

How did consumers react to the COVID-19 pandemic over time?

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Abstract

Non-pharmaceutical interventions (NPIs) have been the key policy instrument utilized to contain the impact of the COVID-19 pandemic. This paper disentangles the effects of NPIs from that of the virus and looks at the specific channels through which the virus impacts consumption. Using geo-located transaction data, we find that consumers' behaviour towards the virus has explanatory power for the drop in consumption in the early stages of the pandemic. This effect disappears in the later stages of the pandemic, suggesting that consumers have adapted their behaviour. As the COVID-19 pandemic progressed, consumers tended to make 'safer' consumption decisions, by avoiding crowded places.

I. Introduction

The novel coronavirus outbreak, along with lockdown measures, that is, non-pharmaceutical interventions (NPIs) intended to contain the spread of COVID-19, resulted in significant and unprecedented changes in society and the economy. NPIs have been for most countries the key policy instrument utilized to contain the impact of the COVID-19 pandemic. The NPIs involved closing (sub)sectors or placing supply restrictions on others, reducing the opportunity for consumers to spend. To understand the potential negative economic impact of COVID-19, it is important to understand the economic transmission channels through which the shocks will adversely affect the economy.

Studies on the effect of COVID-19 on consumption have looked specifically at the effects of NPIs on domestic consumption Chetty *et al.* (2020); Bounie *et al.* (2020); Neuteboom *et al.* (2020); Coibion *et al.* (2020) and Chen, Qian and Wen (n.d.). They find that during the first stages of the COVID-19 pandemic, consumption has been hit directly by both the NPIs and the behavioural response function of consumers towards the virus, that is, voluntary stay at-home-policies by economic agents. According to some, in the absence of NPIs, self-imposed stay at-home-policies would have been at least

JEL Classification numbers: C33, C61, D12, D14, E21.

as damaging to the economy (see Lin and Meissner, 2020 and Igan *et al.*, 2020). This evidence suggests that NPIs are only part of the story, and the behavioural reaction of consumers towards the virus is perhaps even more important to understand the effects of the COVID-19 pandemic on the economy.

In this study, we look more closely at the effect of NPIs and the behavioural response of consumers on consumption, by using bank transaction data. We use a novel approach to geo-locate physical pin transactions and compare consumption patterns in different Dutch municipalities with the local COVID-19 outbreak. The Dutch government's strategy of imposing nationwide NPIs measures presents us with a unique empirical estimation strategy. Both the incidence of the illness and its timing varied substantially across different municipalities, while the NPIs induced homogeneous expenditure dynamics across all municipalities in the Netherlands. We look specifically at the spatial heterogeneity of COVID-19, as the virus has had different effects in different municipalities across the country. While similar empirical studies have been done in other countries, for instance, in China and France (Chen *et al.*, n.d. and Bounie *et al.*, 2020), the Netherlands was the only country where NPI policy that was consistent among regions in terms of strictness and imposition date. Therefore, we do not have to manually control for differences between regions within the country.

This paper makes two main contributions. First, we show, in line with previous studies, that changes in behaviour by consumers towards virus explain a large part of the drop in consumption during the early stages of the pandemic. An explanation put forward to describe this effect is that, when the pandemic eases/surges, people are less willing/more willing to comply with the imposed NPIs and therefore consumption increases/decreases (Coibion *et al.*, 2020). We do not find any evidence for this explanation, as our results show that COVID-19 does not correlate with spending in sectors that were subject to NPIs. Secondly, while a variety of studies have examined the effects of the first lockdown on economic behaviour and evaluating the economic damage, there are no detailed studies that look at the effects over time. In this study, we incorporate the period from the start of the pandemic until the vaccine roll-out. We find no effect of COVID-19 on aggregate consumption in the second COVID-19 wave, which suggests that consumers have changed their behaviour as the pandemic progressed. We do however find an effect of COVID-19 on the substitution of offline spending towards online spending, from which we conclude that consumers continued their consumption patterns but perhaps in a more 'safer' way. This finding taps into the literature of how consumer update their beliefs and behaviour on COVID-19 (Akeson *et al.*, 2020). As the COVID-19 pandemic progressed, more information became available on the infectiousness and on which (social) activities may increase the chances of attracting the virus. Our results suggest that perhaps consumers have adapted their consumer behaviours, avoiding physical consumption in certain sectors that have a high chance of transmissibility of the virus, but continue to shop in sectors that are relatively safe.

This paper is structured as follows: In section II, we review the most relevant literature on the economic impact of the pandemic and the associated fall in consumption. In section III, we describe the geolocation model and our methodology for estimating the differences in consumption between municipalities. Section IV discusses the results of the regressions. In section V, we show that the results are robust to changing the time

frequency and cross-sectional dependence. We also explore the time lags of the dependent variable. Finally, in section VI, we discuss our findings in light of existing research and outline avenues for future work.

II. Literature review

Our work builds on and contributes to a rapidly evolving literature on measuring the economic impacts of COVID-19¹.

In general, the literature on the economic impact of COVID-19 can be broadly divided among four different main transmission channels through which a pandemic can cause economic disruptions (see: Carlsson-Szlezak and Philipp, 2020a,b; Guerrieri *et al.*, 2020): (i) direct impact through economic agents falling ill; (ii) direct impact due to an increase in uncertainty; (iii) direct impact through restrictions by the government to contain the spread of the virus (NPIs); and (iv) the indirect impact through worsening economic conditions. Below we will discuss the four different channels in more detail.

The direct impact of infections is caused by workers who become infected by the virus and do not show up at work, causing supply-side disruptions for producers (Guerrieri *et al.*, 2020). On top of that, consumers who have contracted the virus are not eligible to leave their houses and consume, causing demand-side disruptions. This channel is, for example, taken into account by the canonical epidemiology macro model by Eichenbaum, Rebelo and Trabandt (2020). In their model, the prevalence of infection depends on the degree of interaction between agents when consuming and working, as well as the random chance of contracting the virus. Therefore, the susceptible population can lower the chances of infection by reducing their spending activities and their labour supply (outside of their houses).

The second channel through which the virus cause economic disruption is uncertainty (Dardanoni, 1991; Mengel, Tsakas and Vostroknutov, 2016). A pandemic is a negative unusual and unexpected event that disrupts the status quo; therefore, economic agents may become more uncertain about their future earnings and hence more risk-averse. There is a large stand of literature on uncertainty due to sudden unexpected negative events. In summary, the literature finds that uncertainty is damaging for short-run growth, reducing output, investment, hiring, consumption and trade (Bloom, 2014). Uncertainty impacts the economy in several ways: (i) real-options effects, which act to make firms more cautious about hiring and investing, and consumers more cautious about buying durable goods (Bernanke, 1983), (ii) risk-premium effects raise the cost of finance (Arellano, Bai and Kehoe, 2019) and (iii) from consumers' desire for precautionary saving, which itself reduces consumption expenditure (Bansal and Yaron (2000).

According to Baker *et al.* (2020), COVID-19 has led to massive spikes in uncertainty, and there are no close historical parallels. They show that volatility is affected by specific economic indicators and is sensitive to COVID-19 news. Both negative and positive

¹For a good literature review see Brodeur *et al.* (2020), and for an overview on the economic effect of COVID-19, see our previous work on this topic (Neuteboom *et al.*, 2020).

COVID-19 information is significant, though according to their study negative news has more impact than positive news. Based on a similar approach, Altig *et al.* (2020) conduct an analysis of different forward-looking uncertainty measures during the pandemic and find that uncertainty on what is coming is an important explanatory variable for consumption decisions. Baldwin (2020) argues that the ‘wait-and-see’ attitude by economic agents is common during economic downturns characterized by uncertainties, as there is less confidence in markets. Therefore consumers are less willing to engage in economic transactions. He concludes that the intensity of the shock is determined by the underlying epidemiological properties of the virus, behaviour, and firm decisions in the face of risk-aversion and uncertainty. In this light, also the public response is crucial in influencing uncertainty. Related to this is the economic literature concerning fear and anxiety (Farrés *et al.*, 2021 and Bounie *et al.*, 2020). This may cause supply-side disruptions as workers do not show up at work, afraid to contract the virus in the workplace. From a demand-side perspective, this may cause consumers to impose voluntary stay-at-home policies (Chetty *et al.*, 2020). Yan *et al.* (2021) disentangle voluntary from policy-induced behavioural changes related to stay-at-home time and find that a substantial share of the observed behavioural response was voluntary. Stay-at-home orders increased the time people spent at home by replacing voluntary actions that likely would have emerged as cases rose.

Within the strand of literature that looks at uncertainty of economic agents, the effects of a sudden event – such as a pandemic – can shape an agent’s belief about the future and may cause agents to change their economic behaviour. That is, underlying psychological mechanisms shape economic anxiety in the environment of a pandemic by assessing the role of beliefs and information about pandemic risk factor of infectious disease spread (Akeson *et al.*, 2020). Gallagher (2014) finds that agents have difficulty forming beliefs about the future in the wake of infrequent major events. Therefore, belief formation may differ substantially during the COVID pandemic as compared with an economic shock.

