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The effects of COVID-19 policies on consumer spending in Norway

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Abstract
We examine the effect of COVID-19 policies on consumer spending using bankcard transactions from Norway. Exploiting variation in COVID-19 policies over time and across space in the four largest municipalities, we investigate the heterogeneity of policy effects in their *number* and *type*. First, we document that the number of restrictions is negatively correlated with spending and exhibits decreasing marginal effects. Second, restrictions do not affect all types of spending equally: restrictions tend to have larger impacts on the sector in which they are targeted. Finally, we find suggestive evidence from a difference-in-differences estimation that supports a causal interpretation of our results.

KEYWORDS
consumption, COVID-19, expenditure, pandemic

JEL CLASSIFICATION
C83, D12, E21

1 | INTRODUCTION

This paper investigates the effect of COVID-19 policies on consumer spending in Norway. Although previous research has looked at the impact of COVID-19 restrictions on household consumption and expenditure (see literature review in Appendix A), many of these studies consider packages of policies, overlooking the potential heterogeneity in their *scale* and *scope*—i.e., the number and types of policies within the package. Our study bridges this research gap by analyzing payment card transaction data from one of Norway's largest banks, combined with hand-collected data on COVID-19 measures at the national and municipality levels. We concentrate on *economic* outcomes, as there are already numerous studies on *health* impacts (e.g., Methi et al., 2022; Ursin et al., 2020).

Our contribution is based on three sets of empirical analyses. First, we investigate the relationship between the *number* of COVID-19 policies and household consumption. Second, we examine how the *type* of policies influence different sectors of consumer spending, such as dining, shopping, and nightlife. And third, we set out to estimate the causal effects of policies using a difference-in-differences (DiD) case study, taking advantage of temporal and spatial

Abbreviations: BBVA, Banco Bilbao Vizcaya Argentaria; COVID, COrona VIrus Disease; DiD, Difference in Differences; DNB, Den Norske Bank; EU, European Union; GDP, Gross Domestic Product; OECD, Organization for Economic Cooperation and Development; OLS, Ordinary Least Squares; UK, United Kingdom; UN, United Nations; US, United States; VG, Verdens Gang; WHO, World Health Organization.

variation in COVID-19 restrictions across municipalities. Throughout the paper, we account for the potential correlation of errors over time and across space using Driscoll-Kraay standard errors, which is appropriate for our context because we have few municipalities and a relatively larger number of time periods. Further, in our DiD case study, we have chosen a particular episode of restrictions that provides the "cleanest" comparison possible, to avoid potential biases due to dynamic treatment effects in a staggered DiD design (Goodman-Bacon, 2021).

We find that COVID-19 restrictions are negatively correlated with consumption, but every additional restriction does not necessarily precipitate reduced spending. Importantly, the nature of the restriction seems to matter greatly for expenditure outcomes: restrictions tend to have larger impacts on the sector in which it is targeted. These patterns are also evident in our DiD analysis, thus providing suggestive evidence of a causal interpretation of the results. Although our conclusions are necessarily specific to our Norwegian study setting, there is reason to believe that they generalize to other countries. For example, studies of COVID-19 lockdowns and shelter-in-place orders in China (e.g., Chen et al., 2021), the US (e.g., Alexander & Karger, 2021), and the UK (e.g., Chronopoulos et al., 2021) have also found large expenditure declines in sectors associated with mobility.

With the worst of the COVID-19 pandemic now hopefully in the rear-view mirror, our study offers several broad insights that can inform public policy and research practice in the future. Given our results on the non-linear impacts of pandemic restrictions, policymakers should be cognizant of potential saturation points beyond which additional measures could yield minimal impacts. Moreover, we show that COVID-19 restrictions exhibit substantial effects in some sectors but not others—a finding that is mirrored in many other study contexts. Thus, when closures and similar measures are implemented, policymakers may also consider targeting support policies to the specific sectors hit hardest by these restrictions.

From a practical standpoint, our study also highlights the value of leveraging private sector data for both retrospective and real-time analysis, in line with research by Chetty et al. (2023). Many studies of COVID-19 impacts on the economy have used bank card transactions (e.g., Aastveit et al., 2020; Akerman et al., 2022; Cox et al., 2020; Sheridan et al., 2020), which provide a quicker view of economic activity than government statistics. The threat of a pandemic deadlier than COVID-19 is ever present (UN, 2023), and building infrastructure to facilitate research access to real-time, high-frequency information from the private sector is critical. Doing so will provide policymakers with timely, data-driven guidance to navigate future pandemics or economic crises. Our study also helps to underscore that access to these data should be complemented by rigorous empirical work. Future DiD studies should strive for comparisons that are careful and transparent—a principle we have sought to model in this paper—echoing the arguments of Goodman-Bacon and Marcus (2020).

2 | DATA

We assemble data on the four largest municipalities in Norway: Oslo, Bergen, Stavanger, and Trondheim. These are all the municipalities that are available in our consumer spending data, which we combine with hand-collected data on COVID-19 policies. We provide a brief overview of the data below. Appendix B provides further information about the study setting, Appendix C contains details about COVID-19 policies in Norway, and Appendix D a description and statistics on the bank card data.

