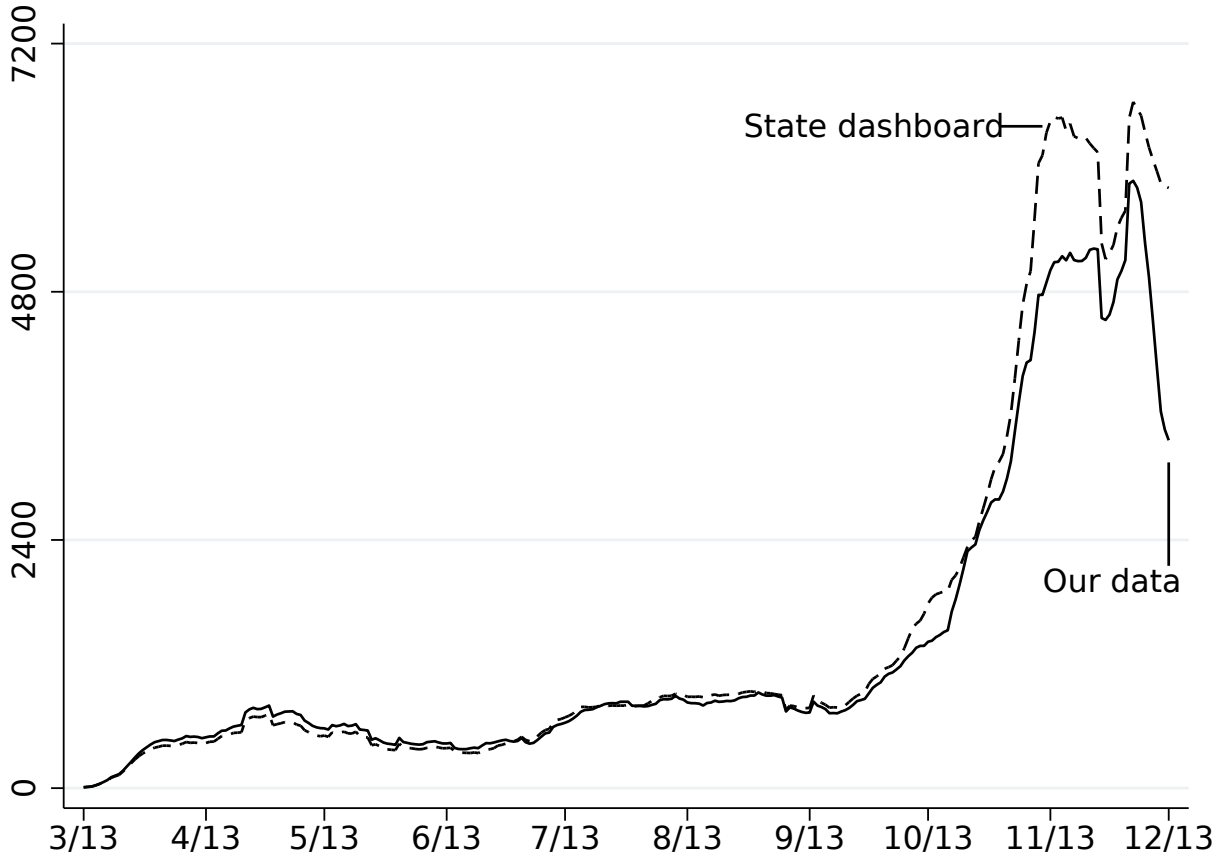
For online publication

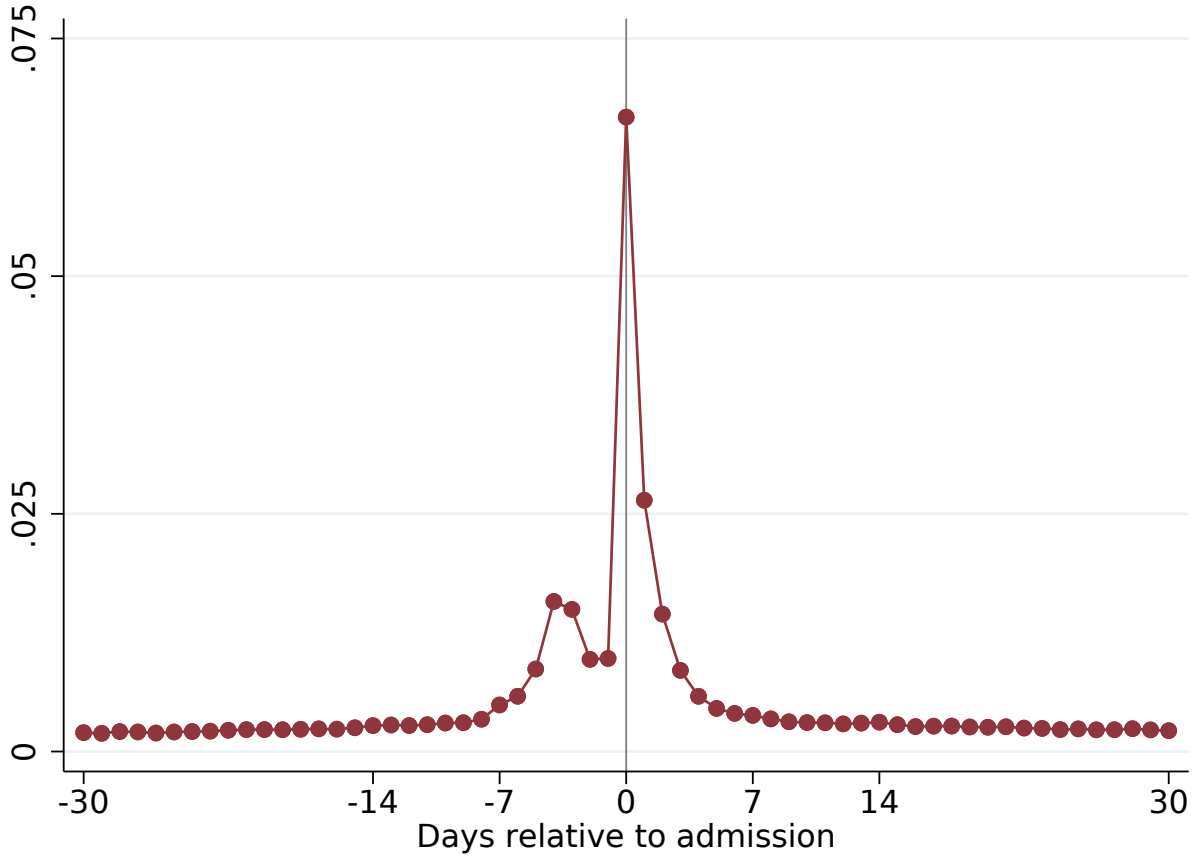
A Appendix figures and tables

Figure A.1: Positive cases in our data and on state dashboard



Notes: Figure plots 7-day moving average of the number of positive cases reported on Indiana's COVID-19 dashboard (Indiana State Department of Health, 2020), as well as the number of positive cases observed in our data.

Figure A.2: Timing of tests relative to hospitalization



Notes: Figure plots the fraction of hospitalized patients who had a SARS-CoV-2 test on the indicated day relative to their admission, for non-ICLI hospitalizations, defind as hospitalizations with no diagnosis for influenza-like or COVID-like illness. Patients who are never tested are in the denominator, and a patient can be tested on multiple days.

Table A.1: Weekly test rates, by sample

Sample	Population	Non-ICLI		Clear Cause		ICLI	
Week	% Tested (1)	N (2)	% Tested (3)	N (4)	% Tested (5)	N (6)	% Tested (7)
13mar	0.001	6,094	0.034	1,363	0.021	1,140	0.294
20mar	0.002	5,173	0.115	1,278	0.065	1,226	0.734
27mar	0.003	4,697	0.155	1,189	0.124	1,342	0.776
03apr	0.003	4,677	0.174	1,273	0.108	1,204	0.754
10apr	0.004	4,841	0.177	1,248	0.114	1,078	0.741
17apr	0.005	5,057	0.194	1,348	0.185	1,191	0.702
24apr	0.006	5,303	0.196	1,431	0.150	1,108	0.621
01may	0.007	5,832	0.302	1,442	0.303	1,195	0.755
08may	0.008	6,187	0.325	1,555	0.273	1,106	0.756
15may	0.009	6,774	0.321	1,576	0.268	1,120	0.776
22may	0.007	6,793	0.315	1,581	0.272	1,059	0.729
29may	0.008	6,949	0.318	1,562	0.291	975	0.715
05jun	0.010	7,359	0.314	1,715	0.291	996	0.753
12jun	0.012	7,554	0.323	1,707	0.251	974	0.728
19jun	0.012	7,437	0.320	1,644	0.279	946	0.759
26jun	0.011	7,434	0.325	1,655	0.291	898	0.739
03jul	0.010	7,441	0.325	1,660	0.289	937	0.719
10jul	0.013	7,591	0.346	1,702	0.340	1,050	0.744
17jul	0.015	7,650	0.340	1,658	0.289	992	0.708
24jul	0.014	7,671	0.311	1,675	0.279	1,028	0.750
31jul	0.015	7,516	0.323	1,634	0.278	1,126	0.690
07aug	0.016	7,722	0.308	1,638	0.278	1,106	0.636
14aug	0.017	7,709	0.321	1,751	0.274	1,048	0.725
21aug	0.016	7,745	0.300	1,770	0.262	1,114	0.679
28aug	0.016	7,733	0.290	1,715	0.247	1,006	0.671
04sep	0.015	7,448	0.299	1,637	0.268	1,015	0.657
11sep	0.016	7,871	0.321	1,717	0.286	1,030	0.637
18sep	0.016	7,857	0.301	1,719	0.246	1,074	0.622
25sep	0.016	7,791	0.310	1,678	0.271	1,146	0.641
02oct	0.018	7,655	0.311	1,713	0.265	1,306	0.647
09oct	0.016	7,476	0.303	1,622	0.256	1,414	0.635
16oct	0.022	7,333	0.343	1,597	0.274	1,457	0.650
23oct	0.023	7,356	0.364	1,603	0.292	1,521	0.689
30oct	0.026	7,379	0.346	1,568	0.311	1,629	0.583
06nov	0.029	7,508	0.360	1,705	0.280	2,126	0.612
13nov	0.032	7,013	0.439	1,579	0.418	2,280	0.624
20nov	0.026	6,268	0.431	1,462	0.405	2,152	0.633
27nov	0.029	6,433	0.475	1,466	0.440	2,151	0.643
04dec	0.022	5,668	0.474	1,217	0.456	1,716	0.668
11dec	0.005	2,284	0.480	426	0.532	689	0.667

Notes: Table reports the weekly test rate for the population, and the number of hospitalizations and test rate, by type of hospitalizations, weighted to match the population age distribution.. (The population size is 6.64 million in all weeks.) ICLI hospitalizations have at least one diagnosis for influenza-like or COVID-like illness. Clear cause hospitalizations are hospitalizations for cancer, labor and delivery, AMI, stroke, fracture or crush, open wound, appendicitis, or accidents (vehicle or other). See Appendix B for definitions.

