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Abstract

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JEL Classification: R21

Keywords: footfall, shopping, COVID-19

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Covid-19 and Shopping Streets*

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March 30, 2021

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1 Introduction

Covid-19 brings about unprecedented economic changes as well as policies that could not have been foreseen before the advent of the pandemic. One of the sectors that suffers the most is the retail sector (including restaurants and bars). This sector contributes typically about 8-10% of GDP and is even larger in terms of employment. Hence, the productivity of retail activities is paramount for the functioning of the economy. Moreover, busy shopping areas are considered by many as being very important for the liveliness of cities (Jacobs 1961, Glaeser et al. 2001, Öner 2017).

The growth in online shopping in the last year has been unprecedented (about 80%, see Emerce 2020). Nevertheless, it accounted for merely 10% of overall retail sales before the start of the pandemic (Van Welie 2020). Moreover, growth in online shopping is concentrated in a few sectors, like clothing and household items, while other sectors, such as the food and beverages (FNB), are unaffected.

In this paper, we consider the impact of Covid-19 and subsequent Covid-19 policies on the productivity of shopping streets. Shopping streets are particularly important in Europe, whereas shopping malls are typically rare. We gained access to unique real-time Wi-Fi data on footfall, *i.e.* the daily visitor flow, measured for more than 500 locations across the Netherlands. Footfall is a standard measure to explain the attractiveness of a shopping location (Graham 2016). Koster et al. (2019) show that footfall is the main determinant of retail rent and report an elasticity of rent with respect to footfall of about 0.50.

Given the obvious importance of footfall for retail income, it makes sense to study the effects of different policies enacted during the pandemic on footfall. We focus on the Netherlands, where since the outbreak of the pandemic, three types of policies have been implemented: *(i)* partial and full lockdowns, *(ii)* the obligation to wear face masks inside shops as well as outdoors in main shopping streets, and *(iii)* social distancing.

The main policy implemented to curtail the spread of the virus is lockdowns. The first lockdown was enacted on March 15, 2020, and lasted for about two months. In October 2020, a 'partial' lockdown was introduced, where FNB stores were obliged to close. Since December 15, 2020, a second full lockdown was implemented, which stipulated that all (offline) shops selling non-daily

(”non-essential”) products were forced to close. One expects these lockdowns to reduce footfall sharply because everyone is advised to stay at home. Although shops were allowed to be open during the first lockdown, many shops closed for a myriad of reasons (*e.g.* health safety, lack of employees, lack of demand) (Schelfaut 2020). Using a temporal Regression Discontinuity Design (RDD) in the spirit of Brodeur et al. (2021), we identify (discrete) shocks to local footfall around the implementation of lockdowns where we further allow the effect of lockdowns to vary by shopping street characteristics (*i.e.*, number of shops in the vicinity; type of shops), as shopping streets are extremely heterogeneous in these characteristics. Important and dense shopping streets are typically located within (historic) city centers in the Netherlands.

Our results show that the lockdown implies reductions in footfall of about 50%. There is considerable heterogeneity in this effect, with dense shopping streets being the most affected.

The second policy we consider is the legal requirement of wearing face masks *inside* shops as well as *outdoors* in busy shopping streets. A priori, the effect of face mask restrictions on footfall is ambiguous. On the one hand, it may increase demand for offline shopping, as it increases health safety. On the other hand, it may decrease demand if the (psychological) cost of wearing a face mask are increased.

Shoppers are obliged to wear face masks *inside* shops to limit transmission of the virus as of December 1, 2020. The face mask requirement was preceded by the government advice to wear masks inside shops from 30 September 2020.[1] Before the pandemic, Dutch shoppers did not wear face masks. In our econometric analysis, we will exploit the exact date of the implementation of the law. We emphasize that this date does not coincide with the introduction of other policies, allowing us to identify the effects of face mask requirements on footfall. We do not find any evidence that the law, or the advice to wear face masks inside shops, reduced footfall.

To measure the effects of *outdoor* face mask restrictions, we exploit a short-lived policy pilot applied to several busy shopping streets in the two largest Dutch cities (*i.e.* Amsterdam and Rotterdam). Using daily panel data and spatial differencing, we identify the effect of the outside face mask restriction on local footfall. We document a substantial 25% reduction in footfall

[1] In contrast to many other countries, the Dutch national government has not announced a state of emergency because of the pandemic, and therefore the introduction of a law which obliged shoppers to wear a mask inside public buildings needed a change of the law, which took time.

for regulated streets, and a slightly smaller effect for shopping streets nearby. These results suggest that the (psychological) costs of wearing a face mask outdoors are non-negligible, but also indicate that spatial substitution may be important as shoppers can go to other shopping streets where no restrictions apply.

Another important policy that was implemented to control the pandemic was social distancing, which was enforced on March 15 2020. More specifically, individuals from different households were advised to keep a 1.5 m distance from each other at all times. We investigate whether this may have reduced demand for shopping as Dutch shops are not very spacious and shopping streets, which are often located in historical city centers, tend to be crowded. We use a difference-in-differences design to compare footfall during social distancing with footfall on the same day of the same week in the previous years, following Ostermeijer et al. (2021). We also focus on heterogeneity in enforcement effects. Social distancing also implies strong reductions in footfall of about 45%, with the densest shopping streets (in terms of the number of shops) being the most affected.[2]

To investigate the implications of these above-mentioned results on retail income, we estimate the elasticity of footfall with respect to retail rents employing an instrumental-variables methodology introduced by Koster et al. (2019). Importantly, we show that this elasticity is positive at 0.50, but also far below one, implying that the rental income losses from a reduction in footfall are less than proportional. Hence, despite the sizable reductions in footfall, the reductions in retail sales are expected to be smaller.[3]

Do our results say something about when the pandemic is over? This is the case for at least two reasons. First, it is plausible that footfall levels in shopping streets located in employment centers do not return to pre-Covid levels, because permanent increases in the number of individuals working from home are expected (see studies cited in Brueckner et al. 2021). Second, many persons are now used to buy their products online and may be inclined to keep doing so when the pandemic is over. In this context, the results on social distancing are interesting, because

[2] Another contributing factor might be restrictions on the use of public transport. This contribution is likely small as the majority of shoppers uses the bicycle or the car. However, the contribution of restrictions on transport may have been stronger for footfall in historic city centers, where a larger share of shoppers makes use of public transport.

[3] We caution that the relationship between retail sales and footfall may be different during the pandemic because shopping trip-chaining is likely to be less common.

there were no other strong restrictions on visiting shops and infection rates were low. Still, we observe reductions in footfall of up to 45% during social distancing periods. Hence, we envisage that the long-run effects of Covid-19 on footfall are not negligible, and that shopping streets may not recover from the pandemic any time soon.

2 Data and context

2.1 Related literature

Our paper relates to an emerging economics literature on the causes and consequences of Covid-19 in an urban setting. These studies typically focus on the effects and consequences of Covid, but ignore the impact of *policies* related to Covid. For example, Kuchler et al. (2020) examine the impact of social networks on the spread of Covid between regions, while Glaeser et al. (2020) investigate how mobility of individuals, measured by cellphone data, can affect the spread of the virus. Brueckner et al. (2021) further examine the effects of Covid on the spatial structure of the economy. Our study on the effect of Covid policies on footfall will be of interest to scholars who seeks to understand the effectiveness of lockdown policies in curtailing the spread of the virus. Findings of this paper are relevant to many other countries as they suspend retail activities and restrict the mobility of individuals - policies akin to those implemented in the Netherlands - to curb the spread of the pandemic.

While we show that Covid-19 policies reduced footfall and adversely affected the retail sector, studies such as Baker et al. (2020) have shown that households initially increased spending on retail and footfall during the first lockdown, before decreasing consumption in the subsequent lockdowns. Binder (2020) further report that 40% of surveyed consumers prioritized spending on food. Hence, expenses on food products are unlikely to fall during lockdowns. Using cellphone data from `SafeGraph`, Althoff et al. (2020) document strong reductions in the visits to specific retail establishments, such as hotels and restaurants, due to the pandemic.[4] To the best of our knowledge, no paper has yet investigated the economic consequences of Covid-19 *policies* through changes in shopping behavior.

Our paper further contributes to a broader literature on retail productivity (Pashigian & Gould 1998, Gould et al. 2005) and in particular retail policy. For example, we find that *local* outside

[4] For a more complete review, see Brodeur et al. (2020).

face mask restrictions induce sizable reductions on local retail demand, in line with studies that show that *local* retail policies have strong spatial effects (Cheshire et al. 2015, Sanchez-Vidal 2016). Moreover, in line with the literature, we observe that the effects of national retail policies have implications that vary strongly over space (Bertrand & Kramarz 2002, Schivardi & Viviano 2011, Haskel & Sadun 2012).

2.2 Footfall data

We exploit a novel dataset on *daily* footfall in shopping streets provided by `Bureau RMC`, which places Wi-Fi sensors for shops, retail associations and municipalities. Footfall is recorded from 2018 onwards using 530 sensors that detect unique Wi-Fi signals emitted by mobile telephones carried by by-passing pedestrians.[5] Figure A1 in Appendix A.1 shows the locations of all these `RMC` sensors across major cities in the Netherlands.

This novel way of measuring footfall allows us to filter out signals that are not relevant for footfall – *e.g.* signals by personnel or residents living nearby – and avoids double counting (which happens when a person passes a sensor several times during a shopping trip), improving the accuracy of footfall measurement. Furthermore, we record daily footfall data, which is a stark improvement from the previous literature that relies on manually collected footfall data on an annual basis (Teulings et al. 2017, Koster et al. 2019). Our high frequency data allows us to accurately capture the effects of lockdown, facemask and social distancing policies on footfall.

