1000$_{it}$ | (1) | (2) | (3) |
|--------------------|----------------------|-----|-----|-----|
| $X_{it}$           | −0.503** (0.206)     | −0.224** (0.105) | −0.220** (0.105) |
| Cases per 1000$_{it-1}$ | 0.588*** (0.058)     | 0.587*** (0.057) |
| Cases per 1000$_{it-2}$ | 0.007 (0.059)       | 0.005 (0.059) |
| Cases per 1000$_{it-3}$ | 0.016 (0.041)       | 0.016 (0.041) |
| Completed vaccinations per 1000$_{it-1}$ | 0.004 (0.003)     | 0.002 (0.002) |
| Completed vaccinations per 1000$_{it-2}$ | 0.002 (0.002)       | 0.002 (0.002) |
| Completed vaccinations per 1000$_{it-3}$ | 0.002 (0.002)       | 0.002 (0.002) |
| Neighborhood FE    | ✓                    | ✓   | ✓   | ✓   |
| Week FE            | ✓                    | ✓   | ✓   | ✓   |
| Observations       | 384                  | 384 | 384 |
| $R^2$              | 0.832                | 0.893 | 0.893 |

Note: Observations are at the neighborhood-week level. $X_t$ equals one for neighborhoods that have received the free WiFi in all weeks after they receive it, and zero otherwise. Completed vaccinations are two doses of AstraZeneca, or any combination of two doses of Pfizer and Moderna. Standard errors are clustered at the neighborhood level.

*p < 0.1; **p < 0.05; ***p < 0.01.
With respect to control variables, we find that only the first week of lagged cases matters—one additional extra case per 1000 people in the prior week leads to 0.587 more cases per 1000 in the current week in Specification (3). In theory, higher cases in a prior week could lead to lower cases in the current week as individuals switch to safer behaviors in the presence of information on heightened risk, which Karaivanov, Lu, et al. (2021) find with respect to case growth rates. In our setting, case information at the fine-grained neighborhood level may be less accessible, and lower-income apartment-dwelling households may be less able to respond by restricting movement, even conditional on having case information (Papanastasiou et al., 2022).

The effect of recently completed vaccinations is estimated as small and insignificant for all three lags. Although completed vaccinations are effective against preventing severe illness due to COVID-19 (Andrews et al., 2022; Grannis et al., 2021), their take-up is highly endogenous to the number of cases. The direction of this endogeneity will tend to bias the expected negative coefficient on vaccines toward zero.

5.2 WiFi usage and treatment effect heterogeneity by age group

For the free WiFi program to affect COVID-19 transmission, there must be an effect in treated apartments on Internet connectivity and usage. As discussed in Section 2, apartments were specifically selected by Digital Canopy based on a perceived need for affordable Internet. Although we do not have information on Internet usage pre-WiFi, we present two pieces of evidence related to the take-up of WiFi in the treated apartments.

First, we have access to weekly snapshots of Internet usage for a subset of the cluster at Rockcliffe-Smythe. We plot the trend in the number of devices connecting in Figure 3. Each week, on average about 426 unique devices connected from three apartment buildings comprising approximately 490 apartment units. Consumption varies across devices but averages about 0.66 of a gigabyte of usage per device per week, while uploads were 0.1 gigabyte per device per week (see Figures D.1 and D.2, Appendix D.) Approximately 69% of devices connected to the WiFi in the Rockcliffe-Smythe data are smartphones, with two-thirds being lower priced Android devices. We also have aggregate data on what content types accounted for most of households’ consumed bandwidth, which we discuss in the mechanism section.

Second, if usage of WiFi is helping reduce cases, then we would expect case reductions to be larger amongst sub-populations that are more likely to use the WiFi. Since younger individuals are more likely to own and use an internet-capable device, we use the patient age categories reported for each COVID-19 case to test whether there is a larger reduction in cases among younger individuals in a neighborhood after the WiFi deployment. We aggregate weekly cases in each neighborhood into three age categories: 29 years or younger, 30–59 years old, and 60 years or older. We then normalize the aggregate cases for an age category by the number of individuals in that neighborhood in that age category, and run Equation (1) using the age-specific dependent variables. Results in Table 3 show that younger individuals (less than 59 years old) benefit from the WiFi rollout, and older individuals (60 years or older) do not. Since we only observe a case response among individuals who are likely to use the WiFi, we take this finding as indirect evidence of treatment take-up.