Table A.2: Weekly test rates, by sample, not age weighted

Sample	Population	Non-ICLI		Clear Cause		ICLI	
	% Tested	N	% Tested	N	% Tested	N	% Tested
Week	(1)	(2)	(3)	(4)	(5)	(6)	(7)
13mar	0.001	6,919	0.034	1,368	0.030	1,143	0.294
20mar	0.002	6,011	0.102	1,281	0.085	1,231	0.726
27mar	0.003	5,546	0.158	1,193	0.142	1,344	0.833
03apr	0.003	5,561	0.173	1,281	0.158	1,207	0.795
10apr	0.004	5,703	0.190	1,250	0.174	1,079	0.772
17apr	0.005	5,914	0.196	1,353	0.217	1,195	0.720
24apr	0.006	6,139	0.196	1,434	0.178	1,110	0.642
01may	0.007	6,711	0.287	1,444	0.328	1,200	0.756
08may	0.008	7,101	0.310	1,556	0.330	1,110	0.746
15may	0.009	7,704	0.322	1,580	0.330	1,128	0.762
22may	0.007	7,717	0.305	1,586	0.308	1,061	0.734
29may	0.009	7,877	0.316	1,569	0.327	982	0.737
05jun	0.010	8,270	0.321	1,721	0.317	1,001	0.744
12jun	0.012	8,432	0.318	1,711	0.310	983	0.702
19jun	0.012	8,313	0.319	1,651	0.319	948	0.694
26jun	0.011	8,381	0.318	1,661	0.332	903	0.708
03jul	0.010	8,367	0.311	1,668	0.321	945	0.705
10jul	0.013	8,547	0.325	1,708	0.341	1,052	0.691
17jul	0.015	8,655	0.327	1,661	0.335	1,006	0.706
24jul	0.015	8,601	0.307	1,680	0.305	1,033	0.712
31jul	0.015	8,505	0.320	1,641	0.315	1,135	0.674
07aug	0.016	8,666	0.304	1,642	0.322	1,108	0.663
14aug	0.017	8,619	0.301	1,755	0.292	1,052	0.649
21aug	0.016	8,707	0.287	1,776	0.293	1,121	0.647
28aug	0.016	8,688	0.284	1,715	0.283	1,010	0.617
04sep	0.015	8,381	0.287	1,647	0.293	1,020	0.635
11sep	0.016	8,860	0.305	1,719	0.308	1,040	0.651
18sep	0.016	8,797	0.297	1,724	0.286	1,075	0.635
25sep	0.016	8,717	0.305	1,687	0.305	1,157	0.624
02oct	0.018	8,585	0.306	1,721	0.310	1,310	0.656
09oct	0.016	8,369	0.302	1,629	0.304	1,422	0.655
16oct	0.022	8,191	0.335	1,599	0.326	1,463	0.638
23oct	0.024	8,190	0.348	1,608	0.341	1,530	0.661
30oct	0.026	8,252	0.332	1,574	0.322	1,639	0.610
06nov	0.029	8,326	0.365	1,710	0.341	2,138	0.640
13nov	0.032	7,900	0.429	1,586	0.465	2,287	0.673
20nov	0.027	7,140	0.418	1,467	0.460	2,159	0.661
27nov	0.030	7,253	0.453	1,473	0.491	2,163	0.671
04dec	0.022	6,384	0.452	1,225	0.491	1,725	0.669
11dec	0.005	2,522	0.406	428	0.467	689	0.597

Notes: Table reports the weekly test rate for the population, and the number of hospitalizations and test rate, by type of hospitalizations. (The population size is 6.64 million in all weeks.) ICLI hospitalizations have at least one diagnosis for influenza-like or COVID-like illness. Clear cause hospitalizations are hospitalizations for cancer, labor and delivery, AMI, stroke, fracture or crush, open wound, appendicitis, or accidents (vehicle or other). See Appendix B for definitions.

Table A.3: Weekly bounds on prevalence under test monotonicity, by sample, March-July

Sample	Pop	Non-ICLI		Clear cause	
Representatives assumption		Hosp-M	Hosp-I	Hosp-M	Hosp-I
Week	(1)	(2)	(3)	(4)	(5)
13mar	[0.0001, 0.097]	[0.0001, 0.043]	[0.0019, 0.043]	[0.0001, 0.044]	[0.0017, 0.044]
	(0.0001, 0.105)	(0.0001, 0.066)	(0.0010, 0.066)	(0.0001, 0.095)	(0.0001, 0.095)
20mar	[0.0003, 0.154]	[0.0003, 0.112]	[0.0139, 0.112]	[0.0003, 0.110]	[0.0107, 0.110]
	(0.0003, 0.162)	(0.0003, 0.140)	(0.0108, 0.140)	(0.0003, 0.157)	(0.0063, 0.157)
27mar	[0.0006, 0.166]	[0.0006, 0.159]	[0.0286, 0.159]	[0.0006, 0.113]	[0.0148, 0.113]
	(0.0005, 0.173)	(0.0005, 0.170)	(0.0234, 0.170)	(0.0005, 0.166)	(0.0089, 0.166)
03apr	[0.0006, 0.154]	[0.0006, 0.131]	[0.0238, 0.131]	[0.0006, 0.083]	[0.0135, 0.083]
	(0.0006, 0.161)	(0.0006, 0.157)	(0.0190, 0.157)	(0.0006, 0.151)	(0.0083, 0.151)
10apr	[0.0006, 0.149]	[0.0006, 0.138]	[0.0236, 0.138]	[0.0006, 0.149]	[0.0132, 0.149]
	(0.0006, 0.155)	(0.0006, 0.154)	(0.0182, 0.154)	(0.0006, 0.154)	(0.0074, 0.154)
17apr	[0.0008, 0.169]	[0.0008, 0.051]	[0.0115, 0.051]	[0.0008, 0.026]	[0.0054, 0.026]
	(0.0008, 0.175)	(0.0008, 0.065)	(0.0091, 0.065)	(0.0008, 0.048)	(0.0027, 0.048)
24apr	[0.0008, 0.137]	[0.0008, 0.086]	[0.0182, 0.086]	[0.0008, 0.057]	[0.0108, 0.057]
	(0.0008, 0.142)	(0.0008, 0.119)	(0.0142, 0.119)	(0.0008, 0.088)	(0.0058, 0.088)
01may	[0.0009, 0.122]	[0.0009, 0.049]	[0.0153, 0.049]	[0.0009, 0.064]	[0.0192, 0.064]
	(0.0009, 0.127)	(0.0009, 0.070)	(0.0113, 0.070)	(0.0009, 0.116)	(0.0094, 0.116)
08may	[0.0008, 0.096]	[0.0008, 0.035]	[0.0119, 0.035]	[0.0008, 0.093]	[0.0215, 0.093]
	(0.0007, 0.100)	(0.0007, 0.050)	(0.0078, 0.050)	(0.0007, 0.099)	(0.0091, 0.099)
15may	[0.0008, 0.099]	[0.0008, 0.035]	[0.0120, 0.035]	[0.0008, 0.043]	[0.0121, 0.043]
	(0.0008, 0.102)	(0.0008, 0.043)	(0.0098, 0.043)	(0.0008, 0.065)	(0.0074, 0.065)
22may	[0.0006, 0.087]	[0.0006, 0.051]	[0.0158, 0.051]	[0.0006, 0.040]	[0.0110, 0.040]
	(0.0006, 0.090)	(0.0006, 0.069)	(0.0113, 0.069)	(0.0006, 0.063)	(0.0067, 0.063)
29may	[0.0006, 0.073]	[0.0006, 0.025]	[0.0083, 0.025]	[0.0006, 0.022]	[0.0070, 0.022]
	(0.0006, 0.076)	(0.0006, 0.038)	(0.0058, 0.038)	(0.0006, 0.038)	(0.0032, 0.038)
05jun	[0.0006, 0.059]	[0.0006, 0.022]	[0.0073, 0.022]	[0.0006, 0.021]	[0.0064, 0.021]
	(0.0006, 0.061)	(0.0006, 0.028)	(0.0055, 0.028)	(0.0006, 0.035)	(0.0023, 0.035)
12jun	[0.0005, 0.045]	[0.0005, 0.022]	[0.0067, 0.022]	[0.0005, 0.018]	[0.0053, 0.018]
	(0.0005, 0.047)	(0.0005, 0.036)	(0.0040, 0.036)	(0.0005, 0.031)	(0.0022, 0.031)
19jun	[0.0006, 0.051]	[0.0006, 0.014]	[0.0047, 0.014]	[0.0006, 0.016]	[0.0046, 0.016]
	(0.0005, 0.053)	(0.0005, 0.018)	(0.0032, 0.018)	(0.0005, 0.029)	(0.0013, 0.029)
26jun	[0.0006, 0.060]	[0.0006, 0.022]	[0.0070, 0.022]	[0.0006, 0.021]	[0.0071, 0.021]
	(0.0006, 0.062)	(0.0006, 0.037)	(0.0040, 0.037)	(0.0006, 0.033)	(0.0037, 0.033)
03jul	[0.0007, 0.069]	[0.0007, 0.015]	[0.0049, 0.015]	[0.0007, 0.033]	[0.0088, 0.033]
	(0.0007, 0.071)	(0.0007, 0.026)	(0.0030, 0.026)	(0.0007, 0.067)	(0.0027, 0.067)
10jul	[0.0009, 0.067]	[0.0009, 0.026]	[0.0086, 0.026]	[0.0009, 0.020]	[0.0067, 0.020]
	(0.0009, 0.069)	(0.0009, 0.038)	(0.0054, 0.038)	(0.0009, 0.032)	(0.0033, 0.032)
17jul	[0.0010, 0.069]	[0.0010, 0.011]	[0.0041, 0.011]	[0.0010, 0.012]	[0.0042, 0.012]
	(0.0010, 0.071)	(0.0010, 0.015)	(0.0029, 0.015)	(0.0010, 0.021)	(0.0016, 0.021)
24jul	[0.0010, 0.072]	[0.0010, 0.019]	[0.0064, 0.019]	[0.0010, 0.021]	[0.0065, 0.021]
	(0.0010, 0.074)	(0.0010, 0.024)	(0.0047, 0.024)	(0.0010, 0.033)	(0.0032, 0.033)
31jul	[0.0011, 0.072]	[0.0011, 0.034]	[0.0104, 0.034]	[0.0011, 0.052]	[0.0110, 0.052]
	(0.0010, 0.074)	(0.0010, 0.052)	(0.0067, 0.052)	(0.0010, 0.073)	(0.0041, 0.073)