Akeson *et al.* (2020) report a rapid increase in economic anxiety during and after the coronavirus has reached a country. Fetzer *et al.* (2021) document that the average participant overestimates both the mortality and contagiousness of the virus relative to the percentages given by the medical literature. They show that beliefs about mortality and contagiousness are associated with participants’ economic worries about the aggregate economy and their personal economic situation. This is in line with the findings of Akeson *et al.* (2020), who report that economic agents dramatically overestimate the danger and infectiousness of COVID-19 relative to expert opinion. These findings on the COVID-19 pandemic are consistent with previous studies on risk perception, which suggest that economic agents are likely to overestimate risks that are unfamiliar, outside of their control, inspire feelings of dread, and receive extensive media coverage (see for an overview on risk perception: Slovic (2012)). Interestingly, Akeson *et al.* (2020) also find that providing people with expert information partially corrects their beliefs about the virus.

Several studies on how economic agents shape their beliefs and behaviour according to new information find that economic agents are well-capable of updating their beliefs as new information comes in (see: Armona, Fuster and Zafar, 2019; Coibion

et al., 2020). For instance, Roth and Wohlfart (2020) find that economic agents update their macroeconomic outlook in response to the expert forecasts extrapolate to expectations about their personal economic circumstances, and adjust their consumption plans and stock purchases².

The third transmission channel contains the economic damage due to the imposition of restrictions to prevent the spread of the virus. A large body of literature has been focusing on the impact of NPIs. These NPIs, which include, among others, international travel restrictions, business closures, prohibition of large-scale private and public gatherings, and mandatory quarantines, have been adopted so as to effectively minimize or reduce the rate at which the virus is transmitted.

Mulligan (2020) assesses the lockdown by extrapolating the welfare loss stemming from non-working days, the fall in the labour-capital ratio resulting from the absence/lay-off of workers, and the resulting idle capacity of workplaces. The author finds the welfare loss to be approximately USD 7 trillion per year of shutdown. In a similar fashion, Demirgüç-Kunt, Lokshin and Torre (2021) estimate the economic impact of the NPIs implemented by countries in Europe and Central Asia during the initial stages of the COVID-19 pandemic. The results suggest that the NPIs led to a decline of about 10% in economic activity across the region. On average, countries that implemented non-pharmaceutical interventions in the early stages of the pandemic appear to have better short-term economic outcomes and lower cumulative mortality, compared with countries that imposed NPIs during the later stages of the pandemic.

Various papers use high-frequency transaction data, analogous to the data we assemble here, to analyse aggregate consumer spending under NPIs. There are a number of empirical studies that have attempted to disentangle the direct effects of NPIs and the behavioural response towards the virus on consumption. In general, there is consensus in the literature that changes in behaviour by consumers towards virus explain a large part of the drop in consumption during the early stages of the pandemic. While these studies typically find that NPIs have had a short-term effect on consumption, the NPIs only account for a small proportion of the observed declines (see: Chetty *et al.*, 2020; Bounie *et al.*, 2020; Neuteboom *et al.*, 2020; and Chen *et al.*, n.d.). This claim is also supported by a quantitative model developed in a theoretical paper of Aum, Lee and Shin (2020). Coibion *et al.* (2020) use surveys to assess the macroeconomic expectations of households in the United States. They find that it is primarily lockdowns, rather than the infections themselves, that lead to declines in consumption spending.

Some authors argue that the direct effect of the virus and the NPIs interplay. As the virus surges, people are more willing to comply with the NPIs than when the virus spread slows. According to Makris and Toxvaerd (2020), people tend to adopt social distancing practices when there is a specific incentive to do so in terms of risk to health and financial cost. Maloney and Taskin (2020) attribute voluntary, cooperative actions to either fear of infection or to a sense of social responsibility. There is a broad range of literature looking at different socio-economic determinants

²Some authors combine the supply-side disruptions from both ill-fallen workers as well as workers who are afraid of showing up at work because of the spread of the virus, see: Bonadio *et al.* (2020); Céspedes, Chang and Velasco (2020) and Chudik, Pesaran and Rebucci (2020)

of the degree of compliance with social distancing. Factors that influence compliance with NPIs include income level, trust and social capital, public discourse, news channel viewership, ethnic diversity, and even gender (see: Galasso *et al.*, 2020; Chiou and Tucker, 2020).

Individual beliefs and preferences should also be taken into consideration, as they affect behaviour and hence compliance with NPIs. Akesson *et al.* (2020) find that the more infectious COVID-19 was deemed to be, the less likely individuals were to undertake social distancing measures. This was perhaps due to the belief that the individual will contract COVID-19 regardless of social distancing practices, that is, a certain amount of fatigue started to kick in. Brodeur, Grigoryeva and Kattan (2021) document the determinants of implementation and compliance with stay-at-home orders in the United States, focusing on trust and social capital. They find that counties in the United States exhibiting relatively more trust decrease their mobility significantly once NPI is implemented. Interestingly, they also show that the estimated effect on postlockdown compliance is especially large if people tend to place trust in the media, and relatively smaller if they tend to trust in science, medicine, or government.

The fourth and last channel that we can distinguish from the literature is the indirect impact, or second-round effect, of worsening economic conditions. In this respect, it is important to understand the inter-linkages between macroeconomic indicators and how they interact on a medium-term horizon.

To understand the longer term implications and second-round effects of the pandemic, a number of researchers have integrated canonical epidemiology models such as the susceptible, infected, resolved model (SIR) with macroeconomic models (see: Moser and Yared, 2020; Alvarez, Argente and Lippi, 2020; and Bognanni *et al.*, 2020). As the crisis progresses and uncertainty remains high, the drop of consumption and investments lowers employment and productivity growth which has a negative impact on consumption (Romer, 1986; Barro, 1990; Diamond, 1982).

An important channel through which these second-round effects take place is unemployment (Pissarides, 1987; Schubert and Turnovsky, 2018). As the macroeconomic environment worsens, businesses go bankrupt, workers get unemployed, which lowers their average income and decreases their spending capacity. A large number of studies document the effects on the variables of hours of work and job losses (e.g. Forsythe *et al.*, 2020). Notably, the authors find that the labour market declines (proxied through reductions in job vacancies and increases in UI claims) were uniform across states, with no notable differences across states that experienced the spread of the pandemic, or implemented stay-at-home orders, earlier than others.

Codagnone *et al.* (2020) focus on the expectations of stakeholders with regards to the postlockdown period. As outcome variables, they use expectations (e.g. economic outlook, labour market situation, recovery), fear (e.g. scenario of new outburst, economic depression, restriction to individual rights and freedom), and behavioural change across the following dimensions: savings, cultural consumption, social capital, and risky behaviour. The authors find that exposure to the COVID-19 shock and the ensuing lockdown led to pessimistic expectations about job opportunities, greater drawdowns of savings than before, and a deterioration in social relations, which might be instrumental in finding job opportunities in the longer term.

III. Data and methods

The Dutch government's strategy of imposing nationwide NPIs measures presents us with a unique empirical estimation strategy, as both the incidence of the illness and its timing varied substantially across different municipalities. While the NPIs induced homogeneous expenditure dynamics across all municipalities in the Netherlands, spatial heterogeneity remains because the COVID-19 virus may have had different effects in different municipalities across the country. While similar empirical studies have been done in other countries, for instance, in China and France (Chen *et al.*, n.d.; Bounie *et al.*, 2020), the Netherlands was one of the only countries that saw an NPI policy that was consistent among regions in terms of strictness and imposition date. Therefore, contrary to other studies so far, we do not have to manually control for these differences between regions within the country.

Transaction data

We use real-time transaction data from ABN AMRO cardholders. ABN AMRO is the third biggest bank in the Netherlands and has approximately 18% of the total market share in the country³. As a consumer bank, ABN AMRO has around 3.1 million unique account holders. This covers around 22% of the total adult (18+) population. ABN AMRO is a broad retail bank present in all parts of the country and catering to all types of customers. Our data are therefore largely representative of the adult population of the Netherlands in terms of gender, age, and income. Moreover, compared with the population statistics data provided by the statistical agency (CBS), our data are equally distributed across Dutch provinces (see Appendix Figure A1).

Collectively, ABN AMRO account holders spend over 65 million euros on a daily basis, with an average transaction size of 23 euros. On average, over the sample period, our dataset comprises 2,745,651 physical pin transactions a day and around 344,753 online transactions a day. From the ABN AMRO transactions data, we acquire point of sale (PoS) data from pin terminals when costumers pay by card, ATM data from cash machines when costumers draw cash, and online transactions by e-banking iDEAL transactions. In 2019, on average, clients paid 57% by card (PoS), 24% online by iDEAL, and 19% by cash. The PoS data include a timestamp, the amount in euros, the corresponding account number, the counterparty description, and the counterparty account number. We find the geolocation of every PoS transaction by using a geolocation model (see section III and Appendix B). As explained in the section below, we use a transaction-weighted dynamic-parameter DBSCAN (TWDP-DBSCAN) to determine the geolocation and find that, based on a ground-truth dataset that comprises 65 million PoS transactions including geolocation, the model correctly predicts around 95% of the geolocations. We have used a labelling function based on keywords in the transaction description to identify the category the transaction belongs to.

Our transaction data only incorporate accounts held by individuals and households. We exclude corporate accounts (SMEs) by excluding transactions backed by a debit card

³See: <https://www.bankinbeeld.nl/thema/betalingsverkeer/>).

issued to a corporation as a ‘company card’. The purpose of this is to ensure that we make a correct analysis of domestic consumption that is not biased by corporate expenditure.⁴

Geolocation model and research design

In order to investigate the impact of the COVID-19 pandemic by municipality, we have geo-located the pin transactions to zip codes of payment point locations.