**FIGURE 3** Trends in WiFi Usage for 3 Apartments in Rockcliffe-Smythe. Data covers August 31, 2020 through October 3, 2021. Data is missing for 2 weeks in late December and early January
Given that the WiFi treatment does not affect the entire neighborhood, the baseline estimates represent intent-to-treat (ITT) effects. We can compute the average effect of treatment-on-the-treated (TOT) as the ITT divided by the population share who received treatment, under the assumptions that (1) the treated share would have been the same in control neighborhoods as treated neighborhoods and (2) outcomes respond to treatment, not treatment assignment (excludability).

From Table A.1, on average the population share of treated apartments is 0.17 in their respective neighborhoods, leading to a TOT of $-1.29$ cases per 1000 per week. Using Table A.2 in Appendix A, we estimate 2.57 cases per week per 1000 residents for the subpopulation of households living in the designated apartment blocks during the sample period, implying a 50.2% reduction from treatment. Our most conservative estimate of the ITT from robustness checks in Section 5.4 is $-0.11$, implying a reduction in COVID-19 cases in the treated population of 25.1%. To benchmark these numbers, Karaivanov, Kim, et al. (2021) estimates that 17%–27% of individuals start wearing face-coverings more frequently due to province-level mask mandates, implying an 81% or larger TOT effect from their ITT estimate of 22%. Having access to WiFi thus seems to be substantially less effective in reducing case rates compared to wearing a mask, although the complier populations likely differ between our study and theirs.

In our setting—and indeed, for most COVID-19 policies—the excludability assumption required to convert the ITT to the TOT is likely to be violated. Treatment assignment may matter for outcomes, since an untreated individual in a treatment neighborhood will benefit from a lower chance of contracting the virus if the free WiFi helps their treated neighbor avoid risky behaviors. While the aggregated data we have cannot separately identify the benefit to untreated individuals in the same neighborhood, our ITT estimate can be understood as an overall general equilibrium effect for the neighborhood. Even with this caveat, we acknowledge that the baseline estimate of the reduction in cases for treated individuals is high. We thus emphasize that the magnitude of our results should be interpreted with caution.

### Robustness checks

#### Different lag lengths for cases and vaccines

We experiment with between one and four weekly lags of neighborhood case rates and completed vaccinations. Our treatment coefficient reported in Table B.1, Appendix B.1 is very stable and similar to the baseline across all specifications.
5.4.2 | Different time window trimming

The baseline regression trims the sample to be 6 months around the rollout date of the WiFi in each neighborhood. We experiment with trimming the sample using 2, 4, 6, 8, and 10 months windows around the WiFi rollout, and report the results in Table B.2, Appendix B.2. The treatment effect is negative in all cases, is largest for the shortest time window, and has $p < 0.1$ or lower on the treatment coefficient in all specifications. This exercise suggests that it is not the specific choice of time window around the WiFi rollout that is leading to the negative estimate, and indeed that the coefficient is not driven exclusively by reductions in cases many weeks out from the WiFi adoption date. The regressions in this robustness check imply weekly reductions in cases between 7.2% and 22.7% for neighborhoods that receive free WiFi.

5.4.3 | Candidate neighborhoods

We also estimate Equation (1) using candidate neighborhoods as the control group instead of matched control neighborhoods. Estimates reported in Table B.3, Appendix B.3 are negative but insignificant. The treatment implies an 8.0% reduction in weekly cases in the specification with all control variables included.

5.4.4 | Log variables

We use the log of the count of cases plus one instead of the rate of cases per 1000 as an alternative dependent variable. Results reported in Table B.4, Appendix B.4 are negative and significant ($p < 0.01$), and the coefficient on $X_i$ can be directly interpreted as a 21.6% percent reduction in weekly cases relative to the baseline.

5.4.5 | Synthetic control

As an alternative to the propensity-score matched difference-in-differences framework, we use a synthetic control approach (Abadie, 2021). Synthetic controls are especially useful for estimation and inference when the number of treated units is small, but the time dimension is long, as we have here. We implement the Generalized Synthetic Control method from Xu (2017) using the gsynth package in R to allow for multiple treated units and varying treatment times. Briefly, in this approach, synthetic control neighborhoods are constructed by projecting the pre-treatment patterns in treated neighborhood cases onto an underlying set of linearly additive time-varying factors, which are themselves estimated using the entire set of control neighborhoods and time periods. We plot the average treated and counterfactual Cases per 1000 in Figure 4, and find that pre-treatment trends

![Figure 4](image_url)

**Figure 4** Generalized synthetic control averages. The treated average is the average weekly cases per 1000 for the four treated neighborhoods, the estimated $Y(0)$ average is the average weekly cases per 1000 for the four weighted, synthetic controls. Each treated neighborhood time period, and the time period for the synthetic control for that treated neighborhood, is normalized relative to the week when WiFi is first deployed.
for the treatment and synthetic control neighborhoods match well. There is a clearly visible reduction in average cases for treated neighborhoods starting a few weeks after treatment, suggesting that our estimated treatment effect in the main regression is not driven by our particular modeling assumptions or control unit selection. Standard errors for weekly estimated treatment effects in the synthetic control are shown in Figure B.1, Appendix B.5. Given that only 7 degrees of freedom in the data are available to estimate each coefficient after estimating the weekly trend, it is unsurprising that no coefficient is strongly significant (the coefficients at 3 and 9 weeks are significant at a 5% level, and coefficients at 2 and 6 weeks are significant at a 10% level.) The estimate of the average treatment effect is −0.248 and is significant at the 5% level.