Notes: Table reports weekly bounds on COVID prevalence in the indicated sample under test monotonicity, as well as the indicated representativeness assumption. Hosp-M means hosptilalization monotonicity, and Hosp-I means hospitalization independence. Hospitalized samples weighted to match the population age distribution. See Table A.1 for sample definitions. Bounds are in brackets, 95% confidence intervals in parentheses.

Table A.4: Weekly bounds on prevalence under test monotonicity, by sample, August-December

Sample	Pop	Non-ICLI		Clear cause	
Representatives assumption		Hosp-M	Hosp-I	Hosp-M	Hosp-I
Week	(1)	(2)	(3)	(4)	(5)
07aug	[0.0010, 0.067]	[0.0010, 0.032]	[0.0087, 0.032]	[0.0010, 0.020]	[0.0065, 0.020]
	(0.0010, 0.068)	(0.0010, 0.048)	(0.0054, 0.048)	(0.0010, 0.033)	(0.0032, 0.033)
14aug	[0.0010, 0.059]	[0.0010, 0.022]	[0.0070, 0.022]	[0.0010, 0.059]	[0.0173, 0.059]
	(0.0010, 0.060)	(0.0010, 0.033)	(0.0043, 0.033)	(0.0010, 0.060)	(0.0079, 0.060)
21aug	[0.0010, 0.060]	[0.0010, 0.022]	[0.0066, 0.022]	[0.0010, 0.041]	[0.0098, 0.041]
	(0.0010, 0.062)	(0.0010, 0.033)	(0.0042, 0.033)	(0.0010, 0.061)	(0.0038, 0.061)
28aug	[0.0010, 0.062]	[0.0010, 0.016]	[0.0052, 0.016]	[0.0010, 0.012]	[0.0035, 0.012]
	(0.0010, 0.063)	(0.0010, 0.021)	(0.0038, 0.021)	(0.0010, 0.022)	(0.0013, 0.022)
04sep	[0.0009, 0.057]	[0.0009, 0.016]	[0.0049, 0.016]	[0.0009, 0.015]	[0.0042, 0.015]
	(0.0008, 0.058)	(0.0008, 0.021)	(0.0036, 0.021)	(0.0008, 0.025)	(0.0020, 0.025)
11sep	[0.0009, 0.053]	[0.0009, 0.016]	[0.0053, 0.016]	[0.0009, 0.014]	[0.0043, 0.014]
	(0.0009, 0.055)	(0.0009, 0.025)	(0.0032, 0.025)	(0.0009, 0.022)	(0.0021, 0.022)
18sep	[0.0009, 0.058]	[0.0009, 0.016]	[0.0051, 0.016]	[0.0009, 0.022]	[0.0064, 0.022]
	(0.0009, 0.059)	(0.0009, 0.021)	(0.0036, 0.021)	(0.0009, 0.036)	(0.0030, 0.036)
25sep	[0.0012, 0.070]	[0.0012, 0.019]	[0.0064, 0.019]	[0.0012, 0.018]	[0.0061, 0.018]
	(0.0011, 0.072)	(0.0011, 0.024)	(0.0046, 0.024)	(0.0011, 0.029)	(0.0026, 0.029)
02oct	[0.0014, 0.081]	[0.0014, 0.018]	[0.0058, 0.018]	[0.0014, 0.024]	[0.0065, 0.024]
	(0.0014, 0.083)	(0.0014, 0.023)	(0.0044, 0.023)	(0.0014, 0.038)	(0.0029, 0.038)
09oct	[0.0017, 0.101]	[0.0017, 0.027]	[0.0086, 0.027]	[0.0017, 0.021]	[0.0072, 0.021]
	(0.0016, 0.102)	(0.0016, 0.040)	(0.0064, 0.040)	(0.0016, 0.031)	(0.0038, 0.031)
16oct	[0.0024, 0.110]	[0.0024, 0.029]	[0.0103, 0.029]	[0.0024, 0.022]	[0.0073, 0.022]
	(0.0024, 0.111)	(0.0024, 0.038)	(0.0078, 0.038)	(0.0024, 0.033)	(0.0039, 0.033)
23oct	[0.0031, 0.132]	[0.0031, 0.030]	[0.0115, 0.030]	[0.0031, 0.047]	[0.0154, 0.047]
	(0.0031, 0.134)	(0.0031, 0.037)	(0.0095, 0.037)	(0.0031, 0.070)	(0.0102, 0.070)
30oct	[0.0042, 0.162]	[0.0042, 0.038]	[0.0136, 0.038]	[0.0042, 0.057]	[0.0190, 0.057]
	(0.0041, 0.163)	(0.0041, 0.048)	(0.0104, 0.048)	(0.0041, 0.093)	(0.0098, 0.093)
06nov	[0.0055, 0.189]	[0.0055, 0.056]	[0.0213, 0.056]	[0.0055, 0.069]	[0.0230, 0.069]
	(0.0054, 0.191)	(0.0054, 0.067)	(0.0180, 0.067)	(0.0054, 0.090)	(0.0170, 0.090)
13nov	[0.0059, 0.181]	[0.0059, 0.060]	[0.0264, 0.060]	[0.0059, 0.115]	[0.0450, 0.115]
	(0.0058, 0.183)	(0.0058, 0.076)	(0.0216, 0.076)	(0.0058, 0.178)	(0.0312, 0.178)
20nov	[0.0052, 0.196]	[0.0052, 0.080]	[0.0352, 0.080]	[0.0052, 0.160]	[0.0519, 0.160]
	(0.0052, 0.198)	(0.0052, 0.099)	(0.0287, 0.099)	(0.0052, 0.197)	(0.0322, 0.197)
27nov	[0.0066, 0.225]	[0.0066, 0.062]	[0.0306, 0.062]	[0.0066, 0.100]	[0.0457, 0.100]
	(0.0066, 0.227)	(0.0066, 0.075)	(0.0253, 0.075)	(0.0066, 0.149)	(0.0332, 0.149)
04dec	[0.0047, 0.213]	[0.0047, 0.048]	[0.0243, 0.048]	[0.0047, 0.060]	[0.0299, 0.060]
	(0.0046, 0.215)	(0.0046, 0.056)	(0.0206, 0.056)	(0.0046, 0.078)	(0.0214, 0.078)
11dec	[0.0010, 0.195]	[0.0010, 0.056]	[0.0277, 0.056]	[0.0010, 0.086]	[0.0480, 0.086]
	(0.0010, 0.199)	(0.0010, 0.084)	(0.0143, 0.084)	(0.0010, 0.177)	(0.0117, 0.177)

Notes: Table reports weekly bounds on COVID prevalence in the indicated sample under test monotonicity, as well as the indicated representativeness assumption. Hospitalized samples weighted to match the population age distribution. See Table A.1 for sample definitions. Bounds are in brackets, 95% confidence intervals in parentheses.

Table A.5: Weekly bounds on prevalence under test monotonicity, by sample, not age-weighted, March-July