In Figure 1, we show average weekly footfall levels for the years 2018, 2019, 2020 and (the first 8 weeks of) 2021. The impacts of the Covid policies on footfall are evidently depicted in this figure. First, we record a sizable drop in footfall after the first lockdown was implemented (March 15 2020). We then observe a slight increase in footfall after the first lockdown is relaxed. Footfall did not change much during the partial lockdowns but dipped sharply after the second lockdown is enforced on Dec 15 2020. One may be worried, as indicated in the figure, that footfall was already lower before the first lockdown. A plausible reason why footfall prior to the first lockdown in 2020 is lower compared to footfall in 2018 and 2019 is that Dutch people are

[5]For some sensors, we do not have observations for each day so we have an unbalanced panel. For that reason, in all our analyses we will include sensor fixed effects. We will also show that our results are insensitive to the inclusion of sensors with missing observations. To comply with privacy regulation, Wi-Fi signals (*i.e.* MAC addresses) are anonymized. Sensors are calibrated at the time of installation using information from manual counts. A (small) proportion of shoppers do not carry a mobile phone (see Soundararaj et al. 2020), which we will treat as random measurement error.

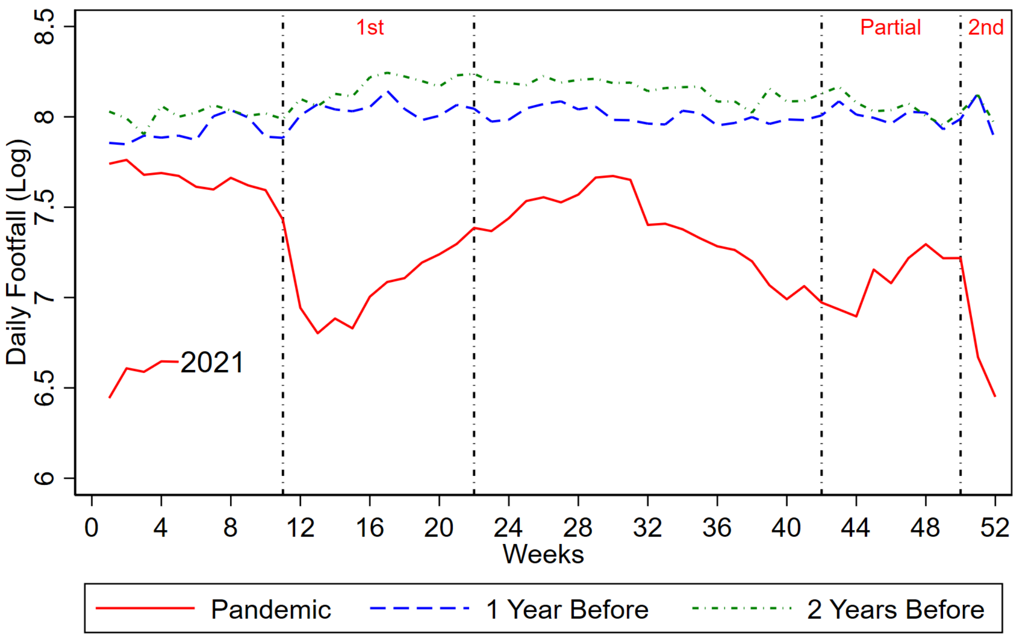

FIGURE 1 – FOOTFALL OVER TIME

Notes: We show the log of weekly footfall during first lockdown *(1^{st})*, partial lockdown *(Partial)* and second lockdown *(2^{nd})* in the year of pandemic (2020 and 2021), 1 year before (2019) and 2 years before (2018).

already avoiding crowded shopping streets as they are concerned about getting infected even before the outbreak. What is important is that our results do not hinge on this pre-trend, as we will show in Appendix A.4.

2.3 Other data

We also use yearly data on the universe of shops within the Netherlands from `Locatus`. For each non-vacant retail establishment, we know the 8-digit retail sector. In the analysis, we distinguish between four aggregated sectors to facilitate interpretation (*i.e.* Daily Shopping, FNB, Clothing and Other Retail). Using the exact location of `RMC` Wi-Fi sensors, we calculate the number of shops, as well as the share of type of shops within 500 m of each measurement point.[6]

In some sensitivity analyses, the number of Covid hospital admissions is used to control for the probability of getting infected by Covid. These data are derived from `RIVM` (*i.e.* the National Institute for Public Health and the Environment). National weather data from `KNMI` (*i.e.* the Royal Netherlands Meteorological Institute) are used to construct control variables. For detailed

[6]Changing the buffers to 200m has an immaterial impact on the estimates. See Table A8 in Appendix A.4.

description of the variables employed in the analysis, refer to Table A1 in Appendix A.1.

We report descriptive statistics in Appendix A.1 for different samples used in the econometric analyses. The main takeaway of these descriptives is that shopping streets are extremely diverse in terms of shop density (the median density is 355 shops within 500 m, whereas the density can be as high as 1622 shops), but also in terms of shop type. In Appendix A.2, we show that in city centers, shopping streets have a high density of shops and a high share of clothing stores: a 5 km increase in distance to the city center reduces density by two-thirds, whereas the share of clothing store drops by almost one third.

3 The effect of lockdowns

3.1 Econometric framework

We exploit the temporal variation in footfall to capture the effects of the *start* of various lockdown events, labelled by the subscript n, on log footfall across the Netherlands.[7] Here, we examine the effects of the start of the 1st lockdown (15th of March 2020), 2nd lockdown (15th of December 2020) and a 'partial' lockdown (14th of October 2020).[8] Hence, $n = 1, 2, p$.

We start by estimating the following temporal regression discontinuity model:

$$\ln(F_{it}) = \alpha_i + \sum_n \gamma_n L_{nt} + f_n(D_{it}) + X'_{it}\delta + \epsilon_{int}, \tag{1}$$

where the dependent variable, $\ln(F_{it})$, is the natural logarithm of footfall recorded by RMC sensor i on day t. Our key variable of interest is L_{nt}, which is a binary variable that takes the value of 1 at day t after type n lockdown is enforced. Hence, γ_n captures (approximately) the percentage change in footfall across all RMC sensors from type n lockdown. $f_n(D_{it})$ is a polynomial function of the number of days from the lockdown event n and we allow these trends of footfall to vary before and after the enforcement by interacting these polynomials with L_{nt}. We allow footfall trends to vary flexibly by including second-order polynomials to ensure that the discontinuous jumps in footfall around the lockdown events are not driven by pre or post event trends in

[7] Hence, we concentrate on the period around the start of a lockdown rather than the end of the lockdown. The main reason we do so is that lockdowns were not announced before, so anticipation effects are minimal. In contrast, the end of the lockdowns is announced, so anticipation effects, as well as induced-demand effects after the lockdown, make the interpretation of the end of the lockdown effects problematic.

[8] The lockdown was partial because only food and beverage (FNB) stores were forced to close.

footfall. α_i denotes RMC sensor fixed effects that partial-out time-invariant unobserved differences between locations. We further control for public and school holidays, and weather conditions.[9] These control variables are captured by X_{it}. To mitigate bias from unobserved time shocks, we constrain our analysis to observations within a window of 90 days around the type n lockdown.

We are particularly interested to examine whether effects of lockdown policies systematically differ across locations, as captured by k shopping street characteristics, i.e. shop density and share of shop type within 500 m. Note that these characteristics strongly vary over space, as analysed in Appendix A.2. We aim to capture heterogeneous spatial effects through structural characteristics that characterize shopping streets to improve on the external validity of our study.[10] Hence, we allow the effects of lockdown to vary across locations by interacting L_{nt} with demeaned shopping street characteristics, $Z_{ik} - \bar{Z}_k$. The estimation equation takes the following form:

$$\ln(F_{it}) = \alpha_i + \sum_n \gamma_n L_{nt} + \sum_n \sum_k \gamma_{nk} L_{nt}[Z_{ik} - \bar{Z}_k] + f_n(D_{it}) + X_{it}'\delta + \epsilon_{int}. \quad (2)$$

These estimates are meaningful as they capture the efficacy of lockdowns across different locations as captured by the shopping street characteristics. In our approach, we treat the interaction term with shop density as exogenous. However, one may argue that the shop count could be correlated with unobserved factors that affect footfall. Moreover, there could be concerns of reverse causality as retail firms are more likely to be situated at locations with high footfall. Interestingly, in our context, endogeneity is not necessarily problematic, because an *interaction term* between endogenous and exogenous variables may be treated as exogenous if the conditional expectation of the endogenous variable and the error term does not depend on the exogenous regressor with which the endogenous variable is interacted, as demonstrated by Bun & Harrison (2019).[11] Nevertheless, to deal with potential endogeneity issues, we instrument shop density with the number of cinemas in 1930 within 500 m. Historically, cinemas were small with one screen only and were located in shopping streets.

[9] We do not control for the number of Covid cases per municipality, as this variable is likely a 'bad control'. Inclusion of this variable does not change the results for lockdowns, see Panel C in Table A8 in Appendix A.4.

[10] For example, dense shopping areas may be located outside city centers in other countries.

[11] The validity of this assumption can be tested using a standard Hausman test, which compares the results of the model where the interaction term is treated as endogenous with the model in which it is treated as exogenous.

3.2 Results

It is common to support regression-discontinuity designs with graphs. In Figure 2, we show the effects of the first and second lockdown on log footfall. Perhaps surprisingly, the effects of both lockdowns appear very similar with a reduction of about 0.6-0.7 log points, despite the differences in restrictions imposed. In the first lockdown, shops were advised, but not required to close, while in the second lockdown, all shops were required to close except for those selling daily products (*e.g.* supermarkets and pharmacies). Our econometric analysis also implies that the point estimates of the first and the second lockdown are virtually the same (see Panel A and B of Table A7 in Appendix A.4). Hence, we will show results where we restrict the effects of these two lockdowns to be the same such that $\gamma_1 = \gamma_2$.

We also examine the impacts of the partial lockdown, but we do not discern any effect of the partial lockdown on footfall (see Figure A3 in Appendix A.3 in Appendix A.4). Hence, we ignore the effects of the partial lockdown from hereon ($\gamma_p \approx 0$).

In column (1) of Table 1, we show the results of the baseline regression discontinuity model (without interacting with shop counts and types) based on equation (1). It appears that these lockdowns attributed to a substantial 47% ($\exp(-0.635) - 1 \approx -47\%$) reduction in footfall. In column (2), we demonstrate that the reduction is much stronger in dense shopping streets. For instance, footfall in the densest shopping streets (with a density that is two log points above the average, which is about two standard deviations) is 67% lower after the lockdowns are enforced.

