B. Background and additional estimates for SCM application to Jena

This appendix presents supporting findings for the comparative case study of Jena.

B.1. Covid-19 cases and cumulative incidence rate in Jena and Germany on April 5

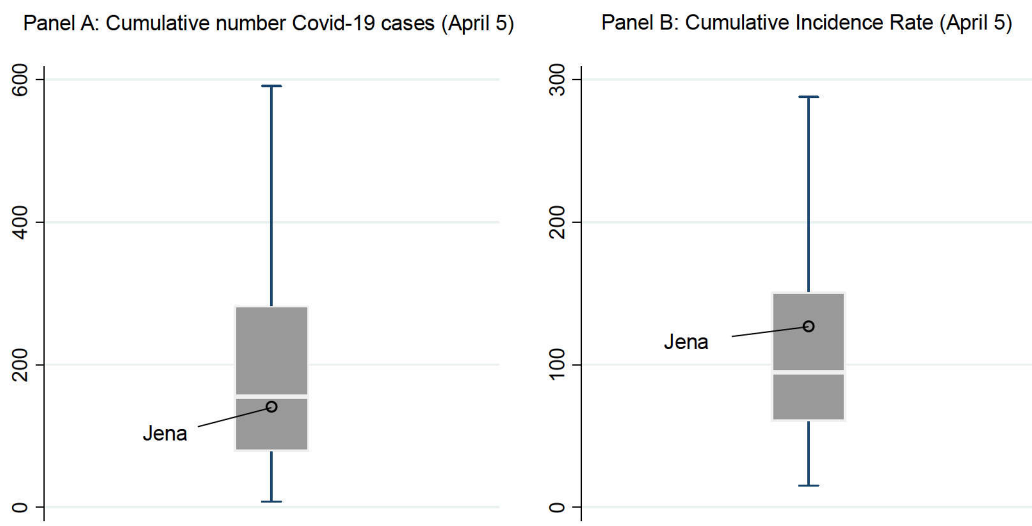

Figure A1: Box plots for distribution of Covid-19 cases across German NUTS3 regions (April 5)

B.2. Evaluation of pre-treatment predictor balance and prediction error (RMSPE)

This appendix shows the balancing properties of the SCM approach together with the root mean square percentage error (RMSPE) as a measure for the quality of the pre-treatment prediction.

Table A2: Pre-treatment predictor balance and RMSPE for SCM in Figure 2

Treatment:	**Introduction of face masks**		**Announcement/ start of campaign**	
	Jena	Synthetic control group	Jena	Synthetic control group
Cumulative number of registered Covid-19 cases (one and seven days before start of treatment, average)	129.5	129.2	93	92.7
Number of newly registered Covid-19 cases (last seven days before the start of the treatment, average)	3.7	3.8	5	5.2
Population density (Population/km^2)	38.4	22.8	968.1	947.9
Share of highly educated population (in %)	968.1	1074.3	38.4	26.3
Share of female in population (in %)	50.1	50.1	50.1	50.1
Average age of female population (in years)	43.5	43.7	43.5	43.9
Average age of male population (in years)	40.5	40.6	40.5	40.8
Old-age dependency ratio (in %)	32.1	29.3	32.1	29.8
Young-age dependency ratio (in %)	20.3	19.6	20.3	19.5
Physicians per 10,000 of population	20.5	19.8	20.5	20.8
Pharmacies per 100,000 of population	28.8	28.7	28.8	28.6
Settlement type (categorial variable)	1	1.3	1	1.9
RMSPE (pre-treatment)	**3.145**		**4.796**	

Notes: Donor pool includes all other German NUTS3 regions except the two immediate neighboring regions of Jena (Weimarer Land, Saale-Holzland-Kreis) as well as the regions Nordhausen and Rottweil since the latter regions introduced face masks in short succession to Jen on April 14 and April 17.

B.3. Selected control regions and their associated sample weights

Table A3: Distribution of sample weights in donor pool for synthetic Jena

Introduction of face masks (Panel A in Figure 2)		
ID	**NUTS 3 region**	**Weight**
13003	Rostock	0.326
6411	Darmstadt	0.311
3453	Cloppenburg	0.118
7211	Trier	0.117
6611	Kassel	0.082
5370	Heinsberg	0.046

Note: Donor pools corresponds to SCM estimation in Panel A of Figure 2. Sample weights are chosen to minimize the RMSPE ten days prior to the start of the treatment.

B.4. Growth rates

Jena has 142 registered cases on April 6 compared to an estimated number of 143 cases in the synthetic control group. On April 26 Jena counts 158 cases and the synthetic control group reports 205 (again estimated) cases. The daily growth rate in Jena is denoted by x_{Jena} and can be computed from 142 $[1+x_{Jena}]^{20} = 158$. The daily growth rate in the control group is denoted by $x_{control}$ and can be computed from 143 $[1+x_{control}]^{20} = 205$. Hence, the introduction of the face mask is associated with a decrease in the number of infections of $x_{control} - x_{Jena}$ percentage points per day.

Table A4: Summary of treatment effects of face mask introduction in Germany

	Single Treatment (Jena)	Multiple treatments (all districts)	Multiple treatments (cities)
Percentage change in cumulative number of Covid-19 cases over 20 days	-22.9%	n.a.	n.a.
Absolute change in cumulative number of Covid-19 cases over 10 days	-23	-5.8	-12.3
Percentage change in cumulative number of Covid-19 cases over 10 days	-12.8%	-2.3%	-4.2%
Difference in daily growth rates of Covid-19 cases in percentage points	-1.32%	-0.23%	-0.42%
Reduction in daily growth rates of Covid-19 cases in percent	60.1%	18.94%	37.28%

These numbers are computed in an Excel-file available on the web pages of the authors.

B.5. SCM results by age groups

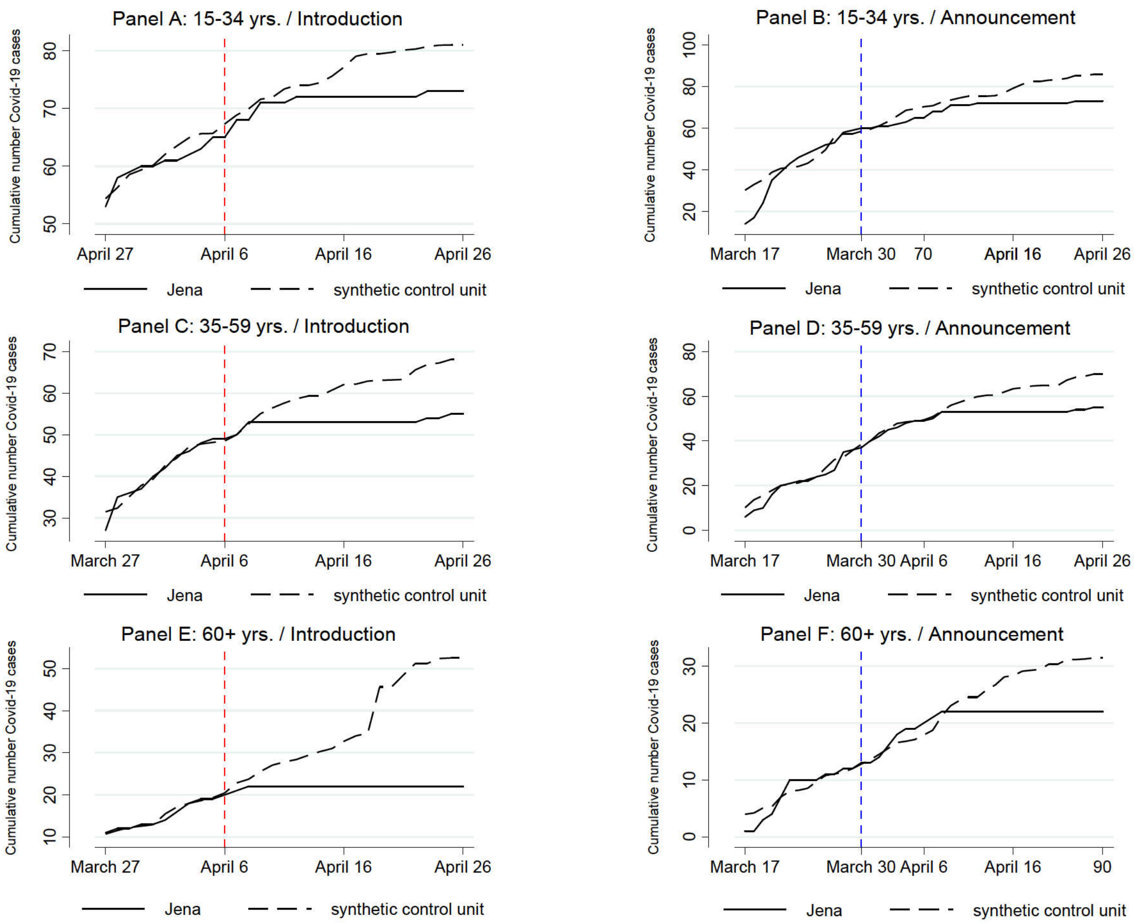

Figure A2: Treatment effects for introduction and announcement of face masks in Jena

Notes: Predictor variables are chosen as for overall specification shown in Figure 2.

Table A5: Sample weights in donor pool for synthetic Jena (cumulative Covid-19 cases; by age groups)

Age Group 15-34 years			Age Group 35-59 years			Age Group 60 years and above		
ID	**NUTS 3 region**	**Weight**	**ID**	**NUTS 3 region**	**Weight**	**ID**	**NUTS 3 region**	**Weight**
1001	Flensburg	0.323	6411	Darmstadt	0.528	6411	Darmstadt	0.522
7211	Trier	0.207	16055	Weimar	0.16	16055	Weimar	0.244
13003	Rostock	0.184	14511	Chemnitz	0.15	7316	Neustadt a.d. Weinstraße	0.068
5370	Heinsberg	0.142	8221	Baden-Baden	0.07	9562	Erlangen	0.06
3453	Cloppenburg	0.107	6434	Hochtaunus-kreis	0.062	3356	Osterholz	0.056
6413	Offenbach am Main	0.038	8435	Bodenseekreis	0.029	5515	Münster	0.027
			5370	Heinsberg	0.001	9188	Starnberg	0.022

Note: Donor pools corresponds to SCM estimations in Figure A2. Sample weights are chosen to minimize the RMSPE ten days prior to the start of the treatment.

B.6. Effects on cumulative number of infections per 100,000 inhabitants

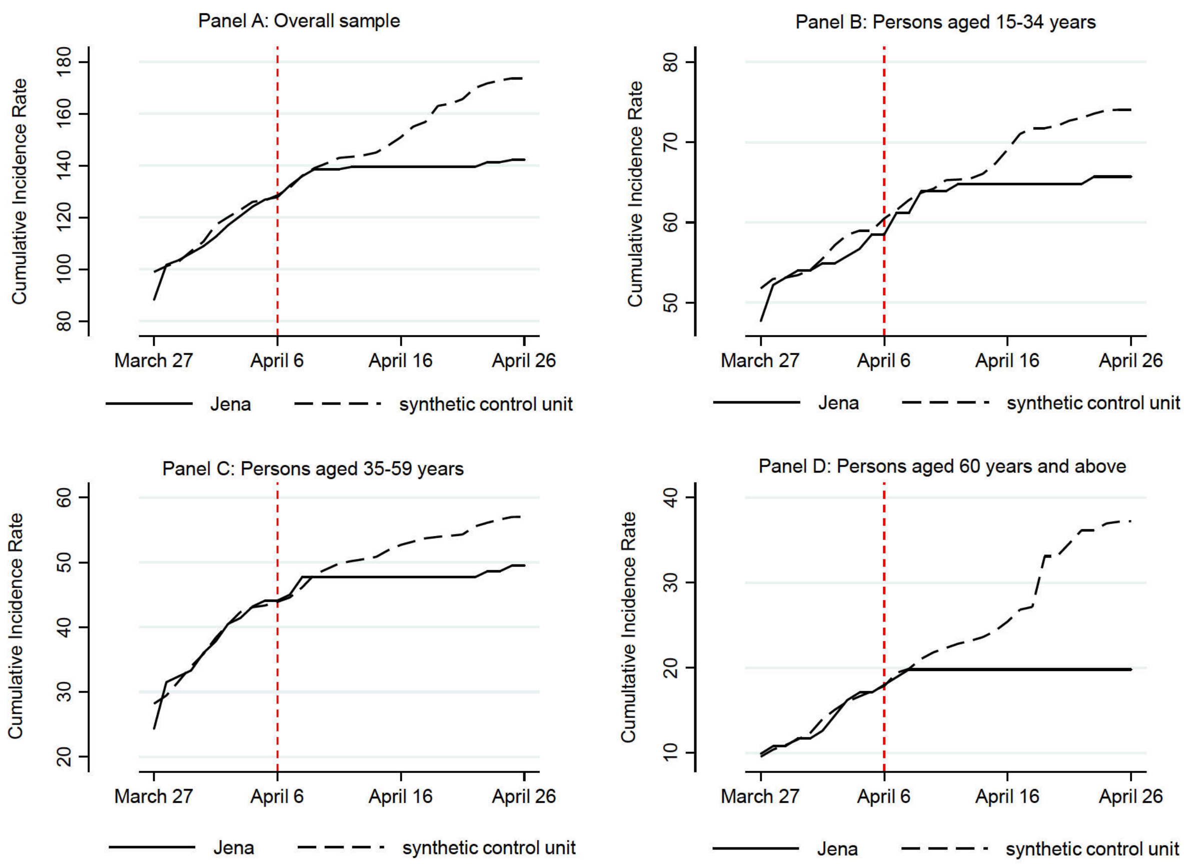

Figure A3: Treatment effects for introduction of face masks on cumulative incidence rate

Notes: See Table 1 for a definition of the incidence rate. Predictor variables are chosen as for overall specification shown in Figure 2.

Table A6: Sample weights in donor pool for synthetic Jena (cumulative incidence rate)

ID	**NUTS 3 region**	**Weight**
6411	Darmstadt	0.46
15003	Magdeburg	0.171
5370	Heinsberg	0.133
13003	Rostock	0.093
5515	Münster	0.066
11000	Berlin	0.035
12052	Cottbus	0.032
6611	Kassel	0.011

Note: Donor pools corresponds to SCM estimation in Figure A3. Sample weights are chosen to minimize the RMSPE ten days prior to the start of the treatment.

Table A7: Sample weights in donor pool for synthetic Jena (cumulative incidence rate; by age groups)

Age Group 15-34 years			Age Group 35-59 years			Age Group 60 years and above		
ID	NUTS 3 region	Weight	ID	NUTS 3 region	Weight	ID	NUTS 3 region	Weight
5370	Heinsberg	0.377	6411	Darmstadt	0.419	6411	Darmstadt	0.448
13003	Rostock	0.288	14511	Chemnitz	0.184	14612	Dresden	0.313
1001	Flensburg	0.14	14612	Dresden	0.154	9188	Starnberg	0.071
6611	Kassel	0.138	8221	Heidelberg	0.138	16054	Suhl	0.069
11000	Berlin	0.058	9188	Starnberg	0.088	5515	Münster	0.06
			5370	Heinsberg	0.016	8221	Heidelberg	0.039

Note: Donor pools corresponds to SCM estimations in Figure A3. Sample weights are chosen to minimize the RMSPE ten days prior to the start of the treatment.

B.7. Google trends and announcement effects

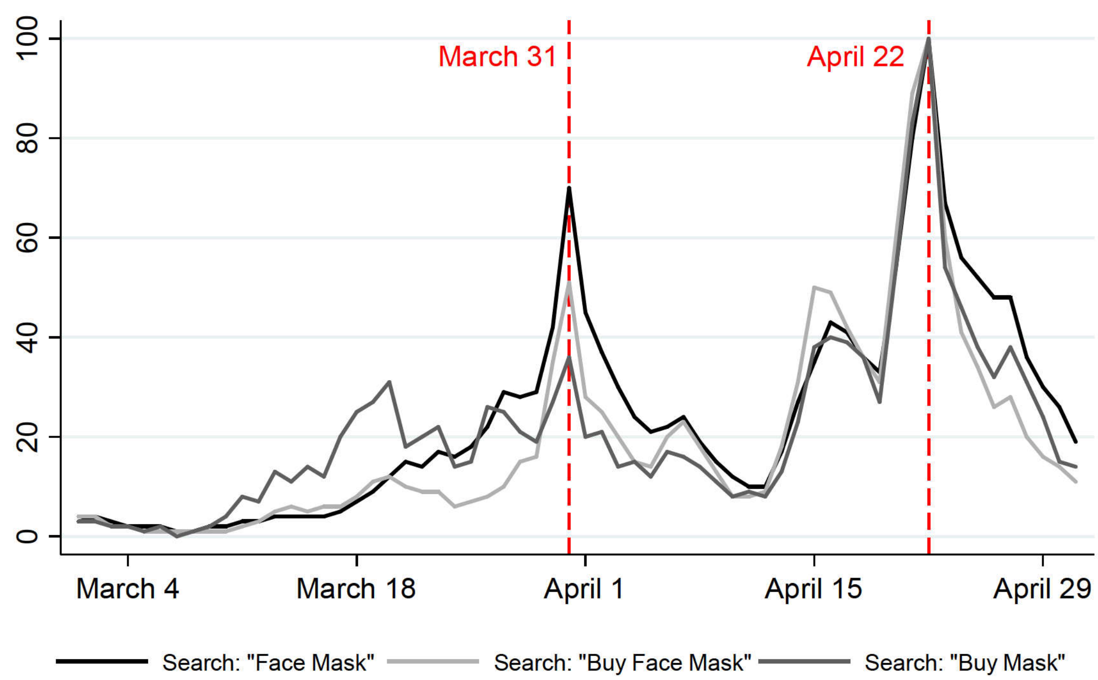

Figure A4: Online search for face masks and purchase options according to Google Trends

Note: Online search for keywords (in German) as shown in the legend as Face Mask (“Mund.-Nasen-Schutz”), Buy Face Mask (“Mundschutz kaufen”) and Buy mask (“Maske kaufen”); alternative keywords show similar peaks but with a lower number of hits; based on data from Google Trends (2020).

B.8. Changes in donor pool for synthetic Jena

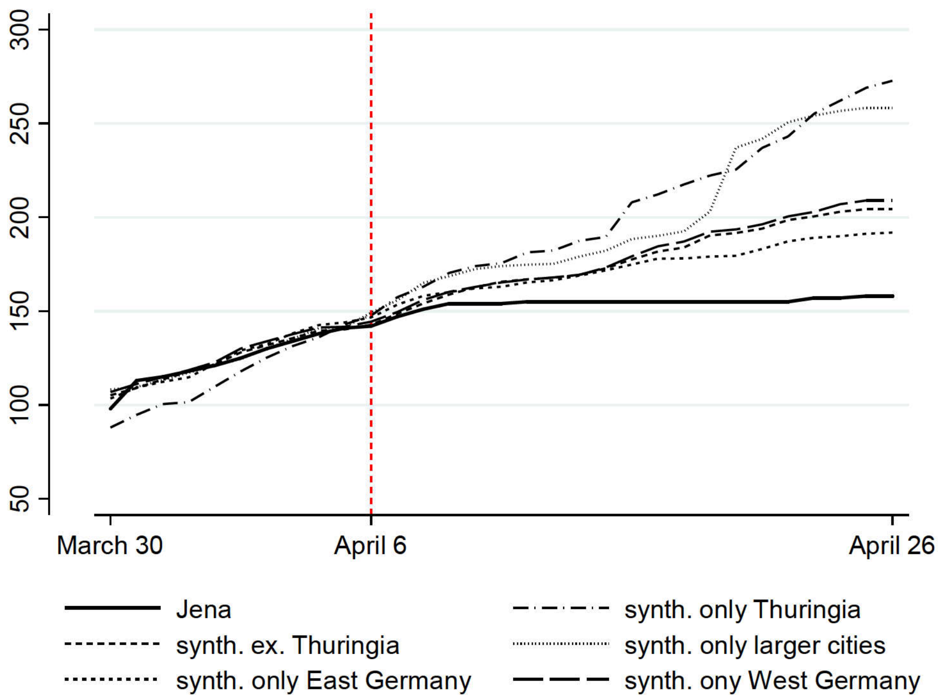

Figure A5: Treatment effects for changes in donor pool used to construct synthetic Jena

Notes: See main text for a detailed definition of the respective donor pools. Predictor variables are chosen as for overall specification shown in Figure 2.

Table A8: Sample weights for alternative donor pools used to construct synthetic Jena

	Only Thuringia			Excluding Thuringia			Only larger cities	
ID	**NUTS 3 region**	**Weight**	**ID**	**NUTS 3 region**	**Weight**	**ID**	**NUTS 3 region**	**Weight**
16076	Greiz	0.533	13003	Rostock	0.318	6411	Darmstadt	0.504
16051	Erfurt	0.467	6411	Darmstadt	0.302	13003	Rostock	0.304
			7211	Trier	0.129	5113	Essen	0.192
			3453	Cloppenburg	0.122			
			6611	Kassel	0.083			
			5370	Heinsberg	0.046			

	Only East Germany			Only West Germany	
ID	**NUTS 3 region**	**Weight**	**ID**	**NUTS 3 region**	**Weight**
16051	Erfurt	0.865	6411	Darmstadt	0.242
14612	Dresden	0.124	3402	Emden	0.198
11000	Berlin	0.011	6611	Kassel	0.169
			7211	Trier	0.168
			4012	Bremerhaven	0.167
			5370	Heinsberg	0.057

Note: Donor pools corresponds to SCM estimations in Figure A5. Sample weights are chosen to minimize the RMSPE ten days prior to the start of the treatment.

B.9. Place-in-space tests for other major cities in Thuringia

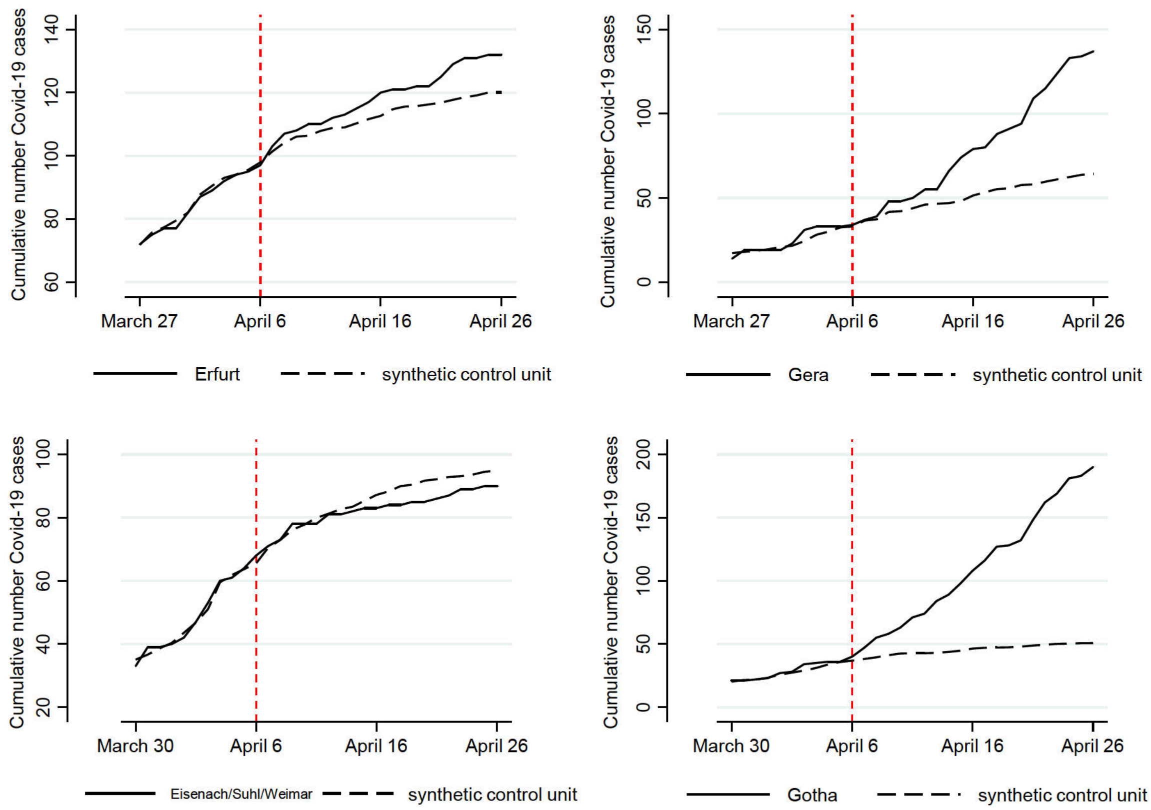

Figure A6: Placebo tests for the effect of face masks in other cities in Thuringia on April 6.