We have for every account holder the zip code of their registered home address. These zip codes are merged with an external dataset featuring all Dutch zip codes and their latitudes and longitudes⁵ in order to obtain geolocations (expressed as latitude and longitude). Customers’ home locations often have a clear relationship to payment point locations, with most payment points being situated in a dense cluster around consumer home location points. The purpose is thus to identify one cluster in these points and to classify other transactions as outliers. Then using only the main cluster points, we proceed by determining the payment point location.

We use transaction-weighted density-based clustering to predict POS geolocations. Given that we want to determine dense clusters and classify all other points as outliers, a density-based method is the most suitable approach. As the dataset is large and clustering will need to happen for many sets of points, a less computationally expensive algorithm is preferred, and hence DBSCAN (density-based spatial clustering of applications with noise) was selected as the clustering algorithm for this problem.

The DBSCAN clustering algorithm⁶ requires two main parameters to be set. The first, ϵ , is the maximum distance that two points can be from each other while still belonging to the same cluster. The second, *min_samples*, is the minimum number of samples required for a group of points to become a cluster in the final results. As there are many varieties of payment points in the data, there is no one size fits all approach. The goal with the setting of these parameters is to find the optimal (most dense while including the most samples) cluster for a set of home locations associated with a payment point, in order to be able to remove points that are outliers or do not improve the location prediction. The initial parameters are set at very ambitious levels based on the label of the payment point and the total number of samples there are. When no cluster is found, the parameters are widened until either a cluster has been found or the parameter limit has been reached, and thus, hypothetically, no dense enough cluster exists in the samples to make a good prediction for the payment point.

The distance between a customer’s home and the payment point can differ according to the time of the day, month of the year, and total amount spent. Therefore, we introduce heuristic transaction weighting, also called transaction-weighted dynamic-parameter DBSCAN (TWDP-DBSCAN). These heuristics determine the weight of a transaction, which can vary between 0 and 2. This weight is used to determine how strongly the transaction influences the final predicted location of the payment point.

⁴Note that this is a drawback of the Carvalho *et al.* (2020) data. The study cannot distinguish the identity of the buyer in each transaction, and therefore the data represent a mixture of final consumption expenditure by households and corporate firms’ intermediate input purchases.

⁵<http://geonames.org>.

⁶<https://scikit-learn.org/stable/modules/generated/sklearn.cluster.DBSCAN.html>.

The final location (after clustering) is calculated as follows (for latitude and longitude separately):

$$location = \frac{\sum_{i=1}^N location_i * weight_i}{\sum_{i=1}^N weight_i}, \quad (1)$$

where N is the number of samples after clustering. Although this is based on analysis of a smaller sample, van der Crujisen and Knoben (2018) suggest that payment behaviours tend to be consistent across different groups of people and thus this sample is assumed to generalize well to the full dataset. See Appendix Figure A2 for an overview of the algorithm used.

After the implementation of the transaction-weighted dynamic-parameter DBSCAN (TWDP-DBSCAN), it was found that certain labels (categories) of payment points, such as hotels, did not perform well. This can be explained by the fact that consumers usually stay in hotels far from their home address. In order to account for the harder-to-predict categories, payment point matching was introduced, where each prediction was assigned a confidence based on the heuristics of the category, number of samples, and cluster size. Payment points were then matched according to the following routine:

1. Partition the transactions by customer
2. Sort the transactions by transaction date and time
3. For each transaction, calculate the time delta between it and the customer's previous transaction
4. If this time delta is smaller than the threshold level, add the previous payment point with its predicted location and confidence as a 'match'

Finally, the lower confidence payment point locations are recalculated based on their own location and the locations of the matched payment points using an algorithm that takes into account the confidence of all predictions, and the time delta of all the matches. For an overview of the validation of the model, see Appendix B.

In order to ensure that we have adequate coverage of transactions within every municipality (i), we delete the lowest 5 percentiles of municipalities based on the ABN AMRO clients to total inhabitants ratio from our dataset. We also delete municipalities for which we have incomplete data, which leaves us with 332 municipalities out of 355 for the main regressions. For the online iDEAL (sepa) transactions, we locate transactions at the home address of the ABN AMRO client, which is part of the meta data that are provided for every ABN AMRO account holder.

Variables constructed with the geo-located transaction data

Our variable of interest is the total spending of ABN AMRO clients in euros for municipality, where we aggregate over week. We calculate $y_{it} = v_{it} \cdot p_{it}$, where v_{it} is the volume of transactions and p_{it} is the price of every transactions in euros. These are physical transactions done by ABN Amro clients by phone or card at a PoS.

To investigate whether people may have changed their behaviour, due to a preference switch towards more online goods, we also use an indicator that includes credit card

and iDeal (internet banking) supermarket expenditures by municipality in euros. That is, $v_{it} \cdot p_{it}$, where v_{it} is the volume of online transactions for groceries and p_{it} is the price of every online transactions for groceries in euros.

For all main regressions, we aggregate the data by week. We pair every week with its equivalent week number in the previous year, therefore using year-on-year data. In addition to adjusting for seasonal patterns, we also manually adjust for calendar effects.

Other variables

COVID-19 data: We report the number of new hospitalized COVID-19 cases each day, for each municipality, using publicly available data from the National Institute for Public Health and the Environment⁷. The National Institute for Public Health and the Environment provides the cumulative number of hospitalized COVID-19 cases on a daily basis by municipality. This file contains data on the residency of the individual who is hospitalized. For example, if a COVID-19 patient is admitted to a hospital in Amsterdam, but lives in Rotterdam, the new hospitalized COVID-19 patient will be included in the number of hospitalized COVID-19 patients in the municipality of Rotterdam. We prefer the number of hospitalized cases over the confirmed COVID-19 infections as over time the testing capacity has been scaled up. If we would use the total COVID-19 infections measured by the number of positive test outcomes, we would drastically underestimate the real number of COVID-19 patients in the early months of the pandemic, as during these months testing capacity was still insufficient. We use the 7-day moving average of new daily COVID-19 patients who have been admitted to the hospital.

Stringency index: We make use of the Oxford COVID-19 Government Response Tracker from the Blavatnik School of Government and the University of Oxford (Thomas Hale and Goldszmidt, 2021). Specifically, we look at the stringency index. This tracker collects information on several different common policy responses that governments have taken to respond to the pandemic, using nine sub-indicators: school closing, workplace closing, cancellation of public events, restrictions on gatherings, public transport closing, stay-at-home requirements, restrictions on internal movement, internal travel controls, and public info campaigns. The data from the nine indicators are aggregated into a set of four common indices, reporting a number between 1 and 100 to reflect the level of government action. While not all sub-indicators in the stringency index do necessarily hamper consumption, it is the best proxy we have of the NPI policy.⁸

Local unemployment: We compiled data from the Dutch UWV (Employee Insurance Agency), an autonomous administrative authority that implements employee insurances and social benefits. We use the weekly figure of the unemployment benefits statistics, based on the number of unemployment benefits registered in the UWV administration for that week by municipality. Similar to the variable of interest, the spending data, we use the year-on-year differences in local unemployment rates.

⁷<https://www.rivm.nl/>.

⁸See: <https://www.bsg.ox.ac.uk/research/publications/variation-government-responses-covid-19> for more information.

Empirical strategy

To measure whether the severity of the COVID-19 outbreak at the local (municipality) level has a significant impact on transactions, we use a variant of the fixed effects (FE) panel regression, put forward by Allison, Williams and Moral-Benito (2017). FE estimation is performed by *time demeaning* the data. Demeaning deals with unobservable factors because it takes out any component that is constant over time and entity. The problem with a traditional FE model is however that they have the core assumption of strict exogeneity:

$$E(\varepsilon_{is}|x_{it}, \alpha_i) = 0, \quad \text{for all } s, t = 1 \dots, T. \quad (2)$$

This assumption is violated in case of reverse causality. In our case, the level of expenditure of consumers may impact COVID-19 as well; as people go out shopping and meet each other, the virus may spread more rapidly and therefore we cannot exclude reverse causality. The assumption of strict exogeneity in the FE model forbids current values of ε_{is} to be correlated with past, present, and future values of x_{it} . However, if y_{it} affects x_{it+1} , thus if reverse causality is present, ε_{it} in a regular FE model is necessarily correlated with x_{it+1} . By violating one of the core assumptions of the FE model, the presence of reverse causality thus introduces bias to estimates from the model (Leszczensky and Wolbring, 2022).

In order to address this issue, we use the ML-SEM specification introduced by Allison *et al.* (2017). This is a cross-lagged panel model with FE, including lags of both the dependent and the independent variables in the specification. They show that the maximum likelihood (ML) method suggested by Moral-Benito (2013) can be implemented in a structural equation modelling (SEM) framework, hence calling it the ML-SEM method.