Sample	Pop	Non-ICLI		Clear cause	
Representatives assumption		Hosp-M	Hosp-I	Hosp-M	Hosp-I
Week	(1)	(2)	(3)	(4)	(5)
13mar	[0.0001, 0.111]	[0.0001, 0.090]	[0.0034, 0.090]	[0.0001, 0.073]	[0.0022, 0.073]
	(0.0001, 0.120)	(0.0001, 0.113)	(0.0020, 0.113)	(0.0001, 0.114)	(0.0001, 0.114)
20mar	[0.0003, 0.181]	[0.0003, 0.159]	[0.0189, 0.159]	[0.0003, 0.174]	[0.0149, 0.174]
	(0.0003, 0.187)	(0.0003, 0.184)	(0.0155, 0.184)	(0.0003, 0.186)	(0.0089, 0.186)
27mar	[0.0006, 0.197]	[0.0006, 0.197]	[0.0390, 0.197]	[0.0006, 0.147]	[0.0210, 0.147]
	(0.0005, 0.202)	(0.0005, 0.201)	(0.0338, 0.201)	(0.0005, 0.196)	(0.0128, 0.196)
03apr	[0.0006, 0.178]	[0.0006, 0.162]	[0.0331, 0.162]	[0.0006, 0.163]	[0.0259, 0.163]
	(0.0006, 0.183)	(0.0006, 0.180)	(0.0285, 0.180)	(0.0006, 0.182)	(0.0170, 0.182)
10apr	[0.0006, 0.168]	[0.0006, 0.134]	[0.0300, 0.134]	[0.0006, 0.106]	[0.0184, 0.106]
	(0.0006, 0.173)	(0.0006, 0.156)	(0.0257, 0.156)	(0.0006, 0.149)	(0.0109, 0.149)
17apr	[0.0009, 0.188]	[0.0009, 0.085]	[0.0194, 0.085]	[0.0009, 0.058]	[0.0126, 0.058]
	(0.0008, 0.191)	(0.0008, 0.101)	(0.0154, 0.101)	(0.0008, 0.088)	(0.0070, 0.088)
24apr	[0.0008, 0.147]	[0.0008, 0.110]	[0.0249, 0.110]	[0.0008, 0.086]	[0.0154, 0.086]
	(0.0008, 0.150)	(0.0008, 0.129)	(0.0208, 0.129)	(0.0008, 0.123)	(0.0098, 0.123)
01may	[0.0009, 0.126]	[0.0009, 0.058]	[0.0190, 0.058]	[0.0009, 0.057]	[0.0187, 0.057]
	(0.0009, 0.129)	(0.0009, 0.069)	(0.0152, 0.069)	(0.0009, 0.078)	(0.0116, 0.078)
08may	[0.0008, 0.098]	[0.0008, 0.039]	[0.0137, 0.039]	[0.0008, 0.047]	[0.0154, 0.047]
	(0.0008, 0.100)	(0.0008, 0.048)	(0.0109, 0.048)	(0.0008, 0.067)	(0.0093, 0.067)
15may	[0.0008, 0.094]	[0.0008, 0.045]	[0.0164, 0.045]	[0.0008, 0.057]	[0.0190, 0.057]
	(0.0008, 0.097)	(0.0008, 0.053)	(0.0136, 0.053)	(0.0008, 0.078)	(0.0129, 0.078)
22may	[0.0006, 0.084]	[0.0006, 0.047]	[0.0162, 0.047]	[0.0006, 0.053]	[0.0164, 0.053]
	(0.0006, 0.086)	(0.0006, 0.055)	(0.0132, 0.055)	(0.0006, 0.074)	(0.0107, 0.074)
29may	[0.0006, 0.069]	[0.0006, 0.029]	[0.0102, 0.029]	[0.0006, 0.027]	[0.0090, 0.027]
	(0.0006, 0.071)	(0.0006, 0.036)	(0.0079, 0.036)	(0.0006, 0.042)	(0.0050, 0.042)
05jun	[0.0006, 0.056]	[0.0006, 0.026]	[0.0092, 0.026]	[0.0006, 0.028]	[0.0087, 0.028]
	(0.0006, 0.058)	(0.0006, 0.032)	(0.0072, 0.032)	(0.0006, 0.042)	(0.0047, 0.042)
12jun	[0.0005, 0.041]	[0.0005, 0.018]	[0.0064, 0.018]	[0.0005, 0.023]	[0.0070, 0.023]
	(0.0005, 0.042)	(0.0005, 0.024)	(0.0046, 0.024)	(0.0005, 0.036)	(0.0035, 0.036)
19jun	[0.0006, 0.049]	[0.0006, 0.017]	[0.0062, 0.017]	[0.0006, 0.015]	[0.0049, 0.015]
	(0.0006, 0.050)	(0.0006, 0.022)	(0.0044, 0.022)	(0.0006, 0.027)	(0.0019, 0.027)
26jun	[0.0006, 0.058]	[0.0006, 0.020]	[0.0070, 0.020]	[0.0006, 0.031]	[0.0103, 0.031]
	(0.0006, 0.060)	(0.0006, 0.025)	(0.0051, 0.025)	(0.0006, 0.046)	(0.0058, 0.046)
03jul	[0.0007, 0.067]	[0.0007, 0.015]	[0.0052, 0.015]	[0.0007, 0.026]	[0.0084, 0.026]
	(0.0007, 0.069)	(0.0007, 0.020)	(0.0037, 0.020)	(0.0007, 0.041)	(0.0037, 0.041)
10jul	[0.0009, 0.067]	[0.0009, 0.020]	[0.0074, 0.020]	[0.0009, 0.024]	[0.0082, 0.024]
	(0.0009, 0.069)	(0.0009, 0.026)	(0.0056, 0.026)	(0.0009, 0.037)	(0.0041, 0.037)
17jul	[0.0010, 0.069]	[0.0010, 0.017]	[0.0061, 0.017]	[0.0010, 0.020]	[0.0066, 0.020]
	(0.0010, 0.071)	(0.0010, 0.022)	(0.0044, 0.022)	(0.0010, 0.032)	(0.0030, 0.032)
24jul	[0.0010, 0.072]	[0.0010, 0.027]	[0.0093, 0.027]	[0.0010, 0.039]	[0.0119, 0.039]
	(0.0010, 0.073)	(0.0010, 0.034)	(0.0073, 0.034)	(0.0010, 0.057)	(0.0072, 0.057)
31jul	[0.0011, 0.071]	[0.0011, 0.028]	[0.0100, 0.028]	[0.0011, 0.035]	[0.0110, 0.035]
	(0.0010, 0.073)	(0.0010, 0.034)	(0.0078, 0.034)	(0.0010, 0.049)	(0.0063, 0.049)

Notes: Table reports weekly bounds on COVID prevalence in the indicated sample under test monotonicity, as well as the indicated representativeness assumption. See Table A.1 for sample definitions. Bounds are in brackets, 95% confidence intervals in parentheses.

Table A.6: Weekly bounds on prevalence under test monotonicity, by sample, not age-weighted, August-December

Sample	Pop	Non-ICLI		Clear cause	
Representatives assumption		Hosp-M	Hosp-I	Hosp-M	Hosp-I
Week	(1)	(2)	(3)	(4)	(5)
07aug	[0.0011, 0.067]	[0.0011, 0.023]	[0.0079, 0.023]	[0.0011, 0.030]	[0.0098, 0.030]
	(0.0010, 0.069)	(0.0010, 0.029)	(0.0059, 0.029)	(0.0010, 0.046)	(0.0054, 0.046)
14aug	[0.0010, 0.061]	[0.0010, 0.023]	[0.0077, 0.023]	[0.0010, 0.047]	[0.0137, 0.047]
	(0.0010, 0.062)	(0.0010, 0.029)	(0.0058, 0.029)	(0.0010, 0.061)	(0.0087, 0.061)
21aug	[0.0010, 0.064]	[0.0010, 0.022]	[0.0071, 0.022]	[0.0010, 0.031]	[0.0090, 0.031]
	(0.0010, 0.065)	(0.0010, 0.027)	(0.0054, 0.027)	(0.0010, 0.046)	(0.0054, 0.046)
28aug	[0.0010, 0.066]	[0.0010, 0.022]	[0.0069, 0.022]	[0.0010, 0.021]	[0.0058, 0.021]
	(0.0010, 0.068)	(0.0010, 0.028)	(0.0051, 0.028)	(0.0010, 0.033)	(0.0024, 0.033)
04sep	[0.0009, 0.060]	[0.0009, 0.023]	[0.0075, 0.023]	[0.0009, 0.025]	[0.0073, 0.025]
	(0.0009, 0.061)	(0.0009, 0.030)	(0.0058, 0.030)	(0.0009, 0.040)	(0.0035, 0.040)
11sep	[0.0009, 0.055]	[0.0009, 0.019]	[0.0064, 0.019]	[0.0009, 0.030]	[0.0093, 0.030]
	(0.0009, 0.056)	(0.0009, 0.023)	(0.0048, 0.023)	(0.0009, 0.046)	(0.0052, 0.046)
18sep	[0.0009, 0.059]	[0.0009, 0.021]	[0.0071, 0.021]	[0.0009, 0.032]	[0.0093, 0.032]
	(0.0009, 0.061)	(0.0009, 0.027)	(0.0052, 0.027)	(0.0009, 0.049)	(0.0047, 0.049)
25sep	[0.0012, 0.072]	[0.0012, 0.024]	[0.0082, 0.024]	[0.0012, 0.031]	[0.0095, 0.031]
	(0.0011, 0.073)	(0.0011, 0.031)	(0.0062, 0.031)	(0.0011, 0.048)	(0.0052, 0.048)
02oct	[0.0015, 0.082]	[0.0015, 0.026]	[0.0090, 0.026]	[0.0015, 0.032]	[0.0099, 0.032]
	(0.0014, 0.084)	(0.0014, 0.033)	(0.0071, 0.033)	(0.0014, 0.047)	(0.0053, 0.047)
09oct	[0.0017, 0.102]	[0.0017, 0.034]	[0.0115, 0.034]	[0.0017, 0.051]	[0.0154, 0.051]
	(0.0016, 0.104)	(0.0016, 0.042)	(0.0094, 0.042)	(0.0016, 0.070)	(0.0095, 0.070)
16oct	[0.0024, 0.111]	[0.0024, 0.038]	[0.0140, 0.038]	[0.0024, 0.046]	[0.0150, 0.046]
	(0.0024, 0.113)	(0.0024, 0.046)	(0.0115, 0.046)	(0.0024, 0.064)	(0.0097, 0.064)
23oct	[0.0032, 0.134]	[0.0032, 0.047]	[0.0181, 0.047]	[0.0032, 0.069]	[0.0237, 0.069]
	(0.0031, 0.135)	(0.0031, 0.055)	(0.0148, 0.055)	(0.0031, 0.091)	(0.0165, 0.091)
30oct	[0.0042, 0.164]	[0.0042, 0.043]	[0.0161, 0.043]	[0.0042, 0.067]	[0.0217, 0.067]
	(0.0042, 0.165)	(0.0042, 0.052)	(0.0132, 0.052)	(0.0042, 0.088)	(0.0145, 0.088)
06nov	[0.0055, 0.191]	[0.0055, 0.071]	[0.0288, 0.071]	[0.0055, 0.105]	[0.0358, 0.105]
	(0.0055, 0.193)	(0.0055, 0.080)	(0.0253, 0.080)	(0.0055, 0.132)	(0.0272, 0.132)
13nov	[0.0059, 0.184]	[0.0059, 0.066]	[0.0317, 0.066]	[0.0059, 0.114]	[0.0532, 0.114]
	(0.0058, 0.186)	(0.0058, 0.074)	(0.0278, 0.074)	(0.0058, 0.139)	(0.0436, 0.139)
20nov	[0.0053, 0.198]	[0.0053, 0.093]	[0.0442, 0.093]	[0.0053, 0.116]	[0.0534, 0.116]
	(0.0052, 0.200)	(0.0052, 0.103)	(0.0387, 0.103)	(0.0052, 0.139)	(0.0420, 0.139)
27nov	[0.0067, 0.227]	[0.0067, 0.084]	[0.0426, 0.084]	[0.0067, 0.142]	[0.0703, 0.142]
	(0.0066, 0.229)	(0.0066, 0.093)	(0.0375, 0.093)	(0.0066, 0.169)	(0.0577, 0.169)
04dec	[0.0047, 0.214]	[0.0047, 0.068]	[0.0344, 0.068]	[0.0047, 0.108]	[0.0534, 0.108]
	(0.0047, 0.216)	(0.0047, 0.077)	(0.0298, 0.077)	(0.0047, 0.133)	(0.0412, 0.133)
11dec	[0.0010, 0.196]	[0.0010, 0.051]	[0.0228, 0.051]	[0.0010, 0.085]	[0.0399, 0.085]
	(0.0010, 0.201)	(0.0010, 0.064)	(0.0177, 0.064)	(0.0010, 0.120)	(0.0219, 0.120)

Notes: Table reports weekly bounds on COVID prevalence in the indicated sample under test monotonicity, as well as the indicated representativeness assumption. See Table A.1 for sample definitions. Bounds are in brackets, 95% confidence intervals in parentheses.