Notes: For the placebo tests in the other cities in Thuringia the same set of predictors as for Jena (Figure 2) has been applied. The reported regions cover all *kreisfreie Städte* plus Gotha (*Landkreis*). The cities Weimar, Suhl and Eisenach have been aggregated since the number of reported Covid-19 is low in these cities.

Table A9: Sample weights in donor pool for synthetic control groups (other cities in Thuringia)

Erfurt			**Gera**		
ID	**NUTS 3 region**	**Weight**	**ID**	**NUTS 3 region**	**Weight**
13003	Rostock	0.28	15001	Dessau-Roßlau	0.501
16055	Weimar	0.244	16054	Suhl	0.222
3356	Osterholz	0.212	7318	Speyer	0.162
7313	Landau in der Pfalz	0.154	8231	Pforzheim	0.061
6413	Offenbach am Main	0.078	7311	Frankenthal (Pfalz)	0.046
5370	Heinsberg	0.029	8211	Baden-Baden	0.005
5515	Münster	0.004	9662	Schweinfurt	0.003
			14521	Erzgebirgskreis	0.001
Weimar/Suhl/Eisenach			**Gotha**		
ID	**NUTS 3 region**	**Weight**	**ID**	**NUTS 3 region**	**Weight**
15001	Dessau-Roßlau	0.263	15081	Altmarkkreis	0.23
12052	Cottbus	0.236	16077	Altenburger Land	0.164
13004	Schwerin	0.202	15086	Jerichower	0.161
9361	Amberg	0.177	3402	Emden	0.111
14626	Görlitz	0.069	16071	Weimarer Land	0.108
9363	Weiden i.d. Opf.	0.036	16074	Saale-Holzland-Kreis	0.063
14521	Erzgebirgskreis	0.008	16061	Eichsfeld	0.058
9184	München	0.005	16070	Ilm-Kreis	0.055
6411	Darmstadt	0.005	3453	Cloppenburg	0.027
			15003	Magdeburg	0.017
			4012	Bremerhaven	0.007

Note: Donor pools corresponds to SCM estimations in Figure A6. Sample weights are chosen to minimize the RMSPE ten days prior to the start of the treatment.

C. The effect in other German cities and regions (single treatment analyses)

In addition to Jena, we test for treatment effects in Nordhausen, Rottweil, Main-Kinzig-Kreis, and Wolfsburg (compare Figure 1). We ignore Braunschweig here as the introduction became effective only two days in advance of its federal state.

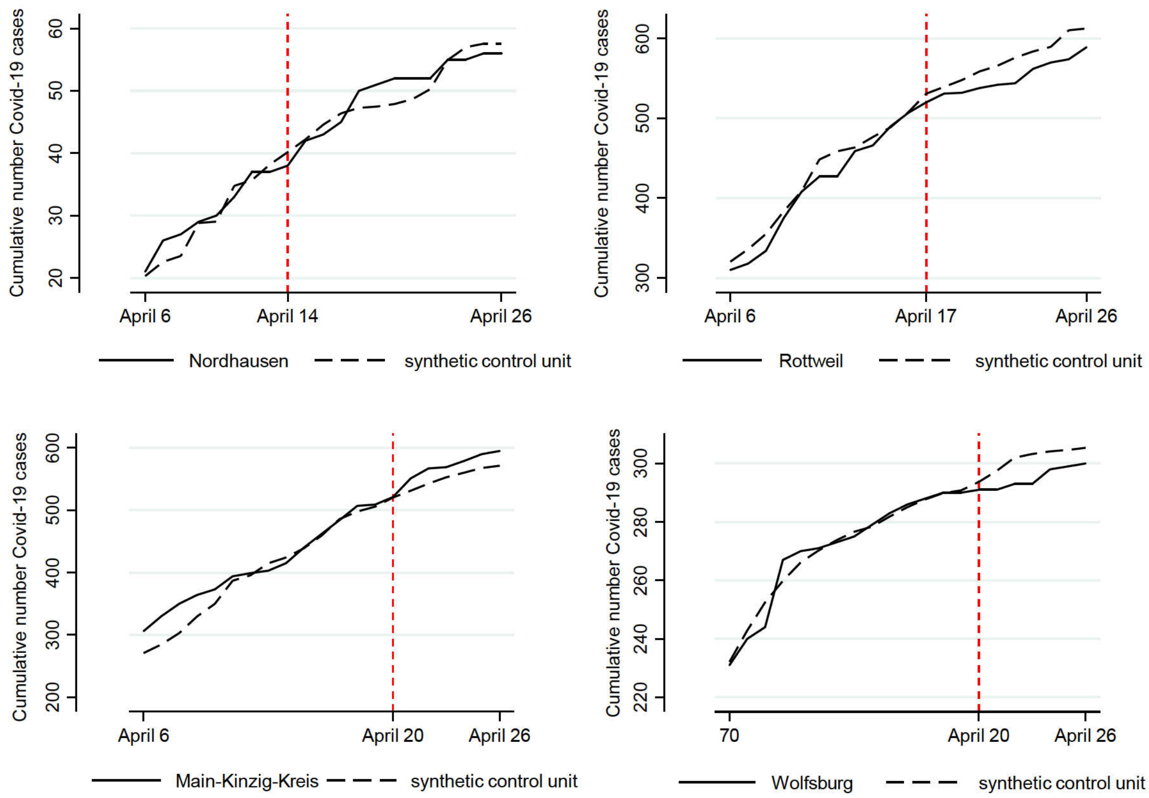

Figure A7: Treatment effects for introduction of face masks in other cities

Notes: Nordhausen (Thuringia, April 14, top left), Rottweil (Baden Württemberg, April 17, top right), Wolfsburg (Lower Saxony, April 20, middle left), Main-Kinzig-Kreis (Hessia, April 20, middle right). Predictor variables are chosen as for overall specification shown in Figure 2.

As the figure shows, the result is 2:1:1. Rottweil and Wolfsburg display a positive effect of mandatory mask wearing, just as Jena. The results in Nordhausen are very small or unclear. In the region of Main-Kinzig, it even seems to be the case that masks increased the number of cases relative to the synthetic control group. As all of these regions introduced masks after Jena, the time period available to identify effects is smaller than for Jena. The effects of mandatory face masks could also be underestimated as announcement effects and learning from Jena might have induced individuals to wear masks already before they became mandatory. Finally, the average pre-treatment RMSPE for these four regions (7.150) is larger than for the case of Jena (3.145). For instance, in the case of the region of Main-Kinzig it is more than three times as high (9.719), which indicates a lower pre-treatment fit. The obtained treatment effects should then be interpreted with some care as the pre-sample error could also translate into the treatment period. In order to minimize the influence of a poor pre-treatment fit for some individual regions, the main text therefore compares the results in Jena mainly with a multiple unit treatment approach.

Table A10: Sample weights in donor pool for synthetic controls (other treated NUTS3 regions)

	Nordhausen			Rottweil	
ID	**NUTS 3 region**	**Weight**	**ID**	**NUTS 3 region**	**Weight**
16069	Hildburghausen	0.228	8327	Tuttlingen	0.324
6636	Werra-Meißner-Kreis	0.209	5966	Olpe	0.216
16064	Unstrut-Hainich-Kreis	0.168	8136	Ostalbkreis	0.2
16054	Suhl	0.109	16071	Weimarer Land	0.063
3402	Emden	0.093	14521	Erzgebirgskreis	0.06
12073	Uckermark	0.071	3102	Salzgitter	0.043
12053	Frankfurt (Oder)	0.07	16061	Eichsfeld	0.035
3354	Lüchow-Dannenberg	0.051	9187	Rosenheim	0.031
			9279	Dingolfing-Landau	0.025
			3455	Friesland	0.003
	Main-Kinzig-Kreis			**Wolfsburg**	
ID	**NUTS 3 region**	**Weight**	**ID**	**NUTS 3 region**	**Weight**
8136	Ostalbkreis	0.193	8212	Karlsruhe	0.357
1062	Stormarn	0.168	8221	Heidelberg	0.189
5966	Olpe	0.113	8211	Baden-Baden	0.158
6433	Groß-Gerau	0.105	10046	St. Wendel	0.128
9473	Coburg	0.092	14511	Chemnitz	0.071
5562	Recklinghausen	0.063	5117	Mülheim an der Ruhr	0.059
7313	Landau in der Pfalz	0.059	5315	Köln	0.028
9171	Altrötting	0.056	15003	Magdeburg	0.007
7338	Rhein-Pfalz-Kreis	0.047	9663	Würzburg	0.004
6437	Odenwaldkreis	0.041			
8236	Enzkreis	0.041			
3159	Göttingen	0.023			

Note: Donor pools corresponds to SCM estimations in Figure A7. Sample weights are chosen to minimize the RMSPE ten days prior to the start of the treatment.

D. A brief survey of public health measures against Covid-19

Our approach goes in line with various studies that have already tried to better understand the effect of public health measures on the spread of Covid-19 (Barbarossa et al., 2020, Hartl et al., 2020, Donsimoni et al., 2020, Dehning et al., 2020, Gros et al., 2020, Adamik et al, 2020). However, these earlier studies all take an aggregate approach in the sense that they look at implementation dates for a certain measure and search for subsequent changes in the national incidence. There are some prior analyses that take a regional focus (Khailaie et al. 2020) but no attention is paid to the effect of policy measures.[11]

There are also many cross-country analyses, both in a structural SIR (susceptible, infectious and removed) sense (Chen and Qiu, 2020) and with an econometric focus on forecasting the end of the pandemic (Ritschl, 2020). Others draw parallels between earlier pandemics and Covid-19 (Barro et al., 2020). These studies do not explicitly take public health measures into account. Some studies discuss potential effects of public health measures and survey general findings (Wilder-Smith et al. 2020, Anderson et al., 2020, Ferguson et al, 2020) but do not provide direct statistical evidence on specific measures.

The synthetic control method (SCM) has been applied by Friedson et al. (2020) to estimate the effect of the shelter-in-place order for California, USA, in the development of Covid-19. The authors find *inter alia* that around 1600 deaths from Covid-19 have been avoided by this measure during the first four weeks. The effects of face masks have been surveyed by Howard et al. (2020) and Greenhalgh et al. (2020). Greenhalgh et al. (2020) mainly presents evidence on the effect of face masks during non-Covid epidemics (influenza and SARS). Marasinghe (2020) reports that they "*did not find any studies that investigated the effectiveness of face mask use in limiting the spread of COVID-19 among those who are not medically diagnosed with COVID-19 to support current public health recommendations*".

In addition to medical aspects (like transmission characteristics of Covid-19 and filtering capabilities of masks), Howard et al. (2020) survey evidence on mask efficiency and on the effect of a population. They first stress that "*no randomized control trials on the use of masks <...> has been published*". The study which is "*the most relevant paper*" for Howard et al. (2020) is one that analyzed *"exhaled breath and coughs of children and adults with acute respiratory illness*" (Leung et al., 2020, p. 676), i.e. used a clinical setting. Concerning the effect of masks on community transmissions, the survey needs to rely on pre-Covid-19 studies.

We conclude from this literature review that our paper is the first analysis that provides field evidence on the effect of masks on mitigating the spread of Covid-19.

[11] In a short note, Hartl and Weber (2020) apply panel methods based on time dummies to understand the relative importance of various public health measures. They employ data at the federal state level and not at the regional level. As a detailed model description is not available, an appreciation of results is difficult at this point.

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Trends in COVID-19 Incidence After Implementation of Mitigation Measures — Arizona, January 22–August 7, 2020

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Mitigating the spread of SARS-CoV-2, the virus that causes coronavirus disease 2019 (COVID-19), requires individual, community, and state public health actions to prevent person-to-person transmission. Community mitigation measures can help slow the spread of COVID-19; these measures include wearing masks, social distancing, reducing the number and size of large gatherings, pausing operation of businesses where maintaining social distancing is challenging, working from or staying at home, and implementing certain workplace and educational institution controls (*1–4*). The Arizona Department of Health Services' (ADHS) recommendations for mitigating exposure to SARS-CoV-2 were informed by continual monitoring of patient demographics, SARS-CoV-2 community spread, and the pandemic's impacts on hospitals. To assess the effect of mitigation strategies in Arizona, the numbers of daily COVID-19 cases and 7-day moving averages during January 22–August 7, 2020, relative to implementation of enhanced community mitigation measures, were examined. The average number of daily cases increased approximately 151%, from 808 on June 1, 2020 to 2,026 on June 15, 2020 (after stay-at-home order lifted), necessitating increased preventive measures. On June 17, local officials began implementing and enforcing mask wearing (via county and city mandates),* affecting approximately 85% of the state population. Statewide mitigation measures included limitation of public events; closures of bars, gyms, movie theaters, and water parks; reduced restaurant dine-in capacity; and voluntary resident action to stay at home and wear masks (when and where not mandated). The number of COVID-19 cases in Arizona peaked during June 29–July 2, stabilized during July 3–July 12, and further declined by approximately 75% during July 13–August 7. Widespread implementation and enforcement of sustained community mitigation measures informed by state and local officials' continual data monitoring and collaboration can help prevent transmission of SARS-CoV-2 and decrease the numbers of COVID-19 cases.

ADHS supports surveillance and investigation efforts of local public health departments, compiles surveillance and investigation information across counties, and provides infrastructure statewide to support infectious disease surveillance. Data on laboratory-confirmed and probable (*5*) COVID-19 cases (based on the Council of State and Territorial Epidemiologists case definitions)† were collected in the centralized Medical Electronic Disease Surveillance Intelligence System (MEDSIS),§ which is used by state, tribal, and county public health agencies to report human-based diseases in Arizona. Information was submitted to or entered into MEDSIS by health care providers, laboratories, local health departments, tribal entities, and ADHS. Multiple laboratory tests submitted for a single patient were combined into a single record. Specimen collection date was used for confirmed cases, and symptom onset date was used for probable cases.

Temporal trends were examined by comparing the number of daily COVID-19 cases (as of September 1)¶ and 7-day moving averages before, during, and after implementation of enhanced community mitigation measures, defined as the following: limitations on persons' time away from their place of residence except for essential activities; certain business closures and service limitations (e.g., occupancy limitations, curbside pickup, and delivery of goods); enhanced sanitation practices**; social distancing, employee mask wearing, and symptom screenings for all businesses operating a physical location; limitations on the occurrence and size of public events; and local mandates enforcing mask use. The 7-day moving average was calculated after the cumulative case count exceeded 20 cases and is presented to describe COVID-19 trends.

On March 11, 2020, Arizona declared a public health state of emergency to prepare for, prevent, respond to, and mitigate the spread of SARS-CoV-2. Additional guidance was provided to local officials, businesses, communities, and individual persons to implement social distancing and close schools statewide

* Mandates and ordinances varied and were county- and city-specific. Enforcement types included educating persons on the dangers of COVID-19 spread, issuing fines to persons and businesses who refused to comply with mandates, and loss of licenses for businesses not enforcing rules or mandates.

† https://cdn.ymaws.com/www.cste.org/resource/resmgr/ps/positionstatement2020/Interim-20-ID-02_COVID-19.pdf.

§ https://azdhs.gov/preparedness/epidemiology-disease-control/infectious-disease-services/index.php#medsis-faqs.

¶ https://www.azdhs.gov/preparedness/epidemiology-disease-control/infectious-disease-epidemiology/covid-19/dashboards/index.php.

** Based on guidance from ADHS, CDC, and the Department of Labor, and Occupational Safety and Health Administration to limit and mitigate the spread of COVID-19, including promoting healthy hygiene practices; and intensifying cleaning, disinfection and ventilation practices.

(March 15); postpone and limit large gatherings to fewer than 50 persons; recommend telework options; restrict access to congregate settings; require restaurants to provide dine-out options only; and close all bars, gyms, and movie theaters in counties with confirmed COVID-19 cases (March 19) (Table). Based on Arizona data and CDC guidance (*1*,*2*), ADHS also recommended limiting persons' time away from their place of residence except for essential activities (i.e., stay-at-home order, "Stay Home, Stay Healthy, Stay Connected")[††] (March 31).

During April 1–May 15, the 7-day moving average of daily cases ranged from 154 to 443 (Figure). During April 29–May 11, Arizona initiated a phased approach for retail shops and stores, cosmetologists, and barbers to reopen and operate, and for restaurants to resume dine-in services; the stay-at-home order ended May 15.

Average daily cases increased 151% from June 1 (808) to June 15 (2,026), necessitating an increased focus on preventive measures by businesses, communities, and individual persons. Updated guidance from state officials provided local governments the authority to implement mask policies (June 17) and enforcement measures tailored to local public health needs (local policies were applicable to approximately 85% of the total Arizona population). Before June 17, mask wearing had not been widely mandated or enforced. Arizona limited organized public events to fewer than 50 persons (with some exceptions); closed bars, gyms, movie theaters, and water parks and recreational tubing facilities (June 29); and limited restaurants' indoor dining to <50% capacity, with at least 6 feet of separation between patrons (July 9). The 7-day moving average of daily cases peaked during June 29–July 2 (range = 4,148–4,377), stabilized during July 3–12 (range = 3,609–4,160), and subsequently decreased 75% from July 13 (3,506) to August 7 (867). Mitigation measures put in place in June were extended through August to further limit transmission.

Discussion

Quantitative data on the effectiveness of community mitigation measures at suppressing the spread of COVID-19 are limited. The primary goal of implementing widespread enhanced mitigation measures in Arizona was to protect and save lives and maintain capacity in the health care system. A combination of voluntary and enforceable measures is more effective than any single measure (*6*). Mitigation measures mandated through public policy can effectively increase social distancing (*7*), and wearing masks has prevented transmission of SARS-CoV-2 (*8*). In Arizona, decreases in daily COVID-19

[††] https://www.azdhs.gov/preparedness/epidemiology-disease-control/infectious-disease-epidemiology/index.php#novel-coronavirus-admin-orders; https://azgovernor.gov/executive-orders.

TABLE. Public policies to implement and enforce COVID-19 community mitigation measures and dates of issue/reissue* — Arizona, March 11–August 7, 2020

Mitigation measure	Date of issue/reissue
Declaration of emergency	Mar 11
School closure (on-site learning)	Mar 15
Limits on senior living facilities visitation	Mar 19
Expanded availability and coverage for telemedicine for persons, pets, and animals	Mar 25, Apr 1
Deferred requirements to renew driver license	Mar 20
Stay-at-home order	Mar 30–May 15
Business/Service closures	
Bars	Mar 19, Jun 29, Jul 23
Movie theaters	Mar 19, Jun 29, Jul 23
Indoor gyms and fitness clubs	Mar 19, Jun 29, Jul 23
Restaurants, on-site dining	Mar 19
Pools	Mar 19
Water parks and recreational tubing facilities	Jun 29, Jul 23
Business/Service limits (requirements)	
All businesses operating a physical location (enhanced sanitation,[†] social distancing, employee mask wearing, symptom screenings)	Jun 17
Retail (limited capacity, social distancing, enhanced sanitation)	Apr 29
Barbers and cosmetologists (employee mask wearing, spaced appointments, enhanced sanitation)	May 4
Restaurants (social distancing, limited capacity, employee mask wearing, patron mask wearing [when not eating or drinking], employee screening, enhanced sanitation)	May 4, Jul 9
Public pools (e.g., at hotels; limited capacity)	Jun 29, Jul 23
Private pools in public areas (e.g., multihousing complexes; limited capacity)	Jun 29, Jul 23
Public events (<50 persons)	Mar 15, Jun 29, Jul 23
Wearing masks (mandatory)	
Local officials able to mandate and enforce wearing masks	Jun 17
Yuma County	Jun 18
Maricopa County	Jun 19
Pima County	Jun 19
Santa Cruz County	Jun 19
Coconino County	Jun 20
>40 other cities/tribal communities	Jun 17–25[§]

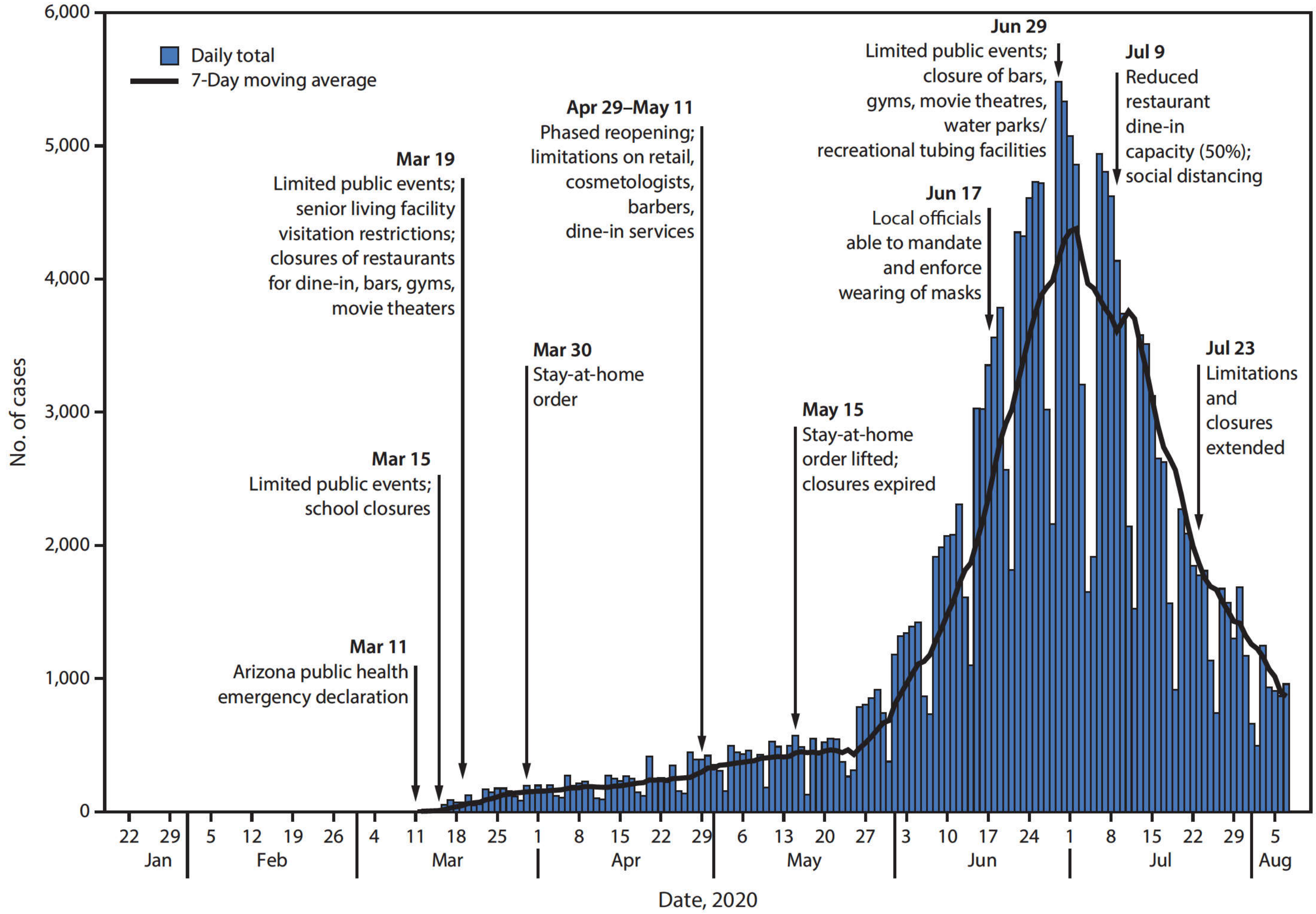
Abbreviation: COVID-19 = coronavirus disease 2019.

* Issue dates are the dates the issuing official signed the order implementing the mandatory mitigation measure. In some instances, mitigation measures were effective either immediately or within 1 to 3 days of issue. https://www.azdhs.gov/preparedness/epidemiology-disease-control/infectious-disease-epidemiology/index.php#novel-coronavirus-admin-orders; https://azgovernor.gov/executive-orders.

[†] Based on guidance from the Arizona Department of Health Services, CDC, Department of Labor, and Occupational Safety and Health Administration (OSHA) to limit and mitigate the spread of COVID-19 including promoting healthy hygiene practices; and intensifying cleaning, disinfection and ventilation practices.