We use the following equation:

$$y_{it} = \alpha_i + \beta_1 x'_{i,t-1} + \beta_2 y'_{i,t-1} + \beta_3 y'_{i,t-2} + \varepsilon_{it}, \quad (3)$$

where y_{it} is the observation for the i th cross-section unit at time t for every $i = (1, 2, \dots, n)$ and $t = (1, 2, \dots, m)$. α_i represents the combined time-invariant effects of all time-invariant unobserved variables, thus being a unit-specific FE. The ML-SEM model, however, does not treat α_i as a fixed parameter but as a latent variable that is allowed to correlate with x_{it} and y_{it} at all points in time. In other words, the ML-SEM provides FE estimates for time-varying covariates (see also Allison *et al.*, 2017), while initial values of Y and X are treated as strictly exogenous. $x'_{i,t-1}$ is a $1 \times k$ (include constants) of observed lagged independent variables, including our variable of interest, new hospitalized COVID-19 cases. ε_{it} are the individual-specific (idiosyncratic) errors that are distributed independently of x_{it} and α_i . By including FE α_i , we are controlling for the average differences across municipalities in any unobservable predictors, which allows us to eliminate the omitted variable bias. This also eliminates any variability between municipalities, for example, the size of the municipality and whether it is urban or rural area. Spatial autocorrelation is also accounted for by including an autoregressive parameter of the order two in the model specification (i.e. $\beta_2 y'_{i,t-1}$ and $\beta_3 y'_{i,t-2}$).

Another issue that may arise is that the unobserved factors α_i could be correlated with x_{it} , and therefore errors are correlated contemporaneously across municipalities. Pesaran (2006) shows that by including the cross-sectional averages in the regression the differential effects of unobserved common factors are eliminated. We therefore run all the main regression in two stages to eliminate any cross-sectional dependence in the robustness checks section (see section V).

We run the regression for two distinctive periods: the first COVID-19 wave and corresponding NPIs, ranging from the first of January until the beginning of June and the second COVID-19 wave and corresponding NPIs, ranging from September to mid-March. We deliberately leave the summer period (July and August) out of the regressions, as the number of COVID-19 cases becomes really low during this period (see Appendix Figure A3). Because FE models rely on within-group action, we need repeated observations for each group, and a reasonable amount of variation of our key variable of interest (COVID-19) within each group. As the number of hospitalized COVID-19 cases is almost zero in July and August, the variation between the municipalities is too small for the FE model to pick it up. To make sure we do not delude our results by measuring the lack of variability in the data, we leave these months out. We have chosen mid-March as the cut-off date as the vaccination pace starts to increase rapidly from that date onward, which may skew the results as we want to measure the behavioural response of consumers to the virus. In mid-March, around 11% of the population have had their first doses. See Appendix C for a timeline of the NPIs introduced in the Netherlands during the COVID-19 pandemic.

IV. Results

Stylized facts

Appendix Figure A4 plots the year-on-year physical pin transactions and the stringency index during the first COVID-19 wave for two municipalities with a relatively small population size; Veendam and Bernheze. Veendam had almost no COVID-19 cases, and although the pin transaction data show an initial spike just before the imposition of the NPIs and a small drop afterwards, it recovers almost immediately to normal levels of expenditure. This is contrary to the municipality of Bernheze, where the drop in pin transactions is very pronounced (50% at its lowest level), and moves alongside the number of new confirmed COVID-19 cases. Notice that for both municipalities the pattern of the consumption recovery mimics the hospitalized COVID-19 cases represented by the blue line. While NPIs were not lifted before half May, consumption already started to recover already in April. This observation shows that during the first phase of the COVID-19 pandemic, pin transactions correlate with the outbreak severity, even though the NPIs were still in place. This is consistent with the aforementioned literature on the relationship between COVID-19 and consumption.

During the second COVID-19 wave and the renewed NPIs, the relationship is less clear. Appendix Figure A5 shows the same comparison between Veendam and Bernheze, but then for the second COVID-19 wave taking place in late 2020. From this figure, no clear relationship between the number of COVID-19 cases and consumption is seen.

TABLE 1
 First COVID-19 wave - dependent variable: $transactions_{it}$

Regressors	(1)	(2)	(3)	(4)
COVID-19 $_{it-1}$	-0.1092*** (0.0405)	-0.0861*** (0.0313)	-0.1012*** (0.0419)	-0.0898*** (0.0336)
Transactions $_{it-1}$		0.5994*** (0.0104)		0.6024*** (0.0105)
Transactions $_{it-2}$		0.0994*** (0.0054)		0.0998*** (0.0056)
Unemp $_{it-1}$			-0.2599*** (0.0671)	-0.0790 (0.0529)
N	332	332	332	319
T	30	30	30	30
FE	Both	Both	Both	Both
R^2	0.0010	0.3800	0.0031	0.3840

Notes: Standard errors clustered by municipality in parentheses.

*** $P < 0.01$; ** $P < 0.05$; * $P < 0.1$.

On the other hand, the sharp drop in transactions, which is noticeable around mid-December, does coincide with the timing of the imposition of the NPIs (also see Appendix Figure A3).

Regression results

Table 1 shows the regression results for the panel regression of equation (3) for the first COVID-19 wave. The dependent variable (y_{it}) is the year-on-year change in total volume of transactions by municipality i by week t , scaled by the number of ABN Amro clients in municipality i . The key explanatory variable is the new number of hospitalized COVID-19 cases by week t for municipality i , scaled by the number of inhabitants of municipality i . We cluster standard errors at municipality level. Regression (1) includes both municipality and time fixed effects. Given that in the Netherlands the strictness and timing of the NPIs were identical for all municipalities, this effect is captured by time fixed effects.

We find a statistically significant negative coefficient for the COVID variable under a 1% confidence interval. For every new hospitalized COVID-19 per 1000 citizens, transactions drop by 0.11% on average. During the peak of the first COVID-19 wave, around 23 new cases were admitted to the hospital daily in Amsterdam. On a population of around 870.000 people, that would result in a drop of total pin transactions of 2% in a week.⁹ In regression (2) in Table 1 we add two lags of the dependent variable (y_{it}). Including lagged dependent variables can reduce the occurrence of autocorrelation arising from model misspecification. The result is robust to the addition of the lags, as the variable

⁹For our fixed effect regression (1), the value of R^2 is relatively low. This is contrary to the findings of Chen *et al.* (n.d.) and Goolsbee and Syverson (2020), who report a relative large R^2 . This is mainly because of differences in specifications. Both of those studies use dummy variables to single out specific subgroups of panel data, and therefore they measure the effects between two different groups over time. In contrast, we look at the fit of all municipalities over time. Moreover, they transform the data into logged values, whereas we prefer to use a linear model and to keep the number of transformations to the data as minimal as possible.

of interest – the number of hospitalized COVID-19 cases – is still significant under a 1% interval level. However, $\hat{\beta}_{covid}$ has become slightly smaller.¹⁰

Table 1 regression (3) and (4) include the local unemployment measure. Another possible explanation for the effect of the COVID-19 variable that is picked up in regression (1) and (2) is that the local virus outbreak adds to the negative sentiment and uncertainty, even resulting in unemployment (Baker *et al.*, 2020, Altig *et al.*, 2020). The unemployment variable in Table 1 regression (3) and (4) therefore may capture the macroeconomic effect of loss of income due to lay-offs. This variable is also significant and shows for every new unemployed worker per 1000 citizens, transactions drop by about 0.25% on average. However, these results are not robust to the inclusion of the additional lags of the dependent variable (see regression (4)). The autocorrelation of the dependent variables absorbs the significance of unemployment. Regression (4) in Table 1 suggests that the heterogeneity of unemployment between municipalities is not an explanatory variable for the heterogeneity in consumption between municipalities. In regression (4), we find a statistically significant negative coefficient for the COVID variable under a 1% confidence interval.

To summarize, during the first COVID-19 wave, we observe that the conditional expectation of consumption is negatively related to the number of hospitalized COVID cases. This effect cannot be explained by differences in unemployment by municipality. This result entails that municipalities that saw a large COVID-19 outbreak also faced a larger drop in local consumption, relatively to municipalities that had little COVID-19 hospitalized cases. This effect can also be partly induced by the fact that consumers living in highly affected areas moved their consumption to municipalities that saw relatively low infection rates, that is, those municipalities benefited.

Non-compliance with NPIs

The findings in the previous section may result from another confounding effect, which is non-compliance or lack of ‘enforcement’ of the NPIs (see Appendix C for an overview of the restrictions that were in place during the first and second COVID-19 wave. See Appendix D for an overview of the institutions responsible for imposing restrictions, information provision on COVID-19, and enforcement). Gianmarco *et al.* (2020) found that varying social compliance or non-compliance with such NPIs has variously facilitated or constrained government action, and compliance has generally decreased over time. In our research, we take the assumption that the NPIs – as they were implemented nationwide – are similar across municipalities. While this should be true theoretically, in practice some municipalities may be stricter in enforcing the national government rules than others, depending on how severe the local COVID-19 outbreak is. Also, depending on the local COVID-19 cases, citizens of municipality i may show different degrees of (non)compliance with the NPIs. To test this assumption, we look at the expenditures in sectors that were subject to NPIs and were either forced to fully close or continue with very limited capacity. We use a subset of transactions that took place in sectors that were

¹⁰We have tested the model with the inclusion of more lags, but after the t_{-2} the included lags are not significant under a 10% confidence interval.

TABLE 2
First COVID-19 wave - dependent variable: transactions in sectors subject to NPIs

<i>Regressors</i>	(1)	(2)	(3)
COVID-19 cases _{it-1}	-0.2133 (0.2018)	-0.2362 (0.1655)	0.0399 (0.1342)
Transactions NPIs _{it-1}		0.4874*** (0.0180)	0.6444*** (0.0161)
Transactions NPIs _{it-2}		0.0596*** (0.0056)	0.0494*** (0.0047)
Stringency _{it-1}			-0.7348*** (0.0372)
<i>N</i>	331	331	331
<i>T</i>	30	30	30
<i>FE</i>	Both	Both	Entity
<i>R</i> ²	0.0002	0.1382	0.4211

Note: Standard errors clustered by municipality in parentheses.