5.4.6  |  Timing test

We estimate weekly treatment coefficients and their standard errors around the time of treatment for the baseline fixed effects regression in an event study approach reported in Figure B.2, Appendix B.6. Almost all post-treatment coefficients are negative, the coefficient at 8 weeks is significant at a 5% level, and coefficients at 1, 4, and 5 weeks are significant at a 10% level; however, as with the synthetic control approach, there are few degrees of freedom to estimate the treatment effects for any given week.

5.4.7  |  Treatment intensity

The intensity of treatment varies by neighborhood: in West Hill, only 4% of the neighborhood population lives in apartment blocks that receive free WiFi, while in Thorncliffe Park the number is 36% (see Table A.1). We include an interaction between the share of the population that receives WiFi and the treatment variable in Equation (1). Results in Table B.5 indicate that the interaction is highly significant and the baseline effect of treatment becomes small and insignificant. One extra percentage point of population with WiFi coverage reduces cases per 1000 by 0.015, or approximately 1%. Neighborhood-specific fixed effect regressions and synthetic control analysis in Table B.6 and Table B.7 reveal that the treatment effect for West Hill is small in magnitude and positive, the treatment effect for Rockcliffe-Smythe is small and negative, and Thorncliffe Park and Scarborough Village are both large and negative.

6  |  MECHANISM

Why are COVID-19 cases lower in a neighborhood after the free WiFi is introduced? On the one hand, free WiFi may have increased access to resources on best safety practices, local restrictions, and other information that could enable risk-mitigating behaviors. On the other hand, WiFi may have directly reduced individuals’ mobility outside the home—limiting encounters that could lead to COVID-19 infection—by incentivizing both leisure and work activities inside the home.

To evaluate whether WiFi improves access to information, we first look at the effect of WiFi on vaccination rates in Section 6.1. WiFi could enable access to information on where and how to book vaccinations—a challenge in Canada, where supply shortages were prevalent throughout 2021.16 Evidence of a positive link between WiFi and vaccinations would be evidence that WiFi enables access to local, high-quality health information. However, a finding of no link or a negative link between WiFi and vaccinations would not be evidence that information doesn’t matter for preventative behaviors: access to online misinformation and conspiracy theories may have reduced or hampered vaccinations (Jennings et al., 2021; Wang et al., 2019), while still inducing preventative behaviors, as some misinformation about vaccines still acknowledged that the virus was deadly (van Mulukom et al., 2022). Even though in Canada vaccine uptake was high and belief in misinformation was low (De Coninck et al., 2021; Karaivanov, Kim, et al., 2021), null or negative findings on vaccines are only suggestive evidence for the information channel.

Our second test in Section 6.2 evaluates jointly whether WiFi affects cases through information and mobility, or just through mobility. In particular, we examine how the introduction of free WiFi affects individuals’ propensity to visit the five most prevalent categories of business in the mobility data: retail stores, restaurants, arts/entertainment, personal services, and health care (US NAICS codes 44–45, 72, 71, 81, and 62 respectively) which account for 90% of the locations visited by at least one mobile device in the sample time period, see Table C.2. Categories for which online activities are not a direct substitute—restaurant dining or personal services—should only experience fewer visits if individuals are becoming more well-informed about safety, while categories for which online activities can be a direct substitute—arts/entertainment and retail stores—could experience fewer visits due to either informational or direct-substitution channels.
On balance, the evidence supports a direct substitution story. There is no effect of WiFi on vaccination rates; moreover, only the arts/entertainment category has a large, significant reduction in the propensity to visit. Cross-apartment-cluster aggregated data on WiFi usage points to leisure activities, notably video streaming, as comprising the largest share of usage. Sections 6.1 and 6.2 provide details of the analysis.

6.1 Effects of WiFi on vaccinations

We estimate the baseline specification of Equation (1) using completed vaccinations per 1000 residents as the outcome measure, but otherwise keeping the sample selection and control variables the same. Results are reported in Table 4 and indicate no significant relationship between free WiFi and vaccinations, with a negative and very imprecisely estimated coefficient on treatment. That vaccinations do not increase suggests that in-home WiFi is not affecting case rates through increased access to high-quality, local health information, although as per the discussion in the previous section, this may simply be evidence that individuals are consuming misinformation about vaccines but still taking correct safety precautions to avoid infection risk.