[§] Other tribal communities with mask mandates (issued June 18–23) included Fort McDowell Yavapai Nation, Gila River Indian Community, Navajo Nation, Salt-River Pima-Maricopa Indian Community, Tohono O'Odham Nation. Other cities with mask mandates (issued June 17–25) included Avondale, Bisbee, Buckeye, Carefree, Casa Grande, Chandler, Clarkdale, Clifton, Coolidge, Cottonwood, Douglas, Flagstaff, Fountain Hills, Gila Bend, Gilbert, Glendale, Globe, Goodyear, Guadalupe, Jerome, Kingman, Litchfield Park, Mammoth, Mesa, Miami, Nogales, Oro Valley, Paradise Valley, Payson, Peoria, Phoenix, San Luis, Sedona, Scottsdale, Somerton, Superior, Surprise, Tempe, Tolleson, Tucson, Youngtown, Yuma. Several other tribal communities and cities encouraged but did not mandate wearing masks.

FIGURE. Selected community mitigation measures* and COVID-19 case counts† and 7-day moving averages§ — Arizona, January 22–August 7, 2020



Abbreviation: COVID-19 = coronavirus disease 2019.
* Issue dates are the dates the issuing official signed the order implementing the mandatory mitigation measure. In some instances, mitigation measures were effective either immediately or within 1 to 3 days of issue. https://www.azdhs.gov/preparedness/epidemiology-disease-control/infectious-disease-epidemiology/index.php#novel-coronavirus-admin-orders; https://azgovernor.gov/executive-orders.
† As of September 1, 2020. Specimen collection date was used for confirmed cases, and symptom onset date was used for probable cases.
§ Plotting of 7-day moving average began when cumulative case count exceeded 20 cases.

cases were observed after widespread sustained community mitigation measures that promoted social distancing, limited large gatherings, paused operations of businesses where mask use and social distancing were difficult to maintain, mandated and enforced mask wearing, and promoted voluntary resident actions to stay at home and wear masks (when and where not mandated). The number of COVID-19 cases stabilized and began to decrease approximately 2 weeks after local officials began mandating mask wearing (throughout several counties and cities) and enhanced sanitation practices. Additional declines in case counts were associated with implementation of statewide limitations and closures sustained throughout July and extended into August.

The findings in this report are subject to at least four limitations. First, the relationship between mitigation measures and changes in case counts are temporal correlations and should not be interpreted to infer causality. Other factors that might have influenced the rate of change (e.g., travel restrictions, neighboring state mitigation measures, and individual choices to reduce movement before implementation of mandates) cannot be ruled out. Second, health centers run by tribal entities and federal health facilities (i.e., Indian Health Service, Veteran's Administration, and Department of Defense) in the state are requested but not required to comply with state reporting rules. Many of these health centers and federal health facilities complied with reporting, but the completeness of reporting by these entities is unknown. Third, adherence to

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Summary

What is already known about this topic?

Community mitigation measures can help slow the spread of COVID-19.

What is added by this report?

The number of COVID-19 cases in Arizona stabilized and then decreased after sustained implementation and enforcement of statewide and locally enhanced mitigation measures, beginning approximately 2 weeks after implementation and enforcement of mask mandates and enhanced sanitations practices began on June 17; further decreases were observed during July 13–August 7, after statewide limitations and closures of certain services and businesses.

What are the implications for public health practice?

Widespread implementation and enforcement of sustained community mitigation measures, including mask wearing, informed by state and local officials' continual data monitoring and collaboration can help prevent transmission of SARS-CoV-2 and decrease the numbers of COVID-19 cases.

mitigation measures was not assessed, nor could the extent to which each individual measure affected the number of incident COVID-19 cases be established. Finally, Arizona might not be representative of other U.S. states, and community mitigation measures might have a different impact in more populous or densely populated states; thus, these findings are not necessarily generalizable to other settings.

Enhanced mitigation measures should be implemented by communities and persons to slow COVID-19 spread, particularly before a vaccine or therapeutic treatment becomes widely available. State, local, and tribal officials are best positioned to continually monitor data and collaborate to determine the level and types of enhanced mitigation required. Mitigation measures, including mask mandates, that are implemented and enforced statewide appear to have been effective in decreasing the spread of COVID-19 in Arizona.

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[1]Arizona Department of State Health Services; [2]CDC COVID-19 Response Team.

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By Wei Lyu and George L. Wehby

Community Use Of Face Masks And COVID-19: Evidence From A Natural Experiment Of State Mandates In The US

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ABSTRACT State policies mandating public or community use of face masks or covers in mitigating the spread of coronavirus disease 2019 (COVID-19) are hotly contested. This study provides evidence from a natural experiment on the effects of state government mandates for face mask use in public issued by fifteen states plus Washington, D.C., between April 8 and May 15, 2020. The research design is an event study examining changes in the daily county-level COVID-19 growth rates between March 31 and May 22, 2020. Mandating face mask use in public is associated with a decline in the daily COVID-19 growth rate by 0.9, 1.1, 1.4, 1.7, and 2.0 percentage points in 1–5, 6–10, 11–15, 16–20, and 21 or more days after state face mask orders were signed, respectively. Estimates suggest that as a result of the implementation of these mandates, more than 200,000 COVID-19 cases were averted by May 22, 2020. The findings suggest that requiring face mask use in public could help in mitigating the spread of COVID-19.

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One of the most contentious issues being debated worldwide in the response to the coronavirus disease 2019 (COVID-19) pandemic is the value of wearing masks or face coverings in public settings.[1] A key factor fueling the debate is the limited direct evidence thus far on how much widespread community use would affect COVID-19 spread. However, there is now substantial evidence of asymptomatic transmission of COVID-19.[2,3] For example, a recent study of antibodies in a sample of customers in grocery stores in New York State reported an infection rate of 14.0 percent by March 29 (projected to represent more than 2.1 million cases), which substantially exceeds the number of confirmed COVID-19 cases.[4] Moreover, all public health authorities call on symptomatic people to wear masks to reduce transmission risk. Even organizations that at the time of our study had not yet recommended widespread community use of face masks for COVID-19 mitigation (that is, everyone without symptoms should use a face mask outside of their home), such as the World Health Organization, strongly recommend that symptomatic individuals wear them.[5] Because mask wearing by infected people can reduce transmission risk, and because of the high proportion of asymptomatic infected individuals and transmissions, there appears to be a strong case for the effectiveness of widespread use of face masks in reducing the spread of COVID-19. However, there is no direct evidence thus far on the magnitude of such effects, especially at a population level.

Researchers have been reviewing evidence from previous randomized controlled trials for other respiratory illnesses, examining mask use and types among people at higher risk of contracting infections (such as health care workers or people in infected households). Systematic reviews and meta-analyses of such studies have provided suggestive, although generally weak, evidence.[6] The estimates from the meta-analyses based on randomized controlled trials suggest declines in transmission risk for influenza or influenza-like illnesses to mask wearers, although estimates are mostly statistically insignificant possibly because of small sample sizes or design limitations, especially those related to assessing compliance.[7–9] There is also a relationship between increased adherence to mask use, specifically, and effectiveness of reducing transmission to mask wearers: In one randomized study of influenza transmission in infected households in Australia, transmission risk for mask wearers was lower with greater adherence.[10] Further, the evidence is mixed from randomized studies on types of masks and risk for influenza-like illness transmission to mask wearers; for example, a recent systematic review and meta-analysis comparing N-95 respirators versus surgical masks found a statistically insignificant decline in influenza risk with N-95 respirators.[11]

Positions on widespread face mask use have differed worldwide but are changing over time. In the US, public health authorities did not recommend widespread face mask use in public at the start of the pandemic. The initially limited evidence on asymptomatic transmission and concern about mask shortages for the health care workforce and people caring for patients contributed to that initial decision. On April 3, 2020, the Centers for Disease Control and Prevention (CDC) issued new guidance advising everyone to wear cloth face covers in public areas where close contact with others is unavoidable, citing new evidence on virus transmission from asymptomatic or presymptomatic people.[12] Guidelines differ between countries, and some, including Germany, France, Italy, Spain, China, and South Korea, have mandated the use of face masks in public.[13–16]

This study adds complementary evidence to the literature on the impacts of widespread community use of face masks on COVID-19 spread from a natural experiment based on whether or not US states had mandated the use of face masks in public for COVID-19 mitigation as of May 2020. Fifteen states plus Washington, D.C., issued mandates for face mask use in public between April 8 and May 15.

We identified the effects of state mandates for the use of face masks in public on the daily COVID-19 growth rate, using an event study that examined the effects over different periods. We considered the impact of mandates for mask use targeted only to employees in some work settings, as opposed to communitywide mandates. This evidence is critical, as states and countries worldwide begin to shift to "reopening" their economies and as foot traffic increases. Mandating the public use of masks has become a socially and politically contentious issue, with multiple protests and even acts of violence directed against masked employees and those asking customers to wear face masks.[17] Face cover recommendations and mandates are part of the current set of measures, following earlier social distancing measures such as school and nonessential business closures, bans on large gatherings, and shelter-in-place orders being considered by states and local governments, especially as regions of the country reopen. For example, during Virginia's phase one reopening, begun May 22, 2020, everyone in the state was required to wear a face mask in public where people congregate.[18] Even though more states have issued such orders since the study was completed, it is critical to provide direct evidence on this question not only for public health authorities and governments but also for educating the public.

Study Data And Methods

DATA We collected information on statewide face cover mandate orders from public data sets on such policies and from searching and reviewing all state orders issued between April 1 and May 21, 2020. Our study focused on state executive orders or directives signed by governors that mandate use. Recommendations or guidelines from state departments of public health were not included, as these largely follow the CDC guidelines and might not necessarily add further information or impact. See online appendix A for a more detailed description of the data sources and measuring of the mandates.[19]

States differ in whether or not they require their citizens to wear face masks (covers) to limit COVID-19 spread. Between April 8 and May 15, governors of fifteen states and the mayor of Washington, D.C., signed orders mandating all individuals who can medically tolerate the wearing of a face mask do so in public settings (for example, public transportation, grocery stores, pharmacies, or other retail stores) where maintaining six feet of "social distance" might not always be practicable. These sixteen jurisdictions also have specific mandates requiring employees in certain professions to wear masks at all times while working.

In addition to these sixteen jurisdictions, twenty additional states have employee-only mandates (but no community mandate) requiring that some employees (for example, close-contact service providers such as in barber shops and nail salons) wear a face mask at all times while providing services. The face mask defined in these orders primarily refers to cloth face coverings or nonmedical masks. The state orders strongly discourage the use of any medical or surgical masks and N-95 respirators, which should be reserved for health care workers and first responders. The orders also clearly specify that the face masks are not a replacement for any other social distancing protocols. More information on dates and links to these state orders are in appendix exhibit A1 and appendices D and E.[19] Fifteen states had not yet issued community or employee mandates when we performed the study.

The main model used publicly available daily county-level data of confirmed COVID-19 cases from March 25 through May 21.[20] The data covered all states plus Washington, D.C., and the analytical sample included 2,930 unique counties plus New York City (five boroughs combined). See appendix A for a more detailed description of COVID-19 data.[19]

STATISTICAL ANALYSIS We employed an event study, which is generally similar to a difference-in-differences design, to examine whether statewide mandates to wear face masks in public affect the spread of COVID-19 based on the state variations noted earlier. This design allowed us to estimate the effects in the context of a natural experiment, comparing the pre-post mandate changes in COVID-19 spread in the states with mandates versus changes in COVID-19 spread in the states that did not pass these mandates, over time. The model also tested whether states issuing these mandates had differential pre-event trends in COVID-19 rates before they were issued. This is a critical assumption of the validity of an event study that must be upheld under testing. In addition, the model allowed us to control for a wide range of time-invariant differences between states and counties, such as population density and socioeconomic and demographic factors, plus time-variant differences between states and counties, such as other mitigation and social distancing policies, in addition to state-level COVID-19 testing rates.

We estimated the effects of face cover mandates on the daily county-level COVID-19 growth rate, which is the difference in the natural log of cumulative COVID-19 cases on a given day minus the natural log of cumulative cases in the prior day, multiplied by 100.[21] This measure gives the daily growth rate in percentage points.

The reference period for estimating the face cover mandate effects was 1–5 days before signing the order. We examined how effects change over five post-event periods: 1–5, 6–10, 11–15, 16–20, and 21 or more days. The model also tested for pre-event trends over the course of 6–10, 11–15, and 16 or more days before signing the mandate. For all counties in the analytical sample, the main model included daily data from March 31 (seven days before the first state signed a face cover mandate) through May 22. The models were estimated by least squares weighted by the county's 2019 population with heteroscedasticity-robust and state-clustered standard errors.

As noted earlier, all of the fifteen states plus Washington, D.C., that mandated face cover use in public also mandated employee mask use. To assess the effects of employee face cover mandates, we employed another event study model that focused solely on the employee face cover mandate as the policy intervention. In this analysis, we excluded the sixteen jurisdictions that enacted both public and employee face cover mandates and focused on the twenty states that enacted an employee-only mandate and the fifteen states with neither a public nor an employee mandate.

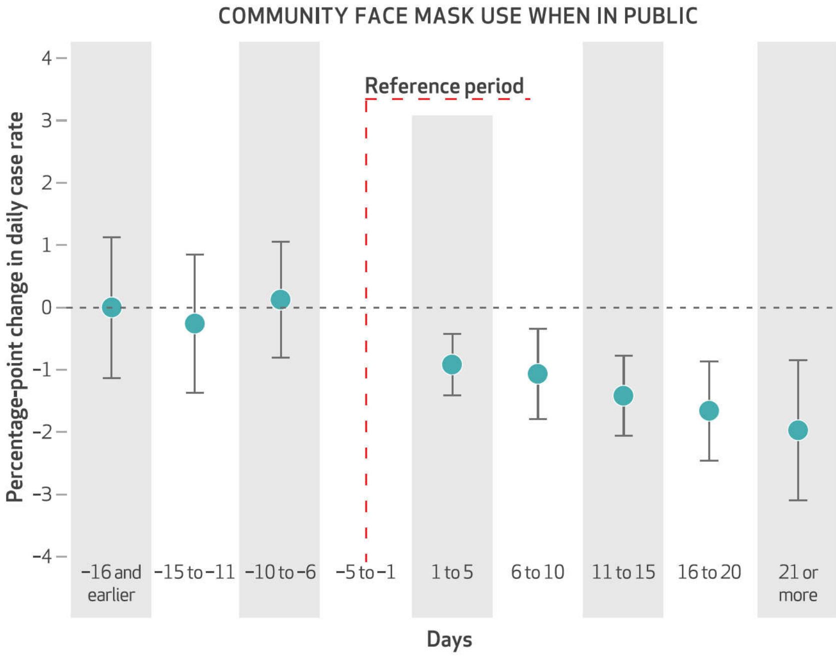
LIMITATIONS We were unable to measure face cover use in the community (that is, compliance with the mandate). As such, the estimates represent the intent-to-treat effects of these mandates—that is, their effects as passed and not the individual-level effect of wearing a face mask in public on one's own COVID-19 risk. Related, we did not measure enforcement of the mandates, which might affect compliance. We also did not have data on county-level mandates for wearing face masks in public. In some states without state-level mandates at the time of our study, such as California,[22] Texas,[23] and Colorado,[24] multiple counties had enacted such mandates. These county-level mandates did not bias the intent-to-treat estimates of effects of state-level mandates as actually passed, but they added local-level heterogeneity not directly accounted for in the model. We did examine the robustness of estimates to the exclusion of some of these states. Finally, we were able to examine only confirmed COVID-19 cases. However, there is evidence of a higher infection rate in the community than is reflected in the number of confirmed cases.[25]

Study Results

EFFECTS OF MANDATES FOR FACE COVERING IN PUBLIC Exhibit 1 plots the event study estimates of effects of state mandates for community face covering in public on the county-level daily growth rate of COVID-19 cases, with 95 percent confidence intervals, obtained from the main regression model (in appendix B),[19] using county-level daily data from March 31 through May22; appendix exhibit C1 (column 1) reports the exact estimates. The effects are shown over the course of five periods after signing the orders, relative to the five days before signing (which is the reference period). Also shown

EXHIBIT 1

Event study estimates of the effects of states mandating community face mask use in public on the daily county-level growth rate of COVID-19 cases, 2020



SOURCE Authors' analysis of US county-level COVID-19 case data between March 31 and May 22, 2020. **NOTES** Event study estimates (dots) and 95% confidence intervals (bars) of the effects of states mandating community use of face covers or masks when people are in public on the county-level daily growth rate of COVID-19 cases over different periods before and after the mandate order was signed. The reference period was the first five days before the mandate order was signed. The model controlled for major COVID-19 mitigation policies as time-varying (closure of K–12 schools, county-level or statewide shelter-in-place orders, nonessential business closure, closure of restaurants for dining in, closure of gyms or movie theaters), COVID-19 tests per 100,000 people, county fixed effects, and day fixed effects. The model was estimated by least squares weighted by the county 2019 population, and the standard errors were robust to heteroscedasticity and clustered at the state level.

are estimated differences in daily COVID-19 growth rates between states with and without the mandates over the course of three periods before the reference period.

There was a significant decline in daily COVID-19 growth rate after the mandating of face covers in public, with the effect increasing over time after the orders were signed. Specifically, the daily case rate declined by 0.9, 1.1, 1.4, 1.7, and 2.0 percentage points within 1–5, 6–10, 11–15, 16–20, and 21 or more days after signing, respectively. All of these declines were statistically significant ($p < 0.05$ or less). In contrast, the pre-event trends in COVID-19 case growth rates were small and statistically insignificant.

We also projected the number of averted COVID-19 cases with the mandates for face mask use in public by comparing actual cumulative daily cases with daily cases predicted by the model if none of the states had enacted the public face cover mandate at the time they did (see details in appendix B).[19] The main model estimates suggested that because of these mandates, 230,000–450,000 cases may have been averted by May 22. Estimates of averted cases should be viewed cautiously and only as general approximations.

ROBUSTNESS CHECKS We estimated multiple extensions of the main event study model to assess the robustness of estimates to different model specifications and sample choices. These checks started the event study on March 26; added flexible controls for social distancing measures, state reopening measures, employee face mask use mandates, and county-specific time trends; and allowed time trends to vary by socio-demographic indicators. Other checks used the mandate effective date instead of the signing date, used hyperbolic sine transformation to account for zero cases, included states as the unit instead of counties, included only urban counties, and excluded some states without state-level mandates but with multiple counties having local mandates. The detailed description and results of these robustness checks are in appendix C.[19] The results were robust across these checks; effects were smaller when we used the effective dates instead of the signing dates, which differ by about two to three days, on average, suggesting earlier compliance, and when we used states as the unit of analysis. But the estimates remained meaningful and statistically significant in all checks.

EFFECTS OF EMPLOYEE-ONLY FACE COVER MANDATES As noted earlier, we also directly assessed the effects of states mandating only that certain employees wear face masks. Twenty states issued employee use mandates but not community use mandates. We reestimated the event study model described earlier for an employee-only mandate including those twenty states (issued between April 17 and May 9) and the fifteen states without mandates, and excluding the sixteen jurisdictions that issued both public and employee use mandates. Exhibit 2 plots the event study estimates of changes in county-level daily COVID-19 growth rates with the employee-only face cover mandates and their 95 percent confidence intervals. All pre- and postmandate estimates were small and insignificant. Overall, these results indicate no evidence of declines in daily COVID-19 growth rates with employee-only mandates.

Discussion

Around the world, governments have been fighting COVID-19 spread through a mix of policies and mitigation measures such as school and nonessential business closures and shelter-in-place orders. Some countries have also recommended or mandated widespread community use of face masks as a mitigation measure. However, the effectiveness of this measure is highly debated. The debate and uncertainty are fueled by the limited direct empirical evidence available on the magnitude of the effects of widespread face mask use in public on COVID-19 mitigation. There is a critical need for empirical evidence on the magnitude of these effects from natural experiments.[8] This evidence is especially relevant as governments reopen their economies and loosen social distancing restrictions while new infections continue to occur and while there is no vaccine or widely accessible or effective treatments in sight.

The study provides direct evidence on the effectiveness of widespread community use of face masks from a natural experiment that evaluated the effects of state government mandates in the US for face mask use in public on COVID-19 spread. Fifteen states plus Washington, D.C., mandated face mask use between April 8 and May 15. Using an event study that examined daily changes in county-level COVID-19 growth rates, the study found that mandating public use of face masks was associated with a reduction in the COVID-19 daily growth rate. Specifically, we found that the average daily county-level growth rate decreases by 0.9, 1.1, 1.4, 1.7, and 2.0 percentage points in 1–5, 6–10, 11–15, 16–20, and 21 or more days after signing, respectively.

These estimates are not small; they represent nearly 16 percent to 19 percent of the effects of other social distancing measures (school closures; bans on large gatherings; shelter-in-place orders; and closures of restaurants, bars, and entertainment venues) after similar periods from their enactment.[21] The estimates suggest that the effectiveness of and benefits from these mandates increase over time. By May 22, 2020, the estimates suggest that 230,000–450,000 COVID-19 cases may have been averted on the basis of when states passed these mandates. Again, the estimates of averted cases should be viewed cautiously, as they are sensitive to assumptions and different approaches to transforming the changes in the daily growth rate estimates to cases.

The early declines in the daily growth rate over the course of five days after signing the order are broadly consistent with the timing of the effects of other social distancing measures such as business closures.[21] Although the median incubation period is estimated to be around five days,[26] there is a wide range from 2.2 days (2.5th percentile) to 11.5 days (97.5th percentile), which suggests that for many people, symptoms may appear relatively early. Further, people may become aware of the mandates early through governors' briefings and related media reports, or they may be anticipating them.

There is no evidence of differential pre-mandate COVID-19 trends with respect to issuing these mandates. The estimates represent the intent-to-treat effects of the statewide face cover mandates as passed, conditional on other national and local measures. In that way, the effects are independent of the CDC national guidance to wear face masks that was issued April 3, 2020.[12] These effects were robust to several model checks. The study provides evidence from a natural experiment on the effectiveness of mandating public use of face masks in mitigating the spread of COVID-19. We found no evidence for effects of states mandating employee face mask use, perhaps because many businesses themselves already required their employees to wear masks.[27,28] In that case, mandating employee mask use reinforce what many businesses already choose to do on their own.

Although the intent-to-treat estimates are of interest for understanding the effectiveness of

EXHIBIT 2

Event study estimates of effects of states mandating only employee use of face masks during working time on daily county-level growth rate of COVID-19 cases

SOURCE Authors' analysis of US county-level COVID-19 case data between March 31 and May 22, 2020. **NOTES** Event study estimates (dots) and 95% confidence intervals (bars) of the effects of states mandating employee use of face covers or masks on the county-level daily growth rate of COVID-19 cases over different periods before and after the mandate order was signed. This model excluded fifteen states plus Washington, D.C., that made the use of face covering mandatory for both the general public and employees. The reference period was the first five days before the mandate order was signed. The model controlled for major COVID-19 mitigation policies as time-varying (closure of K–12 schools, county-level or statewide shelter-in-place orders, nonessential business closure, closure of restaurants for dining in, and closure of gyms or movie theaters), COVID-19 tests per 100,000 people, county fixed effects, and day fixed effects. The model was estimated by least squares weighted by the county 2019 population, and the standard errors were robust to heteroscedasticity and clustered at state level.

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these policies in limiting COVID-19 spread at the community and population levels, understanding how their effects change with compliance and enforcement strategies is important for designing effective policies. Our study has built the first step in estimating the overall effect of these policies as enacted. However, these policies vary in their strictness and the consequences of noncompliance. The mandates generally require wearing a face mask in public whenever the social distance cannot be maintained. States such as Delaware, Maryland, Massachusetts, and Maine clarify what "public" areas are (for example, indoor space in retail establishments, outdoor space in busy parking lots and waiting areas for take-out services, semi-enclosed areas such as at public transportation stops, and enclosed spaces such as in taxis and other public transportation). The language on enforcement and penalties for noncompliance also vary. In states such as Delaware, Hawaii, Maryland, and Massachusetts, the face mask orders state that they have the force and effect of law, with a willful violation subject to a criminal offense with penalties. For example, the order in Maryland states that "a person who knowingly and willfully violates this order is guilty of a misdemeanor and on conviction is subject to imprisonment not exceeding one year or a fine not exceeding $5,000 or both."[29] In contrast, the orders of other states such as Connecticut, Maine, and Pennsylvania, although clearly mandating the wearing of a face mask in public, do not appear to clearly specify that violations of the order are subject to criminal offense or penalties. Future work should examine whether and how differences in strictness and enforcement modify the effects of these mandates.

Compliance and enforcement may also differ across contextual factors (such as other social distancing measures, workforce distribution, population demographics, and socioeconomic and cultural factors). In that regard, it is important to clarify that the suggested benefits from mandating face mask use are not substitutes for other social distancing measures; the effects are conditional on the other enacted social distancing measures and how communities are complying with them. It is also important to extend the evidence into additional measures of exposure to the virus in the community as data become available, such as from serological testing for antibodies. Finally, future work can examine effects on deaths, which lag cases and change not only with the number of cases but also with case severity.

