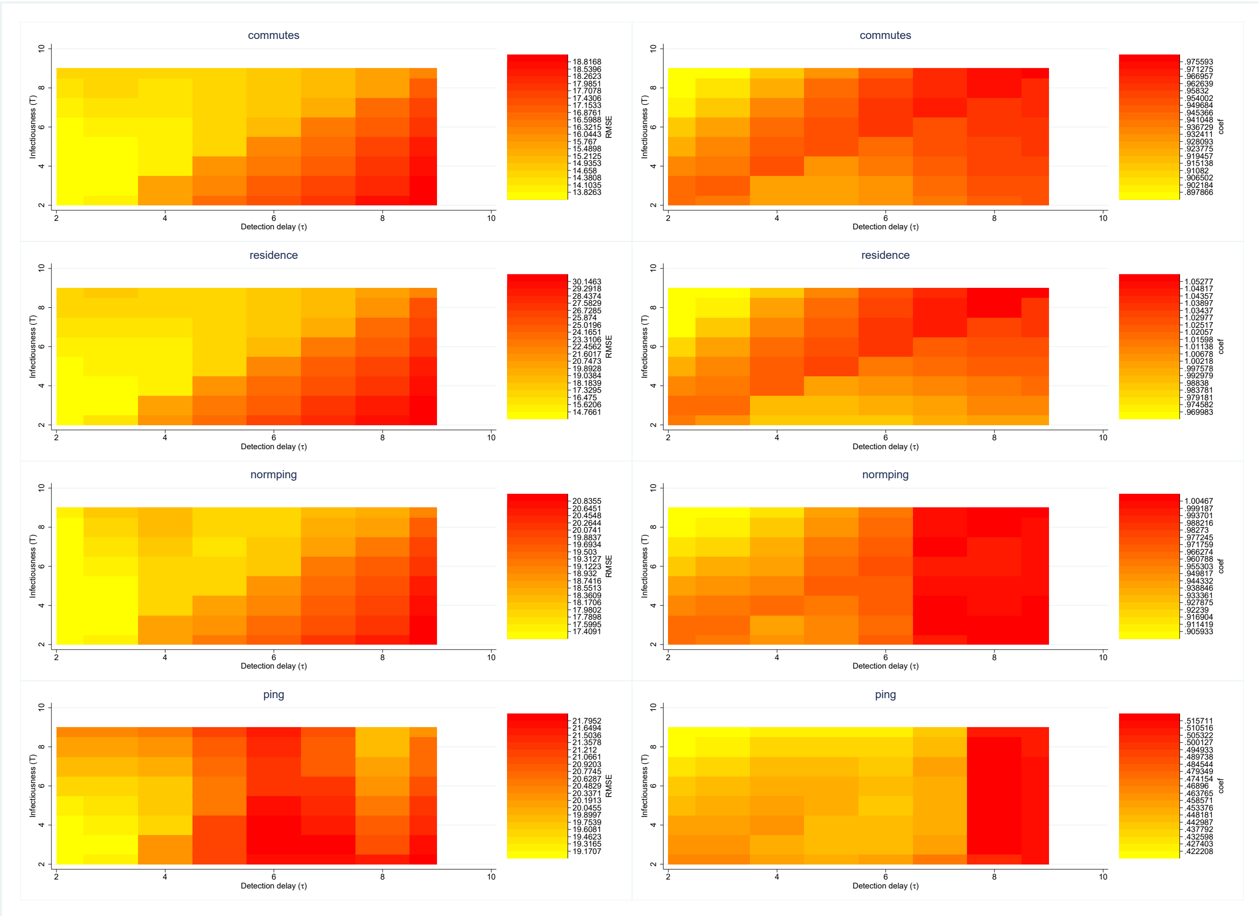
Figure S.2: Transmission coefficient and Root Mean Squared Errors for combinations of exposure (days of infectiousness vertical axis) and lag (days between presumed infection and registration of case).