In column (3), we examine the heterogeneous effects of lockdowns according to shop types without controlling for shop density. It seems that the types of shops in the vicinity influence the impact of lockdowns on footfall. In particular, we observe that shopping streets with clothing shops are more affected by the lockdowns. However, when we control for shop density, in column (4), it turns out that these results are largely spurious as shop density and the shop type are strongly correlated. Specifically, we document, imprecisely estimated, and moderate, differences of lockdowns across different shop types. For example, the maximum share of clothing stores is 0.40, which is 20 percentage points above the mean. For these shopping streets, the effect of lockdowns is around $\exp(-1.215 \times 0.20 - 0.624) - 1 \approx -58\%$. In column (5), we examine the heterogeneous effect while instrumenting for shop density. The instrument is sufficiently strong

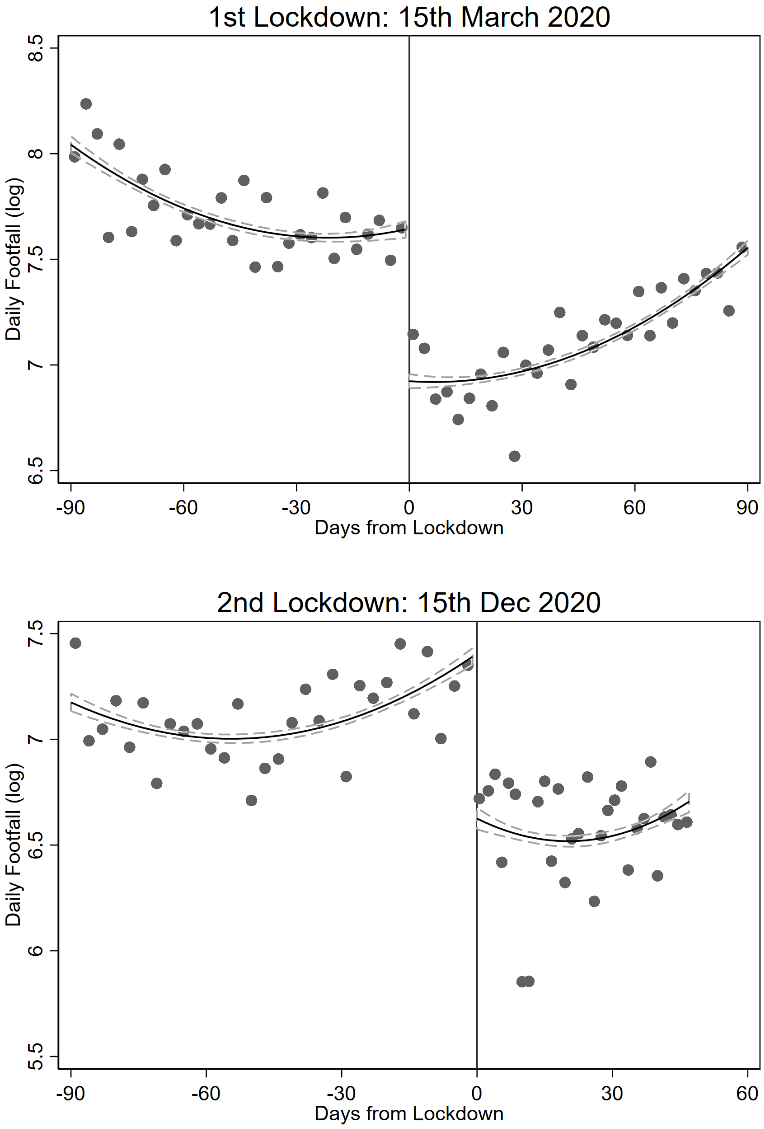

FIGURE 2 – LOCKDOWNS AND FOOTFALL

Notes: We show log daily footfall 90 days before and after first and second lockdown. We employ an Epanechnikov kernel and control for a second-order polynomial on both sides of the threshold. Dashed line denotes 95% confidence interval associated with second-order polynomial footfall trends. For additional figures that employ higher order polynomials for time trends, we refer to Figure A3 in Appendix A.3.

(and has the expected sign).[12] The Hausman T-test of equality between the coefficient of shop density in the IV and the OLS estimate is equal to 2.14, suggesting that the OLS estimate is biased (*e.g.* because of reverse causation). When instrumenting for shop density, we do not find evidence that shop type plays a role, while the effect of shop density is even more pronounced.

For additional results based on alternative polynomial functions and other bandwidths, we refer to Table A6 in Appendix A.4.

[12]The first-stage results are not reported due to space constraints but are available upon request.

Table 1 – The impact of lockdowns and social distancing on footfall

	Panel A: Lockdowns (RDD)				
	(1)	**(2)**	**(3)**	**(4)**	**(5)**
	Baseline	**Shop Count**	**Shop Type**	**Combined**	**Combined (IV)**
Lockdowns	-0.635[a]	-0.622[a]	-0.629[a]	-0.624[a]	-0.616[a]
	(0.078)	(0.071)	(0.075)	(0.071)	(0.071)
Lockdowns × Log Shop Counts		-0.199[a]		-0.157[a]	-0.369[a]
		(0.032)		(0.049)	(0.111)
Lockdowns × Share of Other Retail Shops			-1.497[a]	-0.782[c]	0.184
			(0.354)	(0.433)	(0.721)
Lockdowns × Share of Clothing Shops			-3.229[a]	-1.215[c]	1.503
			(0.445)	(0.706)	(1.541)
Lockdowns × Share of FNB shops			-1.728[a]	-1.005[b]	-0.030
			(0.499)	(0.434)	(0.759)
Obs	91822	90773	90773	90773	90773
Adj R2	0.79	0.78	0.78	0.78	0.36
Kleibergen-Paap F statistic					15.05
	Panel B: Social Distancing (DID)				
	(6)	**(7)**	**(8)**	**(9)**	**(10)**
	Baseline	**Shop Count**	**Shop Type**	**Combined**	**Combined (IV)**
Social Distancing	-0.626[a]	-0.618[a]	-0.627[a]	-0.623[a]	-0.623[a]
	(0.034)	(0.029)	(0.035)	(0.029)	(0.029)
Social Distancing × Log Shop Counts		-0.138[a]		-0.194[a]	-0.203[b]
		(0.029)		(0.049)	(0.089)
Social Distancing × Share of Other Retail Shops			0.312	1.207[a]	1.247[b]
			(0.380)	(0.445)	(0.507)
Social Distancing × Share of Clothing Shops			-0.702[c]	1.768[a]	1.878[c]
			(0.403)	(0.674)	(1.100)
Social Distancing × Share of FNB shops			-0.092	0.825	0.866
			(0.712)	(0.700)	(0.819)
Obs	234152	233148	233148	233148	233148
Adj R2	0.85	0.86	0.85	0.86	0.21
Kleibergen-Paap F statistic					16.16

Notes: The dependent variable is the natural log of visitor footfall at RMC sensor i on day t. Reported variables denote binary variables that take the value of 1 (Lockdowns) for RMC sensors after the 1[st] Lockdown (15[th] of March 2020) and 2[nd] Lockdown (15[th] of Dec 2020) for Panel A. RDD regressions from columns (1) to (5) in Panel A include controls for public and school holidays, weather conditions, RMC fixed effects and time trends (days to event) at second order polynomial of time trends (quadratic). We further restrict the analysis to a window 90 days from the event. Reported variables denote binary variables that take the value of 1 (Social Distancing) for RMC sensors after the social distancing is enforced (from 1st June 2020 to 13th October 2020) for Panel B. DID regressions from columns (6) to (10) in Panel B include RMC fixed effects, day-of-week fixed effects (Monday to Sunday) and week fixed effects (1-52 weeks) and year fixed effects. Baseline effects of lockdowns and social distancing are reported in columns (1) and (5) respectively. In columns (2) and (7), we further interact these binary variables with the demean natural logarithm of shop counts within 500m from the RMC. In columns (3) and (8), we interact binary event variables with demean share of shops (whether Daily shopping, Clothing, Food and Beverages(FNB) or other retail) within 500m from the RMC. In columns (4) and (9), we collectively estimate how shop counts and shop types can affect the impacts of lockdowns and social distancing on footfall. In columns (5) and (10), we repeat the analysis in columns (4) and (9) but we instrument log shop counts with the counts of historical cinemas in 1930. Two-way clustered standard at postcode and date levels are reported in the parentheses. [c] p<0.10, [b] p<0.05, [a] p<0.01.

4 The effect of face mask regulation

4.1 Econometric framework

We now examine the effects of the obligation to wear face masks *outdoors*. As this policy was introduced in a limited number of dense shopping streets for a short period of time, we measure the impact of this regulation using a difference-in-differences approach. We estimate how the face mask requirements, denoted by T_{it}, for Wi-Fi sensor i at day t, affected footfall, F_{it}. Let d_{it} denote the distance to the nearest treatment area. We estimate the following two-way fixed effects model:

$$\ln(F_{it}) = \alpha_i + \zeta_1 T_{it} + \zeta_2 T_{it}^{0-500\text{m}} + \tau_t + \epsilon_{it}, \qquad \text{if } d_{it} < d^{\max} \tag{3}$$

where T_{it} takes the value of 1 for RMC sensors where outdoor face mask wearing is compulsory between the 5th and 30th of August, 2020. The effect on log footfall due to mandatory face mask regulation is captured by ζ_1. To examine spillover effects on neighboring shopping streets, we include $T_{it}^{0-500\mathrm{m}}$, a dummy indicator with a value of 1 for RMC sensors within 500m from a regulated shopping street. The spillover effect is captured by ζ_2. We expect that $\zeta_1 < \zeta_2 < 0$.

Put differently, we are comparing changes in footfall before and after the face mask regulations are enforced between regulated and unregulated shopping streets, where we allow for spillover effects. We include RMC sensor and *date* fixed effects, where the latter control for general trends in footfall across areas, and other time-varying variables, which are thought to be important such as public and school holidays and the number of Covid cases.

Our identification hinges on the assumption that regulated and unregulated shopping streets have similar footfall trends. This assumption may not hold because regulated shopping streets are located around the city centers of the largest cities in the Netherlands. To address this, we restrict our sample to RMC sensors within 10 km from the regulation boundary to ensure that the trends in footfall are comparable between regulated and unregulated areas (hence, we set $d^{\max} = 10$). We further show the sensitivity of the results by restricting the sample to unregulated areas not more than 1km from regulated areas.