6.2 Effects of WiFi on mobility

6.2.1 Mobility data

An observation in the mobility data consists of the number of visits from an ADA \( i \) to a particular business location \( j \) in a given week \( t \). Each cluster of treated apartments in a given neighborhood belongs to a unique ADA; we refer to ADAs containing a treated cluster as treated ADAs. For apartment blocks in the four matched control neighborhoods, we visually identify clusters of apartments on Google Maps and map their latitude/longitude to an ADA, comprising a set of control ADAs. A list of control apartment addresses in each matched control neighborhood is provided in Table C.1, Appendix C.1. Business locations \( j \) correspond to “points-of-interest” or POIs identified in the SafeGraph data. We use the first two digits of the NAICS code of each POI to identify the broad business category.

Eight-five percentage of business receive fewer than four visits from mobile devices originating from any given ADA in any given week. SafeGraph’s differential privacy policy left-censors this data and codes it as four visits. Since the left-censoring makes the number of visits uninformative for the majority of cases, a dummy variable for whether or not any visits were made from an ADA to a business \( j \) will be used as the mobility measure instead of the number of visits.

| TABLE 4 Estimated effect of free WiFi on vaccinations |
|---------------------------------|
| Dependent variable | Completed vaccinations per 1000 \(_i\), \(_t\) | (1) | (2) | (3) |
| \( X_{it} \) | −0.897 (0.660) | −0.842 (0.568) | −0.693 (0.480) |
| Cases per 1000 \(_i\), \(_t-1\) | 0.424 (0.327) | 0.499 (0.354) |
| Cases per 1000 \(_i\), \(_t-2\) | 0.416 (0.438) | 0.284 (0.435) |
| Cases per 1000 \(_i\), \(_t-3\) | −0.776 (0.503) | −0.734 (0.498) |
| Completed vaccinations per 1000 \(_i\), \(_t-1\) | 0.283*** (0.084) |
| Completed vaccinations per 1000 \(_i\), \(_t-2\) | −0.030 (0.103) |
| Completed vaccinations per 1000 \(_i\), \(_t-3\) | −0.102 (0.074) |
| Neighborhood FE | ✓ | ✓ | ✓ |
| Week FE | ✓ | ✓ | ✓ |
| Observations | 384 | 384 | 384 |
| \( R^2 \) | 0.956 | 0.956 | 0.960 |

Note: Observations are at the neighborhood-week level. \( X_{it} \) equals one for neighborhoods that have received the free WiFi in all weeks after they receive it, and zero otherwise. Completed vaccinations are two doses of AstraZeneca, or any combination of two doses of Pfizer and Moderna. Standard errors are clustered at the neighborhood level.

\(* p < 0.1; ** p < 0.05; *** p < 0.01.\)
We summarize the number of POIs by 2-digit NAICS code in Table C.2, Appendix C.1. We use the same 6-month sample trimming window as in the baseline specification when conducting the regression analysis below, and provide summary statistics for the Visited_{ijt} outcome variable for the top five NAICS categories in Table C.3, Appendix C.1.

6.2.2 Mobility modeling and results

We estimate the following fixed effect specification:

\[
\text{Visited}_{ijt} = \gamma X_{it} + \delta^t Z_{it} + \alpha_j + \alpha_t + \epsilon_{ijt}
\]

where Visited_{ijt} is a Bernoulli variable that equals one if at least one mobile device was recorded as visiting location j from ADA i in week t. \(X_{it}\) is the treatment variable, which equals one in every week after an ADA receives WiFi, and zero before. \(Z_{it}\) is the same vector of lagged, neighborhood-level cases and completed vaccinations as in Equation (1). \(\alpha_j\) is an ADA-POI level fixed effect that controls for the average propensity for individuals from i to visit j. \(\alpha_t\) controls for all time-invariant features of j that might make it more attractive to visitors in general, and all time-invariant factors that make it relatively more attractive to visitors from one ADA compared to another. \(\alpha_t\) controls for city-wide time-varying factors, such as lockdown policies, that affect individuals' propensities to visit businesses in general.

\(\gamma\) is the parameter of interest, and captures whether individuals in treated ADAs exhibit a change in the propensity to visit businesses after the free WiFi is introduced. Note that since the outcome variable is at the \(ij\) level, this is the change in the propensity to visit any given POI in the category. If an individual substitutes from one POI to another in the same category, the reduction in propensity to visit the first POI will be offset by an increase in propensity to visit the second; only a differential, aggregate reduction in the number of POIs visited by individuals from treated ADAs will lead to a negative \(\gamma\) estimate.