Conclusion

The study provides evidence that US states mandating the use of face masks in public had a greater decline in daily COVID-19 growth rates after issuing these mandates compared with states that did not issue mandates. These effects were observed conditional on other existing social distancing measures and were independent of the CDC recommendation to wear face covers issued April 3, 2020. As international and state governments begin to relax social distancing restrictions, and considering the high likelihood of a second COVID-19 wave in the fall and winter of 2020,[30] requiring the use of face masks in public could help in reducing COVID-19 spread. ■

An unedited version of this article was published online June 16, 2020, as a Fast Track Ahead Of Print article. That version is available in the online appendix.

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Face Masks and GDP

New US coronavirus cases have risen sharply in recent weeks, leading investors to worry that renewed lockdowns will again depress economic activity. But since the first infection wave in March and April, it has become clear that broad lockdowns are not the only way to lower virus transmission, and many governments have started to require the wearing of face masks in public settings. Should the United States follow suit with a national mandate? This is inherently a political decision, but we can use our analytical tools to answer three questions that are relevant to it. First, how effective is a face mask mandate in increasing face mask usage? Second, does increased face mask usage lower virus transmission, and if so by how much? And third, how economically valuable is a face mask mandate in terms of reducing the need for broad lockdowns with their well-documented negative effects on GDP?

VIEW VIDEO: Jan Hatzius, head of Goldman Sachs Research and the firm's chief economist, explores the link between face masks and coronavirus outcomes, and the economic value of a national face mask mandate in reducing the need for broad lockdowns.

- The sharp increase in confirmed coronavirus cases in the US Sun Belt has led investors to worry about renewed broad lockdowns with large negative effects on GDP. But there are also other ways to reduce infections, including stringent bans on large gatherings and greater use of face masks.
- In particular, we argue that a national face mask mandate could partially substitute for renewed lockdowns. We start by showing that a national mandate would likely increase face mask usage meaningfully, especially in states such as Florida and Texas where masks remain largely voluntary to date.
- We then investigate the link between face masks and coronavirus outcomes. Our analysis includes 1) a US regional panel in which we relate the growth rate of infections and fatalities to the introduction of state face mask mandates, 2) a large country-level cross section in which we relate cumulative infections and fatalities to the lag between

the onset of spread and the introduction of a face mask mandate, and 3) a smaller country-level panel in which we relate the growth rate of infections and fatalities to lagged mask usage.

- We find that face masks are associated with significantly better coronavirus outcomes. Since this is true across all three of our models and the results are robust to the inclusion of a number of control variables, it seems to reflect a largely causal impact of masks rather than correlation with other factors (such as reduced mobility or avoidance of large gatherings). Our baseline estimate is that a national mandate could raise the percentage of people who wear masks by 15pp and cut the daily growth rate of confirmed cases by 1.0pp to 0.6%.
- Finally, we translate our results into GDP terms by asking how much our Effective Lockdown Index (ELI) would need to increase in order to cut infections by as much as a national mask mandate, and then converting the ELI impact into a GDP impact using the estimated cross-country relationship between the two. These calculations imply that a face mask mandate could potentially substitute for lockdowns that would otherwise subtract nearly 5% from GDP.

Face Masks and GDP [1]

New US coronavirus cases have risen sharply in recent weeks, with most of the deterioration concentrated in the "Sun Belt," including Florida, Texas, Arizona, and California. This has led investors to worry that renewed lockdowns will again depress economic activity. By our estimates, the increase in our Effective Lockdown Index (ELI)—a combination of official restrictions and actual social distancing data—subtracted 17% from US GDP between January and April, and other countries with even more aggressive restrictions saw even larger economic effects.

Since the first infection wave in March and April, however, it has become clear that broad lockdowns are not the only way to lower virus transmission significantly. For one thing, public health experts have long believed that bans on large gatherings can bring disproportionate benefits. This belief has only grown with a multitude of studies documenting the importance of "super spreader" events, such as those associated with the Shinjeonji Church in South Korea, the Austrian ski resort Ischgl, various European soccer matches, and the celebrations in New Orleans for Mardi Gras.

A more abrupt shift has occurred in the official view on face masks. As late as March 30, the World Health Organization advised that there was "no specific evidence to suggest that the wearing of masks by the mass population has any potential benefit."[2] Since then, however, the public health community's thinking has changed dramatically and many governments have started to require the wearing of face masks.

Should the United States follow these countries and adopt a national face mask mandate? This is inherently a political decision, but we can use our analytical tools to answer three questions that are relevant to it. First, how effective is a face mask mandate in increasing face mask usage? Second, does increased face mask usage lower virus transmission, and if so by how much? And third, how economically valuable is a face mask mandate in terms of reducing the need for broad lockdowns with their well-documented negative effects on GDP?

Face Mask Mandates and Usage

At present, the United States is among the less restrictive countries with respect to face mask mandates. The federal government did issue a national "recommendation" to wear masks in public settings in April, and many state and local governments have taken more stringent measures. However, a recommendation is not a mandate and the governors of both Florida and Texas—the two most heavily affected large states—recently reiterated their opposition to a statewide mask mandate. By contrast, many European countries now have national mask mandates in place, as shown in Exhibit 1, and much of East Asia has strong social norms of mask wearing when sick and during pandemics.

Exhibit 1: The US Is Among the Less Restrictive Economies with Respect to Face Mask Mandates

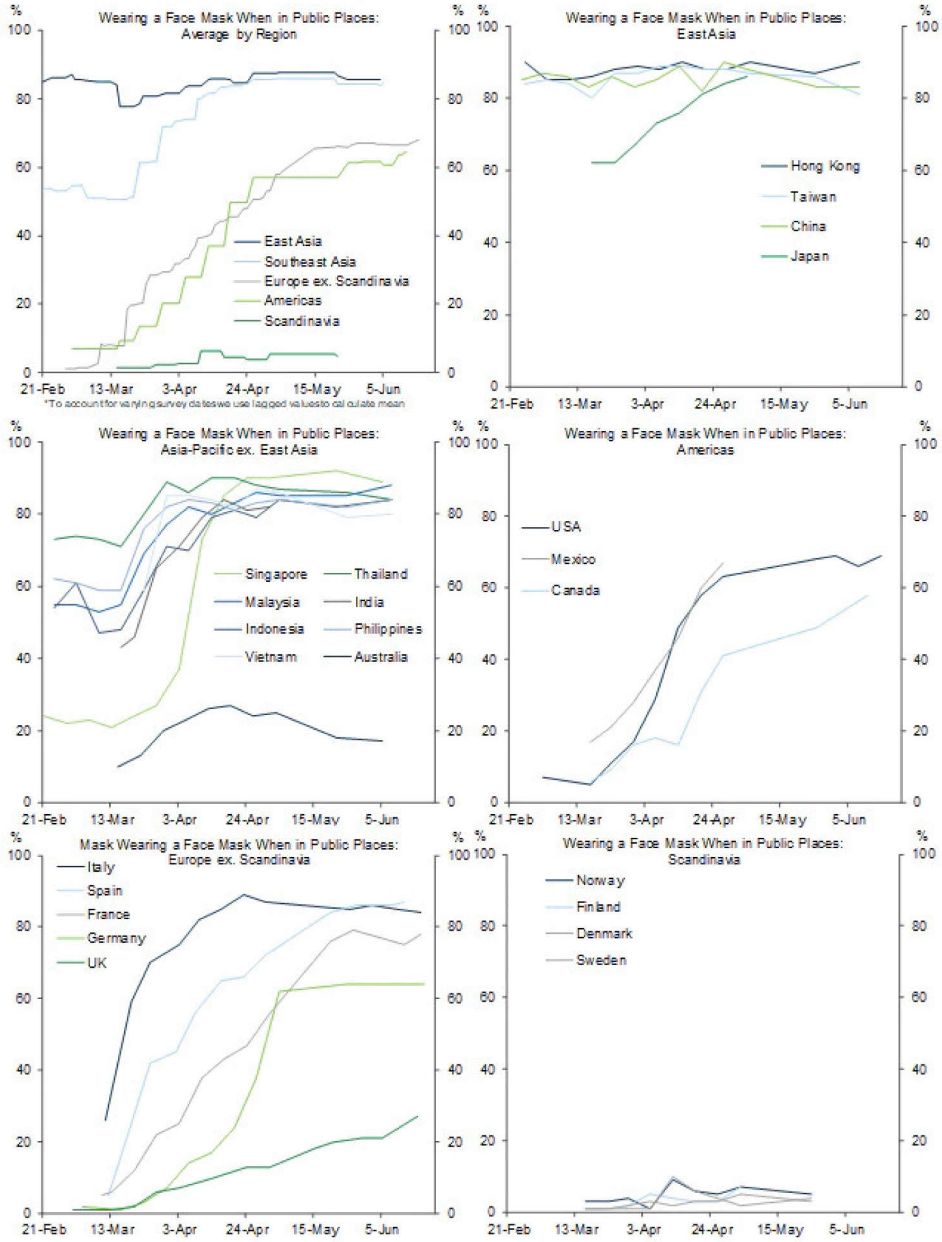
Economy	Region	Mask Policy*	Policy Details*	Date Implemented*
China	East Asia	National Norm/Universal Mask Usage		
Hong Kong	East Asia	National Norm/Universal Mask Usage		
South Korea	East Asia	National Norm/Universal Mask Usage		
Japan	East Asia	National Norm/Universal Mask Usage		
Singapore	Southeast Asia	National Mandate	Everywhere in Public	14-Apr-20
Germany	Europe	National Mandate	Public Transport & Stores	27-Apr-20
India	South Asia	National Mandate	Everywhere in Public	1-May-20
Italy	Europe	National Mandate	Public Transport & Stores	4-May-20
France	Europe	National Mandate	Public Transport & Schools & Stores	11-May-20
Mexico	Americas	National Mandate	Public Transport	20-May-20
UK	Europe	National Mandate	Public Transport	15-Jun-20
Spain	Europe	National Mandate	Public Transport & Stores	21-Jun-20
Brazil	Americas	Regional Mandate	Belo Horizonte, Federal District, Rio Grande do Sul, Rio de Janeiro, Salvador***	
Russia	Central Asia	Regional Mandate	Moscow, St. Petersburg***	
US	Americas	Regional Mandate	CA, CT, DE, DC, HI, IL, KY, ME, MD, MA, MI, NV, NJ, NM, NY, NC, PA, RI, UT, VA, WA	
Switzerland	Europe	National Recommendation	Public Transport & Stores	22-Apr-20
Canada	Americas	National Recommendation	Public Settings**	20-May-20
Australia	Oceania	None		
New Zealand	Oceania	None		
Norway	Scandinavia	None		
Sweden	Scandinavia	None		

* Based on information on MASKS4ALL Website (https://masks4all.co) and Leffler et al. (2020) study.
** Where social distancing is not possible.
*** List not exhaustive.

Source: masks4all.co, Leffler et al. 2020, Goldman Sachs Global Investment Research

What about actual mask usage? In this respect, the US scores somewhat better than one might expect, at least when looking at the national self-reported average. As shown in Exhibit 2, the share of respondents saying that they wear a face mask in public is nearly 90% in East Asia, 80% in Southern Europe, just below 70% in the US and Germany, 30% in the UK, and as low as 10% in Scandinavia. Most countries, including the US, have seen large increases in self-reported mask usage since the start of the pandemic.

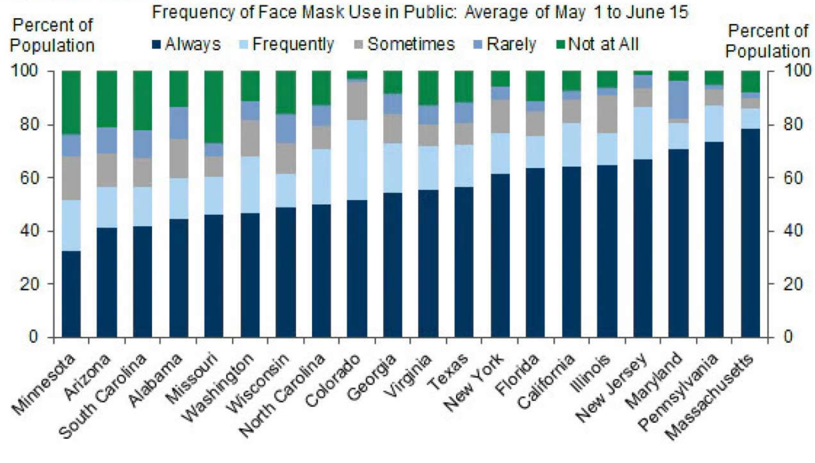
Exhibit 2: The Percentage of People Saying That They Wear a Face Mask in Public Ranges from Less than 10% in Scandinavia to Nearly 90% in East Asia



Source: YouGov, Goldman Sachs Global Investment Research

However, the national data don't tell the full story. As shown in Exhibit 3, face mask usage is highest in the Northeast, where the virus situation has improved dramatically in recent months, and generally lower in the South, where the numbers have deteriorated.[3] For example, only about 40% of respondents in Arizona say that they "always" wear face masks in public, compared with nearly 80% in Massachusetts.

Exhibit 3: The Share "Always" Wearing a Face Mask in Public Ranges from Around 40% in Minnesota and Arizona to 80% in Massachusetts



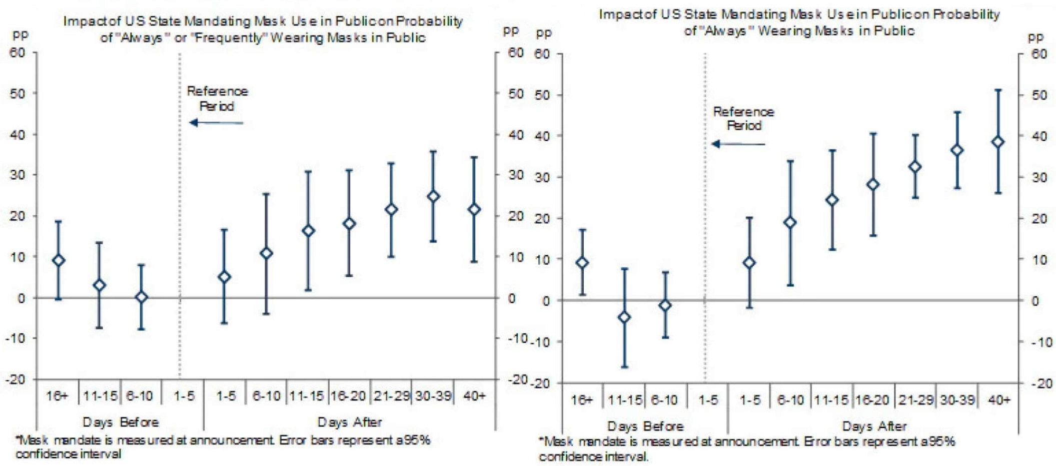
Source: YouGov, Goldman Sachs Global Investment Research

How effective would a national mask mandate be in pushing mask usage to Southern European or East Asian levels? To investigate this, we turn to a statistical event study that relates the adoption of mask mandates across US states to subsequent changes in self-reported mask usage.

We analyze the impact of face mask mandates issued by 20 US states plus DC between April 8 and June 24 in a state panel. We collect the announcement dates of mask mandates from a study in Health Affairs by Wei Lyu and George Wehby and construct statewide time series of face mask usage outside the home using YouGov Covid-19 Behaviour Tracker respondent-level data. We regress state-level mask usage on various event time dummies around the announcement and include state fixed effects and time fixed effects.[4]

Exhibit 4 shows our estimates of a large and highly significant impact of mandates on mask usage. We estimate that statewide mask mandates gradually raise the percentage of people who "always" or "frequently" wear masks by around 25pp in the 30+ days after signing (left panel). The percent of respondents who "always" wear masks rises by nearly 40pp 30+ days after, reflecting some people switching from "frequently" and other categories to "always" (right panel).

Exhibit 4: Mask Mandates Raise the Percentage of People Who "Always" or "Frequently" Wear Masks by Around 25pp in the 30+ Days After Signing



Source: YouGov, Goldman Sachs Global Investment Research

Exhibit 4 suggests that a national mask mandate could increase US face mask usage by statistically significant and economically large amounts, especially in states such as Florida and Texas that currently don't have a comprehensive mandate and are seeing some of the worst outbreaks. Specifically, we estimate that a national mandate would increase the national average share of people who "always" or "frequently" wear masks by 15pp. This estimate is based on two assumptions. First, we assume that states that currently don't have a mandate—which account for 50% of the population—experience a 25pp rise in mask usage in line with the average response to statewide mandates. Second, we assume that states which already have a state mandate see a 5pp increase in mask usage because of increased focus on the issue.

Face Masks and Virus Outcomes

Approach 1: US County Panel

Does increased face mask usage lower virus transmission, and if so by how much? To investigate this, we turn to three statistical approaches relating face mask usage and mandates to virus spread and fatalities.

Our first approach extends our event study analysis of US state-level mandates to the impact on the growth rate of infections and fatalities. Specifically, we regress county-level growth rates of infections and fatalities on event time dummies around the announcement and control for state fixed effects, time fixed effects, and a rich set of county-level controls.[5]

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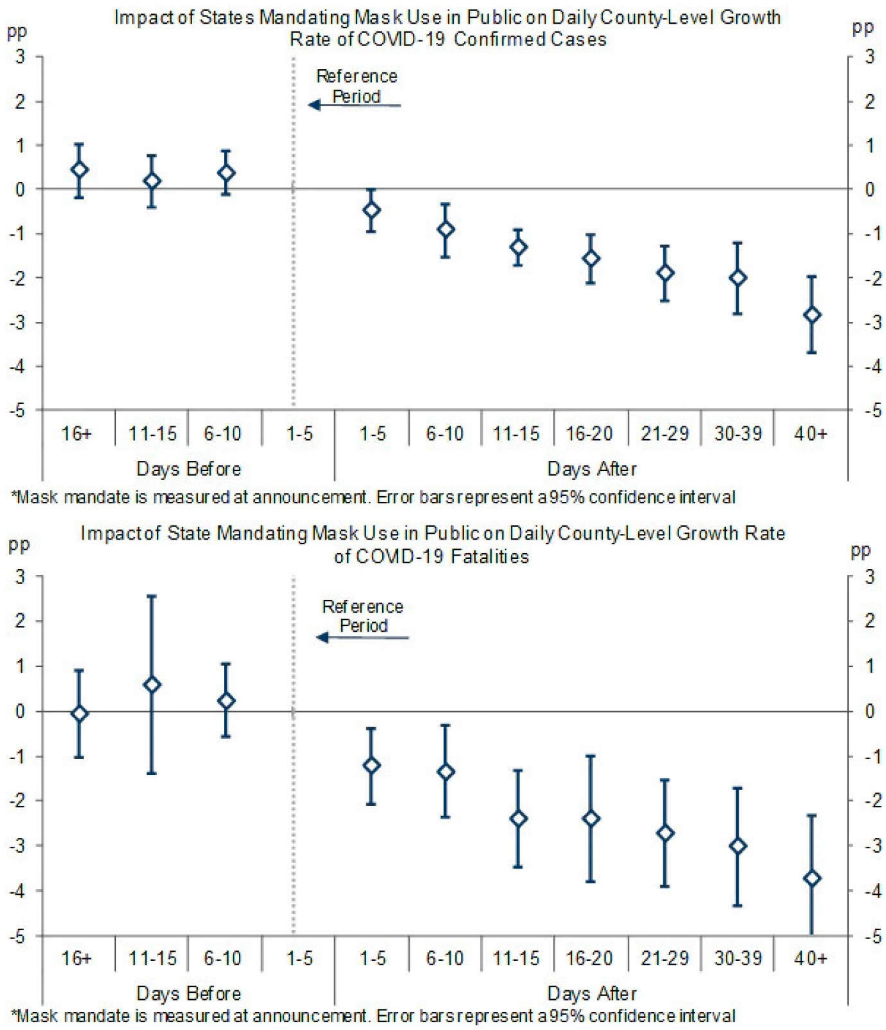
As shown in Exhibit 5, we estimate that face mask mandates have large and highly statistically significant effects on health outcomes. Our estimates imply that mask mandates lower the infection growth rate by 1.3pp in the 11-15 days after announcement. Relative to the 5.4% average infection growth rate prior to announcement, the growth rate of infections is cut by 25%. We also estimate significant and somewhat larger declines in the growth rate of COVID-19 fatalities of 2.4pp in the 11-15 days after announcement and of 3.7pp in the 21-29 days after.

Exhibit 5: Mask Mandates Are Associated with Large Declines in COVID-19 Case and Fatality Growth



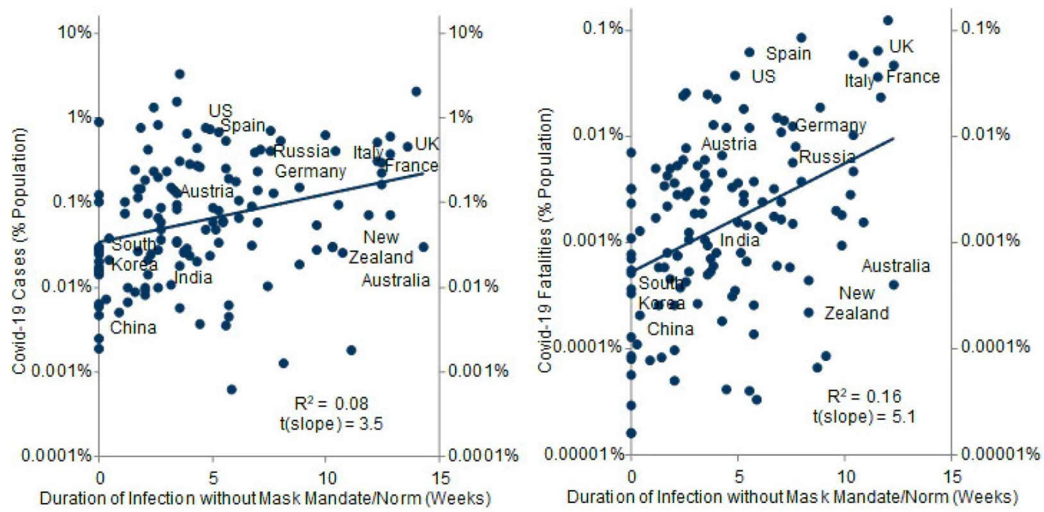
Source: YouGov, Goldman Sachs Global Investment Research

Approach 2: Large Country Cross-Section

Our second approach is a large country cross section in which we relate cumulative case counts and fatalities to the lag between the onset of spread and the introduction of a face mask mandate, building on a study by Christopher Leffler and co-authors.

Exhibit 6 presents the descriptive relationships graphically by plotting the length of the outbreak before masks were widely adopted against cumulative cases per capita (left panel) and cumulative fatalities per capita (right panel). We measure the start of the outbreak as the day of the first fatality. Both graphs show a positive and statistically significant slope, indicating that countries which took longer to reach widespread mask usage (whether by policy or cultural norms) suffered more virus cases and fatalities. The better fit for fatalities than cases likely reflects the relatively better measurement of fatalities.

Exhibit 6: Countries Which Took Longer to Reach Widespread Mask Usage Experienced More COVID-19 Cases and Fatalities



Source: masks4all.co, Leffler et al. 2020, Goldman Sachs Global Investment Research

To formalize this finding, Exhibit 7 presents cross-sectional regression models of log cases and log fatalities for around 125 countries. In both regressions, we find statistically significant negative effects of masks on cumulative cases and fatalities after including controls such as the obesity rate, population density, age structure, and testing policy. Our numerical estimates are that cumulative cases grow 17.3% per week without a mask mandate but only 7.3% with a mask mandate, and that cumulative fatalities grow 29% per week without a mask mandate but only 16% with a mask mandate.

Exhibit 7: Mask Policies and Norms Have Lowered COVID-19 Case Counts and Fatalities

	Country Cross-Section: Impact of Mask Wearing on COVID-19*			
	Log COVID-19 Cumulative Cases per Capita		Log COVID-19 Fatalities per Capita	
	Coefficient [t-stat]	$10^{coefficient}$	Coefficient [t-stat]	$10^{coefficient}$
Intercept	-4.896*** [-18.07]	--	-6.953*** [-23.46]	--
Weeks of Infection	0.069*** [3.22]	1.173	0.109*** [4.27]	1.285
Weeks of Infection with Mask Mandate/Norm**	-0.039*** [-3.34]	0.915	-0.044*** [-3.09]	0.904
Obesity Rate (%)	0.039*** [6.53]	1.093	0.031*** [4.77]	1.074
Population Density***	0.181*** [2.73]	1.519		
Testing Policy****	0.570*** [4.49]	3.712		
% of Population Over 65			0.034*** [3.74]	1.081
Observations	121		131	
R-squared	0.49		0.49	

*Data as of 23 June.
**Calculated upto 28 days before June 23 for Fatalities and 14 days before June 23 for Cases.
***1000 people per square km of land area.
****Proportion of time testing available to anyone showing COVID-19 symptoms (based on Oxford COVID-19 Testing Policy Indicator) from 10 days before first fatality to 14 days before 23 June.