****P* < 0.01; ***P* < 0.05; **P* < 0.1.

restricted by the government's imposed lockdown measures, to construct the variable of interest (see Appendix Table A1). For instance, restaurants were only permitted to provide takeaway goods and are therefore included in the list of sectors that were subject to NPIs. We hypothesize that non-compliance will be bigger in sectors that are subject to NPIs, and if non-compliance is an important explanatory factor, then the local COVID-19 outbreak should negatively correlate with transactions in sectors subject to NPIs.

Appendix Figure A6 shows that the transactions in sectors that we subject to NPIs closely correlate with the measure for NPIs strictness, the stringency index. The regressions in Table 2 show that the variance in COVID-19 cases among municipalities does not explain transactions in sectors subject to NPIs. In other words, the COVID-19 variable has no statistically significant relationship with the spending in sectors that were subject to NPIs. In regression (2) of Table 2, it is shown that the lagged variables of the transactions are significant. Unsurprisingly, regression (3) shows that the stringency index is an extremely good predictor of the drop in transactions in sectors subject to NPIs, with a coefficient of -0.7. This entails that for every unit increase in NPI strictness on a scale of 0-100, the value of consumer transactions fell by 0.7% in sectors that were restricted by NPIs. While non-compliance with NPIs may explain spending differences over time, that is, a larger spending drop in the first lockdown versus the second lockdown, according to our analysis, it does not explain the difference between municipalities.

Consumer response during the second COVID-19 wave

So far, we find that the negative relationship between heterogeneity in COVID-19 cases and local spending by municipality cannot be contributed to non-compliance with NPIs. Table 3 shows the regression results for the panel regression of equation (3) for the second COVID-19 wave. Table 3 regression (1) shows that the number of hospitalized COVID-19 cases is not significant. This suggests that, during the second COVID-19 wave, the behavioural response of consumers had no additional effect on physical spending. What is striking is that unemployment is significant in regressions (3) and (4), and

TABLE 3
Second COVID-19 wave - dependent variable: transactions_{it}

<i>Regressors</i>	(1)	(2)	(3)	(4)
COVID-19 _{it-1}	-0.0010 (0.0391)	-0.0245 (0.0373)	0.0020 (0.0403)	-0.0221 (0.0385)
Transactions _{it-1}		0.1025*** (0.0110)		0.1012*** (0.0117)
Transactions _{it-2}		0.2488*** (0.0107)		0.2489*** (0.0111)
Unemp _{it-1}			-0.0797*** (0.0287)	-0.0505** (0.0267)
<i>N</i>	332	332	332	319
<i>T</i>	30	30	30	30
<i>FE</i>	Both	Both	Both	Both
<i>R</i> ²	4.246e- 05	0.1210	0.0010	0.1210

Note: Standard errors clustered by municipality in parentheses.

****P* < 0.01; ***P* < 0.05; **P* < 0.1.

hence during the second COVID-19 wave robust to the introduction of the lags of the dependent variable. These results show that municipalities that saw larger increases in unemployment also saw a larger drop in transactions. For every new unemployed worker per 1000 citizens, transactions drop by about 0.05% on average (regression (3)).

Our results show that local unemployment had a significant relationship with physical spending during the second COVID-19 wave. This is in line with previous research that focused on the consumption decisions of households that experience unemployment (Gruber, 1994).

Contrary to the finding of Chetty *et al.* (2020), Bounie *et al.* (2020) and Chen *et al.* (n.d.) we find that during the second COVID-19 wave, heterogeneity in spending between municipalities is better explained by labour market conditions than the number of COVID-19 hospitalizations. Given that during the second COVID-19 wave, the hospitalized COVID-19 cases coefficient is not significant also suggests that the main channel through which the virus impacts consumption is not infections by definition. If such a transmission channel would be credible and constant over time, we would have seen significant results for both COVID-19 waves, as the number of infectious people surged.

Changing consumer behaviour

We find that heterogeneity in COVID-19 hospitalizations does not explain the heterogeneity in total spending behaviour by municipalities during the second wave. From our results, we cannot infer *why* consumers react differently in the second COVID-19 versus the first COVID-19 wave. In line with the (growing) literature in this field (Fetzer *et al.*, 2021; Roth and Wohlfart, 2020; see Akesson *et al.*, 2020 and Gallagher, 2014), the most probable explanation is that people have updated their beliefs or changed their behaviour accordingly, as more information on COVID-19 became available. Akesson *et al.* (2020) show that initially individuals dramatically overestimate the dangerousness and infectiousness of COVID-19 relative to expert opinion. This may have caused an

overly strong reaction on consumption, as consumers voluntarily stayed at home (Yan *et al.*, 2021 and Andersen *et al.*, 2020). When people are provided with information, this corrects their beliefs about the virus. As the COVID-19 pandemic progressed, more information became available on the infectiousness and on which (social) activities may increase the chances of attracting the virus. Therefore, consumers may have adapted their consumer behaviours, avoiding physical consumption in certain sectors that have a high chance of transmissibility of the virus, but continue to shop in sectors that are relatively safe.

We investigate this hypothesis further by looking at online supermarket expenditures during the second COVID-19 wave. Given that supermarkets are generally crowded places and a potential source of COVID-19 contamination, we hypothesize that consumers would want to avoid these places more if they live in a municipality that experiences a larger COVID-19 outbreak. Hence, they will be more inclined to order groceries online and get them delivered to their home address. Moreover, what improves our empirical strategy is the fact that total supermarket expenditure was not impacted by the NPIs, as during both COVID-19 waves, supermarkets, convenience stores, and other ‘vital’ food retailers were allowed to stay open and faced no capacity limits. Supermarkets are also likely to be situated closely to consumers, which minimizes any disturbing spatial effects that may occur.

While the total spending on groceries (and hence online groceries) have increased substantially during the COVID-19 crisis, we measure in our FE specification whether differences in the COVID-19 outbreak by municipality explain the differences in online grocery expenditure with our fixed effects regression.

The dependent variable (y_{it}) is the year-on-year change in total online supermarket spending by municipality i by week t , scaled by the number of ABN Amro clients in municipality i . We run the regression on the second COVID-19 wave (see Table 4). As the coefficient is positive, the results show that if there is one additional hospitalized COVID-19 case in a municipality per 1000 inhabitants, the online spending on groceries increases by over 2% on average. Regression (2) shows that this conclusion is robust to the addition of lagged variables to the dependent variable. Regressions (3) and (4) include the stringency index and therefore exclude time effects. The stringency index is also significant and shows that as the strictness of the NPIs increases, the amount of spending on online groceries also increases. It is interesting that when the stringency index is included in the regression, the coefficient of the COVID-19 cases increases substantially. Obviously, these two parameters interact with each other: the stringency index moves along with the number of infections (see Appendix Figure A3. As the stringency indicator is high, there were more *soft* restrictions such as wearing face masks and forced disinfection of hands at the entrance. This results in Table 4 regression (4), showing that whether people decided to buy their groceries online depends on both the NPIs and the severity of the local COVID-19 outbreak¹¹.

These results suggest that, in line with the results of Bounie *et al.* (2020), a local outbreak leads to a substitution from physical expenditures to online. Therefore, consumers

¹¹We have also run the same regression on the first COVID-19 wave, and it shows the same results as during the second COVID-19 wave. We do not include this table because of space limitations

TABLE 4
Second COVID-19 wave – dependent variable: online groceries transactions_{it}

<i>Regressors</i>	(1)	(2)	(3)	(4)
COVID-19 _{it-1}	2.0814** (0.9014)	2.0246** (0.9018)	6.9379*** (0.9289)	7.1766*** (0.9277)
Transactions _{it-1}		0.0247** (0.0112)		0.0616*** (0.0115)
Transactions _{it-2}		0.0166 (0.0113)		0.0231** (0.0107)
Stringency _{it-1}			5.3330*** (0.5565)	4.6276** (0.05705)
<i>N</i>	274	274	274	274
<i>T</i>	30	30	30	30
<i>FE</i>	Both	Both	Entity	Entity
<i>R</i> ²	0.0007	0.0016	0.0243	0.0286

Note: Standard errors clustered by municipality in parentheses.

****P* < 0.01; ***P* < 0.05; **P* < 0.1.

do change their behaviour as the virus surged during the second wave, by switching between physical and safer online expenditures. During the second COVID-19 wave, consumers, instead of refraining fully from physical consumption, made more ‘safer’ decisions on how to consume. Obviously, from our results, we cannot infer why consumers do this, as we have no qualitative data on their decision-making progress or beliefs. This substitution effect that we find in the data diminishes the overall impact of the COVID-19 shock on aggregate consumption expenditure.