To further investigate the national implementation of equirements to wear face masks *inside* shops on September 30th (the advice) and December 1st (the law), we adopt the same temporal RDD approach as in Section 3.

4.2 Results

In Figure A2 in Appendix A.3, we provide a visual plot of log footfall for 90 days before and after the introduction of outdoors face mask regulation. Here, we distinguish between *i)* regulated shopping streets, *ii)* unregulated shopping streets and *iii)* streets within 500 m from face mask shopping streets. Our results suggest comparable trends in footfall across the three types of shopping streets before the regulation, and a reduction in footfall along regulated shopping streets.

Table 2 shows the results from the estimation of equation (3) based on a sample of RMC sensors within 10km from regulated streets. Our baseline estimate in column (1) implies that once outdoor

Table 2 – Effects of Outdoor Face mask regulations on footfall

	(1) Baseline	(2) 50% obs	(3) RMC Trends	(4) Within 1km	(5) Placebo 1 Yr Bef
Face mask regulation	-0.262[a]	-0.290[a]	-0.124[b]	-0.217[a]	-0.008
	(0.051)	(0.050)	(0.048)	(0.031)	(0.073)
within 0-500m	-0.158[b]	-0.192[b]	-0.103[c]	-0.122[c]	-0.089
	(0.066)	(0.071)	(0.055)	(0.059)	(0.086)
Obs	58113	54791	54791	43852	27851
No. of FE	59	51	51	41	40
Adj R2	0.89	0.90	0.93	0.90	0.93
% Δ (Facemask)	-23.06	-25.14	-11.63	-19.51	-0.78
Absolute Effects (Facemask)	-2582.99	-2873.65	-1329.02	-2229.50	-113.35
% Δ (within 0-500m)	-14.63	-17.48	-9.77	-11.48	-8.54
Absolute Effects (within 0-500m)	-956.19	-1022.52	-571.32	-671.30	-647.84

Notes: The dependent variable is the log of visitor footfall. Face mask regulation is a binary variable that takes the value of 1 when face mask regulation is enforced. Within 0-500 m is a binary variable that takes a value of 1 when regulation is enforced within 500 m, but the street is unregulated. All regressions include RMC and date fixed effects and the sample is restricted to 10km from the regulation boundaries unless otherwise stated. Standard errors reported in the parenthesis are clustered at postcode level. Absolute effects denote the changes in footfall due to the face mask regulations and are computed by multiply the relative effects with pre-treatment mean footfall. Two-way clustered standard at postcode and date levels are reported in the parentheses. [c] p<0.10, [b] p<0.05, [a] p<0.01.

face mask wearing is enforced, footfall changes by $\exp(-0.262) - 1 \approx -23\%$, corresponding to a reduction of about 2,500 shoppers per day. These findings are consistent with the notion that mandatory face mask regulations reduce footfall if shoppers care more about the hassle to wear face masks rather than the reduced probability of getting Covid. Footfall in shopping streets near regulated streets is also adversely affected by this regulation. Specifically, we document a smaller, but still substantial drop of 14.6% in footfall, which implies a reduction of around 1,000 shoppers.

To ensure that ζ_1 and ζ_2 measure causal effects, we subject our results to a battery of robustness tests. In column (2), we re-estimate models where we only include sensors with less than 50% missing observations. It appears then that the results become slightly more pronounced. In column (3), we relax the parallel trends assumption by additionally controlling for the interaction of postcode areas (which contain on average about four sensors) with year-month dummies. Despite adding many additional trend controls, we still find a statistically significant negative effect, about half of the original effect size.

In column (4), we further constrain our analysis to RMC sensors within just 1 km from the face mask regulation boundaries to ensure that RMC locations affected and unaffected by the regulation are comparable, as to minimize the risk of unobserved shocks correlated with the regulation. Although our sample now is now substantially smaller, we document a robust 19.5% and 11.5% reduction in footfall in shopping streets where face mask are required and in shopping streets within 500 m of the regulated streets, respectively.

It is common to do placebo tests to ensure that enforcement effects are not documented spuriously around other dates. Therefore, in column (5), we move the regulation window one year before the actual treatment date. Of course, any observations post 2020 are omitted to prevent the Covid-19 from driving our estimates. Here, we observe no statistically significant changes in footfall during the placebo period, suggesting that our results are unlikely to be driven by spurious time trends.

Finally, we also investigate the effect of the (national) advice (from September 30 onwards) as well as the formal requirement (from December 1 onwards) to wear face masks *inside* shops using a similar type of regression discontinuity methodology as applied to lockdowns. Here, we do not find any substantial effects on footfall, either when we plot the unconditional footfall using graphs or when we perform more rigorous regression analysis.[13] The absence of an effect of the national policy to require face masks in shops may be surprising, given that we find an effect for local outside face mask restrictions. We think that this may be indicative that Dutch shoppers have a strong disutility of wearing face masks outside, while they care less about wearing face masks inside buildings. Furthermore, with local face mask regulation, spatial and temporal substitution is likely. That is, people may have visited other shopping streets without restrictions or postponed their shopping trips until the local restrictions were lifted.

5 The effect of social distancing

5.1 Econometric framework

To capture the effect of social distancing on footfall, we apply a difference-in-differences approach where we compare footfall on days when social distancing policies are active with footfall one year or two years before. In other words, observations in years unaffected by the pandemic (in 2018 and 2019), which is the control group, should capture footfall trends in the absence of these

[13]Refer to Figure A3 in Appendix A.3 for a RDD plot of footfall around national face mask regulations. We do not report regression analysis of national facemask regulations on footfall due to space constraints but they are available upon request.

lockdown events. We estimate the following specification:

$$\ln(F_{it}) = \alpha_i + \beta S_t + Q'_{it}\delta + \tau_{t\in d} + \tau_{t\in w} + \epsilon_{it}. \tag{4}$$

Our key variable of interest, S_t, is a binary variable taking the value of one on dates after social distancing is enforced. To disentangle the effect of social distancing from other policies, we exclude observations on days where other Covid policies are active.

Footfall fluctuates over the year due to time-varying factors such as public holidays, school holidays and weather conditions. We add those as controls, denoted by Q_{it}. While this is unlikely to affect the consistency of our estimates, we control for temporal fluctuations in footfall to improve efficiency by including day-of-week fixed effects, $\tau_{t\in d}$, and week-of-the-year fixed effects, $\tau_{t\in w}$. Note further that RMC fixed effects, α_i, are included to control for time-invariant unobserved geographical differences in footfall unrelated to social distancing. The identifying assumption with this approach is that there are no unobserved correlated shocks on footfall in years before the pandemic that could bias our estimates. Earlier footfall trends in Figure 1 suggest that this is unlikely the case as pre-Covid footfall levels are comparable in 2018 and 2019.

5.2 Results

Panel B of Table 1 summarizes the impacts of social distancing on footfall.[14] In column (6) we find a strong reduction in footfall due to social distancing of $\exp(-0.626) - 1 \approx 47\%$. Unsurprisingly, the effect is considerably stronger in dense shopping streets (see column (7)). When we control for (the interaction with) retail type, it appears that the effect of shop density is somewhat stronger. These results hold even when we instrument shop density with historical counts of cinemas in 1930. It implies for example that the densest shopping streets (with a density that is two log points above the average) have reductions in footfall of 60%.

The effects associated with shop types are less clear cut (see columns (8)-(10)). When one does not control for shop density, it is suggested that clothing shops are more negatively affected, but the effect is only marginally significant once we control and instrument for shop density.

[14] Note that we exclude observations in (partial) lockdowns as well as face mask areas in these regressions so that we are only measuring the impacts of social distancing on footfall.

Hence, these results also indicate that shop density is the main shopping street characteristic that explains the heterogeneity of the effect of social distancing, as also found for the effect of lockdowns.

6 Footfall, retail rents and income

We find sizable effects of the different Covid-19 policies on footfall. To interpret these effects, we now provide a back-of-the-envelope assessment on retail income. Following Pashigian & Gould (1998), Gould et al. (2005) and Koster et al. (2019), we use retail rents as a proxy for retail income, which is an intuitive measure as retail firms are willing to pay higher rents at more productive locations.

The productivity effects of Covid policies depend on the elasticity of retail income with respect to footfall, which can be estimated based on the elasticity of rents with respect to footfall.[15] In Appendix A.5, we estimate this elasticity using OLS as well as IV, using the methodology introduced by Koster et al. (2019). Our preferred (IV) estimate is 0.50, with a standard error of 0.11, so it is substantially less than one (these results are essentially identical to Koster et al. 2019). The latter is important, as it implies that the rental income losses of a reduction in footfall are less than proportional to the reduction in footfall. More specifically, these estimates suggest that rental income losses because of Covid-19 policies are approximately half the reduction in footfall and therefore still considerable.

We calculate the total negative external effects of the lockdown and social distancing through reductions in footfall for (approximately) one year of Covid-19 policies for a representative shop in a shopping street. From March 15, 2020, until March 14, 2021, there were 168 lockdown days. We consider the preferred estimate to be the one reported in column (1), Table 1. The loss due to these lockdown days is about 11% of annual rental income ($\approx 0.50 \times (\exp(-0.635) - 1) \times (168/365)$), which is non-trivial. Social distancing was applied the whole year. Hence, the total costs are considerably larger. Using the estimate in column (5), Table 1, we calculate the costs to be 23% of rental income ($\approx 0.50 \times (\exp(-0.632) - 1)$).

These figures mask *extreme* differences between different shopping streets and ignore that

[15] Footfall also decreases shop vacancies. Koster et al. (2019) show that the effect of footfall on rental income through a change in vacancies is very moderate, and will be ignored here.

several policies were combined. For example, outdoor face mask requirements were implemented when social distancing was also required. It appears that dense shopping streets (usually with many clothing stores), bear the largest income losses. For example, the annual losses of lockdowns, despite that lockdowns were absent half of the time, are estimated to be about 16% ($\approx 0.50 \times (\exp(-0.620 - 2 \times 0.265) - 1) \times (168/365)$) of rental income for shops located in dense shopping streets. On the other hand, losses for shops in low-density shopping streets are estimated to be about 2% ($\approx 0.50 \times (\exp(-0.620 + 2 \times 0.265) - 1) \times (168/365)$) and therefore barely noticeable.