Table A.7: Demographics and test rates among hospitalized patients, by group

Group	Number of Admissions	Female	Age						
			Newborn	0-17	18-29	30-49	50-64	65-74	>74
All	781,587	0.555	0.080	0.028	0.105	0.181	0.228	0.182	0.196
Has diagnosis	355,425	0.557	0.100	0.026	0.112	0.178	0.214	0.173	0.198
ICLI	49,904	0.493	0.005	0.023	0.037	0.139	0.269	0.239	0.287
Non-ICLI	305,521	0.568	0.115	0.027	0.124	0.184	0.205	0.162	0.183
Clear cause	61,682	0.592	0.003	0.033	0.165	0.175	0.195	0.179	0.249
Cancer	9,585	0.465	0.001	0.053	0.026	0.122	0.322	0.284	0.192
Labor/delivery	13,304	0.995	0.009	0.023	0.611	0.357	0.000	0.000	0.000
AMI	8,624	0.405	0.000	0.000	0.007	0.112	0.315	0.265	0.301
Stroke	8,297	0.487	0.001	0.004	0.011	0.092	0.269	0.256	0.368
Fracture	13,718	0.546	0.003	0.034	0.063	0.128	0.178	0.187	0.408
Open wound	3,642	0.420	0.002	0.047	0.097	0.197	0.224	0.167	0.266
Appendicitis	1,961	0.465	0.000	0.224	0.199	0.274	0.181	0.084	0.038
Vehicle accident	1,944	0.356	0.001	0.090	0.216	0.297	0.195	0.119	0.082
Other accident	9,782	0.541	0.003	0.033	0.034	0.082	0.154	0.208	0.486

Notes: Table reports the number and age distribution of admissions, for different categories of admissions, over the time period March 13, 2020 through June 18, 2020. See Appendix B for definitions of the different causes of admissions (Cancer-other accident).

B Defining causes of admissions

This section provides more details on our definition of ICLI, non-ICIL, and "clear cause" hospitalization, listing the ICD-10 codes used to define each.

Following Armed Forces Health Surveillance Center (2015), the codes for influenza-like illness are B97.89, H66.9, H66.90, H66.91 H66.92, H66.93, J00, J01.9, J01.90, J06.9, J09, J09.X, J09.X1, J09.2, J09.X3, J09.X9, J10, J10.0, J10.00, J10.01, J10.08, J10.1, J10.2, J10.8, J10.81, J10.82, J10.83, J10.89, J11, J11.0, J11.00, J11.08, J11.1, J11.2, J11.8, J11.81, J11.82, J11.83, J11.89, J12.89, J12.9, J18, J18.1, J18.8, J18.9, J20.9, J40, R05, and R50.9. We say a hospitalizaiton is for an influenza-like illness if it has any of these diagnosis codes in any position. We say a hospitalization is for a COVID-like illness if it has any ICD-10 code among those that is among the CDC's lists of diagnosis codes for COVID-19 Center for Disease Control and Prevention (2020). These codes are J12.89, J20.8, J22, J40, J80, J98.8, O95.5, R05, R06.02, R50.9, U07.1, Z03.818, Z11.58, and Z20.828.

We define ICLI-related hospitalizations as ones with at least one ILI or CLI diagnosis code. We define non-ICLI related hospitalizations as hospitalized with diagnosis codes, but no ILI or CLI code.

We also define "clear cause" hospitalizations. These are hospitalizations for labor and delivery, AMI, stroke, fractures and crushes, wounds, vehicle accidents, other accidents, appendicitis, or cancer. With the exception of cancer, we define a hospitalization as belonging to one of these groups if it has any diagnosis codes for that group, listed below. Cancer is treated differently because it can be a comorbidity. We say a hospitalization is for cancer if a cancer diagnosis (listed below) is an admitting diagnosis, the primary final

diagnosis, or if chemotherapy diagnosis is present. We use the following ICD-10 codes.

- **AMI** I21, I22.
- **Appendicitis** K35-K38.
- **Cancer** C00-C97 (in primary or admitting diagnosis), or Z51.0-Z51.2 (in any position).
- **Fracture/Crush** S02, S12, S22, S32, S42, S52, S62, S72, S82, S92, T02, S07, S17, S37, S47, S57,S67, S77, S87, S97, T07.
- **Labor and delivery** O60-O75, O80-O84.
- **Other accidents** W00-W99, X00-X59.
- **Stroke** I61-I64.
- **Vehicle accident** V01-V99.
- **Wound** S01, S11, S21, S31, S41, S51, S61, S71, S81, S91, T01.

C Calculating negative predictive values with test-retest data

Setup and identification Here we show how to use data on multiple tests to simultaneously identify prevalence and test error rates, and how to use this information to obtain the negative predictive value (NPV) of at test under a narrow set of assumptions. Assume in particular that people have been tested exactly twice, with $R1_i$ the outcome of the first test and $R2_i$ the outcome of the second test for person i. Let C_i be person i's true infection status, which we assume is fixed between the tests. Let $p = Pr(C_i = 1)$ be the prevalence of active SARS-CoV-2 infections in this twice-tested population.

Test outcomes may differ from true infection status because of test errors. In general, therefore, there are four possible sequences of test outcomes: $(0,0), (0,1), (1,0), (1,1)$. We let $P_{ab} = Pr(R1_i = a, R2_i = b)$ for $(a,b) \in \{0,1\}^2$.

We make three strong assumptions to simplify the analysis.

Assumption 4. *The specificity of the test is 1. That is,* $\beta = Pr(Rj_i = 0|C_i = 0) = 1$.

Assumption 5. *The sensitivity of the test,* $\alpha = Pr(Rj_i = 1|C_i = 1)$*, does not depend on the initial test result.*

Assumption 6. *Retesting is random, i.e. independent of* $R1_i$ *and* C_i.

Assumption 4 is the weakest of these assumptions. It implies that there are no false positives, which is consistent with typical practice (UCSF Health Hospital Epidemiology and Infection Prevention, 2020). The remaining assumptions are stronger. Assumption 5 says that the test errors are independent of the initial test result. It would be violated,

for example, if false negatives are more common for patients with high levels of mucus, and mucus levels are correlated across test results. Assumption 6 says that retesting rates do not depend on possible testing errors. We would expect this condition to fail if highly symptomatic people with negative tests are especially likely to test negative. We view this assumption as the most suspect.

Under these assumptions, the test outcome probabilities P_{ab} simplify considerably. Since the probabilities sum to one, and the assumptions imply that $P_{10} = P_{01}$, the only non-redundant probabilities are:

$$P_{00} = (1-p) + p(1-\alpha)^2$$

$$P_{11} = p\alpha^2.$$

We can observe P_{00} and P_{11}. Solving for the unknowns p and α, we have

$$p = \frac{(P_{00} - P_{11} - 1)^2}{4P_{11}}$$

$$\alpha = \frac{2P_{11}}{1 - P_{00} + P_{11}}$$

This shows how to get p and α from two tests, and the assumption that specificity (β) equals 1. Our goal is to find the negative predictive value (NPV), which can be computed given knowledge of α, β and p. In general, for a single test $NPV = Pr(C_i = 0 | R_i = 0)$. Applying Bayes rule shows that:

$$NPV = \frac{1-p}{p(1-\alpha)+(1-p)}$$

Results To implement this approach, we construct a sample of all people who are tested on a given day, not tested the previous day, and then tested again in the next day. There are 835,195 such test pairs. We find $P_{00} = 0.884$ and $P_{11} = 0.113$. Nearly all the mass is on the diagonals; test results switch less than 1% of the time. This fact, together with the assumption that specificity is equal to 1, implies very low false negative rates. Plugging these values into our formula, we have $p = 0.116$ and $\alpha = 0.987$, which implies $NPV = 0.998$. Using instead, all people who are retested once within a three day period, we find similar results: $p = 0.118, \alpha = 0.972, NPV = 0.996$.

We emphasize that these estimates are valid for the twice-tested population and under assumptions 4-6, in particular, random retesting. The prevalence estimate is the prevalence among people tested twice, not the population prevalence. And it is only a valid estimate under assumptions 1-3. In reality, it is likely that retests are most common among suspected false negatives (i.e. when a highly symptomatic patient tests negative). We see some evidence for this: $P_{01} = 0.0013$ and $P_{10} = 0.0016$, a slight but significant difference implying that negative-then-positive is slightly more common than positive-than-negative, inconsistent with the random retesting assumption. We therefore do not view our estimates of prevalence and sensitivity as definitive; rather we think of the sensitivity estimate as a lower bound on sensitivity, because we have selected a retest sample which has a disproportionate number of false negatives. As NPV is increasing in sensitivity, α, our implied estimate of $1 - NPV$ is likely an upper bound on $1 - NPV$.

D Measurement Error In Testing

Virological tests for the presence of SARS-CoV-2 may not be perfectly accurate, and so far there are no detailed studies of the performance of the PCR tests that Indiana is using to test people for SARS-CoV-2. To clarify how error-ridden tests complicate our prevalence estimates, we augment the notation to distinguish between test results and virological status. We continue to use C_{it} and D_it to represent a person's true infection and testing status at date t. But now we introduce R_{it}, which is a binary measure set to 1 if the person tests positive and 0 if the person tests negative. Using this notation, $Pr(C_{it} = 1|D_{it} = 1, R_{it} = 1)$ is called the Positive Predictive Value (PPV) of the test among people who are tested and who test positive. $Pr(C_{it} = 0|D_{it} = 1, R_{it} = 0)$ is called the Negative Predictive Value (NPV) among people who are tested and who test negative. $1 - NPV = Pr(C_{it} = 1|D_{it} = 1, R_{it} = 0)$ is the fraction of people who test negative who are actually infected with SARS-CoV-2.