Source: World Bank, Blavatnik School of Government: Oxford, Leffler et al. 2020, Masks4all.co, Goldman Sachs Global Investment Research

Approach 3: Country Panel

Our third approach consists of a smaller country panel in which we relate the daily growth rate of infections and fatalities to lagged self-reported mask usage, plus a number of control variables. There are three main results, illustrated in Exhibit 8.

First, face masks have a large negative impact on infections and fatalities, controlling for population density and income inequality (columns 1 and 4). This negative and significant impact of face masks is robust to controlling for our Effective Lockdown Index (ELI), the share of the population that say they avoid crowded public places (columns 2 and 5), and country and time fixed effects (columns 3 and 6). Our estimates suggest that a 25pp increase in the self-reported mask usage, for instance as a result of a mask mandate, lowers the growth rate of cumulative cases by 1.9pp and the growth rate of cumulative fatalities by 0.8pp.[6]

Second, the share of respondents that avoid crowded public places also has a large and highly significant negative impact on infections and fatalities. This not only suggests a significant role for "super spreader" events, but also strengthens our main results because it implies that the face mask result is not just driven by the correlation between face masks and other risky activities.

Third, the virus impact of mask usage is large, not just in absolute terms but also relative to the effect of economically costly shutdowns (as measured by our ELI). In fact, the coefficients on the percentage of self-reported mask usage are slightly bigger than those on the ELI, which is interesting as both variables are on a 0-100 scale.

Exhibit 8: Face Masks and Limiting Mass Gathering Lower COVID-19 Case and Fatality Growth

	International Panel: Impact of Mask Wearing on COVID-19 Spread					
	Daily Growth Rate of Confirmed Cases per Million*			Daily Growth Rate of Fatalities per Million*		
	(1)	(2)	(3)	(4)	(5)	(6)
Intercept	-12.37	4.29	8.05	-6.80	4.56	2.32
	[-3.83]***	[0.68]	[1.27]	[-3.77]***	[1.43]	[1.06]
% of Pop. Wearing Maks in Public, Lagged**	-0.11	-0.07	-0.08	-0.06	-0.03	-0.03
	[-5.2]***	[-3.21]***	[-3.63]***	[-5.05]***	[-2.00]**	[-2.19]**
GS Effective Lockdown Index, Lagged**		-0.05	-0.07		-0.04	-0.04
		[-1.59]	[-3.00]***		[-1.94]*	[-2.28]**
% of Pop. Avoiding Crowded Public Places, Lagged**		-0.23	-0.15		-0.15	-0.11
		[-3.11]***	[-2.39]**		[-4.79]***	[-2.30]**
Income Inequality	0.54	0.57	1.10	0.32	0.32	0.43
	[4.96]***	[5.76]***	[2.63]***	[5.98]***	[5.71]***	[2.59]***
Population Density	0.01	0.01	0.02	0.01	0.01	0.01
	[3.83]***	[3.05]***	[2.89]***	[3.03]***	[2.31]**	[3.74]***
Time FEs	No	No	Yes	No	No	Yes
Country FEs	No	No	Yes	No	No	Yes
Num. of Countries	22	18	18	22	18	18
Observations	2086	1743	1743	1711	1491	1491
R-squared	0.19	0.43	0.69	0.11	0.29	0.51

Note: T-statistics in brackets and robust standard errors clustered by country.
* Growth rates are natural log differences*100.
** Lagged 14 days for cases per million regressions; lagged 28 days for fatalities per million regressions.

Source: JHU CSSE, YoGov, Goldman Sachs Global Investment Research

The Impact of a Mandate on Infections

Before we translate our statistical results into a baseline estimate of the impact of face mask mandates on virus outcomes, we need to address two potential concerns about our analysis up front.

The first concern is that the correlation between face masks and virus outcomes might reflect the effect of other unobserved forms of cautious behavior that are correlated with mask mandates or usage, instead of a truly causal effect of masks. But there are some reasons to believe that this type of bias in not a big issue for our analysis. Not only do we obtain remarkably similar estimates across our three approaches, but we also control for a number of other observable forms of cautious behavior. Specifically, our cross-country results on masks include our Effective Lockdown Index among the explanatory variables, and they are largely unchanged when we include the share of respondents who say they stay home from work, don't touch objects, improve personal hygiene, avoid contact with tourists, avoid raw meat, and don't send their children to school.[7]

The second potential concern is that our main results are based on confirmed infections, which can be distorted by a lack of testing. However, it is important to note that this would, if anything, lead to an understatement of the effect assuming that increases in testing are positively correlated with increases in mask usage. Moreover, we control for testing regime indicators in our cross-sectional regression, and we obtain generally similar estimates for fatalities—which are better measured—across all three of our approaches.

So what is a reasonable baseline estimate of the impact of a US national mask mandate on the growth rate of confirmed infections? To generate such an estimate, we apply our country panel results separately to two groups of US states, namely ones with and without a state-level mask mandate in place.

States that currently don't have a state-level mandate account for 40% of US total confirmed cases, 45% of US GDP, half of the population, and two-thirds of new infections. This group has also experienced an average daily growth rate in confirmed infections of 2.9% in the past 7 days. Based on our analysis of state-level mandates, we estimate that a national mask mandate would raise mask usage by 25pp in these states. Our country panel shows that a 25pp increase in self-reported mask usage lowers the infection growth rate by 1.9pp (or just over 60%). The national mandate could therefore lower the daily growth rate in the group of states without a mandate from 2.9% to just over 1%.

States that currently do have a mandate have experienced a lower average daily growth rate in confirmed infections of 0.8% in the past 7 days. Combined with our assumption that a national mandate would lead to a smaller increase in mask usage of 5pp through extra awareness in this group of states, our country panel suggests a 0.4pp decline in the infection growth rate to 0.4-0.5%.

Combining the estimates for these two groups of states, we estimate that a national mandate could cut the national average growth rate of infections by nearly 1.0pp to 0.6-0.7%.

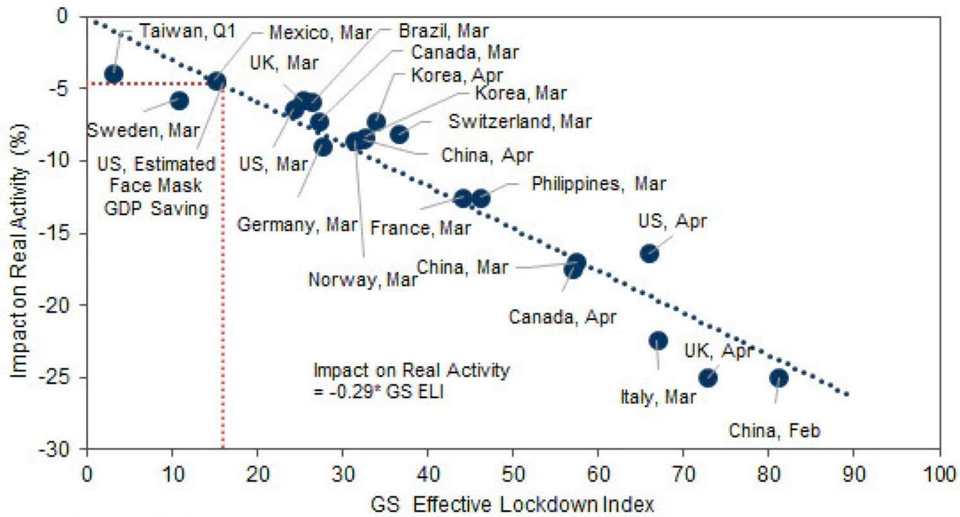
The Impact of a Mandate on GDP

If a face mask mandate meaningfully lowers coronavirus infections, it could be valuable not only from a public health perspective but also from an economic perspective because it could substitute for renewed lockdowns that would otherwise hit GDP.

How big is this potential effect? To generate an answer, we proceed in two steps. First, we use our cross-country panel analysis to ask how much our effective lockdown index (ELI) would need to increase in order to lower the daily case growth rate by 1.0pp, i.e. the estimated impact of a national face mask mandate. The answer is an increase in our ELI of 16pp.

Second, we ask how much a 16pp ELI increase would subtract from the level of GDP. As shown in Exhibit 9, the cross-country relationship implies that such an increase might reduce GDP by just under 5%.

Exhibit 9: A Close Relationship Between the ELI and GDP



Note: Impact on real activity is estimated where data is not available.

References to China are to Mainland China

Source: Goldman Sachs Global Investment Research

Thus, the upshot of our analysis is that a national face mask mandate could potentially substitute for renewed lockdowns that would otherwise subtract nearly 5% from GDP. It is important to recognize that this estimate is quite uncertain because it is based on a number of statistical relationships that are all measured with error. Despite the numerical uncertainty, however, our analysis suggests that the economic benefit from a face mask mandate and increased face mask usage could be sizable.

So will the US adopt a national face mask mandate? This is uncertain, partly because masks have become such a politically and culturally charged issue. However, even in the absence of a national mandate, state and local authorities might well broaden mandates in ways that ultimately mimic the impact of a national mandate. Either way, our analysis suggests that the economy could benefit significantly from such moves, especially when compared with the alternative of a return to broader lockdowns.

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Isabella Rosenberg

[1] We thank Sid Bhushan and Dan Milo for valuable help with this report.

[2] See Jaqueline Howard, "WHO stands by recommendation to not wear masks if you are not sick," CNN, March 30, 2020.

[3] As an aside, note that Minnesota has the lowest self-reported rate of mask usage among larger states in the US. This is interesting because the state is home to a large population of Scandinavian-Americans and Scandinavia has some of the lowest rates of face mask usage in the world.

[4] Our mask usage regressions are weighted by state population, focus on states with more than 4 million people (given the small samples in the respondent-level data) and use robust standard errors at the state level. Our sample starts on April 2nd when respondent-level data become available. We also control for cumulative cases per million and cumulative deaths per million.

[5] The county-level controls are population density, median house value, median household income, homeownership, pollution, maximum summer temperature, and maximum winter temperature, educational attainment, mean Body Mass Index, and the share of population over 65. We use county population weights and cluster robust standard errors at the state level. Our sample covers 2,373 counties and extends from March 31 to June 24. See Lyu and Wehby (2020).

[6] Relative to the sample average growth rates of 3.8% and 2.8%, these estimates imply that a mask mandate raising mask usage by 25pp cuts the growth rates of infections and fatalities by nearly one half and one quarter respectively.

[7] The cross-country panel and cross-county panel results are also robust to controlling for the lagged levels of fatalities and infections per capita, which helps address the concern that the mask effects pick up the impact of other forms of unobserved cautious behavior in response to the size of the outbreak.

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Face Masks, Public Policies and Slowing the Spread of COVID-19: Evidence from Canada
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ABSTRACT

We estimate the impact of mask mandates and other non-pharmaceutical interventions (NPI) on COVID-19 case growth in Canada, including regulations on businesses and gatherings, school closures, travel and self-isolation, and long-term care homes. We partially account for behavioral responses using Google mobility data. Our identification approach exploits variation in the timing of indoor face mask mandates staggered over two months in the 34 public health regions in Ontario, Canada's most populous province. We find that, in the first few weeks after implementation, mask mandates are associated with a reduction of 25 percent in the weekly number of new COVID-19 cases. Additional analysis with province-level data provides corroborating evidence. Counterfactual policy simulations suggest that mandating indoor masks nationwide in early July could have reduced the weekly number of new cases in Canada by 25 to 40 percent in mid-August, which translates into 700 to 1,100 fewer cases per week.

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A Data Appendix is available at https://github.com/C19-SFU-Econ

1 Introduction

When government policies to stem the spread of COVID-19 were introduced in early 2020, the best available evidence supporting them was provided by studies of previous epidemics, epidemiological modeling, and case studies (OECD, 2020). Even when the efficacy of a given precaution in reducing COVID-19 transmission has been established, significant doubts regarding the usefulness of specific policy measures may persist due to uncertainty regarding adherence to the rules and other behavioral responses. For example, even though several observational studies, mostly in medical setting, have shown that face masks reduce the transmission of COVID-19 and similar respiratory illnesses (see Chu et al. (2020) for a comprehensive review), a face mask mandate may not be effective in practice if it fails to increase the prevalence of mask wearing (compliance), or if it leads to increased contacts due to a false sense of security. It is therefore important to directly evaluate and quantify the relationship between various policy measures and the rate of propagation of COVID-19.

The low cost and high feasibility of mask mandates relative to other containment measures for COVID-19 has generated keen interest worldwide for studying their effectiveness. This attention has been compounded by substantial variation, across jurisdictions and over time, in official advice regarding the use of masks. Figure B1 in the Appendix plots self-reported mask usage in select countries (Canada, United States, Germany and Australia) in the left panel, and across Canadian provinces in the right panel. The figure shows large differences in mask usage, both across countries and within Canada.[1]

We estimate and quantify the impact of mask mandates and other non-pharmaceutical interventions (NPI) on the growth of the number of COVID-19 cases in Canada. Canadian data has the important advantage of allowing two complementary approaches to address our objective. First, we estimate the effect of mask mandates by exploiting within-province geographic variation in the timing of indoor face mask mandates across 34 public health regions (PHUs) in Ontario, Canada's most populous province with a population of nearly 15 million or roughly 39% of Canada's population (Statistics Canada, 2020). The advantage of this approach is that it exploits variation over a relatively small geographic scale (PHU), holding all other province-level policies or events constant. In addition, the adoption of indoor face mask mandates in these 34 sub-regions was staggered over approximately two months, creating sufficient intertemporal policy variation across the PHUs.

[1] We show mask usage for the U.S. and Germany because related work by Chernozhukov et al. (2020) and Mitze et al. (2020) studies the effect of mask mandates in these countries respectively. We show Australia as an example of a country which did not mandate mask usage, except for Melbourne in late July. See Hatzius et al. (2020) for more cross-country comparisons of mask usage.

Second, we evaluate the impact of NPIs in Canada as a whole, by exploiting variation in the timing of policies across the country's ten provinces. By studying inter-provincia variation, we are able to analyze the impact of not only mask mandates, but also other NPIs, for which there is little or no variation across Ontario's PHUs (regulations on businesses and gatherings, schooling, travel and long-term care). In addition, our province-level data include both the closing period (March-April) and the gradual re-opening period (May-August), providing variation from both the imposition and the relaxation of policies.

Our panel-data estimation strategy broadly follows the approach of Chernozhukov, Kasahara and Schrimpf (2020), hereafter CKS (2020), adapted to the Canadian context. We allow for behavioural responses (using Google Community Mobility Reports geo-location data as proxy for behaviour changes and trends), as well as lagged outcome responses to policy and behavioral changes. Our empirical approach also allows current epidemiological outcomes to depend on past outcomes, as an information variable affecting past policies or behaviour, or directly, as in the SIR model framework.

We find that, in the first few weeks after their introduction, mask mandates are associated with an average reduction of 25 to 31% in the weekly number of newly diagnosed COVID-19 cases in Ontario, holding all else equal. We find corroborating evidence in the province-level analysis, with a 36 to 46% reduction in weekly cases, depending on the empirical specification. Furthermore, using survey data, we show that mask mandates increase self-reported mask usage in Canada by 30 percentage points, suggesting that the policy has a significant impact on behaviour. Jointly, these results suggest that mandating indoor mask wear in public places is a powerful policy measure to slow the spread of COVID-19, with little associated economic disruption at least in the short run.[2]

Counterfactual policy simulations using our empirical estimates suggest that mandating indoor masks nationwide in early July could have reduced weekly new cases in Canada by 25 to 40% on average by mid-August relative to the actually observed numbers, which translates into 700 to 1,100 fewer cases per week.

We also find that the most stringent restrictions on businesses and gatherings observed in our data are associated with a decrease of 48 to 57% in weekly cases, relative to a lack of restrictions. The business/gathering estimates are, however, noisier than our estimates for mask mandates and do not retain statistical significance in all specifications; they appear driven by the smaller provinces and the re-opening period (May to August). School closures and travel restrictions are associated with a large decrease in weekly case growth in the

[2]Hatzius et al. (2020) estimate that a national mask mandate in the USA could replace alternative restrictions costing 5% of GDP.

closing period. Our results on business/gathering regulations and school closure s ggest that reduced restrictions and the associated increase in business or workplace activity and gatherings or school re-opening can offset, in whole or in part, the estimated effect of mask mandates on COVID-19 case growth, both in our sample and subsequently.

An additional contribution of this research project is to assemble, from original official sources only, and make publicly available a complete dataset of COVID-19 cases, deaths, tests and policy measures in all 10 Canadian provinces.[3] To this end, we constructed, based on official public health orders and announcements, time series for 17 policy indicators regarding face masks, regulations on businesses and gatherings, school closures, travel and self-isolation, and long-term care homes.

Our paper relates most closely to two recent empirical papers on the effects of mask mandates using observational data.[4] CKS (2020) and Mitze et al. (2020) study the effect of mask mandates in the United States and Germany, respectively. CKS (2020), whose estimation strategy we follow, exploit U.S. state-level variation in the timing of mask mandates for employees in public-facing businesses, and find that these mandates are associated with 9 to 10 percentage points reduction in the weekly growth rate of cases. This is substantially smaller that our estimates, possibly because the mask mandates that we study are much broader: they apply to all persons rather than just employees, and most apply to all indoor public spaces rather than just businesses. Mitze et al. (2020) use a synthetic control approach and compare the city of Jena and six regions in Germany that adopted a face mask policy in early to mid April 2020, before their respective state mandate. They find that mandatory masks reduce the daily growth rate of cases by about 40%.

Our paper has several advantages compared to the above two papers. First, we exploit both regional variation within the same province (like Mitze et al., 2020) and provincial variation in the whole country (like CKS, 2020), and find similar results, which strengthens the validity of our findings. Second, we show that self-reported mask usage has increased after introducing mask mandates. We view this "first-stage" result on mask usage as informative, as the effectiveness of any NPI or public policy critically depends on the compliance rate. Moreover, this result mitigates possible concerns that the estimated mask mandate effect on COVID-19 case growth may be caused by factors other than mask policy. Third, a key

[3] All data are available for download at https://github.com/C19-SFU-Econ. The COVID-19 cases, deaths and tests data that we collected and use in this paper incorporate all official ex-post revisions as of mid-August, unlike data from the Government of Canada COVID-19 website or other aggregator websites.

[4] Howard et al. (2020), a comprehensive review of the medical literature, stresses that "no randomized controlled trial (RCT) on the use of masks as source control for SARS-CoV-2 has been published." It is unlikely that an RCT on masks' effectiveness against COVID-19 will be feasible or ethical during the pandemic.

difference between our paper and CKS (2020) is that we evaluate the effect of *universal* (or *community*) mandatory indoor mask wearing for the public rather than the effect of mandatory mask wearing for *employees only*.[5] While other factors such as differences in mask wear compliance between Canada and the U.S. may contribute to the different estimated magnitude of the policy impact, our results suggest that more comprehensive mask policies can be more effective in reducing the case growth rate.

Other Related Literature

Abaluck et al. (2020) discuss the effectiveness of universal adoption of homemade cloth face masks and conclude that this policy could yield large benefits, in the \$3,000–\$6,000 per capita range, by slowing the spread of the virus. The analysis compares countries with pre-existing norms that sick people should wear masks (South Korea, Japan, Hong Kong and Taiwan) and countries without such norms.[6]

In the medical literature, Prather et al. (2020) argue that masks can play an important role in reducing the spread of COVID-19. Howard et al. (2020) survey the medical evidence on mask efficiency and recommend public use of masks in conjunction with existing hygiene, distancing, and contact tracing strategies. Greenhalgh et al. (2020) provide evidence on the use of masks during non-COVID epidemics (influenza and SARS) and conclude that even limited protection could prevent some transmission of COVID-19. Leung et al. (2020) study exhaled breath and coughs of children and adults with acute respiratory illness and conclude that the use of surgical face masks could prevent the transmission of the human coronavirus and influenza virus from symptomatic individuals. Meyerowitz et al. (2020) present a recent comprehensive review of the evidence on transmission of the virus and conclude that there is strong evidence from case and cluster reports indicating that respiratory transmission is dominant, with proximity and ventilation being key determinants of transmission risk, as opposed to direct contact or fomite transmission.

Our paper also complements recent work on COVID-19 policies in Canada. Mohammed et al. (2020) use public opinion survey data to study the effect of changes in mask-wear policy recommendations, from discouraged to mandatory, on the rates of mask adoption and public trust in government institutions. They show that Canadians exhibit high compliance with mask mandates and trust in public health officials remained consistent across time. Yuksel et al. (2020) use an outcome variable constructed from Apple mobility data along

[5] Lyu and Wehby (2020) provide suggestive evidence that community mask mandates are more effective than employees-only mandates.

[6] The authors report average daily case growth rate of 18% in countries with no pre-existing mask norms vs. 10% in countries with such norms. On a weekly basis, this translates to a reduction of 49 log points $(100(\log(1.18^7) - \log(1.1^7)))$ in case growth, or 39% reduction in weekly cases.

with weather data and lagged COVID-19 cases or deaths as dependent variables to study compliance with social distancing measures.

2 Data

We use three main data sources, respectively for epidemiological variables, NPI and mask mandates, and behavioral responses. The time period is from the start of detected community transmission in Canada in March to mid-August, 2020.

We located and accessed the original official sources to collect a complete dataset of COVID-19 cases, deaths, tests and policy measures in all ten Canadian provinces.[7] In addition, our data include cases and policy measure indicators for each of the 34 public health units (PHUs) in Ontario. A detailed description is provided in the data source files shared at the project's Github webpage.

Implementation dates of NPIs and other public policies were collected from government websites, announcements, public health orders and staged re-opening plans collected from their official sources. In the national data, the raw policy measures data contain the dates or enactment and relaxation (if applicable) of 17 policy indicators including: mandatory mask wear; closure and re-opening of retail and non-essential businesses, restaurants, recreation facilities, and places of worship; school closures; limits on events and gatherings; international and domestic travel restrictions and self-isolation requirements; restrictions on visits and staff movement in long-term care homes. All policy indicator variables are defined in Table C1 in the Appendix.[8] Since many of these indicators are highly correlated with each other, we combine them into five policy aggregates in the empirical analysis (see Table A17 and Section 3.2). In the Ontario PHU data, the implementation dates of mask mandates and the relaxation dates of policies for businesses and gatherings vary across PHUs. Decisions about the former were made at the PHU level, while decisions about the latter were made by the province, which classified PHUs into three groups, with some exceptions.

Regarding behavioral responses, we use the Google COVID-19 Community Mobility Reports, which summarize daily cellphone geo-location data for each province as indices calculated relative to the median value for the same day of the week in the five-week baseline

[7]The provinces differ in the ease of accessibility of their official time series of COVID-19 cases, deaths and test numbers. In some cases, we located and used the hidden json sources feeding the public dashboard charts. In few instances in which data were not available, we used the numbers reported in the daily provincial government announcements. All COVID-19 outcome data sources are referenced and web-linked in Appendix Table C3.

[8]Additional survey data on mask usage is described and used in Section 4.4.

period January 3 to February 6, 2020.[9] In Ontario, these location data are available for each of the 51 first-level administrative divisions (counties, regional municipalities, single-tier municipalities and districts).[10]

3 Empirical method

We follow the approach of CKS (2020), but modify and adapt it to the Canadian context. The empirical strategy uses the panel structure of the outcome, policy and behavioral proxy variables, and includes lags of outcomes as information, following the causal paths suggested by the epidemiological SIR model (Kermack and McKendrick, 1927). Specifically, we estimate the effect of policy interventions on COVID-19 outcomes while controlling for information and behaviour. In contrast to CKS (2020) and Hsiang et al. (2020), who study variation in NPIs across U.S. states or across countries, our identification strategy exploits policy variation at the sub-provincial level (Ontario's PHUs) in addition to cross-province variation, and our data captures both the closing down and gradual re-opening stages of the epidemic.

3.1 Estimation strategy

The main data used in our empirical analysis are summarized below; Section 3.2 describes the variables in detail. Everywhere i denotes province for national analysis, and health region (PHU) for Ontario analysis, and t denotes time measured in days.