Our focus is on the effects of Covid policies on the income of shops *through reductions in footfall*. Our estimates are silent about the increases in online sales on shops in shopping streets induced by Covid policies, which may compensate for the income losses induced by reductions in footfall. Given that the online sale increases are small as a percentage of overall sales (in the order of about 5-10%), combined with the plausible assumption that increases in online sales are not systematically related to shop location, our estimates can be essentially interpreted as the effect of Covid policies on retail income including online sales.

7 Conclusions and implications

The current paper has four main messages. First, we show sizable economic costs of the different Covid-19 policies that restricted the mobility of the population to curb the spread of the virus through substantial reductions in footfall in shopping streets. We find large, persistent and heterogeneous effects of lockdowns, with an average reduction of 50% in footfall in Dutch shopping streets. A 6-months lockdown as observed in the Netherlands has induced losses equivalent to a reduction of 11% of yearly rental income for the retail sector. According to our estimates, the cost of the first and second lockdown in the Netherlands are of equal size, despite the fact that these lockdowns were quite different in terms of the number and types of shops that had to cease operation. This makes sense as positive shopping externalities play an important role: when a substantial number of shops closes, neighboring shops are negatively affected by the reduction in footfall.

Second, we observe that these policies are particularly effective in reducing footfall in *dense* shopping streets, mostly located in city centres. This result is important not only because of the spatial implications of these policies on reducing rental income, but also because the spread of the virus is more likely to occur along these streets. Hence, if these policies aim to reduce the spread of the virus through changes in shopping behavior in busy shopping streets, then these policies can be considered successful.

Third, we find that the obligation to wear face masks *outdoors* in busy shopping streets induces strong reductions in footfall in these streets (with an average reduction of 25%), but also substantial reductions in adjacent shopping streets, showing once more the importance of positive agglomeration externalities in shopping behavior. Finally, we report negative estimates of social distancing on footfall, *i.e.* we find a strong reduction in footfall during periods where there are no other Covid-19 policies.

Although our estimates are only informative on the current situation, we reckon that social distancing, lockdowns and face mask regulations is unlikely to be relaxed in the short run given the impediments involved in vaccinating the population. It is plausible that pandemic could bring about long-term changes in offline retail even when the pandemic is over because of the increase in work from home arrangements and the reliance on online shopping platforms. Hence, the substantial reductions in footfall we documented during the enforcement of Covid policies provide useful insights on what may happen to the retail sector in the near future, and suggest that long-run effects of Covid-19 on the retail income of shops in shopping streets may be non-negligible.

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Online Appendix

The appendix reports auxiliary details and analyses to the main paper. We first provide more details and additional visualizations of the data used in the empirical analysis. We then report additional results and several robustness checks of our main specifications.

A.1 Data Description

A.1.1 Spatial distribution of RMC sensors

The RMC network consists of around 500 different Wi-Fi sensors distributed across shopping districts within the Netherlands. These sensors require an internet connection and access to electricity, which is provided by the shop where the sensor is placed. To comply with privacy regulation, shops need to give permission for the use of these sensors. Therefore, sensor placement is based on contracts with retailers, municipalities and retail associations. The sensor coverage of cities is heterogeneous, since certain municipalities have a contract for many sensors, while others have a contract for only one Wi-Fi sensor.

Figure A1 presents the spatial distribution of RMC sensors across the Netherlands. We observe that most of the RMC sensors are located in major cities such as Amsterdam, Rotterdam, and The Hague, with some sensors located in other cities such as Groningen, Eindhoven, and Utrecht. Most of these sensors are saturated around city centers. Another noticeable observation is that the recorded footfalls are much higher for the RMC sensors in Rotterdam and Amsterdam.

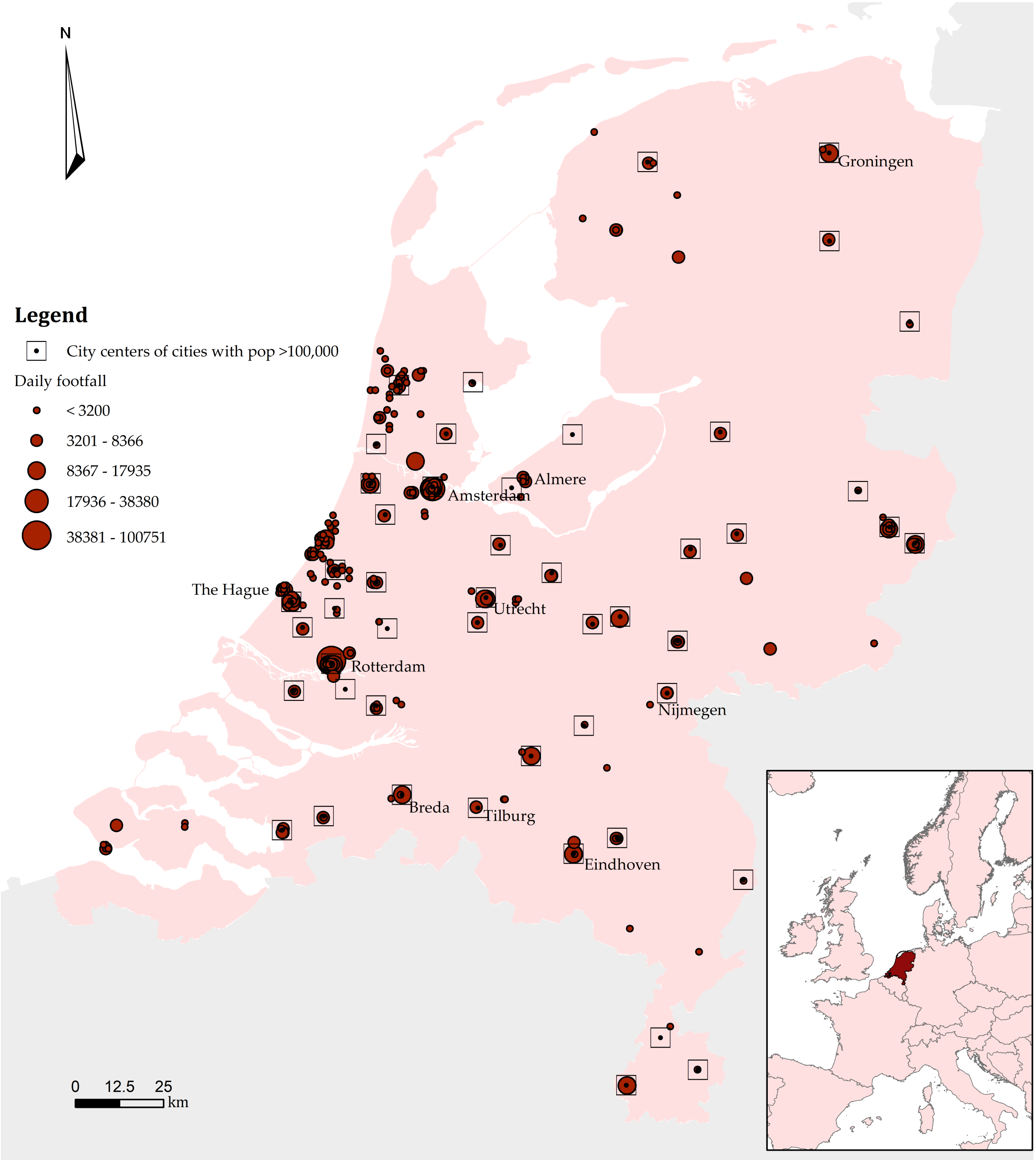


Figure A1 – Spatial distribution of RMC sensors

A.1.2 Variable definitions

TABLE A1 – VARIABLE DEFINITIONS

Variable Name	Definition	Source
Daily Footfall	Absolute footfall counts captured by RMC sensor i on date t	RMC
Retail Rents	Rents per sqm paid by retail firm i in year t	Strabo
Covid 19 Hospital Admissions	Total number of Covid-19 hospital admissions on date t	Dutch agency of public health (RIVM)
Number of Shops	Total number of shops within 500m from RMC sensor i	Locatus
Share of Daily Shops	Number of Daily Shops divided by Total Number of shops within 500m from RMC sensor i	Locatus
Share of FNB Shops	Number of FNB shops divided by Total Number of shops within 500m from RMC sensor i	Locatus
Share of Clothing Shops	Number of Clothing shops divided by Total Number of shops within 500m from RMC sensor i	Locatus
Share of Other Retail Shops	Number of Other Retail shops divided by Total Number of shops within 500m from RMC sensor i	Locatus
Number of Historical Cinemas (in 1930)	Total number of historical cinemas (in 1930) within 500m from RMC sensor i	Locatus
Daily Rainfall	Daily amount of rainfall (in 0.1mm) recorded on date t	Dutch agency of meteorology (KNMI)
Mean Temperature	Average temperature (in Celsius) recorded on date t	Dutch agency of meteorology (KNMI)
Minimum Temperature	Minimum temperature (in Celsius) recorded on date t	Dutch agency of meteorology (KNMI)
Maximum Temperature	Maximum temperature (in Celsius) recorded on date t	Dutch agency of meteorology (KNMI)
Wind Speed	Average daily wind speed (in 0.1 m/s) recorded on date t	Dutch agency of meteorology (KNMI)
Sunshine	Number of Hours with Sunshine recorded on date t	Dutch agency of meteorology (KNMI)

Table A2 – Descriptive Statistics of shops within 500 m of RMC sensors

	Mean	Std. Dev	Median	Min	Max
Number of Shops	433.95	331.17	355.00	2.00	1622.00
Number of Shops (Log)	5.69	1.03	5.87	0.69	7.39
Share of Daily Shopping shops	0.11	0.06	0.09	0.00	0.35
Share of Clothing shops	0.20	0.09	0.23	0.00	0.39
Share of Food and Beverage (FNB) shops	0.26	0.11	0.24	0.01	1.00
Share of Other Retail shops	0.42	0.13	0.43	0.00	0.94

A.1.3 Summary statistics

Table A2 provides summary statistics for the entire sample of RMC sensors. There are, on average, around 434 shops within 500m from these RMC sensors. Around 20% of the shops sell clothing, 11% of the shops are for daily shopping (*e.g.* supermarkets, pharmacies), 26% of the shops are restaurants and bars etc., while the other 42% of shops refer to other retail types.