V. Robustness checks

Cross-sectional dependence

In the main regression Table 1, the standard errors are clustered by municipality level. However, the unobserved factors α_i could be correlated with x_{it} and therefore errors are correlated contemporaneously across municipalities. Pesaran (2006) shows that by including the cross-sectional averages in the regression, the differential effects of unobserved common factors are eliminated.¹² We therefore run the main regression from Table 1 in two stages to eliminate any cross-sectional dependence:

First-stage regressions:

$$\begin{aligned}
 y_{it} &= \beta_1 \mu_t^y + \beta_2 \mu_t^x + e_{it}^y \\
 x_{it} &= \beta_1 \mu_t^y + \beta_2 \mu_t^x + e_{it}^x,
 \end{aligned}
 \tag{4}$$

¹²This approach is favoured above the principal components approach brought forward by Coakley, Fuertes and Smith (2002) because we want to avoid inconsistent results in the situation where the unobserved factors and the included regressors are correlated. Moreover, the approach by Pesaran (2006) allows us to use ordinary least squares (OLS) when we specify an auxiliary regression where the observed regressors are augmented by cross-sectional (weighted) averages of the dependent variable (y_{it}) and observable variables x_{it} and possibly other control variables x_i and x_t (see also Kapetanios and Pesaran, 2007).

TABLE 5

Dependent variable: residuals for percentage change in transactions year on year

<i>Regressor</i>	<i>(1)</i>
Residual COVID-19 cases	-1.5224*** (0.1093)
<i>N</i>	332
<i>T</i>	30
<i>FE</i>	None
<i>R</i> ²	0.0030

Note: Standard errors in parentheses.

*** $P < 0.01$; ** $P < 0.05$; * $P < 0.1$.

where μ_t^y and μ_t^x are the cross-sectional averages of y_{it} and x_{it} respectively over time t . e_{it}^y and e_{it}^x capture the residuals of equation (4).

Second-stage regression:

$$e_{it}^y = e_{it}^x + w_{it}. \quad (5)$$

Under the assumptions explained in Pesaran (2006), for any fixed n in α_i these residuals provide consistent estimates of ε_{it} in the multifactor model and could be used as ‘observed data’ to obtain estimates of the factors α_i . The factor estimates can then be used directly as (generated) regressors in regression equation (5). Effectively, in the second stage, we try to explain the variance of y_{it} with the variance of x_{it} , thereby eliminating all the other FE.

Table 5 shows the results of the two-stage regression (see equation (5)). The result is significant at the 1% level. The interpretation of the beta coefficient is not straightforward because it is the beta coefficient of the residual of the original regression. The sign of the coefficient is as anticipated: the number of hospitalized COVID-19 cases has a negative effect on year-on-year average transactions. If we include time-variant control variables (x_t) in the first stage of regression (equation 4) and estimate (x_{it}) in the second stage, the variable is still statistically significant and the beta coefficient does not change. This suggests that the set of control variables x_t has no correlation with the variable of interest (x_{it}).

Time frequency

In this section, we validate the geolocation model used to geo-locate transaction data. Table 6 and 7 show the same regressions as in section IV, but on a daily frequency. For the aggregations by day, in order to control for seasonality trends, we pair every day with its equivalent day in the previous year and calculate the year-on-year differences.

The daily variance is captured by the error term ε_{it} . As we only have unemployment data on a weekly frequency, this regression excludes the unemployment variable. The regression results are similar to the weekly aggregates. The COVID-19 variable is significant during the first COVID-19 wave and not during the second wave.

Time lags

We also investigate the sensitivity of time lags by performing inter-temporal shifting. Figure 1 shows the P -value on the explanatory variable hospitalized COVID-19 cases,

TABLE 6
First COVID-19 wave – dependent variable: transactions_{it}

<i>Frequency: daily (7-days rolling average)</i>				
<i>Regressors</i>	(1)	(2)	(3)	(4)
COVID-19 _{it-1}	-0.1324*** (0.0462)	-0.1550*** (0.0382)	-0.1172*** (0.0496)	-0.1443*** (0.0444)
Trans _{it-1}		0.4757*** (0.0051)		0.3817*** (0.0052)
Trans _{it-2}		0.2330*** (0.0054)		0.1807*** (0.0057)
Stringency _{it-1}			-0.0690*** (0.0031)	-0.0021 (0.0028)
<i>N</i>	332	332	332	332
<i>T</i>	157	157	157	157
<i>FE</i>	Both	Both	Entity	Entity
<i>R</i> ²	0.0002	0.3354	0.0247	0.2232

Note: Standard errors clustered by municipality in parentheses.
 *** $P < 0.01$; ** $P < 0.05$; * $P < 0.1$.

TABLE 7
Second COVID-19 wave – dependent variable: transactions_{it}

<i>Frequency: daily (7-days rolling average)</i>				
<i>Regressors</i>	(1)	(2)	(3)	(4)
COVID-19 _{it-1}	0.05437 (0.0344)	-0.0188 (0.0274)	-0.0154 (0.0378)	-0.0339 (0.0317)
Trans _{it-1}		0.4398*** (0.0044)		0.4150*** (0.0046)
Trans _{it-2}		0.1450*** (0.0043)		0.07547*** (0.0043)
Stringency _{it-1}			-1.5898*** (0.0072)	-0.8399*** (0.0076)
<i>N</i>	332	332	332	332
<i>T</i>	192	192	192	192
<i>FE</i>	Both	Both	Entity	Entity
<i>R</i> ²	3.943e-05	0.3690	0.4421	0.6078

Note: Standard errors clustered by municipality in parentheses.
 *** $P < 0.01$; ** $P < 0.05$; * $P < 0.1$.

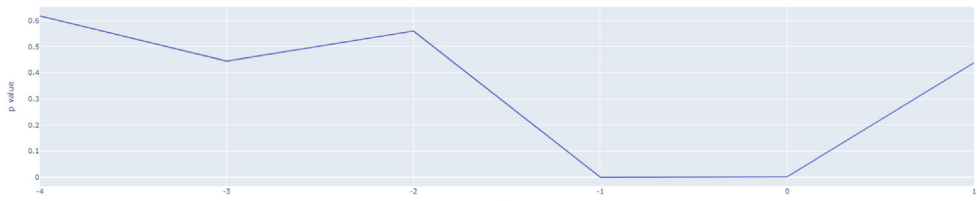


Figure 1. Inter-temporal shifting of the P -value of the explanatory variable (x_{it}): the new number of hospitalized COVID-19 cases. The horizontal axis t indicates the time in weeks and the vertical axis shows the P -value of the explanatory variable

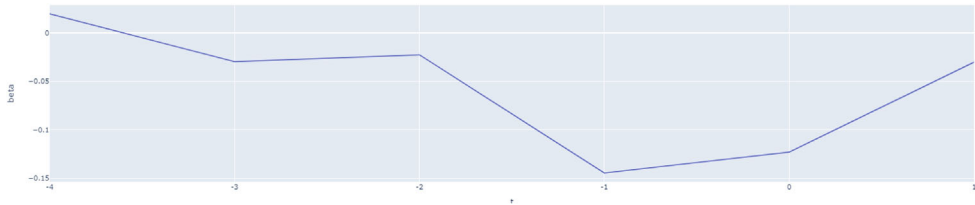


Figure 2. Inter-temporal shifting of the beta coefficient of the explanatory variable (x_{it}): the new number of hospitalized COVID-19 cases. The horizontal axis t indicates the time in weeks and the vertical axis shows the beta coefficient of the explanatory variable

shifted over time. The figure shows that the P -value is significant both at t and t_{-1} , suggesting that the current COVID-19 cases and the COVID-19 cases from a week ago are both explanatory of the drop in transactions during the first COVID-19 wave. The explanatory variable (x_{it}) is not statistically significant when the time is shifted further backwards or forwards. The beta coefficient in figure 2 has the highest negative explanatory power when t_{-1} . This suggests that the expenditure variable y_t is quite sensitive to the timing of the independent variable x_{t-1} .

VI. Conclusion and discussion

By geo-locating transactions by municipality, we find that, during the first COVID-19 wave, the number of new hospitalized COVID-19 cases in a municipality has a strong and statistically significant negative correlation with the change in physical transactions by consumers. This suggests that people living in badly affected areas altered their economic behaviour differently from people living in little affected areas. This supports the claim by Chetty *et al.* (2020), Bounie *et al.* (2020), Chen *et al.* (n.d.), who all find similar results.

An important caveat to our finding is that while we use the ML-SEM specification to circumvent reverse causality and control for unobserved factors that could be correlated with our explanatory variable in the robustness checks, our methodological design does not allow us to infer causality.

In our paper, we try to disentangle the different mechanisms that might be at play during the first COVID-19 wave, to understand how the virus interacts with local spending. We argue that it is unlikely that the interaction of the virus and spending is caused by non-compliance of NPIs. To test this assumption, we look at the expenditures in sectors that were subject to NPIs, but we do not find a significant relation between the number of hospitalized COVID-19 cases and the expenditure in sectors subject to NPIs. This suggests that, at least during the first stage of the pandemic, the consumer reaction function towards the virus, that is, self-imposed stay-at-home policies, is the most likely mechanism through which the virus interacts with consumption expenditure.