Our initial worst case bounds assumed no test errors. Relaxing that assumption yields a different set of upper and lower bounds on prevalence. Following Manski and Molinari (2020), we assume that (i) $PPV = 1$ so that none of the positive tests are false, but (ii) $Pr(C_{it} = 1|D_{it} = 1, R_{it} = 0) \in [\lambda_l, \lambda_u]$. The second condition imposes a bound on $1-NPV$, which is the fraction of people who test negative who are actually infected. Under these two restrictions, the new worst case bounds work out to:

$$L_{w,\lambda} = L_w + \lambda_l Pr(R_{it} = 0|D_{it} = 1)Pr(D_{it} = 1)$$

$$U_{w,\lambda} = U_w + \lambda_u Pr(R_{it} = 0|D_{it} = 1)Pr(D_{it} = 1)$$

Allowing for test errors increases the worst case lower bound by the best-case fraction of missing positives, and increases the worst case upper bound by the worst-case fraction of missing positives. Similar expressions hold for prevalence bounds under test monotonicity and other independence assumptions.

The upshot is that knowledge of test accuracy is important for efforts to learn about prevalence. In their study of the cumulative prevalence of SARS-CoV-2 infections, Manski and Molinari (2020) computed upper and lower bounds on prevalence under the assumption that $\lambda_l = .1$ and $\lambda_u = .4$, citing Peci et al. (2014). Manski and Molinari (2020) view this choice of $.1 \leq 1 - NPV \leq .4$ as an expression of scientific uncertainty about test errors, and they refer to the resulting prevalence bounds as "illustrative." However, the structure of the test error bounds makes it clear that assumptions about the numerical magnitude of test errors have inferential consequences. For example, setting $\lambda_u = .4$ implies that, regardless of the outcome of the test, at least 40 percent of the people who are tested for SARS-CoV-2 are infected.

Although there is little published evidence on the properties of the SARS-CoV-2 PCR test, previous research suggests that PCR test errors are uncommon in other settings. For example, Peci et al. (2014) study the performance of rapid influenza tests using PCR-based tests as a *gold standard*. PCR tests are used as a gold standard because they are expected to have very high PPV and NPV.

To shed more light on test errors, we constructed a sample of people who are tested and retested in a short interval, specifically people who were (i) tested on day t, (ii) not tested on day $t-1$, and (iii) were tested again on day $t+1$. We show in Appendix C how these data can be used to estimate error rates, under assumptions of random retesting

and no false positives. Our data include 835,000 test-retest events. Using $R1_i$ and $R2_i$ to represent the results of a person's first and second test, we found that $Pr(R1_i = 1, R2_i = 1) = .11$ and $Pr(R1_i = 0, R2_i = 0) = .88$ among the people in the twice-tested sample. The two tests were discordant for less than 1 percent of the twice-tested sample. These results imply a negative predictive value of 99.8 percent.

This estimate of NPV depends on our assumptions of random retesting and no false positives. While the no false positive assumption appears plausible, random retesting is not necessarily satisfied. In particular, a patient with a suspected COVID case who initially tests negative may be retested; this selective retesting would bias us towards finding false negatives. Another reason for retesting is delays in processing results. If a patient was tested prior to a planned hospitalization, and the result is not available at the time of the hospitalization, the attending physician may order an in-hospital test, which would be available within hours. This type of retesting is less likely to lead to bias. As we explain in Appendix C, we can test for selection into retesting by looking for symmetry in test results. Under random retesting (and no false positives), the sequences "positive-then-negative" and "negative-then-positive" should be equally likely. In practice we find that "negative-then-positive" is slightly more common, meaning that our test-retest sample likely disproportionately selects people with initial false negatives.

Overall, we think that a plausible value for λ_l is nearly zero, and a plausible value for λ_u is 0.005. Accounting for test errors in this range would have almost no effect on the upper and lower bounds reported in the paper. Test-retest data are potentially informative about test errors, but a limitation of is that retested people are not necessarily representative of the population.

E Small bias from excluding ICLI-hospitalizations

Our main sample uses non-ICLI hospitalizations to bound COVID prevalence in the general population. This approach therefore yields bounds on the prevalence of non-severe COVID-19, where "non-severe" means "not severe enough to induce a COVID-related hospitalization." These bounds are of course biased for bounds on overall COVID-19 prevalence. However this bias is quite small, small enough that it is unlikely to be decision relevant. We show this in two separate arguments.

To begin we abuse notation slightly and let H in this section be an indicator for an ICLI-related hospitalization, rather than any hospitalization. Both arguments start from the observation that COVID prevalence is equal to COVID prevalence among the hospitalized population plus its prevalence among the unhospitalized population:

$$Pr(C = 1) = Pr(C = 1, H = 1) + Pr(C = 1, H = 0).$$

Since our main sample is limited to non-ICLI hospitalizations, our bounds can be interpreted as bounds on $Pr(C = 1, H = 0)$, and the bias is (at most) the bias from omitting $Pr(C = 1, H = 1)$.

E.1 Argument from rarity of ICLI-related hospitalizations

Our first argument that this bias is small is to observe that $Pr(C = 1, H = 1) \leq Pr(H = 1)$. That is, the overall rate of ICLI-related hospitalizations in the population is an upper bound on the fraction of people in the population who are COVID-19 positive

and have an ICLI-related hospitalization. Fortunately, $Pr(H = 1)$ is nearly observable in our data.

In particular, we don't quite observe $Pr(H = 1)$ because not all hospitals report diagnosis information. We can therefore bound $Pr(H = 1)$ by assuming that when diagnosis information is not reported, $H = 1$. Taking this approach, Figure E.1 shows $Pr(H = 1)$ in our data. This is the weekly count of ICLI-related hospitalizations, scaled by the population of Indiana. An alternative approach, also shown in Figure E.1 is to measure ICLI-related hospitalizations as the total number of hospitalizations, scaled by the share of ICLI-related hospitalizations among hospitalizations with diagnoses. We see that $Pr(H = 1)$ is always less than 0.3%, typically less than 0.2%, using the more conservative bound. Thus the population prevalence of COVID-19 exceeds our upper bound by at most 0.3%. A more precise estimate of the bias uses the estimated $Pr(H = 1)$ from observed diagnoses, about 0.05 percent, and uses Figure 3 to infer that $Pr(C = 1|H = 1)$ is typically less than 50 percent, and so the bias from excluding ICLI related hospitalizations is likely less than 0.025 percent, that is, about 1700 cases out of a population of 6.8 million. Reassuringly, this number is similar to the average reported COVID-19 hospitalizations in the state of Indiana in 2020 (Indiana State Department of Health, 2020).[13]

E.2 Argument from low infection hospitalization rate

A second argument shows, similarly, that there is little bias from conditioning on ICLI-unrelated hospitalizations. This argument is based on the fact that the infection hospital-

[13] Because hospitalizations last a few days, our weekly admission count is comparable to the state's daily count of then umber of people in the hospital.

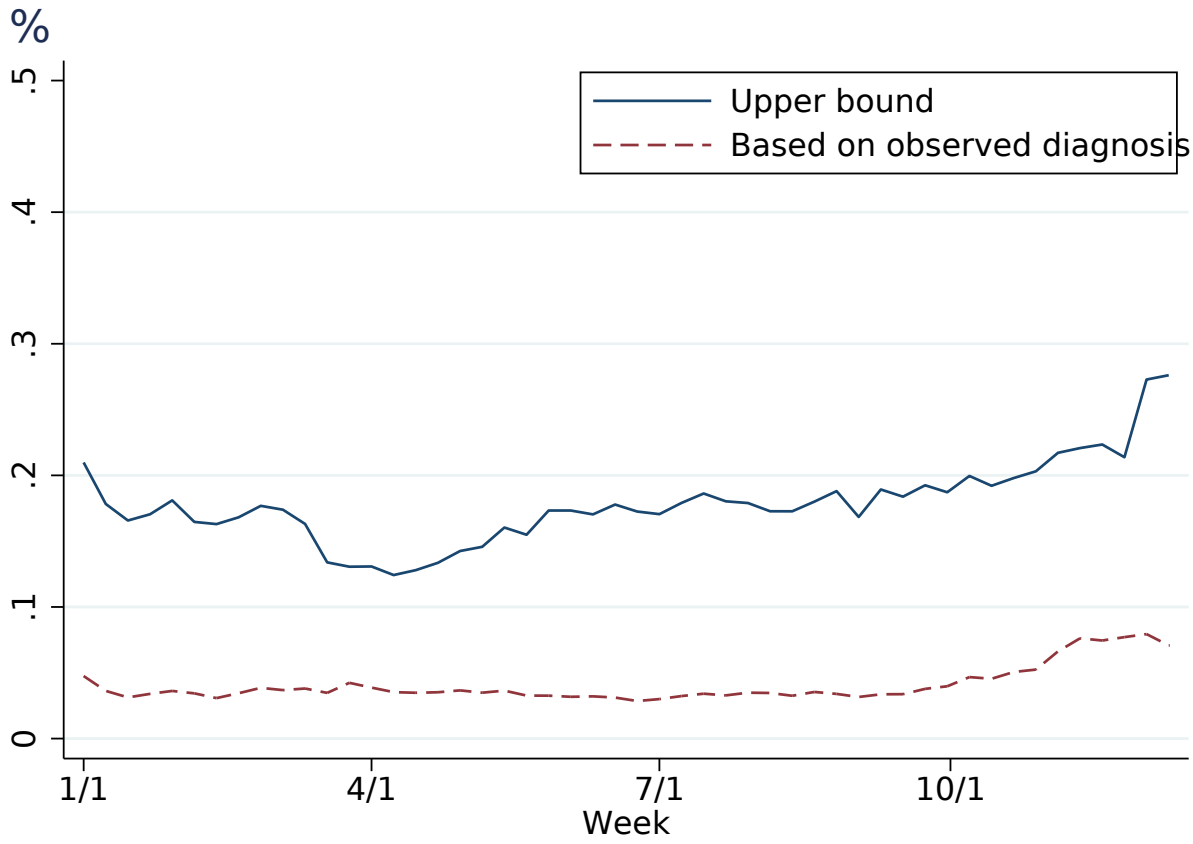
ization rate, $Pr(H = 1|C = 1)$, is known to be low.