Table A3 further presents summary statistics associated with RMC sensors within the outdoor face mask regulated areas and unregulated areas. Shop profiles are quite different between regulated and unregulated face mask areas. There are a total of 23 RMC sensors within face mask regulated areas. Compared to areas outside, footfall levels are much higher within these regulated areas, suggesting that policy makers select populous streets for mandatory face mask wearing to prevent the spreading of the virus. These areas are typically closer to the city center, are more densely populated with shops and have a greater share of food and beverage shops compared to unregulated areas. We further constraint our analysis to areas 0 to 500m, 500 to 1000m and 1 to 5km from the regulation boundaries. Here, we observe that the daily footfall and shop profiles are more comparable for areas right outside (0-500m) but footfall is still considerably lower than those reported in regulated areas.

Finally, in Table A4, we present the mean levels of footfall and hospital admissions due to Covid-19 90 days before and after the first, second and partial lockdowns. Consistent with the main findings of this paper, we observe a stark decrease of around 46% in footfall after the first and second lockdowns are implemented. The relative effects on footfall are quite similar between the first and second lockdown, while the impact of the partial lockdown is negligible. It is also evident that the lockdowns are stop-gap measures strategically implemented to curb the sudden surge of cases. We observe a surge in the number of Covid-19 hospital admissions right after the lockdowns are enforced.

Table A3 – Descriptive Statistics for regulated and unregulated outdoor face mask zones

	Regulated	Unregulated	0-500m	500-1000m	1-5km
Daily Footfall	11282.44	3624.33	5773.15	4018.30	4010.46
	(75.77)	(9.93)	(43.23)	(32.33)	(42.54)
Daily Footfall (Log)	8.81	7.64	8.25	8.21	7.90
	(0.01)	(0.00)	(0.01)	(0.01)	(0.01)
Number of Shops	961.32	392.60	677.47	449.93	252.14
	(2.27)	(0.55)	(1.57)	(1.57)	(0.68)
Share of Daily Shopping shops	0.12	0.11	0.11	0.11	0.17
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
Share of Clothing shops	0.28	0.20	0.24	0.21	0.24
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
Share of Food and Beverage (FNB) shops	0.32	0.26	0.34	0.37	0.19
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
Share of Other Retail shops	0.28	0.43	0.31	0.31	0.40
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
Obs	25510	274824	16137	2205	10939
RMC sensors	23	312	16	2	10

Notes: Mean and standard error of means for regulated areas and non-regulated areas but within 0-500m, 500-1000m and 1-5km from the face mask regulation areas.

Table A4 – Descriptive Statistics before and after various lockdown events

	1st Lockdown		Partial		2nd Lockdown	
	Before	After	Before	After	Before	After
Daily Footfall	3230.09	1718.59	2448.00	2325.02	2234.21	1201.09
	(48.61)	(10.77)	(16.85)	(21.53)	(16.87)	(9.79)
Daily Footfall (Log)	7.58	7.07	7.33	7.12	7.11	6.57
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
COVID-19 hospital admissions (Daily)	0.22	2.11	1.59	4.11	3.96	3.17
	(0.01)	(0.03)	(0.02)	(0.05)	(0.04)	(0.03)
Obs	5229	20350	26443	19626	28228	16159

Notes: Mean and standard error of means for footfall and Covid-19 admissions 90 days before and after the 1st, Partial and 2nd lockdowns are enforced.

A.2 Distance to the city center and shopping streets characteristics

Here we explore the associative relationship between distance to the city center and 'structural' shopping street characteristics (shop type shares and shop density). We first determine the city center for all major cities. Then, for each RMC sensor, we calculate the distance to the nearest city center. While the average distance is 5 km, most RMC sensors are within 25 km from the city center. Table A5 reports the results of a regression of shopping street characteristics on distance (in kilometres), while including municipality fixed effects.

We find a strong association between distance to the city center and shop density. For a 1 km increase to the city center, shop density decreases by 22%. Furthermore, the share of daily stores is considerably higher away from city centers. For a kilometer increase in distance to the city

Table A5 – Distance to city center and shop counts and types

	(1) Log Shop counts	**(2) Share daily**	**(3) Share clothing**	**(4) Share FNB**
Distance to the city centre	-0.2262[a]	0.0107[a]	-0.0119[a]	-0.0043
	(0.0305)	(0.0027)	(0.0039)	(0.0056)
Municipality fixed effects	Yes	Yes	Yes	Yes
Number of observations	502	502	502	502
R^2	0.5223	0.3610	0.4183	0.3927

Notes: Robust standard errors are in parentheses. ^{a} $p < 0.01$, ^{b} $p < 0.05$, ^{c} $p < 0.10$.

center, the share of daily shops decreases by 1.1 percentage point (about 10%). By contrast, the share of clothing stores is substantially higher in city centers. A 1 km increase in distance to the center, decreases the share of clothing stores by 1.2 percentage points, so approximately 6%. For other shop shares we do not see a robust pattern.

Overall, in the Netherlands, the type of shops varies considerably with distance to the city center. Hence, as footfall affects the type of shops differently, Covid-19 policies also has a differential spatial impact within cities.

A.3 Additional graphical evidence

Figure A2 plots the natural logarithm of daily footfall 90 days before and after the face mask areas are designated for areas within the regulation boundaries, for areas outside, and for areas outside but within 0-500m from the regulation boundaries. Consistent with the regression results recorded in Table 2, we document a larger drop in footfall in areas within the regulated areas compared to areas outside in terms of unconditional footfall. We also observe that footfall 0-500m outside are more comparable to the footfall within face mask regulation areas, justifying our strategy of limiting the analysis to areas right outside the regulation boundaries to mitigate endogeneity concerns driven by unobserved differences between areas.

In Panel A and B in Figure A3 we plot the natural logarithm of daily footfall 90 days before and after various events and we control for a *fourth-order* polynomial (instead of a quadratic polynomial) on both sides of the threshold. Similar to Figure 2, we document a stark discontinuous drop in footfall after the 1st and 2nd lockdown is enforced. These results illustrate that the discontinuous drop in the footfall after the lockdowns are enforced is not sensitive to the specification of the time trend.

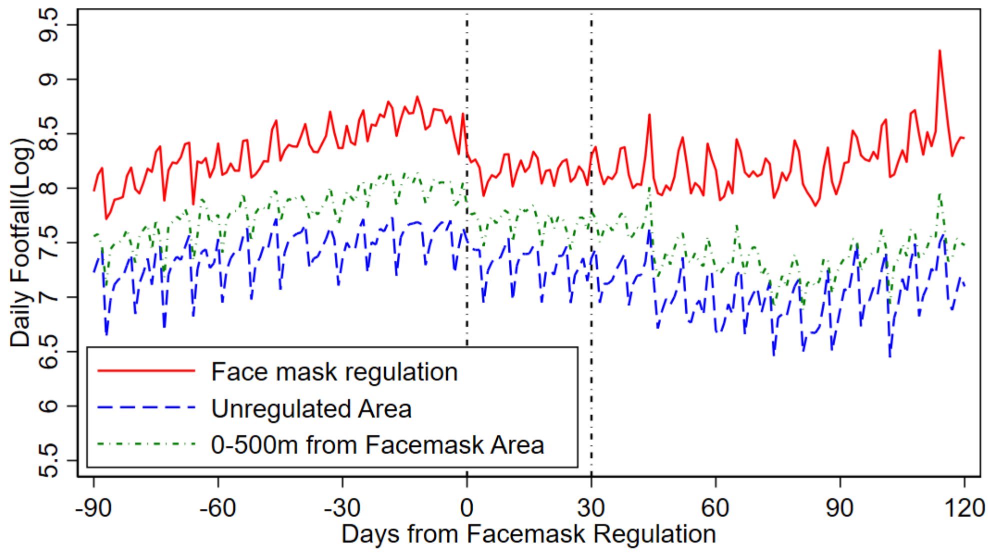


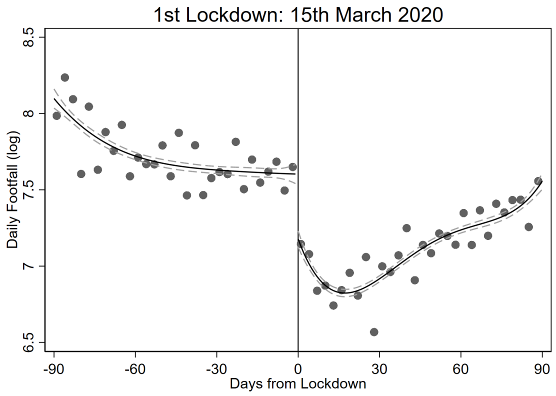
FIGURE A2 – OUTDOOR FACE MASK REGULATION AND FOOTFALL

Notes: We show the log of footfall 90 days before and after face mask regulation for areas inside, outside and within 0-500m from regulated areas.

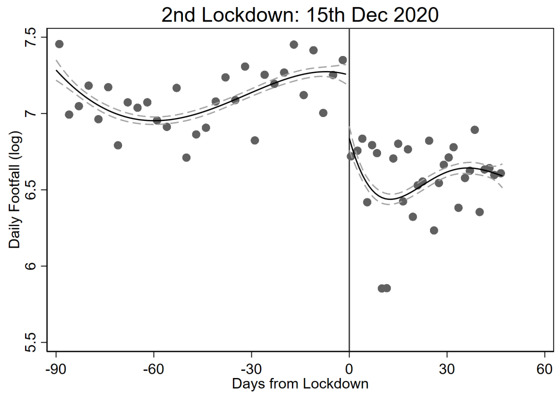
In Panel C in Figure A3 we investigate the effects of the partial lockdown. It is very clear that we do not observe any discontinuity in footfall around the implementation of the partial lockdown. We think this is not too surprising, as shops were still allowed to be open during the partial lockdown. Only part of the FNB sector (such as restaurants and pubs) was forced to cease operation while fast-food stores and 'on-the-go' food stores were still allowed to be open.