We also find that the consumer reaction function towards the virus is not constant over time, as our results show that the relationship between the local COVID-19 outbreak and transactions does *not* appear during the second wave. This seems to suggest that people may have adopted their behaviour. Perhaps a certain amount of fatigue with the NPIs rules came into play, or, alternatively, as more information on the COVID-19 virus became available, consumers could adapt their behaviour in such a way that it did not impact their

aggregate consumption. When looking at online and offline supermarket expenditures, we see that municipalities that saw a large number of COVID-19 hospitalized cases during the second wave, also saw a larger increase in online grocery shopping. When the COVID-19 pandemic progressed, more information became available on the infectiousness and effective measures to mitigate the transmission of the virus (Falcone and Sapienza, 2020). Our result suggests that consumers have adopted their behaviour, in such a way that they may have found different (safer) channels to continue their consumption patterns. This view is consistent with earlier research by Akesson *et al.* (2020) on how individuals update their beliefs towards COVID-19 when new information comes in. The results that we find may also be related to the influence of framing of news on public perceptions and economic expectations in times of high uncertainty (Chong and Druckman, 2007). How the mechanisms works, however, that is, how consumers exactly update their belief system about COVID-19 on the basis of news, information, or public perception and how this translate into consumer decisions, is still unknown to us and cannot be derived from our data. Further research that combines transaction data with survey data – ensuring a constant panel over time – could shed more light on the underlying mechanisms.

As we find no significant correlation between COVID-19 and consumption during the second wave, this also reduces the credibility of the theory that ill-fallen consumers have a significant impact on total consumption. While some theoretical models are built around this assumption that infected consumers and workers stay at home and refrain from spending and producing (Eichenbaum *et al.*, 2020), we cannot support this assumption with our findings. That said, Appendix Figure A3 shows that the peak of hospitalization during the first wave was higher than during the second, possibly, therefore ill-fallen consumers/workers may only start to take effect on consumption after a certain percentage of the population has been infected.

Related to this, nonlinearities could be at play. Perhaps a very high level of local infections does trigger a behavioural effect, also during the second COVID-19 wave, which is currently not picked up by our model. Baqae and Farhi (2020) consider possible nonlinearities in response to the pandemic in a multi-sectoral model. They demonstrate how these shocks are amplified or mitigated by nonlinearities, and quantify their effects using disaggregated data from the United States. A similar method could be used to look at nonlinearities between consumption and COVID-19 on a local level.

We do find a negative significant relationship between unemployment and consumption during the second COVID-19 wave. This is in line with previous research that focused on the consumption decisions of households that experience unemployment (Gruber, 1994). Although the Netherlands has generous unemployment insurance benefits, which is smoothing consumption, the benefit is at most 70% of the last wage, which still entails a significant drop in income. Also, not everyone is eligible for unemployment benefits, self-employed, flexworkers, or workers with insufficient earnings or length of employment are disqualified. An increase in the unemployment rate contains new information on future income streams. Our finding that unemployment negatively impacts spending on a local level is as predicted by economic theory, and consistent with the permanent income hypothesis where households adjusted their consumption downward accordingly (Campos and Reggio, 2015) and Codagnone *et al.*, 2020).

By having a better understanding of how the different mechanisms that impact consumption expenditure interplay, policymakers can make better informed decisions in relation to the strictness of the NPIs, that is, balancing the negative economic impact of NPIs versus limiting the spread of the virus. Our results show that as the pandemic progresses, consumers display different behaviour. Unemployment becomes a more important factor in understanding consumer decisions in the later stages of the pandemic, and in order to level regional disparities, policymakers can address labour market disparities by working together with local councils.

Measuring physical transactions is not a perfect substitute for economic activity. The drop in physical transactions can be partly offset by an increase in online transactions. Our regression results show that the incidence of COVID-19 has a large positive correlation with online grocery spending. Further research could investigate to what extent online consumption substitutes the reduction in offline consumption. Also relevant in this respect is the reallocation from spending at local businesses to online retail businesses. The local community does not necessarily profit from an increase in online consumption at the expense of offline consumption, because many of the online retailers may not be located within the same municipality. Moreover, while we look at the compliance with NPIs by taking a sub-set of spending data in sectors that (partially) had to close, this is not a perfect measure of compliance with NPIs either. Other NPIs, such as face masks, restrictions on large gatherings, working-from-home, etc., were not tested for in our regressions.

Moreover, this study was done specifically using Dutch transaction data, whereas the reaction function of consumers towards the virus may be different among different cultures and countries. Individual beliefs and social preferences should also be taken into consideration, as they affect behaviour. Previous literature has found that the trust that the population has in healthcare system and/or the governments' response towards the pandemic differs by region and country (Brodeur *et al.*, 2021). These institutional and cultural aspects may lead to different conclusions in other countries.

It would be interesting to further look at the impact of the loss of income on consumer expenditure, especially in light of the generous support packages by the government. Cajner *et al.* (2020) and Ganong, Noel and Vavra (2020) argue that income losses were relatively modest because relatively few high-income individuals lost their jobs and lower-income households who experienced job losses had their loss in income offset by unemployment benefits. This also holds to some extent for the Netherlands, where the COVID-19 crisis support packages provided financial support to around 2.9 million workers (out of a labour force of about 9 million).¹³ Further research into the dynamics of the labour market and consumer spending at the local level in relation to the COVID-19 outbreak is needed to understand its full economic impact.

Although vaccination programmes are widely rolled out, and many (mainly developed) countries have almost fully relaxed the NPIs, the COVID-19 pandemic has not ended yet. New strains of the virus and/or low efficacy rates for the vaccines can still prolong the pandemic. Our study incorporates a medium-term horizon, including two distinctive COVID-19 waves. But new waves could take a different shape or occur under different macroeconomic conditions, and therefore the conclusion from this study may again change as well.

¹³https://www.tweedekamer.nl/debat_en_vergadering/plenaire_vergaderingen/details?date=24-06-2020

Appendix A: Figures and Tables

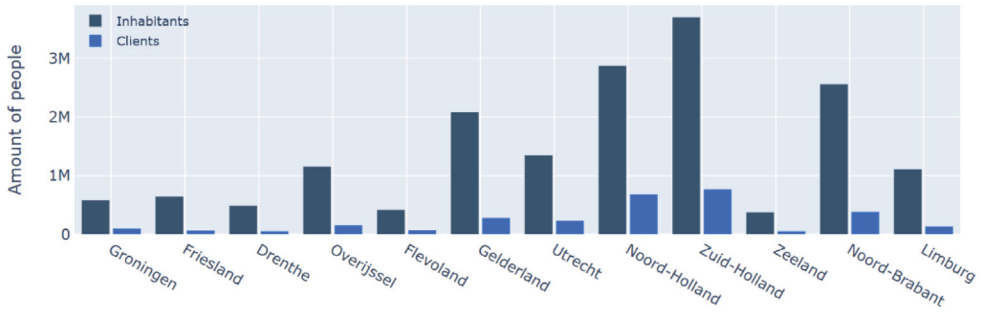


Figure A1. Number of ABN AMRO clients in different provinces of the Netherlands compared with the official population statistics of the Netherlands

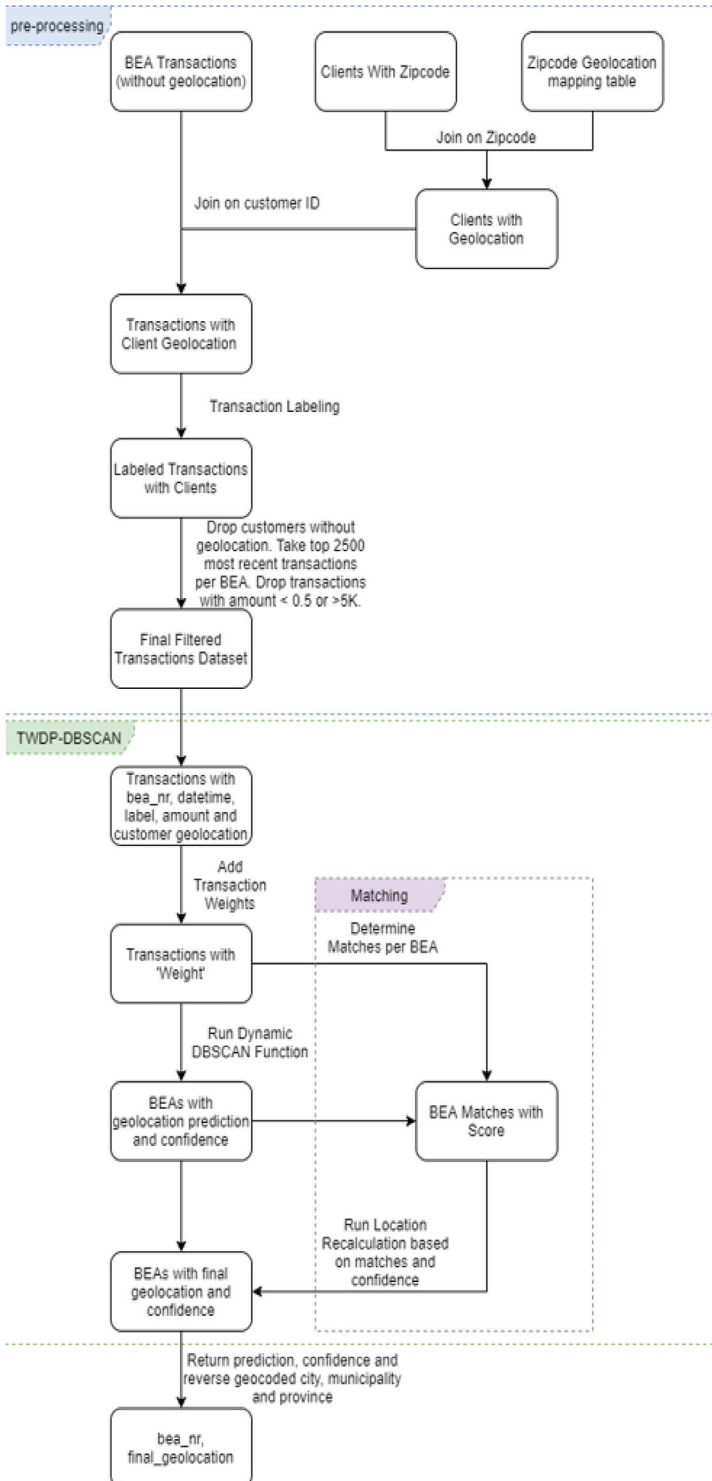


Figure A2. Transaction-weighted dynamic-parameter DBSCAN



Figure A3. Year-on-year transactions by ABN Amro clients (euros), new hospitalized COVID-19 cases, and the proxy of the NPIs measured by the stringency index