Figure 5 displays the estimated change (in percentages) in the propensity to visit any given business for each category after the free WiFi is introduced, which we compute by scaling estimated coefficients and standard errors by the average propensity to visit a business for each category (reported as the “Mean” column in Table C.3, Appendix C.1). Full estimates are reported in Table C.4, Appendix C.2.

The largest and most significant negative effect is for NAICS code 71, the Arts, Entertainment and Recreation category (comprising 7% of visited POIs), with a 18.5% drop in the propensity to visit a business in treated ADAs. Other effects are imprecisely estimated.

Within each of the five estimators, between 20% and 22% of the individual heterogeneous treatment effects have negative weights in the TWFE estimator, which could lead the true effects to be the opposite sign of the TWFE estimates. We compute average treatment effects using the alternative stacked difference-in-differences estimator of De Chaisemartin and d’Haultfoeuille (2020) in Table C.5 which addresses negative weights by focusing on a much narrower set of control unit time periods and treatment time periods. Other than NAICS 62 (healthcare), the sign and magnitudes of the estimated coefficients remain largely

![Figure 5](image_url)  
**Figure 5** Effect of free WiFi on mobility, by business category. Each point corresponds to the \(\gamma\) coefficient from Equation (2) estimated on the mobility sample restricted to a NAICS 2-digit category, normalized by the average propensity to visit in that category, and multiplied by 100. NAICS codes are 72, 71, 62, 81, 44 from left to right. Error bars are 95% confidence intervals, standard errors are clustered at the neighborhood level. Average propensities and regression estimates are available in Tables C.3 and C.4
the same, albeit with much reduced significance, confirming that the TWFE estimator recovers the correct sign for the arts and entertainment mobility effect.

Online leisure activities may provide a substitute for visits to arts and entertainment venues. The large drop in visits to arts and entertainment venues implies that facilitating access to a direct substitute for leisure activities outside the home may be a key benefit of the WiFi program. Using the complete aggregate consumption broken down by application category across all apartment clusters in Figure D.3, Appendix D, we find that of the traffic that is categorizable, over 75% of the bandwidth in the free WiFi program is consumed by video streaming, social media, and music streaming, which are predominantly leisure activities. Figure D.4, Appendix D shows that the majority of video streaming for treated households is from YouTube, which is a free, ad-supported service, and therefore more accessible to low-income individuals than a paid streaming service like Netflix. Although the WiFi service was not optimized for streaming high definition video, in Section 5.2 we found that the majority of devices connecting to the WiFi are mobile phones, which do not need high resolution video for a good streaming experience owing to their small screen sizes (Cermak et al., 2011).

7 | DISCUSSION

The estimates from the paper can be used to make a back-of-the-envelope calculation of the benefits of the free WiFi policy. We focus on the savings from averted hospital visits, keeping in mind that WiFi may have provided direct and indirect (via reduced cases) benefits to employment, education, and long-run health outcomes.

Recent research suggests that the average cost of a COVID-19 hospital stay in Ontario, the home province for the city of Toronto, including both ICU and non-ICU admissions, is $23,000. By the end of the sample period in the data (September 30, 2021) the overall hospitalization rate for positive COVID-19 cases is 5.1%; the expected hospital cost of each positive COVID-19 case is therefore $1173. The estimated reduction in case rates from the free WiFi is 0.22 per thousand per week. Multiplying this number by the average population in a treated neighborhood and again by expected hospital costs yields weekly savings of $5643 or monthly savings of $22,572 per neighborhood on average.

While estimated costs of the WiFi deployment in Toronto are not available, one can estimate whether it would make sense to pay for lower-income individuals’ Internet access based on the number of treated households in each neighborhood and the cost of Internet access. For a 15 Mb/s plan with a 100 Gb monthly cap from Bell in Toronto, a household would pay $49.95 per month. From Table A.1, the average number of households (apartment units) in each treated neighborhood is 1110.25, implying per-neighborhood costs of $55,468 per month—larger than averted hospital costs. Any Internet plan that costs less than $20 per person per month could be fully subsidized for lower-income households and would generate net savings on hospital costs alone.

8 | CONCLUSION

In this paper, we evaluate the effect of introducing free, in-home WiFi to low-income apartment blocks during COVID-19. We find that WiFi leads to an approximately 14.4% reduction in the cases in a neighborhood. Analysis of the vaccination and mobility data suggests that WiFi is helping by providing a direct in-home substitute for leisure activities that would otherwise be conducted outside the home.