After substituting $Pr(H = 1|C = 1)Pr(C = 1)$ for $Pr(C = 1, H = 1)$ in the equation above, and a bit of algebra, we have

$$Pr(C = 1)\frac{Pr(C = 1, H = 0)}{1 - Pr(H = 1|C = 1)}.$$

By focusing on ICLI-unrelated hospitalizations, we bound the numerator. The expression above shows that our bound is off by a factor of at most $(1 - Pr(H = 1|C = 1))^{-1}$. If $Pr(H = 1|C = 1)$ were known to be low, then the bias in our bound would be low as well. The available evidence indicates that the infection hospitalization rate – $Pr(H = 1|C = 1)$ – is small, not more than 10 percent in unvaccinated populations, and likely smaller. Menachemi et al. (2021) estimate 2.1 percent in Indiana (excluding nursing homes) and Mahajan et al. (2021) estimate 7 percent in Connecticut, both using random sample testing to establish population prevalence and treating the number of hospitalizations as known. Salje et al. (2020) estimate 2.9 percent, using a model-driven approach. All estimates imply that our upper bound is too low by, at most, 7.5 (1/.93) percent (we emphasize: percent, not percentage point). As our upper bound is usually less than 5 percent, we are left with a bias of, at most, .4 percentage points.

Figure E.1: Estimate ICLI-related hospitalizations as a share of the population, by week



Notes: Figure plots, for each week, two estimates of the share of the population of Indiana admitted for an ICLI-related hospitalization. Not all hospitals report diagnosis information, so the upper bound assumes hospitalizations are ICLI-related if the diagnosis information is unreported. The "based on observed diagnosis" line assumes that the share of ICLI-related hospitalizations among the hospitals with missing diagnosis information is equal to their share among the hospitals with reported information.

F Inference and age adjustment details

F.1 Inference for Intersection Bounds

The sample analogue estimators we use to construct the test monotonicity, hospital monotonicity, and hospital independence bounds reported in the paper are all asymptotically consistent. However, the hospital monotonicity and hospital independence bounds are examples of "intersection bounds". The sample analogue estimators are asymptotically consistent but their sampling distribution is somewhat complicated and the point estimates may include finite sample bias because the minimum and maximum operators are non-linear.

To understand the finite sample bias of the intersection bounds, consider the upper bound on population prevalence under under test monotonicity and hospital monotonicity:

$$\begin{aligned} U_{mH} &= \min\left\{Pr(C=1|D=1), Pr(C=1|D=1,H=1)\right\} \\ &= \min\left\{\text{Population test positivity}, \text{Hospitalized test positivity}\right\}. \end{aligned}$$

We estimate this bound by using the sample analogs of $Pr(C=1|D=1)$ and $Pr(C=1|D=1,H=1)$, say $\hat{P}(C=1|D=1)$ and $\hat{P}(C=1|D=1,H=1)$. Because the minimum operator is not linear, $E[U_{mH}]$ is not equal to the minimum of the two expectations. Suppose for illustration that, in the population, $Pr(C=1|D=1,H=1) < Pr(C=1|D=1)$, so that the hospital test positivity binds. In that case, finite sample bias may arise because in any given random sample, there is a positive probability that $\hat{P}(C=1|D=1) < \hat{P}(C=$

$1|D = 1, H = 1)$. But if this probability is small, then so is the bias.

We using the bootstrap method described in Manski and Pepper (2009); Kreider and Pepper (2007) to estimate confidence intervals for the test monotonicity, hospital monotonicity, and hospital independence bounds and to assess concerns about finite sample bias in the hospital monotonicity and hospital independence bounds.

We use 500 bootstrap simulations. In each bootstrap replication we formed each set of bounds. We used percentiles of the bootstrap distribution of the upper and lower bounds to form a 95 percent confidence interval around the identified set. The lower bound of the 95 percent confidence interval is the 2.5th percentile of the bootstrapped lower bounds, and the upper bound of the 95 percent confidence interval is the 97.5th percentile of the upper bounds.

We also used the bootstrap to estimate the degree of finite sample bias associated with the hospital monotonicity and hospital independence bounds. We estimate the finite sample bias as the difference between the average estimate in the bootstrap sample and the actual point estimate in the full sample. Table F.1 shows bootstrap estimates of the bias in the upper bound under test monotonicity and hospitalization monotonicity applied to the non-ICLI hospitalized population. The bootstrap results suggest that the finite sample bias is negligible in our application. The estimated bias is less than 1 percent (not percentage point) in most weeks. It makes sense that the bias is small because the sample size in our analysis is very large, and also because there is such a large gap between population test positivity and hospitalized test positivity. The results is that there is a low probability across bootstraps that $\hat{P}(C = 1|D = 1) < \hat{P}(C = 1|D = 1, H = 1)$. We show this probability week-by-week in Figure F.1. In the first month of the sample there is a non

trivial probability – 10-30% – that the inequality does not hold in a given random sample. After mid-April, however, this probability becomes essentially zero in every week. Accordingly, finite sample bias is not an important worry in our application and we do not attempt to correct our point estimates or confidence intervals for finite sample bias.

F.2 Age Adjustment

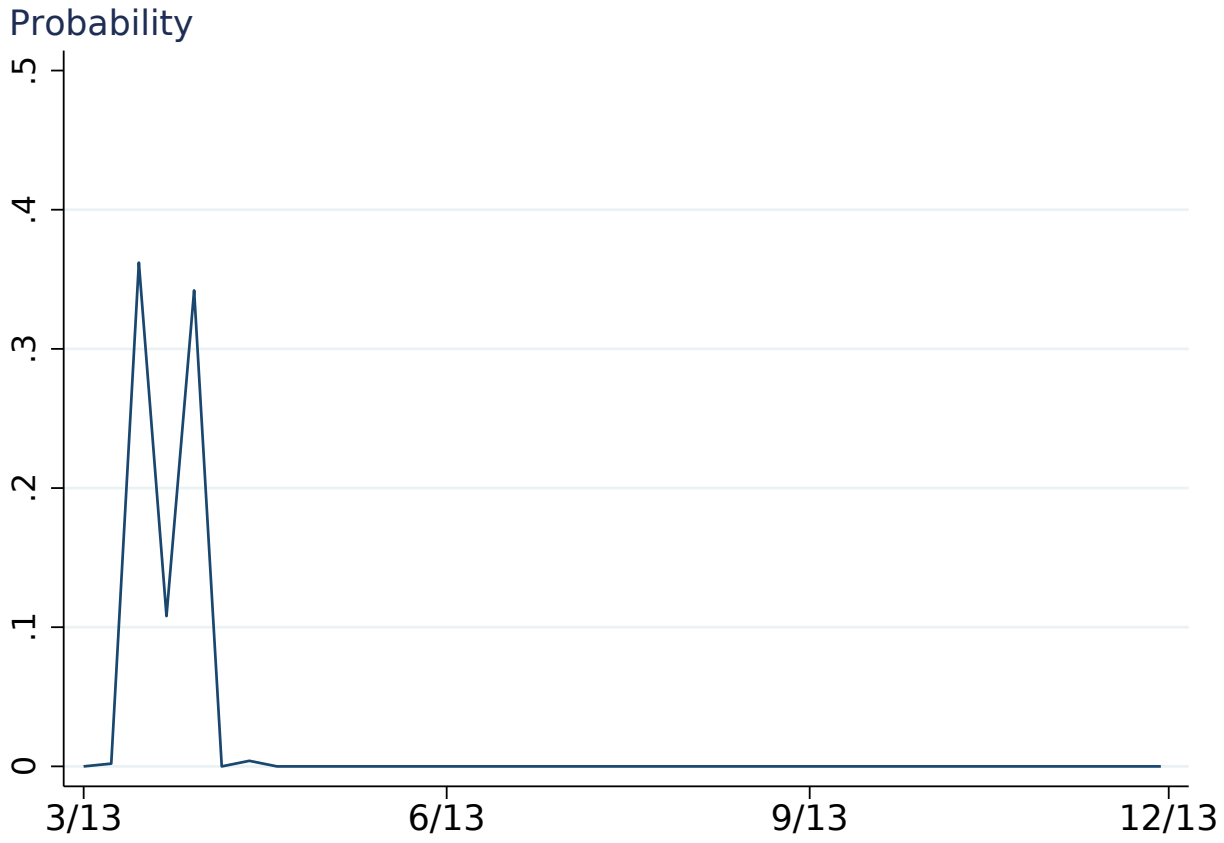
Because the tested and hospitalized samples are not age representative of the general population, throughout the paper, we report both unadjusted results and age-standardized upper and lower bounds. This simply means that we stratify the data six age groups (0-17, 18-30, 30-50, 50-64, 65-74, and 75 and older) and then compute the upper and lower bounds within each age-strata. Afterwards, we average the age group specific bounds by weighting each age-specific bound by that age group's share of the Indiana population. We construct confidence intervals for the age-adjusted bounds using the bootstrap; in each bootstrap iteration we calculate the age-adjusted bound or intersection bound (as appropriate), and our confidence intervals for the bound are the 2.5th percentile of the lower bound confidence interval and the 97.5th percentile of the upper bound confidence interval.

Table F.1: Small bias in intersection bounds

	Upper bound	Bias	Bias/bound
mean	0.044	-0.000	-0.003
min	0.011	-0.005	-0.037
p25	0.020	-0.000	-0.009
p50	0.031	-0.000	-0.002
p75	0.053	0.000	0.004
max	0.159	0.001	0.016

Notes: Table reports statistics on the estimated upper bound (under test monotonicity and hospitalization monotonicity applied to the non-ICLI hospitalization), the bias in the upper bound, and the ratio of the bias to the bound. These statistics vary across weeks in the sample. All bounds are age-adjusted. We estimate the bias as the difference between the average estimate in the bootstrap samples and the actual estimate.