1. Outcomes, Y_{it} – growth rate of weekly cases or deaths.

2. Information, I_{it} – lagged outcomes, i.e. past levels or growth rate of cases (or deaths). We also consider a specification that includes the past cases/deaths and case/death growth at the national level as additional information variables.

3. Behavioral responses, B_{it} – Google mobility data capturing changes in people's geolocation relative to a baseline period in January-February.

4. Policy/NPIs, P_{it} – for the national analysis, five policy aggregates by province and date; for the Ontario analysis, two policy variables (mask mandates and regulation on business and gathering) by PHU and date.

[9]The reports are available for download at https://www.google.com/covid19/mobility/.

[10]Each of these divisions is either entirely (in most cases) or predominantly located within a single PHU. In cases where a PHU corresponds to multiple divisions, 2016 Census population counts were used as weights to compute the PHU's mobility index.

5. Controls, W_{it} – province or PHU fixed effects, growth rate of weekly new COVID-19 tests, and a time trend.

To assess and disentangle the impact of NPIs and behavioral responses on COVID-19 outcomes, we estimate the following equation:

$$Y_{it} = \alpha B_{it-l} + \pi P_{it-l} + \mu I_{it} + \delta_Y W_{it} + \varepsilon_{it}^Y \tag{1}$$

where l denotes a time lag measured in days. Equation (1) models the relationship between COVID-19 outcomes, Y_{it}, and lagged behaviour, B_{it-l}, lagged policy measures, P_{it-l} and information (past outcomes), $I_{it} = Y_{it-l}$. For case growth as the outcome, we use $l = 14$. For deaths growth as the outcome, we use $l = 28$.[11] The choice of these lags is discussed in Appendix D.

By including lagged outcomes, our approach allows for possible endogeneity of the policy interventions P_{it}, that is, the introduction or relaxation of NPIs based on information on the level or growth rate of cases or deaths. Also, past cases may be correlated with (lagged) government policies or behaviors that may not be fully captured by the policy and behaviour variables.

In Appendix Table A18, we also report estimates of the following equation:

$$B_{it} = \beta P_{it} + \gamma I_{it} + \delta_B W_{it} + \varepsilon_{it}^B \tag{2}$$

which models the relationship between policies P_{it}, information, I_{it} (weekly levels or growth of cases or deaths) and behaviour, B_{it}. It is assumed that behaviour reacts to the information without a significant lag. We find strong correlation between policy measures and the Google mobility behavioral proxy measure.

Equation (1) captures both the direct effect of policies on outcomes, with the appropriate lag, as well as the potential indirect effect on outcomes from changes in behaviour captured by the changes in geo-location proxy B_{it-l}. In Appendix Tables A19 and A20, we also report estimates of equation (1) without including the behavioral proxy, that is, capturing the total effect of policies on outcomes. Since our estimates of the coefficient α in equation (1) are not significantly different from zero, the results without controlling for the behavioral proxy are very similar to those from estimating equation (1).

[11] Our lag for deaths is one week longer than that used by CKS (2020). The difference is due to additional evidence from the medical literature and the construction of the weekly variables (see Appendix D).

3.2 Variables and descriptive analysis

Outcomes. Our main outcome of interest is the growth rate of weekly new positive COVID-19 cases as defined below.[12] We use weekly outcome data to correct for the strong day-of-the-week effect present in COVID-19 outcome data.[13] Weekly case growth is a metric that can be helpful in assessing trends in the spread of COVID-19, and it is highlighted in the World Health Organization's weekly epidemiological updates (see, for example, World Health Organization (2020)).

Specifically, let C_{it} denote the cumulative case count up to day t and define ΔC_{it} as the weekly COVID-19 cases reported for the 7-day period ending at day t:

$$\Delta C_{it} \equiv C_{it} - C_{it-7}.$$

The weekly case (log) growth rate is then defined as:

$$Y_{it} = \Delta \log(\Delta C_{it}) = \log(\Delta C_{it}) - \log(\Delta C_{it-7}), \quad (3)$$

that is, the week-over-week growth in cases in region i ending on day t.[14] The weekly death growth rate is defined analogously, using cumulative deaths data.

Policy. In the Ontario analysis, we exploit regional variation in the timing of indoor mask mandates staggered over two months in the province's 34 regions ("public health units" or PHUs). Figure 1 displays the gradual introduction of mask mandates across the 34 PHUs in Ontario. The exact implementation dates of the mask mandates are reported in Table C2. Mandatory indoor masks were introduced first in the Wellington-Dufferin-Guelph PHU on June 12 and last in the Northwestern PHU on August 17.[15]

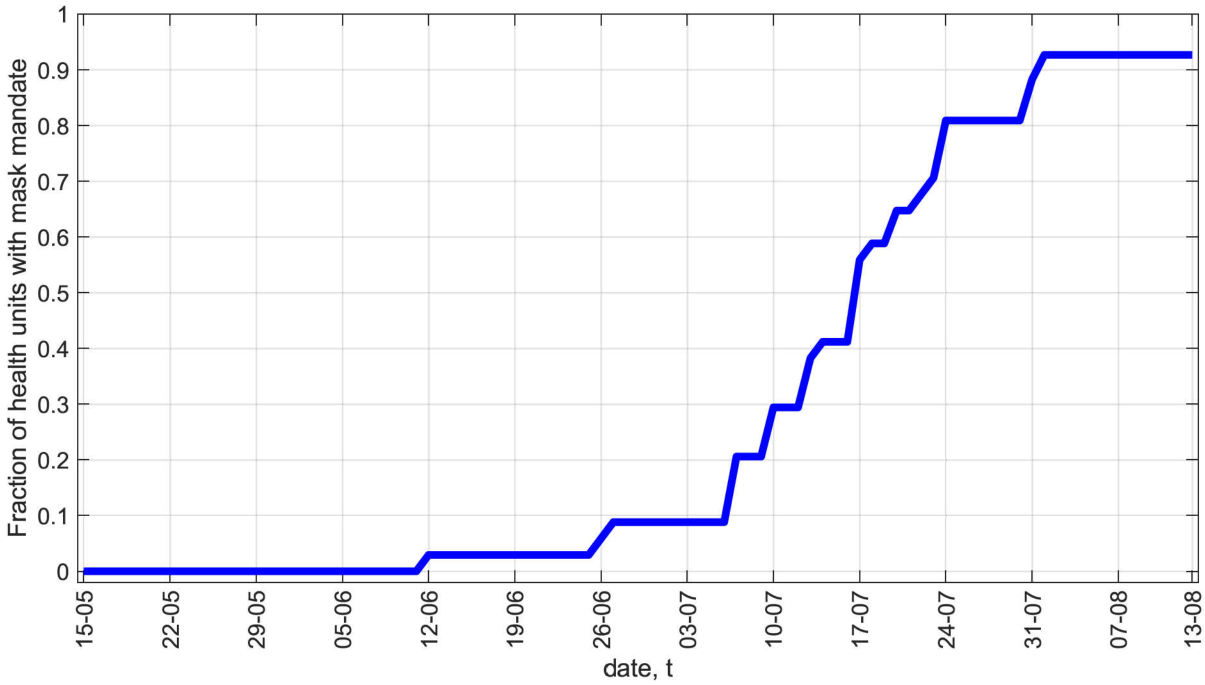
[12] We also report results using the growth rate of deaths as supplemental analysis in Section 4.5.

[13] Figures B9 and B10 in the Appendix respectively display the weekly and daily cases, deaths and tests in each Canadian province over time. There are markedly lower numbers reported on weekends or holidays.

[14] To deal with zero weekly values, which mostly occur in the smaller regions, as in CKS (2020), we replace log(0) with -1. We also check the robustness of our results by adding 1 to all ΔC_{it} observations before taking logs, by replacing log(0) with 0, and by using population weighted least squares; see Tables A5 and A8.

[15] There is no PHU-wide mask mandate in Lambton as of August 31, but its main city, Sarnia, enacted a mask mandate on July 31.

Figure 1: Ontario - mask mandates over time



Notes: There are a total of 34 public health units (PHU) in Ontario. See Table C2 for the exact date of mask mandate implementation in each PHU.

In the province-level analysis, we assign numerical values to each of the 17 policy indicators listed in Table C1 in Appendix C. The values are on the interval [0,1], with 0 meaning no or lowest level of restrictions and 1 meaning maximal restrictions. A policy value between 0 and 1 indicates partial restrictions, either in terms of intensity (see more detail and the definitions in Table C1) or by geographical coverage (in large provinces). The numerical values are assigned at the daily level for each region (PHU or province, respectively for the Ontario and national results), while maintaining comparability across regions.

Many NPIs were implemented at the same time, both relative to each other and/or across regions (especially during the March closing-down period), which causes many of the policy indicators to be highly correlated with each other (see Appendix Table A4). To avoid multi-collinearity issues, we group the 17 policy indicators into 5 policy aggregates via simple averaging: (i) *travel*, which includes international and domestic travel restrictions and self-isolation rules; (ii) *school*, which is an indicator of provincial school closure; (iii) *business/gathering*, which comprises regulations and restrictions on non-essential businesses and retail, personal businesses, restaurants, bars and nightclubs, places of worship, events, gyms and recreation, and limits on gathering; (iv) *long-term care (LTC)*, which includ s NPIs governing the operation of long-term care homes (visitor rules and whether staff are required to work on a single site) and (v) *mask* which takes value 1 if an indoor mask mandate has been introduced, 0 if not, or value between 0 and 1 if only part of a province has enacted such policy.[16]

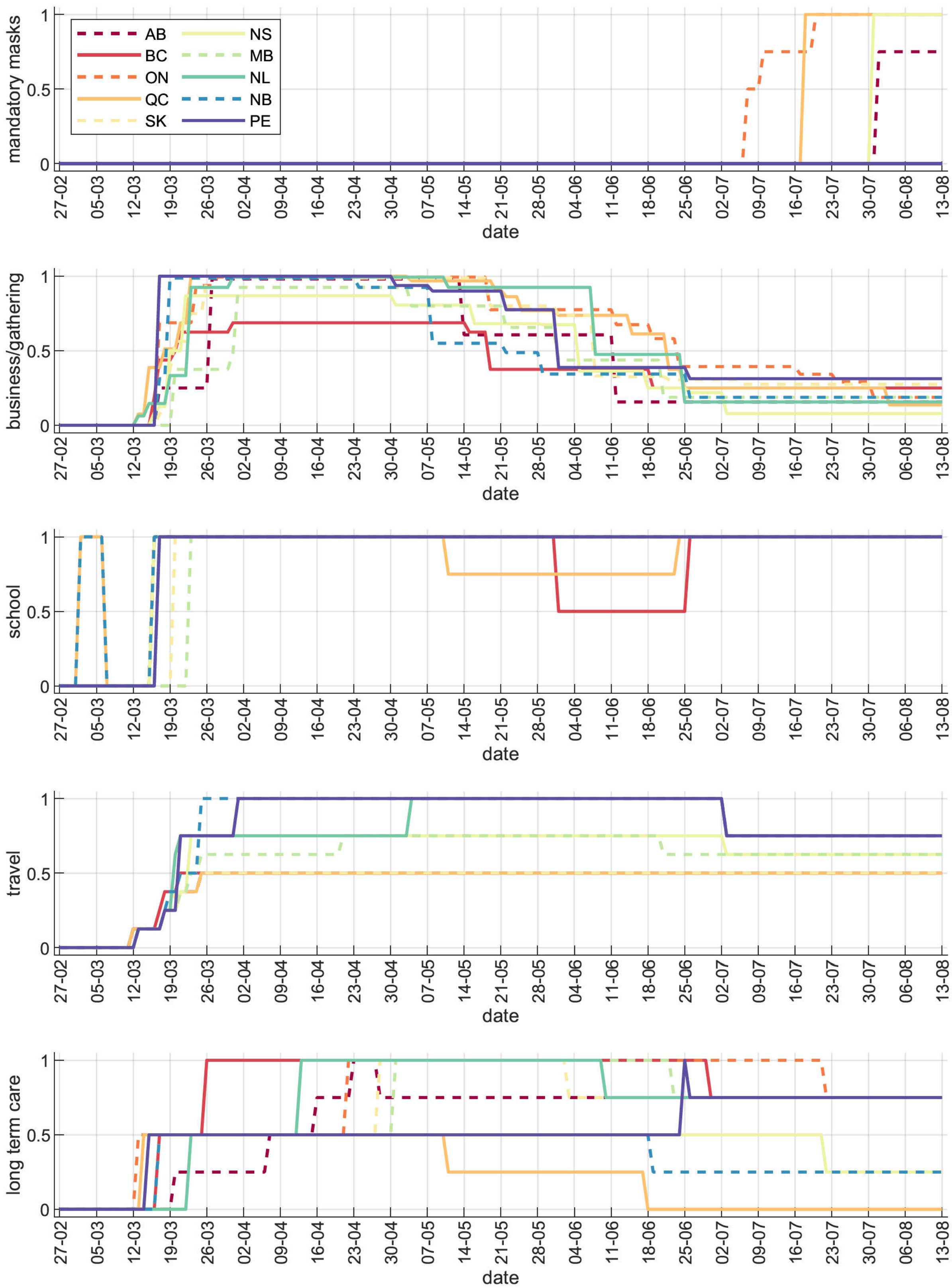
The five policy aggregates are constructed at the daily level and capture both the closing-down period (an increase in the numerical value from 0 toward 1) and the re-opening period (decrease in the numerical value toward zero). In comparison, the policy indicators compiled by Raifman (2020) for the USA used in CKS (2020) are binary "on (1)"/"off (0)" variables.[17] For consistency with the weekly outcome and information variables and the empirical model timing, we construct the policy aggregates P_{it}^{j} used in the regressions (where j denotes policy type) by taking a weekly moving average of the raw policy data, from date $t-6$ to date t.

Figure 2 plots the values of the 5 policy aggregates over time for each of the 10 provinces. Travel restrictions, school closures (including Spring and Summer breaks) and business closures were implemented in a relatively short period in the middle of March. There is some variation in the travel policy aggregate since some Canadian provinces (the Atlantic provinces and Manitoba) implemented inter-provincial domestic travel or self-isolation restrictions in addition to the federal regulations regarding international travel. Restrictions on long-term care facilities were introduced more gradually. In the re-opening period (May-August), there is also more policy intensity variation across the provinces, especially in the business and gatherings category, as the different provinces implemented their own re-opening plans and strategies. Mask mandates were first introduced in Ontario starting from June in some smaller PHUs and early July in the most populous PHUs such as Toronto, Ottawa and Peel (see Appendix Table C2). In Quebec, indoor masks were mandated province-wide on July 18. Nova Scotia and Alberta's two main cities implemented mask mandates on July 31 and August 1, respectively.

[16]We do not use provincial declarations of emergency in our analysis as they are mostly legal tools enabling other restrictions rather than restrictions *per se*.

[17]The daily numerical values for each of the 17 basic policy indicators and the 5 policy aggregates for each province and date are available on the project's Github repository.

Figure 2: Policy aggregates - Canada



Notes: The figure plots the numerical values of the 5 policy aggregates (Mask, Business /gathering, School, Travel and Long-term care, LTC) over time, for each of the 10 provinces. The mask policy values for ON reflect the gradual adoption of mandates and the respective PHUs population sizes.

There are two empirical challenges specific to our Canadian context and data. The first challenge is the presence of small provinces and sub-regions with very few COVID-19 cases or deaths. In Section 4.3, we perform a number of robustness checks using different ways of handling the observations with very few cases (in particular zero cases). The second data limitation is that there are only 10 provinces in Canada and 34 public health units in Ontario, unlike the 51 U.S. jurisdictions in CKS (2020). To account for the resulting small number of clusters in the estimation, we compute and report wild bootstrap standard errors and p-values, as proposed by Cameron et al. (2008).[18] On the flip side, our data has the advantage of a longer time horizon (March to August) and non-binary, more detailed policy variables compared to Raifman et al. (2020).

Behaviour proxy. We follow CKS (2020) and other authors in interpreting the location change indices from the Google Community Mobility reports as proxies for changes in people's behaviour during the pandemic, keeping in mind that location is only one aspect of behaviour relevant to COVID-19. The general pattern in the data (see Figure B3) shows sharply reduced frequency of recorded geo-locations in shops, workplaces and transit early in the pandemic (March), with a subsequent gradual increase back toward the baseline (except for transit), and a flattening out in July and August.

Several of the six location indicators (retail, grocery and pharmacy, workplaces, transit, parks and residential) are highly correlated with each other (see Tables A1 and A2) and/or contain many missing observations for the smaller provinces. To address these data limitations and the possible impact of collinearity on the estimation results, we use as proxy for behavioral changes the simple average of the following three mobility indicators: "retail", "grocery and pharmacy" and "workplaces". To be consistent with the weekly outcome variables and to mitigate day-of-week behavioural variation, we construct the Behaviour proxy B_{it} by taking a weekly moving average of the $\frac{1}{3}$ (retail + grocery and pharmacy + workplaces) data, from date $t-6$ to date t.[19,20] As a result, our empirical analysis uses weekly totals (for cases, tests and deaths) or weekly moving averages (for policies and the behaviour proxy) of all variables recorded on daily basis.[21]

[18] Alternative methods for computing the standard errors are explored in Section 4.3.

[19] We drop the "transit", "parks", and "residential" location indicators because, respectively, 10.6%, 13.7%, and 2.8% of the observations are missing in the provincial data, and 20.7%, 52.1%, and 11.1% are missing in the Ontario data. The "transit" and "residential" variables are also highly correlated with the three indicators we include in our aggregate behaviour proxy B_{it}. Furthermore, the "parks" indicator does not have clear implication for COVID-19 outcomes.

[20] In the Ontario analysis, 1.4% of the B_{it} values are imputed via linear interpolation.

[21] In estimation equation (1), we take moving average from date $t-14$ to date $t-20$ for policies and behaviour when the outcome is weekly case growth, and from date $t-28$ to date $t-34$ if the outcome is

Tables A3 and A4 display the correlation between our behaviour proxy B_{it} and the five NPI policy aggregates P_{it}^{j}. Importantly, the behaviour proxy and mask mandate variables are not highly correlated, suggesting that the effect of mask mandates on COVID-19 outcomes should be independent of location behaviour changes.

Information. We use the weekly cases and case growth variables defined above, ΔC_{it} and Y_{it}, to construct the information variables I_{it} in equation (1). Specifically, we use as information the lagged value of the weekly case growth rate Y_{it-l} ($= \Delta \log(\Delta C_{it-l})$ and the log of past weekly cases, $\log(\Delta C_{it-l})$. We also use the lagged provincial (Ontario analysis) or national (Canada analysis) case growth rate and log of weekly cases as additional information variables in some specifications. A two-week information lag $l = 14$ is used in the baseline results. In the supplementary regressions using the death growth rate as the outcome, we use information on past deaths and a four-week lag (see Section 4.5).

Control variables. In all regressions, we control for region fixed effects (PHU or province) and the weekly COVID-19 tests growth rate $\Delta \log(\Delta T_{it})$, where T_{it} denotes cumulative tests in region i until date t and ΔT_{it} is defined analogously to ΔC_{it} above. We include a time trend: our baseline uses a cubic polynomial in days, but we also report results with no time trend and with week fixed effects. Robustness checks also include news or weather variables as controls (see Section 4.3).

Time period. We use the period May 15 to August 13 for the analysis with Ontario PHU level data and the period March 11 to August 13 for the national analysis with provincial data. The end date reflects data availability at the time of empirical analysis and writing. The start date for the Ontario sample (May 15) is approximately two weeks after the last restrictive measures were implemented and four weeks before the first mask mandate was introduced in Ontario. Robustness checks with different initial dates (May 1, June 1 and June 15) are reported in Section 4.3, with our results remaining stable. The initial date for the national sample (March 11) was chosen as the first date on which each province reported at least one COVID-19 test (so that cases could be potentially reported). Again, alternative initial dates are explored in Section 4.3.

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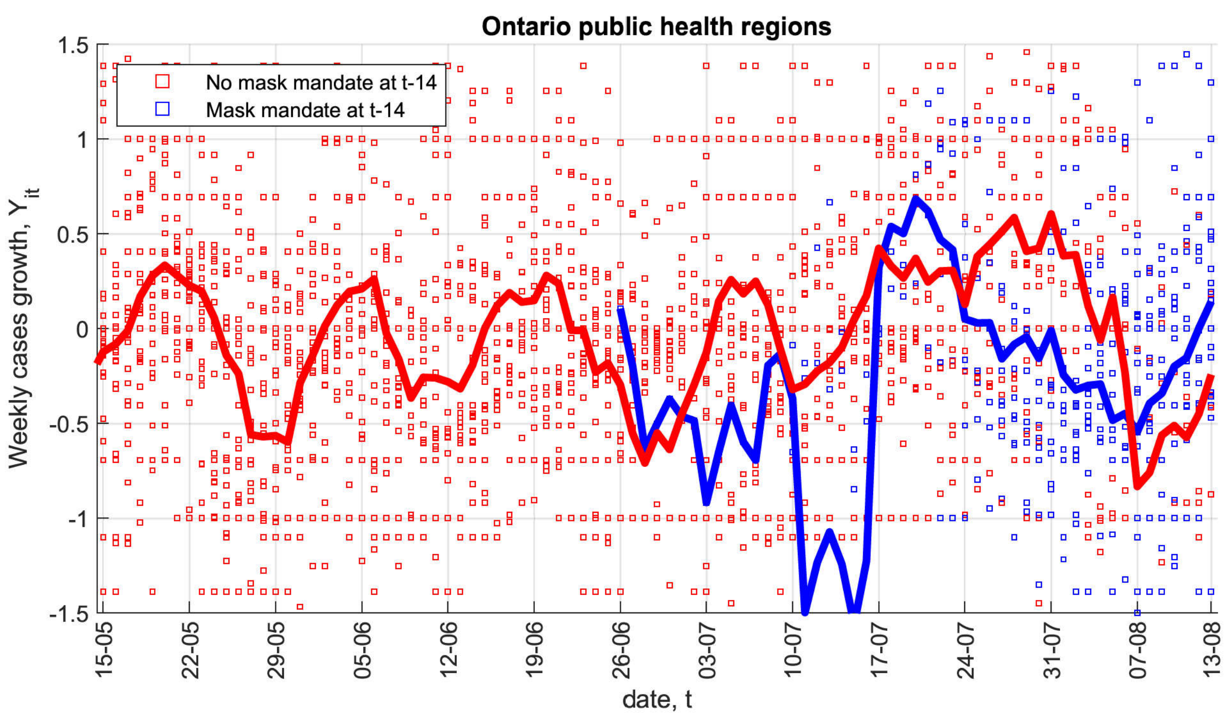
deaths growth. Alternative lags are explored in Section 4.3.

4 Results

4.1 Mask mandates in Ontario public health regions

We start with a simple graphical illustration of the effect of mask mandates on COVID-19 cases growth. Figure 3 displays the average log case growth, $Y_{it} = \Delta \log(\Delta C_{it})$ in Ontario PHUs with or without mask mandates. It shows that, on average, the PHUs with a mask mandate two weeks prior have lower case growth than the PHUs without a mask mandate two weeks prior.

Figure 3: Ontario - mask mandates and weekly case growth



Notes: The figure plots the average log weekly case growth $\Delta\log(\Delta C)$ in the PHUs with mask mandate (blue) vs. without (red) mask mandate 14 days prior.

Table 1 shows the estimates of equation (1), in which we control for other policies, behaviour and information, as explained in Section 3.1.[22] We report wild bootstrap p-values clustered at the PHU level to account for the small number of clusters.[23] The odd-numbered

[22]Mask mandates and regulations on business and gatherings vary at the PHU level. Long-term care policy changed only province-wide. The other policies (schooling and travel) do not vary during the sample period and hence are omitted from the regressions with Ontario PHU data.

[23]Table A6 in the Appendix reports alternative standard error specifications: regular clustering at the PHU level (Stata command "cluster"), wild bootstrap standard errors clustered at the PHU level, and wild

columns in Table 1 use lagged cases and lagged cases growth at the PHU level as information; the even-numbered columns also include lagged cases and lagged case growth at the province level as additional information variables. In the tables, *Variable_14* indicates a 14-day lag of *Variable*.