The potential effect of free, in-home Internet access extends beyond its immediate impact on COVID-19 transmission rates. For instance, in-home Internet access for lower-income individuals increases employment (Zuo, 2021) and improves access to virtual healthcare services (Hollander & Carr, 2020). These benefits may be especially pronounced during COVID-19, as free WiFi may have kept lower-income students from falling behind during periods when school was strictly online or may have enabled individuals to access healthcare services that moved purely online. This research calls for further evaluation of the effect of Internet access on health, education, and labor market outcomes of low-income populations during the pandemic.

ACKNOWLEDGMENTS

Thanks to Hamish Goodwin, Alex Lougheed and the City of Toronto Digital Canopy Project for providing access to the data, and to Anindya Sen, Erin Strumpf, and participants at the CEA 2022 and CHESG 2022 conferences for valuable comments. Yilin Huang provided exceptional research assistance. The author declares that he has no relevant or material financial interests that relate to the research described in this paper.
CONFLICT OF INTEREST
The authors declare that there is no conflict of interest that could be perceived as prejudicing the impartiality of the research reported.

DATA AVAILABILITY STATEMENT
The data that support the findings of this study are available from the corresponding author upon reasonable request.

ORCID
Daniel Goetz https://orcid.org/0000-0002-5010-900X

ENDNOTES
1 See Hale et al. (2021) for information on government policies worldwide.
2 Brodeur et al. (2021) conduct a literature survey on the effectiveness of NPIs in promoting distancing; for reductions in cases, see Karaivanov, Lu, et al. (2021) and Stevens et al. (2021) for NPIs in Canada; Flaxman et al. (2020) for NPIs in Europe; and Lai et al. (2020) for NPIs in China.
3 See https://www.toronto.ca/news/city-of-toronto-and-partners-help-connect-vulnerable-populations-with-internet-access-during-COVID-19-pandemic/.
4 Rogers, the dominant cable Internet provider in Canada, provides unlimited 25 Mbps Internet for $10 per month to public housing households through its Connect for Success program.
5 The cheapest available in-home option at many of the treated apartments was 15 Mb/s speed Internet with a 100 Gb monthly download cap from Bell (the largest Internet provider in Canada) for $49.95 per month. There were no full-speed unlimited cell phone plans offered at the time of the WiFi rollout, and a plan with a 20 Gb monthly download cap was $80.00 per month from Bell.
6 See https://www.toronto.ca/community-people/health-wellness-care/COVID-19-wellness-during-the-pandemic/COVID-19-seniors-vulnerable-people/COVID-19-free-wi-fi-pilot-project/.
7 See https://open.toronto.ca/dataset/covid-19-cases-in-toronto/ for the raw data on daily cases and case attributes in Toronto.
8 See https://www.toronto.ca/home/COVID-19/COVID-19-pandemic-data/COVID-19-vaccine-data/ for data on the weekly cumulative share of completed vaccinations by neighborhood.
9 Data from SafeGraph has been used frequently during COVID-19 to measure social distancing (Brodeur et al., 2021). Who use the discontinued Social Distancing Metrics dataset from SafeGraph.
10 Matching is done using the MatchIt package in R.
11 In Ontario, Stevens et al. (2021) find that up to a week of lagged cases are important in predicting cases, while Karaivanov, Lu, et al. (2021) find that a 2 week lag of case growth is optimal, with some sensitivity across lag choices.
12 Our weekly snapshot data includes several WiFi broadcast nodes that have good coverage of approximately half of the apartment units in the Rockcliffe-Smythe cluster.
13 See https://crct.gc.ca/eng/publications/reports/policymonitoring/2019/cmrl.htm
14 We continue to use aggregate lagged cases and vaccinations as controls.
15 We showed in Section 5.2 that there was substantial take-up of the WiFi in treated apartments for which we have device data, but our TOT estimates in this section assume full compliance with treatment in treated apartments, and should be considered a lower bound.
16 See for instance: Lexchin, Joel, “The roots of Canada’s COVID-19 vaccine shortage go back decades,” The Conversation, February 8, 2021.
17 See https://www.cihi.ca/en/COVID-19-hospital-stays-cost-3-times-more-than-a-stay-for-heart-attack.
18 See https://www.publichealthontario.ca/en/data-and-analysis/infectious-disease/COVID-19-data-surveillance/COVID-19-data-tool.