Panel D shows no jump in footfall after the 1st lockdown is relaxed. A plausible reason is that many shops remained closed right after the 1st lockdown. However, it is evident that footfall increases over time, as shops reopen with lockdown restrictions relaxed. Imperceptible changes in footfall are also recorded around the implementation of partial lockdown and face mask regulations (30th September and 1st of December). This could be due to the fact that these events are not so stringent in restricting the mobility of people.[16]

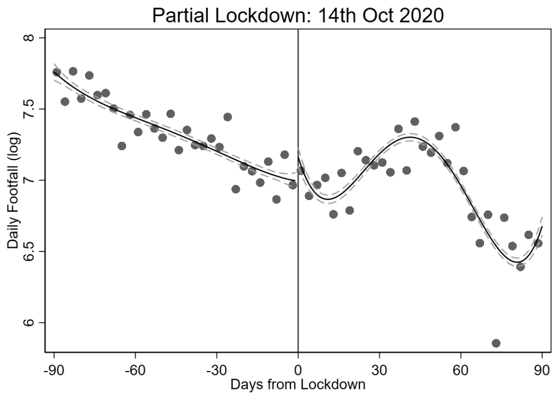
[16] We also estimate the impact of these events on footfall using difference-in-differences specifications. Similar to Figure A3, we do not record a discernible change in footfall around the partial lockdown, the removal of the 1st lockdown and the enforcement of face mask regulation on 30th September and 1st December 2020. We do not report these results due to space constraints but they are available upon request.



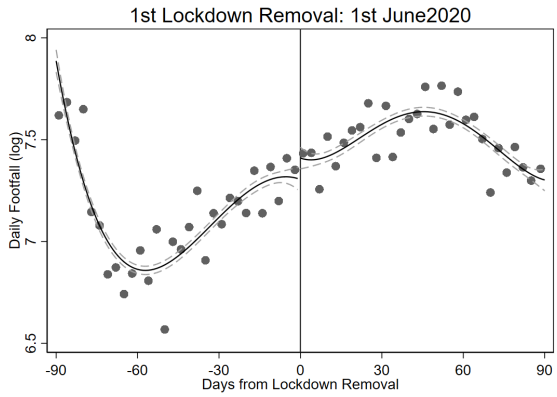
(A) 1ST LOCKDOWN



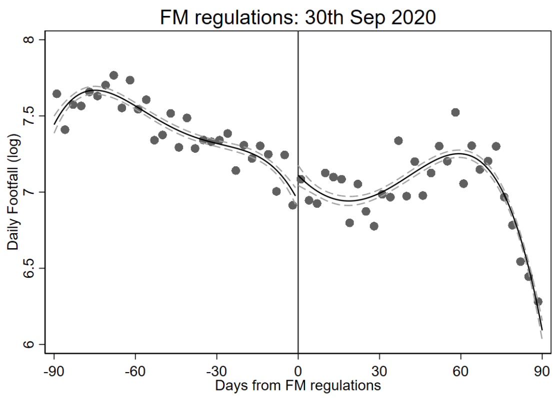
(B) 2ND LOCKDOWN



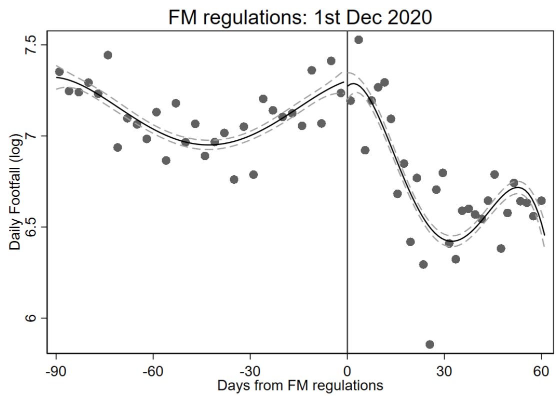
(C) PARTIAL LOCKDOWN



(D) 1ST LOCKDOWN REMOVAL



(E) FACEMASK REGULATION (30TH SEP)



(F) FACEMASK REGULATION (1ST DEC)

FIGURE A3 – LOCKDOWNS, FACEMASK REGULATIONS AND FOOTFALL

Notes: We show the log daily footfall 90 days before and after various events. We employ an Epanechnikov kernel and control for a fourth-order polynomial on both sides of the threshold. Dashed line denotes 95% confidence interval associated with fourth-order polynomial footfall trends.

Table A6 – Effects of lockdowns on footfall (alternate specifications)

	(1)	(2)	(3)	(4)	(5)	(6)
Polynomials/Event Window	**First**	**Second**	**Third**	**Fourth**	**60days**	**30days**
Lockdowns	-0.653^{a}	-0.635^{a}	-0.580^{a}	-0.646^{a}	-0.640^{a}	-0.647^{a}
	(0.081)	(0.078)	(0.085)	(0.106)	(0.077)	(0.102)
Obs	91822	91822	91822	91822	66773	35981
Adj R2	0.78	0.79	0.79	0.79	0.78	0.78

Notes: The dependent variable is the natural log of visitor footfall at RMC sensor i on day t. Reported variables denote binary variables that take the value of 1 (Lockdowns) for RMC sensors after the 1st Lockdown (15th of March 2020) and 2nd Lockdown (15th of Dec 2020). All regressions include RMC fixed effects and time trends (days to event) at various orders of polynomial as denoted by column headers. In (1)-(4), we estimate the effects of various events using single (linear) to fourth order polynomials of time trends. In (5) and (6), we further restrict the analysis to 60 and 30 days to a lockdown after controlling for second order polynomials of time trends (quadratic). Two-way clustered standard at postcode and date levels are reported in the parentheses. p<0.10, ^{b} p<0.05, ^{a} p<0.01.

A.4 Additional regression results

Table A6 presents baseline results of lockdowns on footfall from alternate specifications related to the Regression Discontinuity Design (RDD). In columns (1) to (4), we include first to fourth order polynomials of time trends before trimming down the sample to observations 60 and 30 days from the lockdowns respectively in columns (5) and (6). The rationale is to test whether baseline estimates in Table 1 holds under alternative specifications. It is comforting to observe that the estimates remain fairly robust in size and statistical significance across specifications and are comparable to main findings we report in Table 1, suggesting that our main findings are robust to other specifications and time windows.

Panels A and B of Table A7 present the stratified effects of the first and second lockdown on footfall separately. These results inform us on whether the effects associated with the 1st and 2nd lockdown on footfall are different from one another. As observed, we report a substantial drop in footfall after the 1st and 2nd lockdown are enforced and the estimated effects are quite comparable in size between the two events, and also similar to those reported in Table 1. Additional regressions suggest that density of shops matter as the effects of lockdowns are much greater along streets with a higher shop concentration. These results remain robust even we instrument shop counts with historical counts of cinema within 500m. These findings are again quite similar to those reported in Table 1.

Panels A and B of Table A8 repeat our analysis in Table 1, but we now examine the impact of shop counts and types for shops within 200m from the RMC sensors. The concern is whether computing shop counts and types within 500m is too broad. As observed, our reported estimates are quite similar to our initial findings reported in Table A8 in terms of size and direction. In

Table A7 – Effects of 1st and 2nd lockdowns on footfall

	Panel A: 1st Lockdown				
	(1)	**(2)**	**(3)**	**(4)**	**(5)**
	Baseline	**Shop Count**	**Shop Type**	**Combined**	**IV**
1st Lockdown	-0.728[a]	-0.714[a]	-0.730[a]	-0.725[a]	-0.722[a]
	(0.145)	(0.140)	(0.143)	(0.141)	(0.141)
1st Lockdown × Log Shop Counts		-0.248[a]		-0.226[a]	-0.379[a]
		(0.038)		(0.055)	(0.131)
1st Lockdown × Share of Other Retail Shops			-0.400	0.597	1.274[c]
			(0.359)	(0.452)	(0.737)
1st Lockdown × Share of Clothing Shops			-2.773[a]	0.102	2.056
			(0.463)	(0.778)	(1.743)
1st Lockdown × Share of FNB shops			-1.399[b]	-0.411	0.261
			(0.641)	(0.518)	(0.737)
Obs	49620	49258	49258	49258	49258
Adj R2	0.81	0.82	0.82	0.82	0.41
Kleibergen-Paap F statistic					15.28
	Panel B: 2nd Lockdown				
	(6)	**(7)**	**(8)**	**(9)**	**(10)**
	Baseline	**Shop Count**	**Shop Type**	**Combined**	**IV**
2nd Lockdown	-0.708[a]	-0.699[a]	-0.692[a]	-0.688[a]	-0.673[a]
	(0.122)	(0.122)	(0.123)	(0.122)	(0.122)
2nd Lockdown × Log Shop Counts		-0.138[a]		-0.074	-0.356[a]
		(0.036)		(0.053)	(0.116)
2nd Lockdown × Share of Other Retail Shops			-3.132[a]	-2.787[a]	-1.466[c]
			(0.496)	(0.565)	(0.822)
2nd Lockdown × Share of Clothing Shops			-3.981[a]	-3.032[a]	0.602
			(0.567)	(0.836)	(1.605)
2nd Lockdown × Share of FNB shops			-2.375[a]	-2.019[a]	-0.656
			(0.532)	(0.534)	(1.006)
Obs	42202	41515	41515	41515	41515
Adj R2	0.80	0.79	0.79	0.79	0.25
Kleibergen-Paap F statistic					14.84

Notes: The dependent variable is the natural log of visitor footfall at `RMC` sensor i on day t. Reported variables denote binary variables that take the value of 1 (1st Lockdown) for `RMC` sensors after the 1st Lockdown (15th of March 2020) for Panel A, and denote binary variables that take the value of 1 (2nd Lockdown) for `RMC` sensors after the 2nd Lockdown (15th Dec 2020) for Panel B. All regressions control for public and school holidays, weather conditions, `RMC` fixed effects and time trends (days to event) at second order polynomial of time trends (quadratic). We further restrict the analysis to a window 90 days from the event. In columns 5 and 10, we repeat the analysis in columns 4 and 9 (for all Panels) but we instrument log shop counts with the counts of historical cinemas in 1930. Two-way clustered standard at postcode and date levels are reported in the parentheses. [c] p<0.10, [b] p<0.05, [a] p<0.01.