Figure F.1: Estimated probability that $\hat{P}(C = 1|D = 1) < \hat{P}(C = 1|D = 1, H = 1)$, by week



Notes: Figure shows the estimated probability, for each week, that $\hat{P}(C = 1|D = 1) < \hat{P}(C = 1|D = 1, H = 1)$, estimated using a bootstrap. All bounds are age-adjusted.

G Validity Tests

Our main results show that the test monotonicity bounds on prevalence are much tighter for the non-ICLI hospitalized population than for the population as a whole. These tighter bounds are informative for general population prevalence only under additional assumptions about hospital representativeness, either a monotonicity assumption or an equal prevalence assumption. How valid are these assumptions? Assessing them directly is of course impossible because we lack data on prevalence in the population as a whole or in the hospital sample.

Our main analysis provides one type of indirect evidence in support of our hospital representativeness assumptions. The non-ICLI and clear-cause samples generate similar bounds, and, within the clear-cause sample, there are not large differences in bounds across different causes of admission. This suggests that prevalence does not vary with the exact set of hospitalizations studied, although of course this does not prove hospitalization monotonicity or hospitalization independence are credible assumptions.

In this section, we provide two additional pieces of evidence on the hospital IV assumptions. First we show that the hospital bounds are consistent with the estimates of population prevalence from the Indiana COVID-19 Random Sample Study (Menachemi et al., 2020; Richard M. Fairbanks School of Public Health, 2020).[14] Second, we compare the hospital sample to the general population in terms of their likelihood of prior testing (prior to the hospital data) and the test rate of their home counties. We take these to be proxies for their concern about COVID, although other interpretations are possible.

[14] Our data do not contain the test results from the Random Sample Study, so we compare our bounds to the published results.

G.1 Comparison to random sample testing

A valuable benchmark for the hospital-based prevalence bounds comes from a large-scale study of SARS-CoV-2 prevalence in Indiana. The study invited a representative sample of Indiana residents (aged 12 and older) to obtain a SARS-CoV-2 test. The first wave of the study took place April 25-29, and the second wave took place June 3-7. The preliminary results are reported in Menachemi et al. (2020) and Richard M. Fairbanks School of Public Health (2020). The response rate was roughly 25 percent, and no attempt was made to correct for non-random response. Nonetheless this survey appears to be the best benchmark available. We report the point estimates for prevalence (assuming random nonresponse) and their confidence intervals in the top panel of Table G.1. The first wave estimates 1.7 percent prevalence and the second 0.5 percent.[15]

We compare our prevalence bound during the weeks containing the random sample survey, in the bottom panel of the table. Using population testing we obtain very wide bounds that contain the random sample study estimates. This fact provides some support for the test monotonicity assumption. Under our hospital representativeness assumptions, the bounds are tighter, especially in June. Our bounds under hospital monotonicity always contain the random sample study point etimates. Under hospital independence, the point estimate lies slightly below the lower bound. However the 95% confidence interval always overlap. Thus for both dates the prevalence point estimates are consistent with the bounds we obtain under our hospital representativeness assumptions.

[15] The estimates in Table G.1 are slightly different from those reported by Richard M. Fairbanks School of Public Health (2020). We report updated calculations, based on correspondence with the authors.

Table G.1: Do our bounds contain estimates of prevalence from random-sample testing?

Time period	April 25-29	June 3 -7
Random Sample Study		
Prevalence estimates	0.0170	0.005
95% confidence interval	(0.011, 0.025)	(0.002, 0.013)
Bounds from...		
Population testing	[0.0008, 0.137]	[0.0006, 0.059]
	(0.0008, 0.142)	(0.0006, 0.061)
(0.0008, 0.001)	(0.0006, 0.001)	
Non-ICLI hospitalizations (H-M)	[0.0008, 0.086]	[0.0006, 0.022]
	(0.0008, 0.119)	(0.0006, 0.028)
(0.0008, 0.118)	(0.0006, 0.028)	
Non-ICLI hospitalizations (H-I)	[0.0182, 0.086]	[0.0073, 0.022]
	(0.0142, 0.119)	(0.0055, 0.028)
(-0.0551, 0.118)	(-0.0087, 0.028)	
Clear cause hospitalizations (H-M)	[0.0008, 0.057]	[0.0006, 0.021]
	(0.0008, 0.088)	(0.0006, 0.035)
(0.0008, 0.088)	(0.0006, 0.035)	
Cause hospitalizations (H-I)	[0.0108, 0.057]	[0.0064, 0.021]
	(0.0058, 0.088)	(0.0023, 0.035)
(-0.0407, 0.088)	(-0.0117, 0.035)	

Notes: The first two rows of the table report the estimated population prevalence and 95% confidence interval from the Indiana COVID-19 Random Sample Study, conducted over the indicated dates, which assumes random nonresponse (Menachemi et al., 2020; Richard M. Fairbanks School of Public Health, 2020). The remaining rows report the (age-adjusted) bounds on prevalence, in brackets, with 95-percent confidence intervals, in parentheses, from our different samples, under test monontonicity, as well as hospital monotonicity (H-M) or hospital independnence (H-I) as indicated, for the week containing the random sample study period.

G.2 Comparison of prior testing and community testing

A standard way of measuring representativeness is to compare the distribution of covariates in a study population to their distribution in the target population. In our case, this approach is most convincing if we have well-measured covariates that proxy for SARS-CoV-2 infection risk. Two candidate covariates are the community SARS-CoV-2 testing rate and the prior testing rate. The idea behind these proxies is that people who come from areas with high test rates, or who have been tested in the past, may themselves have a higher current likelihood of being infected with the virus.

To operationalize these measures, we define the community testing rate for person i as the fraction of people in i's county who have ever been tested, as of the end of our sample period. We define the prior test rate of person i as of date t as the probability that i was tested at least once during the week-long period $[t-15, t-9]$. We focus on this window because it is the second week prior to our hospital testing window (which runs from $t-2$ to $t+4$ for a patient admitted at t). We allow for a week of time to elapse between the hospitalization and the "prior" testing because it is possible that some pre-hospital testing would occur in the window $[t-8, t-3]$. When studying prior tests, we limit the sample to each person's first hospitalization after March 1, 2020, to avoid picking up the higher testing that mechanically results from the fact that people hospitalized once are more likely than the general population to have been previously hospitalized. As with our bounds, we weight the data to match the population age distribution.

Table G.2 shows the community testing rate. The average county in Indiana has a testing rate of 25%, with an interquartile range of 22% to 28%. The average person lives

Table G.2: Hospitalized patients are not drawn from counties with high test rates

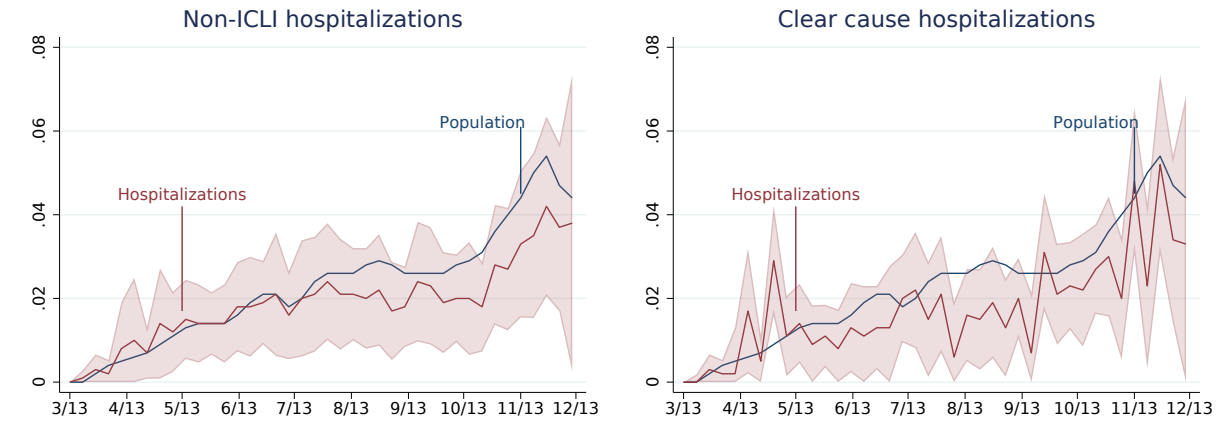
	County test rate
Average	0.252
25th percentile	0.219
75th percentile	0.280
Population	
Average person	.267
Hospitalizations	
Non-ICLI	.266
	[20.8]
Clear cause	.265
	[16.9]
ICLI	.267
	[2.5]

Notes: The county test rate is the share of the county population tested at least once in our test data. Table reports county-level statistics, as well as the average county test rates for the general population, the non-ICLI hospitalizations, clear cause hospitalizations, and ICLI hospitalizations, as well as t-statistic (in brackets) for the null hypothesis that the average person and the average hospitalization have the same county test rate.

in a county with a test rate of 26.7%. The average non-ICLI hospitalized patient comes from a county with a test rate of 26.6%, and the average clear-cause hospitalization patient comes from a county with a test rate of 26.5%. Among ICLI hospitalizations it is 26.7%. Our sample size is large enough that these differences are all statistically significant. Practically, however, the differences are very small. Hospitalized patients appear to come from counties that are roughly representative in terms of their testing rates. These rates are all significantly different from the population average.

Figure G.1 shows the prior testing rate as a function of admission date for the non-ICLI hospitalization sample, the clear-cause hospitalization sample, and the general population (for which the prior test rate on day t is defined as the fraction tested between $t - 15$

Figure G.1: Prior test rates, population and hospitalization samples



Notes: The prior test rate is the fraction of the group at date t that was tested between $t-15$ and $t-9$. Figure plots the average prior test rate for the population, for non-ICLI hospitalizations (in the left panel) and for clear cause hospitalizations (right panel). The shaded area is the 95% confidence interval for each week and hospitalization sample.

and $t-9$). The rates in the hospitalization samples are initially close to the population rate (when testing is low in general), but the lines diverge. By the last week of the sample, the prior testing rate is 1-2 percentage points lower in the hospitalization samples, than in the population. Although the differences in weekly testing rates are not statistically significant, the lower prior testing rate in the hospital sample could indicate that the hospital sample is negatively selected on SARS-CoV-2 infection risk.