REFERENCES
Abadie, A. (2021). Using synthetic controls: Feasibility, data requirements, and methodological aspects. Journal of Economic Literature, 59(2), 391–425. https://doi.org/10.1257/jel.20191450
Abadie, S., Imbens, G. W., & Wooldrige, J. (2017). When should you adjust standard errors for clustering? Tech. rep. National Bureau of Economic Research.
Adams-Prassl, A., Boneva, T., Golin, M., & Rauh, C. (2020). Inequality in the impact of the coronavirus shock: Evidence from real time surveys. Journal of Public Economics, 189, 104245. https://doi.org/10.1016/j.jpubeco.2020.104245
Alstadseter, A., Bratsberg, B., Eielsen, G., Kopczuk, W., Markussen, S., Raaum, O., & Røed, K. (2020). The first weeks of the coronavirus crisis: Who got hit, when and why? Evidence from Norway. Tech. rep. National Bureau of Economic Research.
Andrews, N., Tessier, E., Stowe, J., Gower, C., Kirsebom, F., Simmons, R., Gallagher, E., Thelwall, S., Groves, N., Dabrera, G., Myers, R., Campbell, C. N., Amirthalmangam, G., Edmunds, M., Zambon, M., Brown, K., Hopkins, S., Chand, M., Ladhani, S. N., & Lopez Bernal, J. (2022). Duration
of protection against mild and severe disease by Covid-19 vaccines. *New England Journal of Medicine*, 386(4), 340–350. https://doi.org/10.1056/nejmoa2115481

Bertrand, M., Duflo, E., & Mullainathan, S. (2004). How much should we trust differences-in-differences estimates? *Quarterly Journal of Economics*, 119(1), 249–275. https://doi.org/10.1162/003355304772839588

Brodeur, A., Gray, D., Islam, A., & Bhuiyan, S. (2021). A literature review of the economics of Covid-19. *Journal of Economic Surveys*, 35(4), 1007–1044. https://doi.org/10.1111/joes.12423

Callaway, B., & Sant’anna, P. H. (2021). Difference-in-differences with multiple time periods. *Journal of Econometrics*, 225(2), 200–230. https://doi.org/10.1016/j.jeconom.2020.12.001

Cameron, A. C., & Miller, D. L. (2015). A practitioner’s guide to cluster-robust inference. *Journal of Human Resources*, 50(2), 317–372. https://doi.org/10.3368/jhr.50.2.317

Cermak, G., Pinson, M., & Wolf, S. (2011). The relationship among video quality, screen resolution, and bit rate. *IEEE Transactions on Broadcasting*, 57(2), 258–262. https://doi.org/10.1109/tbc.2011.2121650

Chan, J. (2020). The geography of social distancing in Canada: Evidence from Facebook. *Canadian Public Policy*, 46(S1), S19–S28. https://doi.org/10.3138/cpp.2020-050

Chernozhukov, V., Kasahara, H., & Schrimpf, P. (2021). Causal impact of masks, policies, behavior on early Covid-19 pandemic in the US. *Journal of Econometrics*, 220(1), 23–62. https://doi.org/10.1016/j.jeconom.2020.09.003

Choi, L., & Tucker, C. (2020). Social distancing, internet access and inequality. Tech. rep. National Bureau of Economic Research.

Choi, D., & D’Haultfouville, X. (2020). Two-way fixed effects estimators with heterogeneous treatment effects. *The American Economic Review*, 110(9), 2964–2996. https://doi.org/10.1257/aer.20181169

De Coninck, D., Frissen, T., Matthijs, K., D’Haenens, L., Lits, G., Champagne-Poirier, O., Carignan, M.-E., David, M. D., Pigney-Chynel, N., Salerno, S., & Genereux, M. (2021). Beliefs in conspiracy theories and misinformation about Covid-19: Comparative perspectives on the role of anxiety, depression and trust in exposure to and trust in social media stories. *Frontiers in Psychology*, 12, 646394. https://doi.org/10.3389/fpsyg.2021.646394

Flaxman, S., Mishra, S., Gandy, A., Wein, H. J. T., Yellen, T. A., Coupland, H., Whitaker, C., Zhu, H., Berah, T., Eaton, J. W., Monod, M., Perez-Guzman, P. N., Schmit, N., Cilloni, L., Ainslie, K. E. C., Baguelin, M., Boonyasiri, A., Boyd, O., Cattarino, L., … Bhatt, S. (2020). Estimating the effects of non-pharmaceutical interventions on Covid-19 in Europe. *Nature*, 584(7820), 257–261. https://doi.org/10.1038/s41586-020-2405-7

Goldfarb, A., & Prince, J. (2008). Internet adoption and usage patterns are different: Implications for the digital divide. *Information Economics and Policy*, 20(1), 2–15. https://doi.org/10.1016/j.infoecopol.2007.05.001

Goodman-Bacon, A. (2021). Difference-in-differences with variation in treatment timing. *Journal of Econometrics*, 225(2), 254–277. https://doi.org/10.1016/j.jeconom.2021.03.014

Goosbee, A., & Syverson, C. (2021). Fear, lockdown, and diversion: Comparing drivers of pandemic economic decline 2020. *Journal of Public Economics*, 193, 104311. https://doi.org/10.1016/j.jpubeco.2020.104311