Panel C, we repeat the analysis of Panel A in Table A8 but we further control for the daily number of hospital admissions due to Covid-19. We exclude this variable from our main analysis due to the concern that this variable could be a 'bad control', but we observe that the inclusion does not matter much to our estimates. If anything, the estimated effects of lockdowns on footfall are slightly smaller but they remain statistically significant. This is expected given that we do not expect much changes in the number of Covid-19 hospital admissions around the enforcement of the lockdowns.

A.5 Footfall and retail rents

In this subsection, we replicate the findings by Koster et al. (2019), who uses an alternative (and arguably, inferior) measure of footfall, to measure the effect of footfall on retail rents. We use two data sources (`Strabo`; `Vastgoeddata`) for the period 2010-2020 with information on

Table A8 – Effects of lockdowns and social distancing on footfall (Robustness Tests)

	Panel A: Lockdowns(RDD) (200m)				
	(1)	**(2)**	**(3)**	**(4)**	**(5)**
	Baseline	**Shop Count**	**Shop Type**	**Combined(OLS)**	**Combined (IV)**
Lockdowns	-0.635[a]	-0.618[a]	-0.628[a]	-0.623[a]	-0.609[a]
	(0.078)	(0.073)	(0.076)	(0.073)	(0.076)
Lockdowns × Log Shop Counts		-0.179[a]		-0.121[a]	-0.411
		(0.032)		(0.045)	(0.372)
Lockdowns × Share of Other Retail Shops			-1.173[a]	-0.973[a]	-0.495
			(0.203)	(0.237)	(0.656)
Lockdowns × Share of Clothing Shops			-2.314[a]	-1.479[a]	0.522
			(0.259)	(0.370)	(2.628)
Lockdowns × Share of FNB shops			-1.241[a]	-1.098[a]	-0.757
			(0.253)	(0.231)	(0.566)
Obs	91822	90456	90456	90456	90456
Adj R2	0.79	0.78	0.78	0.78	0.35
Kleibergen-Paap F statistic					3.98
	Panel B: Social Distancing(DID) (200m)				
	(6)	**(7)**	**(8)**	**(9)**	**(10)**
	Baseline	**Shop Count**	**Shop Type**	**Combined(OLS)**	**Combined (IV)**
Social Distancing	-0.626[a]	-0.621[a]	-0.631[a]	-0.627[a]	-0.629[a]
	(0.034)	(0.034)	(0.038)	(0.034)	(0.038)
Social Distancing × Log Shop Counts		-0.091[b]		-0.149[b]	-0.071
		(0.039)		(0.061)	(0.188)
Social Distancing × Share of Other Retail Shops			-0.186	0.066	-0.066
			(0.244)	(0.321)	(0.367)
Social Distancing × Share of Clothing Shops			-0.484[b]	0.547	0.007
			(0.189)	(0.448)	(1.342)
Social Distancing × Share of FNB shops			-0.361	-0.192	-0.281
			(0.375)	(0.344)	(0.459)
Obs	234152	232689	232689	232689	232689
Adj R2	0.85	0.85	0.85	0.85	0.21
Kleibergen-Paap F statistic					3.77
	Panel C: Lockdowns (RDD) (Control for Covid-19 Hospital Admissions)				
	(11)	**(12)**	**(13)**	**(14)**	**(15)**
	Baseline	**Shop Count**	**Shop Type**	**Combined(OLS)**	**Combined (IV)**
Lockdowns	-0.620[a]	-0.607[a]	-0.613[a]	-0.607[a]	-0.595[a]
	(0.081)	(0.076)	(0.079)	(0.076)	(0.076)
Lockdowns × Log Shop Counts		-0.155[a]		-0.114[b]	-0.362[a]
		(0.029)		(0.047)	(0.111)
Lockdowns × Share of Other Retail Shops			-1.406[a]	-0.881[c]	0.268
			(0.374)	(0.467)	(0.757)
Lockdowns × Share of Clothing Shops			-2.746[a]	-1.287[c]	1.904
			(0.413)	(0.698)	(1.516)
Lockdowns × Share of FNB shops			-1.397[a]	-0.864[b]	0.300
			(0.450)	(0.411)	(0.786)
Obs	72095	71192	71192	71192	71192
Adj R2	0.78	0.77	0.77	0.77	0.24
Kleibergen-Paap F statistic					14.91

Notes: The dependent variable is the natural log of visitor footfall at RMC sensor i on day t. Reported variables denote binary variables that take the value of 1 (Lockdowns) for RMC sensors after the 1st Lockdown (15th of March 2020) and 2nd Lockdown (15th of Dec 2020) for Panel A. RDD regressions from columns (1) to (5) in Panel A include controls for public and school holidays, weather conditions, RMC fixed effects and time trends (days to event) at second order polynomial of time trends (quadratic). We further restrict the analysis to a window 90 days from the event. Reported variables denote binary variables that take the value of 1 (Social Distancing) for RMC sensors after the social distancing is enforced (from 1st June 2020 to 13th October 2020) for Panel B. DID regressions from columns (6) to (10) in Panel B include RMC fixed effects, day-of-week fixed effects (Monday to Sunday) and week fixed effects (1-52 weeks) and year fixed effects. Baseline effects of lockdowns and social distancing are reported in columns (1) and (5) respectively. In columns (2) and (7), we further interact these binary variables with the demean natural logarithm of shop counts within 200m from the RMC. In columns (3) and (8), we interact binary event variables with demean share of shops (whether Daily shopping, Clothing, Food and Beverages(FNB) or other retail) within 200m from the RMC. In columns (4) and (9), we collectively estimate how shop counts and shop types can affect the impacts of lockdowns and social distancing on footfall. In columns (5) and (10), we repeat the analysis in columns (4) and (9) but we instrument log shop counts with the counts of historical cinemas in 1930. RDD regressions from columns (11) to (15) in Panel C is similar to Panel A of Table 1 but we further control for daily Covid-19 hospitalizations. Two-way clustered standard at postcode and date levels are reported in the parentheses. [c] p<0.10, [b] p<0.05, [a] p<0.01.

commercial retail rents, size and construction year of 966 retail establishments close to RMC points (within 100m). We also extend the RMC data back to 2010.

Let p_{ijt} be the rent paid by retail firm i in shopping district j in year t which is a function of footfall (f_{ijt}). Furthermore, let x_{ijt} be other shop and location characteristics (*e.g.* shop size, construction year, etc.). The basic equation to be estimated yields:

$$\log p_{ijt} = \alpha \log f_{ijt} + \beta x_{ijt} + \eta_j + \theta_t + \epsilon_{ijt}, \tag{A.1}$$

where α and β are parameters to be estimated, η_j are district fixed effects, θ_t are year fixed effects and ϵ_{ijt} is a random error term. Because our dataset is not very large, we cannot include the detailed shopping street fixed effects as in Koster et al. (2019), but rely on slightly more aggregate district fixed effects.

We are worried that footfall is correlated to unobserved locational characteristics so we instrument footfall with the number of cinemas in 1930 <200m. Historically, most cinemas were small with one screen only, and were located in shopping streets. Hence, the buildings hosting these cinemas were not very different from the surrounding buildings. One may be concerned that cinemas themselves create footfall, while this does not necessarily imply shopping externalities (as people may only visit the cinema and not visit other shops). We therefore control for the number of cinemas in the vicinity in 2010. The buildings of the closed cinemas from the 1930s are now frequently used as shops, but also attract other businesses.

The main identifying assumption when relying on long-lagged instruments is that past unobservable characteristics of either stores or locations are uncorrelated to current unobservables. Conditional on the controls and district fixed effects, we think this assumption is tenable (but for a longer discussion and a host of robustness checks, we refer to Koster et al. 2019).

We report the results in Table A9. In column (1) we estimate a somewhat naive specification where we only control for property controls and year fixed effects. The elasticity is 0.312, implying that doubling footfall leads to rents that are 22% higher. The effect is somewhat smaller when we control for location characteristics and district fixed effects.

In the next two columns of Table A9 we instrument for footfall with the number of cinemas in

Table A9 – Results for the effect of footfall on retail rents

	(1) OLS	(2) OLS	(3) 2SLS	(4) 2SLS	(5) 2SLS	(6) 2SLS
Log of Footfall	0.3124[a]	0.2196[a]	**0.6030**	**0.6291**[c]	**0.5145**[a]	**0.4864**[b]
	(0.0300)	(0.0414)	(**0.3844**)	(**0.3701**)	(**0.1144**)	(**0.1902**)
Cinemas in 2010 <200m				-0.0088	-0.0498	-0.0124
				(0.0449)	(0.0422)	(0.0394)
Property controls	Yes	Yes	Yes	Yes	Yes	Yes
Location controls	No	Yes	Yes	Yes	Yes	Yes
District fixed effects	No	Yes	Yes	Yes	No	No
Municipality fixed effects	No	No	No	No	No	Yes
Year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Number of observations	983	966	966	966	983	975
R^2	0.3641	0.5339				
Kleibergen-Paap F-statistic			1.931	3.135	36.97	8.211

Notes: The dependent variable is the log of rent per m^2. **Bold** indicates instrumented. In columns (3) and (4) we instrument for footfall with the number of cinemas in 1930. Property controls include the log of size of the property and 10 construction year decade dummies. Location controls include the number of busstops <200m, as well as the number of listed buildings <200m. Robust standard errors are clustered at the RCM scanner level and in parentheses. [a] $p < 0.01$, [b] $p < 0.05$, [c] $p < 0.10$.

1930 within 200m of the property. The elasticity increases considerably to about 0.60. Because of a weak instrument problem (*i.e.* the first stage F-statistic is substantially below the required rule-of thumb-of 10), and the estimate is not significant at conventional significance levels.

To address the latter issue, we remove the district fixed effects in column (5). This implies that the instrument is now considerably stronger. However, the point estimate is hardly affected. In column (6) we include municipality fixed effects rather than district fixed effects. As in Koster et al. (2019) the elasticity is now 0.50, implying that doubling footfall leads to rents that are 35% higher. Note that the Hausman T-tests of equality between the corresponding OLS and the latter IV estimates are 1.8 and 1.5 for columns (5) and (6) respectively, suggesting that the IV estimates are preferred. However, note that the Hausman T-tests are only statistically significant at the 10% and 15%, respectively.