S2 Results without instrumentation (not in manuscript – for review only)

Table S.1: Transmission equation (OLS) conditioned on different definitions of population density

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
		full sample				1st month after first 10 cases		
exposure	0.99***	0.86***	0.94***	0.86***	1.13***	0.47***	0.76***	0.51***
	(0.01)	(0.05)	(0.03)	(0.05)	(0.05)	(0.11)	(0.07)	(0.10)
log density		0.02***				0.10***		
X exposure		(0.01)				(0.02)		
ln density		0.39***				-0.05		
		(0.11)				(0.13)		
log built density			0.01**				0.06***	
X exposure			(0.00)				(0.01)	
log built density			0.24***				-0.04	
			(0.07)				(0.10)	
log house density				0.02***				0.09***
X exposure				(0.01)				(0.02)
log house density				0.43***				-0.09
				(0.11)				(0.13)
Observations	583,301	583,301	577,393	582,656	87,146	87,146	86,449	87,056
State-day fixed effects	yes	yes	yes	yes	yes	yes	yes	yes

*Notes. Estimated in first differences. Twoway (county and day) clustered standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1. Exposure is the 6-day lagged 5-day average number of infected, weighted for each destination county with the origin-normalized commuting flows, multiplied with the origin population.*

Table S.2: Transmission equation conditioned on different urban covariates (OLS)

	(1)	(2)	(3)	(4)	(5)	(6)
		Full sample			First month	
exposure	0.86***	2.02***	1.86*	0.46***	3.34	-1.07
	(0.07)	(0.74)	(0.95)	(0.16)	(2.43)	(1.63)
exposure interacted with..						
log density	0.02*		0.00	0.10***		0.16***
	(0.01)		(0.02)	(0.02)		(0.02)
public transport share		0.04***	0.05***		0.08**	-0.02
		(0.02)	(0.02)		(0.04)	(0.04)
health insurance coverage		0.22	0.19		1.07	1.27
		(0.17)	(0.22)		(1.50)	(1.36)
log average wage		0.17	0.18		-0.19	-0.04
		(0.11)	(0.12)		(0.17)	(0.15)
unemployment rate		-0.66	-0.51		-2.37	-3.02*
		(0.95)	(0.97)		(2.23)	(1.58)
proximity index of jobs		-0.55*	-0.53		-0.46	0.12
		(0.30)	(0.35)		(1.09)	(0.97)
work-from-home		-2.79***	-2.83***		-0.82	-1.36
share of jobs		(0.79)	(0.80)		(1.87)	(1.82)
share elderly (>70)		-0.03	-0.03		1.89	0.95
		(0.44)	(0.35)		(1.69)	(1.31)
share children (<18)		-0.35	-0.34		2.69	2.10
		(0.47)	(0.41)		(1.63)	(1.49)
Observations	86,620	86,620	86,620	12,234	12,234	12,234
State-day fixed effects	yes	yes	yes	yes	yes	yes
Interaction arguments	yes	yes	yes	yes	yes	yes

*Notes. Estimated in first differences. Twoway (county and day) clustered standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1. Exposure is the 6-day lagged 5-day average number of infected, weighted for each destination county with the origin-normalized commuting flows, multiplied with the origin population.*

S3 Containment policies (not in manuscript – for review only)

Containment policies and non-pharmaceutical interventions could change the estimated impact of density on transmissions. In order to understand the role of containment policies, I update the transmission equation to allow policies to affect transmission estimates as:

$$\Delta i_{ot} = \beta_1 \Delta Exposure_{ot} + \beta_2 NPI_{ot} + \beta_3 \Delta Exposure_{ot} * NPI_{ot} + \alpha_{st} + u_{ot}. \tag{7}$$

The variable NPI_{ot} (for non-pharmaceutical interventions) reflects whether containment policies (stay at home orders, public school closures, restaurant closures, foreign travel bans) were active for county i at day t. The data are from Killeen et al. (2020).

The regression results should not be interpreted as evidence on which policies are effective or not, because the state-day fixed effects eliminate most variation in policies. Instead, the aim of this regression is to understand whether the policies show differentiation in the impact, according to the population density of the area.