We present estimates of equation (1) from three specifications that handle possible time effects differently. Columns (1) and (2) in Table 1 are the most basic specifications, without including a time trend. The estimates in columns (1) and (2) suggest that, controlling for behavioural changes, mandatory indoor face masks reduce the growth rate of infections by 29–32 log points ($p < 0.05$), which is equivalent to a 25–28% reduction in weekly cases.[24]

In order to control for potential province-wide factors affecting the spread of COVID-19 such as income support policies or adaptation to the pandemic over time (so-called COVID fatigue), we also estimate (1) with a cubic time trend in days from the beginning of the sample, in columns (3) and (4) of Table 1, and with week fixed effects, in columns (5) and (6). Columns (3)-(6) show that our estimates of the mask mandate policy remain robust to the inclusion of a cubic time trend or week fixed effects. The results indicate that, depending on the specification, mask mandates are associated with a reduction of up to 38 log points in weekly case growth or, equivalently, a 31% reduction in weekly cases. The magnitude of the mask policy estimate is not very sensitive to whether lagged province-level data are included as additional information.

The results in Table 1 suggest that indoor mask mandates can be a powerful preventative measure in the COVID-19 context. Our estimates of the mask mandate impact across Ontario's PHUs are equivalent to a 25–31% reduction in weekly cases. This estimate is larger than the 9–10% reduction estimated by CKS (2020) for the U.S. One possible explanation is that Ontario's mask policy is more comprehensive: we evaluate the effect of *universal* indoor mask-wearing for the public rather than the effect of mask wearing for *employees only* in CKS (2020). Differences in the compliance rate may also contribute to this difference; we discuss this potential channel in Section 4.4.

The results in Table 1 also show a statistically significant negative association between information (log of past cases, log(ΔC)_14) and current weekly case growth ($p < 0.01$ in all specifications), indicating that a higher level of cases two weeks prior is correlated with lower current case growth. While B_{it} allows for behavioural responses to information, the negative estimate on log(ΔC)_14 in Table 1 suggests that our location-based proxy does not capture

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bootstrap standard errors clustered by both PHU and date. Our results are robust to alternative ways of calculating standard errors.

[24] Using equation (3), a coefficient of x translates into a $1 - \exp(x)$ reduction in weekly cases $\Delta C_{it}/\Delta C_{it-7}$.

Table 1: Main Results - Ontario public health regions

	Outcome: weekly case growth $Y_{it}=\Delta\log(\Delta C_{it})$					
	(1)	(2)	(3)	(4)	(5)	(6)
	no time trend		cubic time trend		week fixed effects	
Mask_14	-0.291 **	-0.323 **	-0.366 **	-0.376 ***	-0.319 **	-0.327 **
	[0.017]	[0.016]	[0.010]	[0.008]	[0.021]	[0.019]
Business/gathering_14	-0.625	-0.457	-0.137	0.279	-0.098	0.054
	[0.209]	[0.473]	[0.877]	[0.689]	[0.890]	[0.935]
Long-term care_14	0.643	0.544	0.747	-0.097	-1.044	-1.997
	[0.463]	[0.549]	[0.677]	[0.930]	[0.388]	[0.102]
Behaviour proxy_14	-0.020	-0.016	-0.018	-0.018	-0.016	-0.014
	[0.160]	[0.215]	[0.266]	[0.272]	[0.302]	[0.352]
$\Delta\log(\Delta C)$_14	0.030	0.029	0.024	0.028	0.013	0.012
	[0.614]	[0.649]	[0.692]	[0.665]	[0.817]	[0.834]
$\log(\Delta C)$_14	-0.214 ***	-0.214 ***	-0.203 ***	-0.209 ***	-0.199 ***	-0.201 ***
	[0.000]	[0.000]	[0.001]	[0.001]	[0.001]	[0.001]
$\Delta\log(\Delta PC)$_14		0.287		0.184		0.543 **
		[0.307]		[0.566]		[0.046]
$\log(\Delta PC)$_14		-0.028		0.528		0.112
		[0.907]		[0.124]		[0.744]
$\Delta\log(\Delta T)$	-0.313 *	-0.409 *	-0.260	-0.382	-0.230	-0.480
	[0.087]	[0.058]	[0.287]	[0.125]	[0.492]	[0.138]
R-squared	0.046	0.050	0.051	0.058	0.091	0.094
N	3,094	3,094	3,094	3,094	3,094	3,094
public health unit FE	X	X	X	X	X	X
cubic time trend (days)			X	X		
week fixed effects					X	X

Notes: The sample time period is May 15 to August 13, 2020. P-values from wild bootstrap (cgmwildboot) standard errors clustered by public health unit (PHU) with 5000 repetitions are reported in the square brackets. Mask_14, Business/gathering_14, Behaviour_14, $\Delta\log(\Delta C)$_14, and $\log(\Delta C)$_14 are measured at the PHU level, while Long-term care_14, $\Delta\log(\Delta PC)$_14, $\log(\Delta PC)$_14, and $\Delta\log(\Delta T)$ are measured at the province level. PC denotes provincial total cases. ***, ** and * denote 10%, 5% and 1% significance level respectively. Missing values (1.3% of all observations) for Behaviour proxy_14 are imputed via linear interpolation.

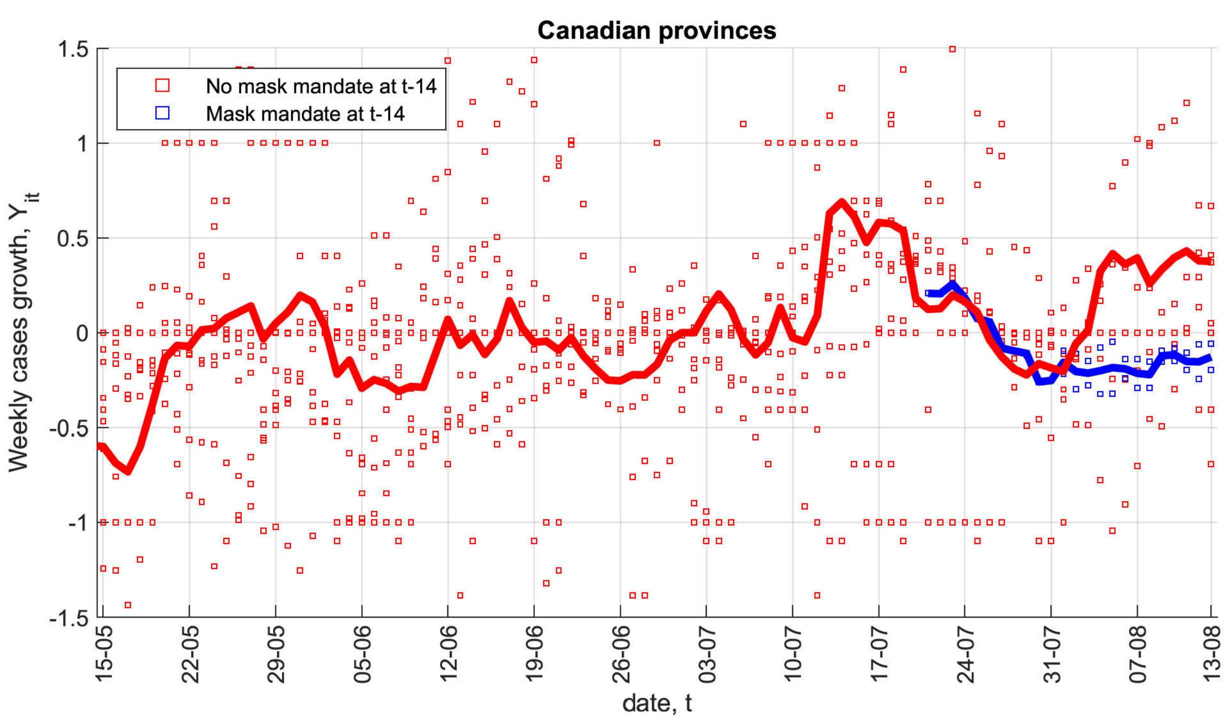
important aspects of behaviour, such as frequent hand-washing or physical distancing. In fact, our coefficient estimate on the behavioral proxy B_{it} is very close to zero (both in Table

1 and in Section 4.2's province-level results), unlike in CKS (2020).[25] In Appendix Table A18, we find strong contemporaneous correlations between the policy measures, log cases, and the Google mobility behavioral proxy from estimating equation (2). This suggests that the information (lagged cases) and the lagged policy variables included in equation (1) may absorb lagged behavioral responses proxied by B_{it-l} or other latent behavioral changes not captured by B_{it-l}.

4.2 Province-level results

We next evaluate the impact of NPIs on COVID-19 cases growth in Canada as a whole by exploiting variation in the timing of policies across the 10 provinces. Here, we examine NPIs for which there is no variation across Ontario's PHUs (i.e., schooling, travel, and LTC) in addition to mask mandates. Also, provincial data contain variation in the timing of policy changes in both the closing and re-opening phases, allowing us to study both the imposition and relaxation of restrictions.

Figure 4: Canada - mask mandates and weekly case growth



Notes: The figure plots the average weekly case growth $\Delta \log(\Delta C)$ in the provinces with mask mandate (blue) vs. without mask mandate (red) 14 days prior.

[25] We also tried including each location change measure separately and the results are similar (not shown).

CDC_TMO_000127

As in the Ontario analysis, we begin with a graphical illustration of mask mandates and COVID-19 case growth across Canadian provinces, in the period March 11 to August 13, 2020. Figure 4 plots the average log weekly case growth in the provinces with vs. without mask mandates. While mask mandates are implemented relatively late in our sample period, average case growth in the provinces with a mask mandate (Ontario and Quebec) diverged from the average case growth in the provinces without a mandate begin roughly four weeks after the mandates are imposed.[26]

Table 2 displays the estimates of equation (1) for weekly case growth, along with wild bootstrap p-values, clustered at the province level (see Table A9 for other methods of computing the standard errors). The odd-numbered columns use lagged cases and lagged case growth at the provincial level as information while the even-numbered columns include in addition lagged cases and case growth at the national level as additional information variables.

As in the Ontario analysis, we present in Table 2 estimates from three specifications: no time trend (columns (1)-(2)), including cubic time trend in days (columns (3)-(4)) and including week fixed effects (columns (5)-(6)). The most robust result is the estimated effect of mask mandates: they are associated with a large reduction in weekly case growth of 45 to 62 log points, which is equivalent to a 36 to 46% reduction in weekly cases across the different specifications. The estimates are statistically significantly different from zero in all cases, with a p-value of less than 0.001 in columns (1)-(4). It is reassuring that these results regarding mask mandates are consistent with the Ontario analysis in the previous section.

Table 2 further shows that restrictions on businesses and gatherings are associated with a reduction in the weekly case growth of 65 to 85 log points or, vice versa, that relaxing business/gathering restrictions is associated with higher case growth. The estimate is equivalent to a 48 to 57% decrease in weekly cases in our sample period. The business/gathering estimates are, however, more noisy than our estimates for mask mandates and do not retain statistical significance in the specifications with week fixed effects ($p = 0.15$ and 0.14). Tables A8 and A15 further suggest that the results on business and gathering NPIs are driven by the smaller provinces and the re-opening period (May to August). Still, these results suggest that lowered restrictions and the associated increase in business/workplace activity or gatherings can be an important offsetting factor for the estimated effect of mask mandates on COVID-19 case growth, both in our sample and in the future.

We also find that school closures (the School_14 variable in Table 2) can be negatively

[26] Figure 4 assumes a July 7 mask mandate implementation date for Ontario (when its most populous PHU, Toronto, adopted a mask mandate, along with Ottawa), and July 18 for Quebec (province-wide mandate).

Table 2: Main Results – Canada

	Outcome: weekly case growth $Y_{it}=\Delta\log(\Delta C_{it})$					
	(1)	(2)	(3)	(4)	(5)	(6)
	no time trend		cubic time trend		week fixed effects	
Mask_14	-0.446 ***	-0.484 ***	-0.618 ***	-0.613 ***	-0.581 **	-0.567 **
	[0.000]	[0.000]	[0.000]	[0.000]	[0.030]	[0.026]
Business/gathering_14	-0.654 **	-0.827 **	-0.835 **	-0.846 **	-0.648	-0.694
	[0.018]	[0.019]	[0.031]	[0.033]	[0.146]	[0.137]
School_14	-0.336	-0.480	-0.425 **	-0.433 **	-0.261	-0.347
	[0.352]	[0.196]	[0.015]	[0.019]	[0.235]	[0.130]
Travel_14	-0.585	-0.772	-0.375	-0.412	-0.396	-0.553
	[0.146]	[0.118]	[0.613]	[0.636]	[0.695]	[0.559]
Long-term care_14	-0.052	-0.119	0.023	0.032	0.063	0.056
	[0.824]	[0.715]	[0.958]	[0.920]	[0.889]	[0.898]
Behaviour proxy_14	-0.009	-0.008	-0.001	0.000	-0.003	0.001
	[0.257]	[0.350]	[0.880]	[0.972]	[0.858]	[0.935]
$\Delta\log(\Delta C)$_14	-0.061	-0.062	-0.078 *	-0.072	-0.055	-0.054
	[0.177]	[0.262]	[0.090]	[0.198]	[0.449]	[0.459]
$\log(\Delta C)$_14	-0.223 ***	-0.244 ***	-0.227 **	-0.227 *	-0.224	-0.232
	[0.000]	[0.003]	[0.019]	[0.090]	[0.102]	[0.113]
$\Delta\log(\Delta NC)$_14		0.015		-0.107		-0.050
		[0.895]		[0.631]		[0.807]
$\log(\Delta NC)$_14		0.141		0.055		0.302 **
		[0.326]		[0.825]		[0.048]
$\Delta\log(\Delta T)$	0.112	0.166 *	0.172 **	0.169 *	0.158	0.166 *
	[0.170]	[0.074]	[0.043]	[0.056]	[0.110]	[0.073]
R-squared	0.406	0.410	0.414	0.414	0.430	0.433
N	1,560	1,560	1,560	1,560	1,560	1,560
province fixed effects	X	X	X	X	X	X
cubic time trend (days)			X	X		
week fixed effects					X	X

Notes: The time period is March 11 to August 13, 2020. P-values from wild bootstrap (cgmwildboot) standard errors clustered by province with 5000 repetitions are reported in the square brackets. ***, ** and * denote 10%, 5% and 1% significance level respectively. NC denotes national total cases.

associated with case growth. However, the estimates are statistically significant from zero only in the specifications with cubic time trend (columns (3) and (4)). As seen in Figure

2, provincial school closures occurred in a very short time interval during M rch, so we may lack statistical power to separately identify its effect from other NPIs (especially the travel-related). Hence, we interpret this result with caution.

As in Table 1, the level of past cases, $\log(\Delta C)$, is negatively and statistically significantly associated with current weekly case growth in columns (1)-(4).

Since the specification with cubic time trend in Tables 1 and 2 allows for possible non-monotonic aggregate time trends in case growth in a parsimonious way, we choose it as our baseline specification with which to perform robustness checks in the next section. Robustness checks with the other specifications are available upon request.

4.3 Robustness

Policy collinearity

A possible concern about our data for the national analysis is that some NPIs (e.g. international travel restrictions or closing of schools) were implemented within a very short time interval.[27] Thus, we may lack enough regional variation to distinguish and identify the separate effect of each policy.[28] Collinearity could also affect the standard errors and the signs of the estimated coefficients.

To check robustness with respect to potential collinearity in the NPI policies, Tables A7 and A10 report estimates from our baseline specification, omitting one policy at a time, for Ontario and Canada respectively. First, it is reassuring that the mask mandate estimates are hardly affected by omitting any of the other policies. This is expected since mask mandates were imposed during a period where other NPIs changed little (see Figure 2). Similarly, the effects of business/gathering regulations and school closures in Table A10 are not sensitive to omitting other policies one at a time, which suggests that there is sufficient statistical power and variation to identify them in the national analysis.

Treatment of zero weekly cases

Another concern for our empirical strategy is that the usual formula for our dependent variable, $\Delta \log(\Delta C_{it})$, cannot be applied when the weekly case total ΔC_{it} is zero. We follow CKS (2020) and replace ln(0) with -1 in our baseline specifications in Tables 1 and 2. We now check the robustness of our estimates to alternative treatments of zero weekly cases.

For easier comparison, the first two columns in Table A5 repeat columns (3) and (4)

[27] For example, Table A4 shows a correlation of 0.61 between the Travel and School policy aggregates.

[28] Aggregating the 17 basic policy indicators into five groups mitigates this issue. Here, we test whether any remaining collinearity poses a problem.

from Table 1 for Ontario.[29] Our main results on mask mandates across Ontario PHUs are robust to replacing log(0) with 0 and to adding 1 to all ΔC_{it} observations before taking logs, as shown in columns (3)-(6) of Table A5. Another way to mitigate the issue of PHUs with very few cases is to estimate a weighted least squares regression where PHUs are weighted by population. Columns (7) and (8) in Table A5 show that the resulting mask estimate has a slightly smaller magnitude and, due to the reduced effective sample size, weaker statistical significance.

Similarly, Table A8 shows that our province-level estimates, in particular for mask mandates, are also robust to the same manipulations as above.[30] In columns (9) and (10) of Table A8, we restrict the sample to only the largest 4 provinces (British Columbia, Ontario, Quebec and Alberta), which have only 0.3% (2 out of 624) zero observation cases. Again, the estimated mask effects are little changed.

Alternative dates

Figure B4 shows that our estimates and confidence intervals for the effect of mask mandates in the Ontario baseline regressions do not vary much by the initial date of the sample. Similarly, Figure B5 shows that, in the national analysis, our results about mask mandates and business/gathering restrictions are also robust to alternative sample start dates.

Alternative lags

We explore alternative time lags, either shorter or longer in duration, centered around the baseline value of 14 days. Figure B6 (with Ontario data) and Figure B7 (with province-level data) plot the estimates and confidence intervals from the baseline regressions and show that our mask effect estimates remain fairly consistent for different lags.

Omitted variables

Our behaviour proxy variable (Google geo-location trends) likely misses some aspects of behaviour relevant for COVID-19 transmission. One factor that may meaningfully impact behaviour is weather. For example, good weather could entice more people to spend time outside, lowering the chance of viral transmission. Columns (3) and (4) in Table A11 report national estimates with lagged weather variables (daily maximum and minimum temperatures and precipitation for the largest city in each province[31]) as additional regressors. Our NPI estimates, in particular mask mandates, are little changed from the baseline results in columns (1) and (2).

[29] 535 out of 3,094 observations (17%) had to be replaced.
[30] 230 out of 1,560 observations (15%) had to be replaced.
[31] Vancouver, BC; Calgary, AB; Saskatoon, SK; Winnipeg, MB; Toronto, ON; Montreal, QC; Moncton, NB; Halifax, NS; Charlottetown, PE; and St. John's, NL.

Another possible concern is that our information variables, lagged cases and lagged case growth, may not fully capture the information based on which people react or adjust their behaviour, possibly affecting the observed weekly case growth. Columns (5) and (6) in Table A11 add a national-level "news" variable to the baseline specification. The news variable is defined as the number of daily search results from a news aggregator website (*Proquest Canadian Newsstream*) for the terms "coronavirus" or "COVID-19" (see Appendix C for more details). In column (6), the lagged news variable approaches the 10% significance level ($p = 0.103$). Our estimates on masks and business/gathering remain very close to those in the baseline.

4.4 Self-reported mask usage

The effectiveness of any NPI or public policy crucially depends on whether it affects behaviour. In this section, we use self-reported data on mask usage to examine whether mask mandates indeed increase mask use in Canada ("first-stage" analysis).

We use data from the YouGov COVID-19 Public Monitor, which includes multiple waves of public opinion surveys fielded regularly since early April in many countries.[32] Here, we focus on inter-provincial comparison within Canada. Our variable of interest is based on responses to the question "Thinking about the last 7 days, how often have you worn a face mask outside your home (e.g. when on public transport, going to a supermarket, going to a main road)?" The answer choices are "Always", "Frequently", "Sometimes", "Rarely", and "Not at all". We create a binary variable taking value 1 if the response is "Always" and 0 otherwise, as well as another variable taking value of 1 if the respondent answered either "Always" or "Frequently" and 0 otherwise.

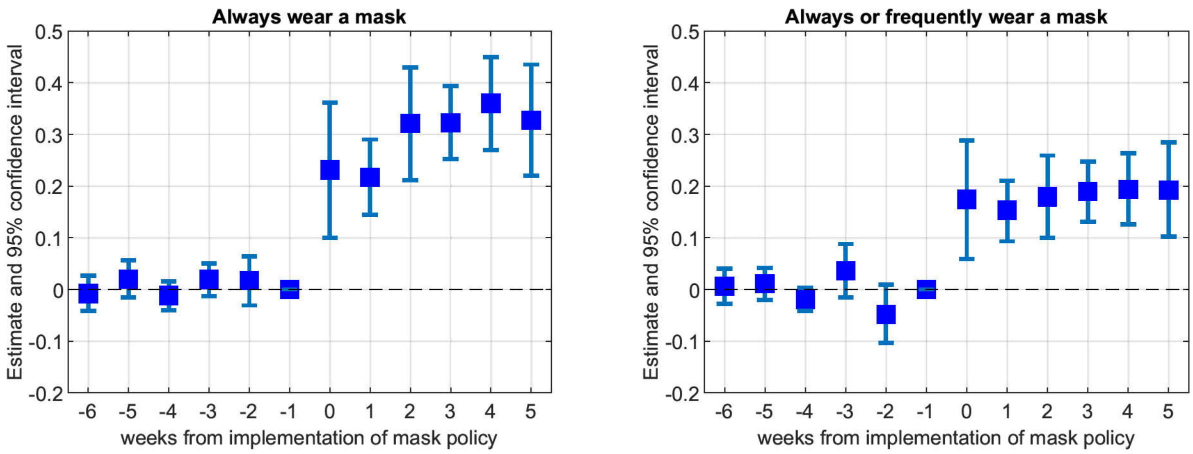
We begin with a simple illustration of self-reported mask usage in Canada from April to August 2020. Figure B2 plots the average self-reported mask usage (the response "Always") in the provinces with and without mask mandates.[33] The figure clearly shows that self-reported mask usage is higher, by up to 50 percentage points, in the provinces with a mask mandate than in the provinces without mask mandates. Since Figure B2 does not account for compositional changes in the data, we formally estimate equation (2), using self-reported mask usage as the behavioral outcome.[34]

[32]The YouGov data can be accessed at: https://yougov.co.uk/covid-19.

[33]As on Figure 4, we use July 7 as the mask mandate implementation date in Ontario.

[34]Since mask usage is reported only for specific dates within each survey wave, we use our mask policy variable daily values for these same dates instead of the weekly moving average.

Figure 5: Event Study of Self-reported Mask Usage – Canada



Notes: The data source is YouGov. The outcome is a binary variable taking value 1 if the respondent respectively answered "Always" (in the left panel) or "Always" or "Frequently" (in the right panel) to "Thinking about the last 7 days, how often have you worn a face mask outside your home?" The figure plots the estimates from a version of equation (2) where the mask policy variable is replaced by the interaction of the variables corresponding to being in the treatment group (imposed mask mandate) and a series of dummies for each week, ranging from 6 weeks before the mask mandate to 6 weeks after ($T = -6$ to $+5$, where $T = 0$ is the mandate implementation date). The reference point is 1 week before the implementation ($T = -1$). Wild bootstrap (cgmwildboot) standard errors clustered by province with 5000 repetitions are used to construct the confidence intervals. Sample weights are used.

Figure 5 shows a graphical event study analysis on mask mandates and changes in mask usage. The event study approach is appropriate for the mask usage outcome variable, since the policy impact is expected to be immediate, unlike the other outcomes we study, for which any impact is expected to occur with a lag and we use weekly totals or moving averages. We replace the mask policy variable in equation (2) by the interaction of variables corresponding to being in the treatment group (i.e. under a mask mandate), and a series of dummies for each week, ranging from 6 weeks before the mask mandate to 5 weeks after the mask mandate ($T = -6$ to $+5$, where $T = 0$ is the implementation date of the mask mandate). The reference point is one week before the implementation of the mask mandate ($T = -1$), and we use the same y-axis scale on both panels.

The left and right panels of Figure 5 present the results from the event study analysis for the "Always" and "Always" or "Frequently" mask usage answers, respectively. We make several observations. First, neither panel shows a pre-trend – the estimates are close to zero before the mask mandates. This addresses the potential concern that provinces that implemented mask mandates may have had a different trend in mask usage than provinces that did not. Second, the effect of mask mandates on mask usage is immediate: an increase of roughly 20 percentage points as soon as the mask policy is implemented at ($T = 0$). Third, the effect appears persistent rather than transitory, since mask usage after $T = 0$ does not revert to its level before $T = 0$.