Grannis, S. J., Rowley, E. A., Ong, T. C., Stenchjem, E., Klein, N. P., Desilva, M. B., Naleway, A. L., Natarajan, K., Thompson, M. G., & Network, V. (2021). Interim estimates of Covid-19 vaccine effectiveness against Covid-19–Associated emergency department or urgent care clinic encounters and hospitalizations among adults during SARS-COV-2 B. 1.617. 2 (Delta) variant predominance—Nine states, June–August 2021. *Morbidity and Mortality Weekly Report*, 70(37), 1299–1321. https://doi.org/10.15585/mmwr.mm7037e2

Hale, T., Angrist, N., Goldsmithid, R., Kira, B., Petherick, A., Phillips, T., Webster, S., Cameron-Blake, E., Hallas, L., Majumdar, S., & Tatlow, H. (2021). A global panel database of pandemic policies (Oxford Covid-19 government response tracker). *Nature Human Behaviour*, 5(4), 529–538. https://doi.org/10.1038/s41562-021-01079-8

Hollander, J. E., & Carr, B. G. (2020). Virtually perfect? Telemedicine for Covid-19. *New England Journal of Medicine*, 382(18), 1679–1681. https://doi.org/10.1056/nejmp2003539

Jennings, W., Stoker, G., Bunting, H., Valgardsson, V. O., Gaskell, J., Devine, D., Mckay, L., & Mills, M. C. (2021). Lack of trust, conspiracy beliefs, and social media use predict Covid-19 vaccine hesitancy. *Vaccines*, 9(6), 593. https://doi.org/10.3390/vaccines9060593

Karaivanov, A., Kim, D., Lu, S. E., & Shigeoka, H. (2021). COVID-19 vaccination mandates and vaccine uptake. Tech. rep. National Bureau of Economic Research.

Karaivanov, A., Lu, S. E., Shigeoka, H., Chen, C., & Pamplona, S. (2021). Face masks, public policies and slowing the spread of Covid-19: Evidence from Canada. *Journal of Health Economics*, 78, 102475. https://doi.org/10.1016/j.jhealeco.2021.102475

Lai, S., Ruktanonchai, N. W., Zhou, L., Prosper, O., Luo, W., Floyd, J. R., Wesolowski, A., Santillana, M., Zhang, C., Du, X., Yu, H., & Tatem, A. J. (2020). Effect of non-pharmaceutical interventions to contain Covid-19 in China. *Nature*, 585(7825), 410–413. https://doi.org/10.1038/s41586-020-2293-x

Papanastasiou, A., Ruffle, B. J., & Zheng, A. (2022). Compliance with social distancing: Theory and empirical evidence from Ontario during Covid-19. *Canadian Journal of Economics/Revue canadienne d’économique*, 55(S1), 705–734. https://doi.org/10.1111/caje.12565

Stevens, N. T., Sen, A., Kiwon, F., Morita, P. P., Steiner, S. H., & Zhang, Q. (2021). Estimating the effects of non-pharmaceutical interventions (NPIs) and population mobility on daily Covid-19 cases: Evidence from Ontario. *Canadian Public Policy*, e2021022.

Van Mulukom, V., Pummerer, L. J., Alper, S., Bai, H., ČAVOJOVÁ, V., Farias, I., Kay, C. S., Lazarevic, L. B., Lobato, E. J., Marinthe, G., Pavela Banai, I., Srol, J., & Zezelj, I. (2022). Antecedents and consequences of Covid-19 conspiracy beliefs: A systematic review. *Social Science & Medicine*, 301, 114912. https://doi.org/10.1016/j.socscimed.2022.114912

Wang, Y., Mckee, M., Torbica, A., & Stuckler, D. (2019). Systematic literature review on the spread of health-related misinformation on social media. *Social Science & Medicine*, 240, 112552. https://doi.org/10.1016/j.socscimed.2019.112552
Xu, Y. (2017). Generalized synthetic control method: Causal inference with interactive fixed effects models. *Political Analysis, 25*(1), 57–76. https://doi.org/10.1017/pan.2016.2

Zuo, G. W. (2021). Wired and hired: Employment effects of subsidized broadband internet for low-income Americans. *American Economic Journal: Economic Policy, 13*(3), 447–482. https://doi.org/10.1257/pol.20190648

**SUPPORTING INFORMATION**

Additional supporting information can be found online in the Supporting Information section at the end of this article.

**How to cite this article:** Goetz, D. (2022). Does providing free internet access to low-income households affect COVID-19 spread? *Health Economics*, 1–16. https://doi.org/10.1002/hec.4601