Table S.3: Transmission rates under different containment policies

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
		full sample				first month		
Exposure	0.02	0.01	0.61**	0.65**	2.04***	1.17***	0.44**	0.65**
	(0.03)	(0.03)	(0.24)	(0.25)	(0.44)	(0.12)	(0.19)	(0.25)
Stay-at-home order	39.54*	39.60*	39.53*	39.60*	9.64***	6.86***	9.65***	39.60*
	(20.61)	(20.60)	(20.61)	(20.65)	(3.24)	(2.50)	(3.25)	(20.65)
Stay-at-home order X exposure	0.01	0.07***	0.01	0.06***	-0.91**	-0.73***	-0.91**	0.06***
	(0.01)	(0.01)	(0.01)	(0.01)	(0.45)	(0.19)	(0.44)	(0.01)
Public school order X exposure			-1.30***	-0.74***			0.06	-0.74***
			(0.40)	(0.23)			(0.37)	(0.23)
Restaurant order			9.69***	9.87***			-7.57**	9.87***
			(1.04)	(0.59)			(3.78)	(0.58)
Restaurant order X exposure			0.10	-0.23			0.73***	-0.23
			(0.26)	(0.19)			(0.22)	(0.19)
Foreign travel ban X exposure			0.42	0.32			0.56	0.32
			(0.33)	(0.21)			(0.31)	(0.21)
Stay-at-home order X		-0.01***		-0.01***		0.10***		-0.01***
log density X exposure		(0.00)		(0.00)		(0.02)		(0.00)
Public school order X				-0.00				
log density X exposure				(0.00)				
Restaurant order X								-0.00
log density X exposure								(0.00)
Observations	602,332	602,117	602,332	602,117	87,858	87,826	87,858	602,117
State-day fixed effects	yes	yes	yes	yes	yes	yes	yes	yes

*Notes. Estimated in first differences. Twoway (county and day) clustered standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1. Exposure is the 6-day lagged 5-day average number of infected, weighted for each destination county with the origin-normalized commuting flows, multiplied with the origin population.*

Table S.3 shows the main results. Columns 1 and 5 focus on stay-at-home measures associated to lockdown, in the full sample and in the first month after developing 10 infections. Stay-at-home-orders are associated with significantly lower transmission rates in the first month of the outbreaks (columns 5) but not over the full sample. When allowing the assocation of stay-at-home orders with transmission rate estimates to vary with population density, I find that places with a log point higher density had higher estimated transmission rates under a stay-at-home order, but slightly lower transmission estimates over the full sample. It is important to bear in mind that these are not causal impact estimates of the policies, but only descriptives that help understand whether containment policies bias the main results.

Stay-at-home orders are not (first month) or only marginally (full sample) correlated to lower transmission estimates in more densely populated areas, as compared to less densely populated areas. Hence, differences in containment policies can at best explain a part of the result that densely populated areas saw higher transmission estimates initially than other areas, but converge over time.

Columns 3 and 7 introduce other containment measures, next to the stay-at-home-order. In the full sample, the interaction of public school orders with exposure are significant. However, the impact of public school orders on transmission estimates does not vary with density, witnessing the insignificance of the triple interaction between exposure, public school orders and log density entered in column 4. In the first month sample, restaurant orders show significant (albeit positive) interactions with exposure. However, introducing the triple interaction of restaurant orders, exposure and density does not point to variation in the impact of restaurant order on transmission with differences in density.

S4 Shelter in place mobility choices (not in manuscript – for review only)

Table S.4 reports regressions for different forms of trips. A daily additional infection per 1,000 people in the preceding two weeks (140 in 100,000 in the two-week case numbers) leads to 28% decline in trips to workplaces. A sample standard deviation increase in the exposure rate is linked to about 30% decline in workplace trips. Phone tracking in the residence increases significantly, while tracking in transit stations, parks, groceries, pharmacies, retail and recreation all decline significantly. The share of phone pings in the county of residence also increases significantly, so the devices leave their resident county less often by comparison.

Table S.4: Mobility responses to infection rates

	(1) Workplace	(2) Residence	(3) Transit	(4) Parks	(5) Groceries & pharmacies	(6) Retail & recreation	(7) Residential county ping share
cases per 1,000 cap	-28.34***	11.26***	-67.08***	-172.09***	-66.03***	-81.65***	23.67***
	(3.04)	(1.82)	(12.98)	(54.98)	(8.99)	(9.38)	(4.19)
Observations	472,892	224,208	183,742	107,269	294,250	321,639	217,969
county fixed effects	yes	yes	yes	yes	yes	yes	yes
state-day fixed effects	yes	yes	yes	yes	yes	yes	yes

*Notes. Estimated in first differences. Twoway (county and day) clustered standard errors in parentheses. *** $p<0.01$, ** $p<0.05$, * $p<0.1$. Exposure is the 6-day lagged 5-day average number of infected, weighted for each destination county with the origin-normalized commuting flows.*