Table 3: Self-reported mask usage – Canada

	Outcome: "Always wear mask"					
	(1)	(2)	(3)	(4)	(5)	(6)
	no time trend		cubic time trend		week fixed effects	
Mask	0.404 ***	0.396 ***	0.304 ***	0.315 ***	0.310 ***	0.310 ***
	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]
$\Delta\log(\Delta C)$	-0.017	-0.006	-0.008	-0.006	-0.004	-0.008
	[0.663]	[0.611]	[0.524]	[0.595]	[0.656]	[0.464]
$\log(\Delta C)$	-0.025	0.015 **	0.004	0.006	0.006	0.007
	[0.127]	[0.025]	[0.662]	[0.544]	[0.504]	[0.502]
$\Delta\log(\Delta NC)$		-0.106 *		-0.023		0.191
		[0.054]		[0.324]		[0.108]
$\log(\Delta NC)$		-0.089 ***		-0.028		-0.068
		[0.000]		[0.669]		[0.582]
R-squared	0.157	0.169	0.172	0.172	0.173	0.174
N	8,859	8,859	8,859	8,859	8,859	8,859
individual characteristics	X	X	X	X	X	X
province fixed effects	X	X	X	X	X	X
cubic time trend (days)			X	X		
week fixed effects					X	X
† average mask usage rate without mask mandate = 0.298						

Notes: The time period is April 2 to August 13, 2020. P-values from wild bootstrap (cgmwildboot) standard errors clustered by province with 5000 repetitions are reported in the square brackets. NC denotes national total cases. The data source is YouGov. The outcome is a dummy which takes value 1 if the respondent answered "Always" to the survey question "Thinking about the last 7 days, how often have you worn a face mask outside your home?" Sample weights are used. Individual characteristics include a gender dummy, age dummy (in years), dummies for each household size, dummies for each number of children, and dummies for each employment status. ***, ** and * denote 10%, 5% and 1% significance level respectively.

Table 3 displays the estimates on self-reported mask usage (answer "Always") in equation (2) along with wild bootstrap p-values clustered at the province level. The odd-numbered columns use lagged cases and lagged case growth at the provincial level as information while the even-numbered columns include in addition lagged cases and case growth at the national level as additional information variables. As in Table 1 and Table 2, we present estimates without time trend, including cubic time trend (in days), and including week fixed effects. Our preferred specification with cubic time trend, column (4) of Table 3, shows that mask mandates are associated with 31.5 percentage point increase in self-reported mask usage ($p < 0.001$), from a base of self-reported mask usage without mask mandate of 29.8%.[35,36]

These "first-stage" results show that mask mandates exhibit significant compliance in Canada and establish a basis for the significant impact of mask mandates on the spread of COVID-19 that we find. That said, given that mask mandates do not change everyone's behaviour, our estimates in Tables 1 and 2 represent intent-to-treat effects. The full effect of the entire population shifting from not wearing to wearing masks is likely significantly larger.[37]

There is a heated debate on whether community use of masks may create a false sense of security that reduces adherence to other preventive measures. We also investigate this question using YouGov survey data. As Tables A13 and A14 indicate, we find no evidence that mask mandates in Canada have had an offsetting effect on other preventive measures such as hand washing, using sanitizer, avoiding gatherings, and avoiding touching objects in public during the period we study. On the contrary, mask mandates may slightly increase social distancing in one out of the eight precaution categories (avoiding crowded areas) ($p < 0.10$).[38]

4.5 Counterfactuals

We evaluate several counterfactuals corresponding to replacing the actual mask policy in a province or Canada-wide with a counterfactual policy, including absence of mask mandate.

Letting t_0 be the implementation date of a counterfactual policy, we set the counterfactual weekly case count, ΔC^c_{it}, equal to ΔC_{it} for all $t < t_0$. For each date $t \geq t_0$, using the definition of Y_{it} from (3), we then compute the counterfactual weekly cases, ΔC^c_{it} and the counterfactual

[35]Similarly, in Table A12, column (4) shows that "Always" or "Frequent" mask usage increases by 21.5 percentage points. The finding that the increase in mask usage among the "Always" respondents is larger than among the "Always" or "Frequent" respondents is consistent with some people switching from wearing masks "frequently" to "always."

[36]Hatzius et al. (2020) document that state mask mandates in the US increased mask usage roughly by 25 percentage points in 30 days. The compliance with mask mandates may differ across countries or regions based on social norms, peer effects, political reasons or the consequences of noncompliance (e.g., fines).

[37]If we take the increase of about 30 percentage points in reported mask usage induced by mask mandates at face value, the full effect of mask wearing (treatment-on-the-treated effect) would be roughly triple our estimates. It could be larger still if there is desirability bias in answering the mask usage survey question, so that the actual increase in mask use may be smaller than our estimate.

[38]Consistent with this result, Seres et al. (2020) find that wearing masks increased physical distancing based on a randomized field experiment in stores in Germany.

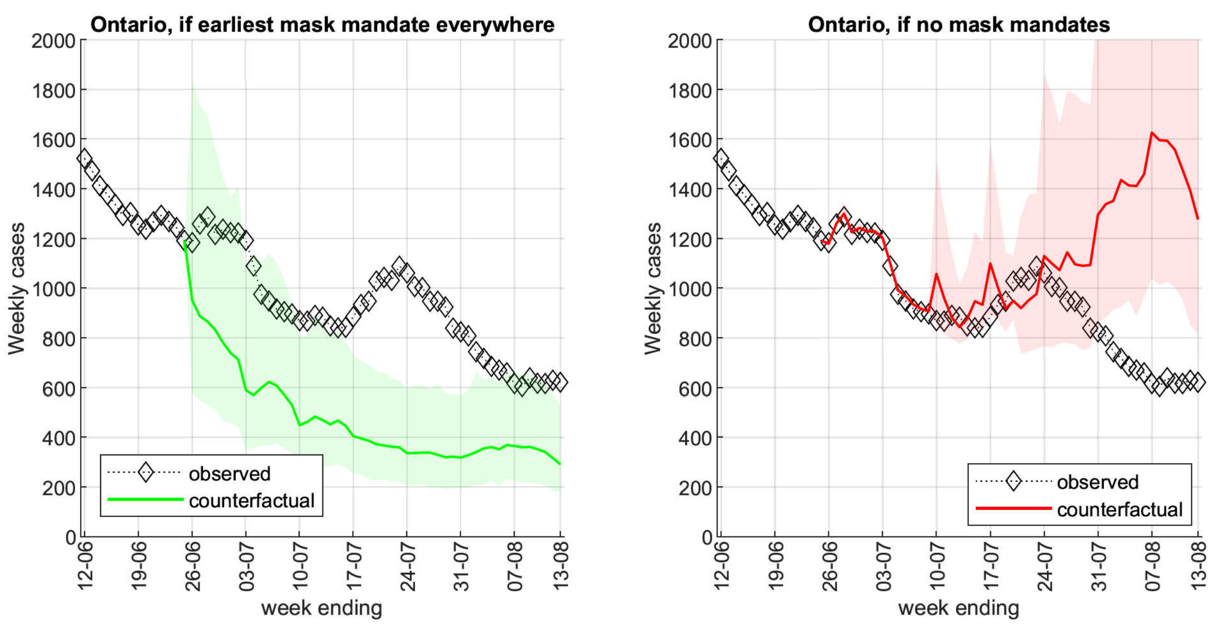
case growth rate, Y^c_{it}, as follows:

$$\Delta C^c_{it} = \exp(Y^c_{it})\left(\Delta C^c_{it-7}\right) \text{ and}$$

$$Y^c_{it} = \hat{Y}_{it} + \beta_{Mask\_14}\left(\text{Mask}^c\text{\_14} - \text{Mask\_14}\right) + \beta_{log\_dC\_14}\left(\ln(\Delta C^c_{it-14}) - \ln(\Delta C_{it-14})\right),$$

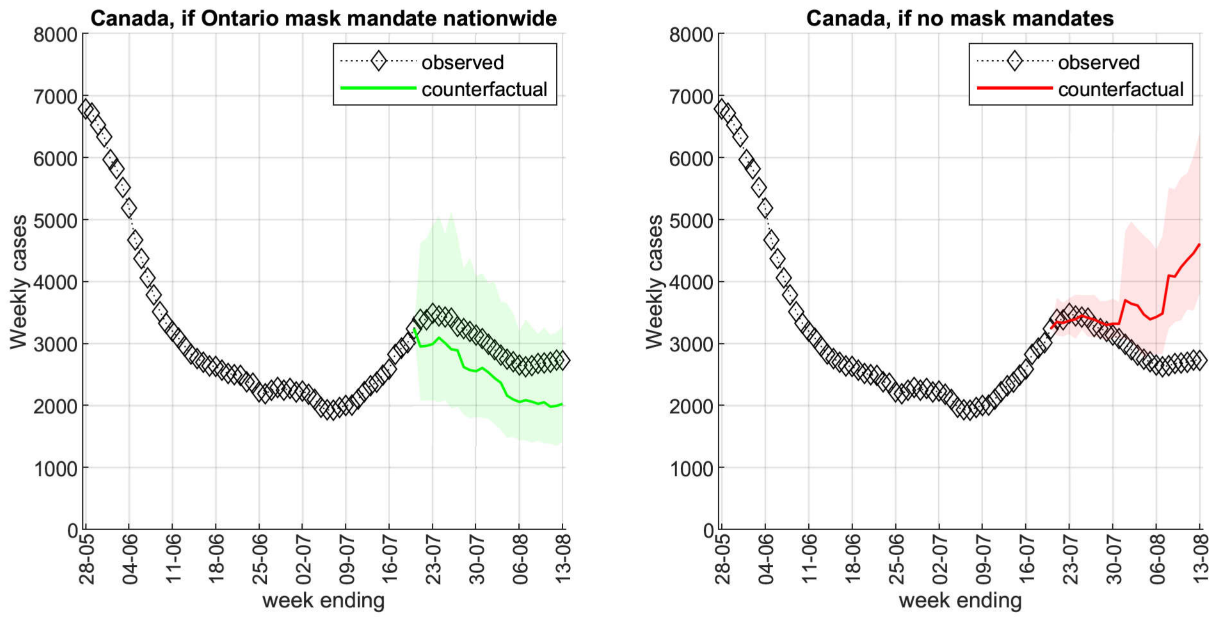
where $\hat{Y}_{it}$ is the regression-fitted value of weekly case growth; β_{Mask_14} is the coefficient estimate on the mask mandate variable Mask_14 in baseline specification (4) in Table 1 or 2, depending on the counterfactual; Mask^c_14 is the counterfactual mask policy (e.g. different implementation date, wider geographic coverage or absence of mask mandate); and $\beta_{log\Delta C\_14}$ is the coefficient estimate (-0.227 or -0.209) on lagged cases $\log(\Delta C)$_14 in Table 1 or 2, column 4. The coefficient $\beta_{log\Delta C\_14}$ adjusts the counterfactual case growth rate for the negative statistically significant association between the weekly case total two weeks prior and time-t case growth. This effect may be due to people being more careful when they perceive the risk of infection to be higher or less careful vice versa.

Figure 6: Counterfactuals - Ontario public health regions



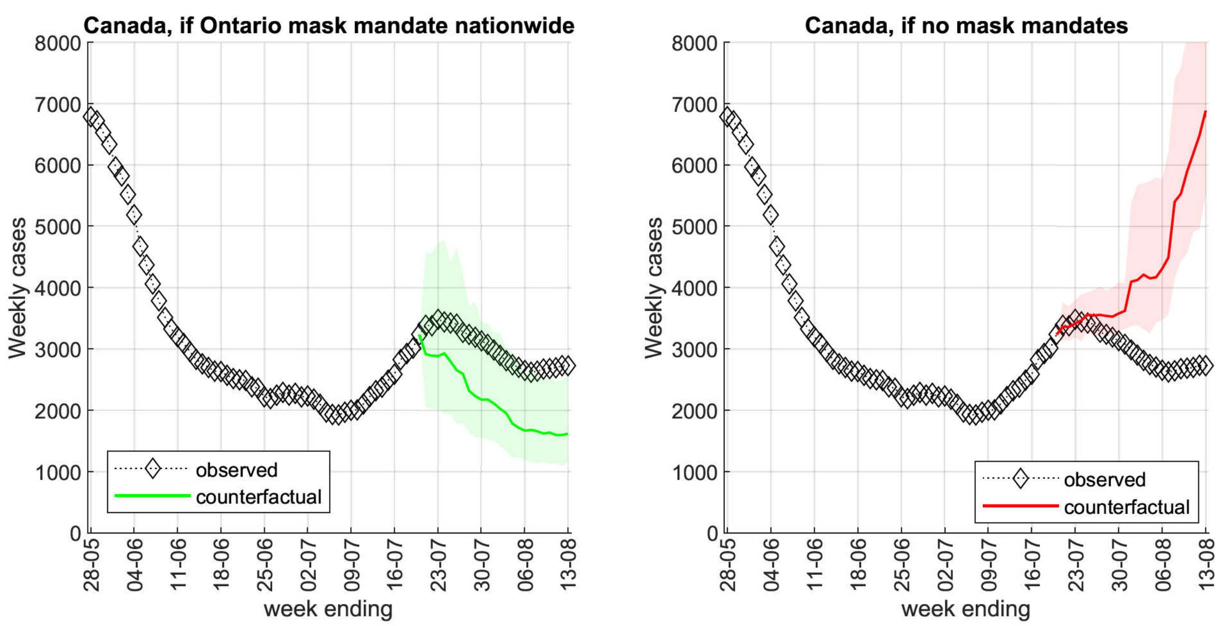
Notes: The left panel assumes that mask mandates were adopted in all PHUs on June 12 (date of the first mask mandate in ON). The right panel assumes that mask mandates were not adopted in any PHU. We use the mask estimate (-0.376) from column (4) of Table 1.

Figure 7: Counterfactuals – Canada (Table 1 mask estimate)



Notes: The left panel assumes that mask mandates were adopted in all provinces on July 7 (the adoption date in Toronto and Ottawa). The right panel assumes that mask mandates were not adopted in any province. We use the mask estimate (-0.376) from column (4) of Table 1.

Figure 8: Counterfactuals – Canada (Table 2 mask estimate)



Notes: The left panel assumes that mask mandates were adopted in all provinces on July 7 (the adoption date in Toronto and Ottawa). The right panel assumes that mask mandates were not adopted in any province. We use the mask estimate (-0.613) from column (4) of Table 2.

Figures 6, 7 and 8 show results from two counterfactual policy evaluations. The first exercise, depicted in the left-hand side panel of the figures, assumes that masks are adopted everywhere at the earliest date observed in the data. Specifically, Figure 6 considers the counterfactual of all Ontario PHUs adopting mask mandates on June 12, while Figures 7 and 8 assume that all provinces adopt a mask mandate on July 7.[39]

Using our mask policy estimate from Table 1, Figure 6 shows that an earlier face mask mandate across Ontario PHUs could have lead to an average reduction of about 300 cases per week as of August 13, holding all else equal. For Canada as a whole, a nation-wide adoption of mask mandates in early July is predicted to reduce total cases per week in the country by 700 to 1,100 cases on average as of August 13, depending on whether we use the more conservative mask estimate (-0.376) from column (4) of Table 1 (see Figure 7) or the larger estimate (-0.613) from column (4) of Table 2 (see Figure 8). In all cases, the indirect feedback effect via $\beta_{log\Delta C\_14}$ (lagged cases as information) starts moderating the decrease in cases two weeks after the start of the counterfactual mask policy.

In the right-hand side panel of Figures 6, 7 and 8, we perform the opposite exercise, namely assuming instead that mask mandates were *not* adopted in any Ontario PHU or any Canadian province. Our estimates imply that the counterfactual absence of mask mandates would have led to a large increase in new cases, both in Ontario and Canada-wide, especially when using the larger mask coefficient estimate from Table 2 (see Figure 8).

Finally, in Figure B11 in the Appendix, we also evaluate the counterfactual in which British Columbia and Alberta, the third and fourth largest Canadian provinces by population, adopt province-wide mask mandates on July 15. The results, using the Mask_14 estimate from Table 2, suggest a reduction of about 300 cases per week in each province by mid-August.

The counterfactual simulations assume that all other variables, behaviour and policies (except the mask policy and $t - 14$ cases) remain fixed, as observed in the data. This is a strong assumption, but it may be plausible over the relatively short time period that we analyze. Moreover, the counterfactuals assume that regions without a mask mandate would react in the same way, on average, as the regions that imposed a mandate. Therefore, these results should be interpreted with caution and only offer a rough illustration and projection of the estimated effect of mask mandates on COVID-19 cases.

[39] June 12 is the date of the earliest mask mandate in Ontario. For the national analysis, July 7, the effective date for Toronto and Ottawa, is considered Ontario's first significant date of mask mandate enactment: PHUs with earlier mandates account for less than 10% of Ontario's population.

4.6 Additional analysis

Closing and re-opening sub-periods

We investigate whether policy impact varied in different phases of the pandemic by splitting the full sample period into two sub-periods: "closing" (March 11 to May 14) and "re-opening" (May 15 to August 13). The dividing date of May 15 (referring to the NPIs in place around May 1) was chosen because very few policies were relaxed before May 1, and very few non-mask policies were tightened after May 1 in our sample period (see Figure 2).

In Table A15, we report estimates and wild bootstrap standard errors using our baseline specification with cubic time trend, separately for the closing and re-opening periods. We find that the imposition of school closures and travel restrictions early in the closing period is associated with a very large subsequent reduction in weekly case growth, as can be also seen on Figure B8 – the average observed log growth rate of cases $\Delta \log(\Delta C)$ falls from 2.4 (ten-fold growth in weekly cases) to -0.4 (33% decrease in weekly cases) between March 15 and April 5. Long-term care restrictions are also associated with reduced case growth two weeks later during the March to May closing period. We interpret these results with caution, however, since many of these policy measures and restrictions were enacted in a brief time interval during March and there is not much inter-provincial variation (see Figure 2). No mask mandates were present in the closing period.

In the re-opening period, our results in Table A15 are in line with our full-sample results for mask mandates and business/gathering regulations (Table 2), with slightly larger coefficient estimates and less statistically significant p-values, possibly due to the smaller sample. Travel and school closures are not statistically significant in the re-opening period. This is unsurprising: relaxation of travel policies was minor and endogenous (only re-open to safe areas within Canada), and the schools that re-opened (in parts of Quebec and, on a part-time basis, in British Columbia) did so on voluntary attendance basis, yielding smaller class sizes.

Deaths

We also examine the weekly death growth as an outcome. We only have access to disaggregated deaths data at the province level (not at PHU levels in Ontario). We thus estimate regression equation (1) using $Y_{it} = \Delta \log(\Delta D_{it})$ for each province i as the dependent variable. In addition, we use a 28-day lag for the policy, behaviour proxy, and information variables to reflect the fact that deaths occur on average about two weeks after case detection; see Appendix D for details and references.[40]

[40] In Table 4, *Variable_28* denotes the *Variable* lagged by 28 days.

Table 4 reports the estimates from the same specifications as those for case growth in Table 2. In all specifications, mask mandates are associated with a large reduction in the observed weekly deaths growth rate four weeks later (more than 90 log points, or equivalently more than 60% reduction in weekly deaths). These results are larger than our case growth results, but consistent with them given the substantial uncertainty. See also Figure B12, which plots the average weekly death growth in the provinces without a mask mandate four weeks prior vs. that for Ontario, the only province with mask mandate four weeks prior in our sample period.

The robustness checks in Table A16, however, show that, unlike for case growth, the mask mandate estimates in Table 4 are not robust to weighing by population or to restricting the sample to the largest 4 provinces. This suggests that the estimated effect is largely driven by observations from the small provinces, which have a disproportionately larger number of zero or small weekly death totals.[41] Furthermore, given the 28-day lag, there are only 9 days with observations (from Ontario only) for which the mask mandate variable takes value of 1. Due to these serious data limitations, the relation between mask mandates and COVID-19 deaths in Table 4 is suggestive at best, and we urge caution in interpreting or extrapolating from these results.

That said, our main findings about the growth in cases may have implications about future growth in deaths, particularly if the affected demographics become less skewed toward the young in later periods.

[41] 205 out of the 1,470 observations (14%) had log(0) replaced by -1.

Table 4: Canada – deaths growth rate and policies

	Outcome: weekly deaths growth, $\Delta\log(\Delta D)$					
	(1)	(2)	(3)	(4)	(5)	(6)
	no time trend		cubic time trend		week fixed effects	
Mask_28	-1.391 ***	-1.453 ***	-0.922 **	-0.983 **	-0.904 **	-0.915 **
	[0.000]	[0.000]	[0.022]	[0.032]	[0.036]	[0.045]
Business/gathering_28	0.241	0.271	-0.134	-0.224	-0.279	-0.268
	[0.529]	[0.521]	[0.762]	[0.748]	[0.712]	[0.732]
School_28	0.002	0.018	0.441	0.440	0.624	0.630
	[0.974]	[0.924]	[0.317]	[0.341]	[0.114]	[0.113]
Travel_28	-0.176	-0.287	-0.005	-0.027	-0.191	-0.161
	[0.553]	[0.432]	[0.972]	[0.935]	[0.638]	[0.718]
Long-term care_28	-0.091	-0.140	-0.035	-0.036	-0.024	-0.017
	[0.592]	[0.600]	[0.900]	[0.900]	[0.936]	[0.948]
Behaviour proxy_28	0.003	0.000	0.002	0.003	0.005	0.005
	[0.718]	[1.000]	[0.815]	[0.737]	[0.675]	[0.695]
$\Delta\log(\Delta D)$_28	0.151	0.175	0.141	0.152	0.154	0.153
	[0.194]	[0.245]	[0.361]	[0.345]	[0.266]	[0.266]
$\log(\Delta D)$_28	-0.238 ***	-0.248 ***	-0.216 ***	-0.220 ***	-0.229 ***	-0.227 ***
	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]
$\Delta\log(\Delta ND)$_28		-0.110		-0.121		-0.019
		[0.471]		[0.476]		[0.806]
$\log(\Delta ND)$_28		-0.015		0.018		-0.053
		[0.743]		[0.858]		[0.557]
$\Delta\log(\Delta T)$	0.081	0.018	-0.038	-0.051	-0.037	-0.037
	[0.409]	[0.922]	[0.758]	[0.735]	[0.752]	[0.748]
R-squared	0.233	0.239	0.251	0.254	0.286	0.286
N	1,470	1,470	1,470	1,470	1,470	1,470
province fixed effects	X	X	X	X	X	X
cubic trend in days			X	X		
week fixed effects					X	X

Notes: The time period is March 11 to August 13, 2020. P-values from wild bootstrap (cgmwildboot) standard errors clustered by province with 5000 repetitions are reported in the square brackets. ***, ** and * denote 10%, 5% and 1% significance level respectively. ND denotes national total deaths.

5 Conclusion

The wearing of face masks by the general public has been a very contentious policy issue during the COVID-19 pandemic, with health authorities in many countries and the World Health Organization giving inconsistent or contradictory recommendations over time. "Conspiracy theories" and misinformation surrounding mask wear abound in social media, fuelled by some individuals' perception that mask mandates constitute significant restrictions on individual freedoms. Given the absence of large-scale randomized controlled trials or other direct evidence on mask effectiveness in preventing the spread of COVID-19, quantitative observational studies like ours are essential for informing both public policy and the public opinion.

We estimate the impact of mask mandates and other public policy measures on the spread of COVID-19 in Canada. We use both within-province and cross-province variation in the timing of mask mandates and find a robust and significantly negative association between mask mandates and subsequent COVID-19 case growth – 25 to 46% average reduction in weekly cases in the first several weeks after adoption, depending on the data sample and empirical specification used. These results are supported by our analysis of survey data on compliance with the mask mandates, which show that the mandates increase the proportion of reporting as always wearing a mask in public by around 30 percentage points. However, our sample period does not allow us to determine whether their effect lasts beyond the first few weeks after implementation. We conclude that mask mandates can be a powerful policy tool for at least temporarily reducing the spread of COVID-19.

Mask mandates were introduced in Canada during a period where other policy measures were relaxed, as part of the economy's re-opening. Specifically, we find that relaxed restrictions on businesses or gatherings are positively associated with subsequent COVID-19 case growth – a factor that could offset and obscure the health benefits of mask mandates. Past case totals were also found to matter for subsequent COVID-19 outcomes, suggesting that riskier behaviour based on favourable lagged information may limit how low mask mandates and other restrictions – short of a lockdown – can push the number of new cases.

We have deliberately abstained from studying the direct economic impacts of COVID-19, focusing instead on the unique features of the Canadian data for identifying the effect of NPIs, in particular mask mandates, on COVID-19 case growth. Future research combining epidemiological finding with the economic benefits and costs of various public policies or restrictions would enrich the ongoing policy debate and provide further guidance.

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