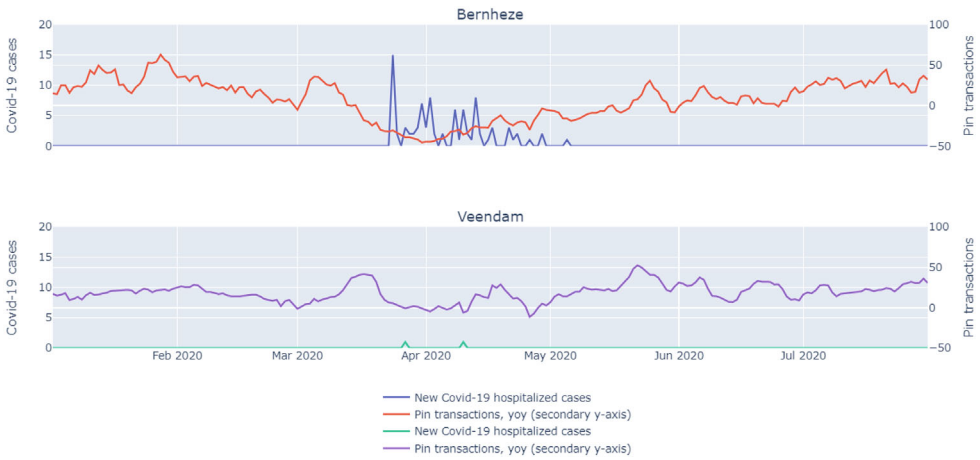


Figure A4. Pin transactions in Bernheze and Veendam during the first COVID-19 wave, 7-day rolling average

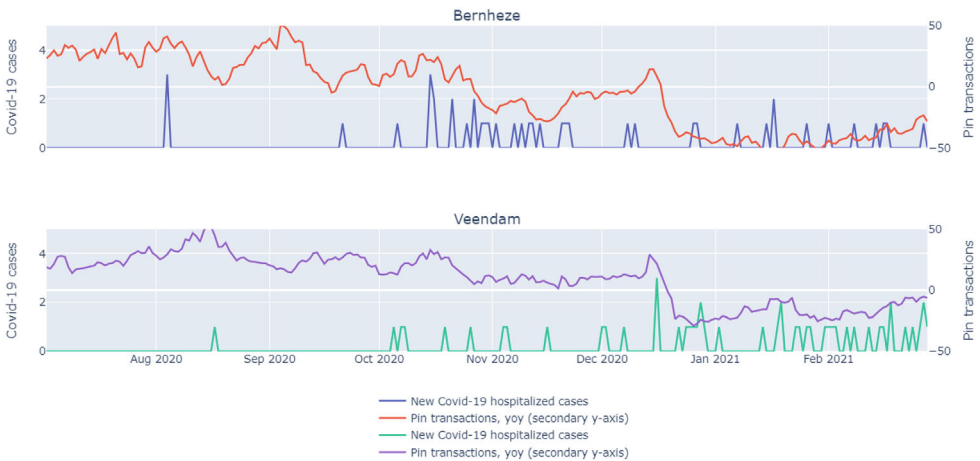


Figure A5. Pin transactions in Bernheze and Veendam during the second COVID-19 wave, 7-day rolling average



Figure A6. Average year-on-year change in expenditures in sectors that were subject to NPIs, 7-day rolling average

TABLE A1
Categories of consumer spending that were target to NPIs

<i>Consumer categories</i>
Restaurants
Wellness
Casinos
Cinemas
Concert halls
Museums
Theme parks
Bars
Dining
Fastfood
Hairdressers
Solariums
Spas
Other entertainment

Appendix B

Table B1 describes the test sets that were used for the final testing, the size of the datasets, and how the label (location) was obtained. The ground-truth dataset for validation was a dataset from 2015 from a company collecting PoS locations. As there is no training with unsupervised learning (clustering), there is no need to split the dataset into training and testing samples, and the full dataset can be used for final tests. Also, both test sets went through the same preprocessing function where payment points with less than 10 or more than 50,000 transactions were removed and transactions with a value under 0.5 euros or over 5,000 euros were removed. This is done to remove non-significant transactions and outliers.

The results of the 2015 test data can be found in Table B2. In this table, MHE represents the *Mean Haversine Error*, CA is the *City Accuracy*, and PA is the *Province Accuracy*. Also, the run time of each model is included. The algorithm was run with all transactions, but only the results for the smaller subset reported in B1 are reported as these are the only payment points that have an available ground-truth location.

As the method has been developed primarily with exploratory data analysis on the 2015 dataset (as specific geolocations of payment points were available), a good method of

TABLE B1
The two test sets used for evaluation

<i>Data</i>	<i>Year</i>	<i>Trans</i>	<i>PoS</i>	<i>Clients</i>
Ground-truth	2015	64M	54,700	2.7M
Descriptive	2020	393M	1.88M	3.24M

TABLE B2
Results 2015 test set (N = 64,000,000)

<i>Model</i>	<i>MHE</i>	<i>CA</i>	<i>PA</i>	<i>Runtime</i>
Baseline	4,213M	34.88%	85.05%	4minutes
DBSCAN	981M	71.03%	91.71%	39minutes
DBSCAN match	981M	71.03%	91.71%	94minutes

TABLE B3
Results on 2020 test set (N = 393,000,000)

<i>Model</i>	<i>MHE</i>	<i>CA</i>	<i>PA</i>	<i>Runtime</i>
Baseline	N.A.*	33.66%	85.0%	8 minutes
DBSCAN	N.A.*	67.63%	94.56%	30 minutes
DBSCAN match	N.A.*	67.61%	94.55%	104 minutes

* Not Available because the test set only contains cities, not exact locations. Cities are as large as 220km, and so would yield a very imprecise estimate.

validation is to test the method on more recent data without changing the implementation or heuristics. The results, for the first 5 months of transactions of 2020, are presented in Table B3. On this much larger, diverse, and high-quality sample, it is found that the baseline and TWDP-DBSCAN (with or without matching) results are similar to the 2015 results. This shows that the method is built in a robust way and generalizes well over time, as the samples are 5 years apart and no modifications to the method have been made prior to testing.¹⁴ This methodology predicts the correct province with 95% accuracy, the correct municipality with 86% accuracy, and the correct city with 67% accuracy.

Appendix C

On 12 March, the Dutch government announced an ‘intelligent lockdown’. Effectively, the next day, all events (concerts, sports) and all meetings with more than 100 people were forbidden. Bars, restaurants, and other public places or venues where people gather had to close as well. The timing and severity of the measures were generally comparable to most of northern Europe (such as Germany and Norway), but less restrictive than in southern Europe, where the virus spread more rapidly (such as Italy, France, and Spain). Similar

¹⁴The validation data used are from 2015 and only represent a limited sub-sample of all payment points. They thus only contain small numbers of observations for certain labels and areas. This means that the validation set is biased towards certain labels and areas in the Netherlands. Therefore, we have manually created more validation data where we extracted the geolocation from the transaction description. By matching this to our geolocation algorithm, we found a 97% overlap.

to other European countries, the Netherlands largely reopened again in the summer. As the number of hospitalized cases picked up during the autumn of 2020, on 14 October, a partial lockdown came into effect. As this did not help to bring down the COVID-19 cases sufficiently, from 15 December 2020, a hard lockdown was imposed, with strict NPIs. All non-essential shops were closed and a curfew was imposed. As the vaccination pace picked up in the first months of 2021, measures were slowly being relieved. On 28 April, shops and outdoor seating areas at restaurants and cafes were allowed to reopen and the curfew was lifted. On 19 May, most of the NPIs were relaxed.

Appendix D

The National Institute for Public Health and the Environment (Rijksinstituut voor Volksgezondheid en Milieu) is coordinating the response against the COVID-19 pandemic. The NPIs were organized by The Minister of Health. Also, the information on restrictions was distributed by the national government, as it was put on the website and broadcasted on national television. Enforcement of the restrictions is organized by the municipalities. The Public Health Act gives extensive powers to the mayor of the central municipality of the so-called safety regions (Veiligheidsregio's). These powers include the authority to hospitalize a person for isolation without delay, to quarantine individuals, to check grounds, buildings, means of transport, or goods for the presence of contamination and, if necessary, disinfecting them; to close buildings or areas or parts thereof. Besides the Public Health Act, there is Dutch Safety Regions Act (Wet veiligheidsregio's). On the basis of this act, the mayor of the central municipality can take several public order measures in the whole region to fight or prevent disasters. By municipal bye-laws, the mayors of the central municipality can impose penalties.

Final Manuscript Received: November 2021

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