2.1 | Municipality-level COVID-19 policies

Since January 30, 2020—when the World Health Organization declared COVID-19 as a public health emergency of international concern—the Norwegian authorities have continuously implemented measures to limit the spread of the virus. On top of national policies, each municipality (*kommune*) is responsible for local regulations.

Our independent variable of interest concerns these municipality-level policies, particularly: (1) the closure of shops; (2) the closure of restaurants; (3) the closure of bars (including pubs, nightclubs, etc.); (4) home office requirement; and (5) prohibition of guests at home. We focus on these five restrictions because they are directly related to consumption and expenditure. While there were also regulations on other aspects of daily life (e.g., sports activities, religious gatherings, school), many of these restrictions tended to follow national guidelines, and therefore have no spatial variation.

To obtain the dates when the policies were implemented in each municipality, we hand collected information from official press releases (available on the municipalities' respective websites) and keyword searches in Norwegian national and regional newspapers. Additionally, we obtained structured data from *Verdens Gang* (VG), the most widely read newspaper in Norway (Norwegian Media Businesses' Association, 2021). VG compiled data on national and local regulations as part of their website's COVID-19 tracker, which was updated daily until late 2022. We use these data to cross check the information we manually collected. We collect data from Week 11 of 2020, when restrictions were first put in place, until Week 30 of 2021, the last week in our consumer spending data.

Figure 1 summarizes the presence of restrictions across municipalities and over time. Since national-level measures also apply to municipalities, we included both national and municipality policies in our coding. For example, in Weeks 11–22 of 2020, the national government closed all bars, pubs, and clubs as part of the only nationwide COVID-19 lockdown. As such, our indicator for the closure of bars equals one for all four municipalities in this period. In contrast, in Weeks 9–21 of 2021, restaurants closed in Oslo but not in the other three municipalities. Hence, during this period, our indicator for the closure of restaurants equals one only for Oslo. We also note that in our coding, the indicator equals one only for full-scale restrictions.[1] For example, if in a given municipality, home office was required in the public sector but not the private sector, then the indicator for the home office restriction remains zero. We have opted to code the restrictions in this way to facilitate interpretation of the variables, as there have been exceptions to the restrictions throughout our study period.

2.2 | Consumer spending

Our data on consumer spending come from DNB, one of the largest banks in Norway (DNB, 2021). The data are from the card transactions of around 1.4 million private DNB customers, which is approximately one-third of taxpayers in Norway. The data include all card transactions, both in-person and online, regardless of the payment method (e.g., debit, credit, tap, online, physical card, app, etc.). Moreover, the data contain a variable for municipality, which is based on the municipality where the customer lives. To maintain customer privacy, the data are aggregated at the municipality-week level.

The data cover the period from Week 1 of 2019 to Week 30 of 2021 for the municipalities of Oslo, Bergen, Stavanger, and Trondheim. For each municipality and week, there are two variables on consumer spending: total spending (in Norwegian kroner) and the number of card transactions. Further, these variables are broken down into several categories, which we compile into the following: (1) shopping, which consists of retail trade (e.g., groceries, clothing, books), home (e.g., gardening centers, hardware, interior), and hobby (e.g., toys, arts and crafts, sewing); (2) dining, which includes restaurants, canteens, bakeries, fast food and catering; (3) nightlife, which includes bars, nightclubs,

FIGURE 1 COVID-19 restrictions across municipalities over time. Hand-collected data from official press releases and newspapers. The figure indicates the closure of restaurants, bars, and shops; home office requirement; and prohibition of guests at home.

clubs, taverns, discos, etc.; (4) travel and personal services, which includes airlines, hotels, car rentals, as well as hairdressers and beauty salons; and (5) all other categories.

We use the weekly 2020–2021 data to construct the percentage change in outcomes relative to the same week in 2019. That is, for municipality i in week w of year $y \in \{2020, 2021\}$, our dependent variable is $100 * \frac{s_{i,w,y} - s_{i,w,2019}}{s_{i,w,2019}}$, where the variable s is either spending or number of card transactions. We measure outcomes relative to the same week in 2019 to account for seasonal effects. In our analysis, we exclude data for Week 1 of each year. This is because for Week 1 of 2019, the DNB data are not aggregated from the full week.[2] We also drop data for Week 53 of 2020, since there are only 52 weeks in 2019.

3 | EMPIRICAL METHOD

We conduct three sets of analyses to investigate the effects of COVID-19 policies on consumer spending. As we describe below, each analysis provides us with separate but complementary insights on the implications of restrictions for expenditures. Throughout, our dependent variables y are the percentage change in the value of transactions or the number of transactions, measured relative to the same week in 2019.

3.1 | Number of restrictions as the independent variable

Our first analysis uses the number of restrictions as the independent variable of interest:

$$y_{it} = \alpha + \beta NumRestrictions_{it} + \gamma_i + \phi_q + \epsilon_{it}, \tag{1}$$

where i and t index municipalities and year-weeks respectively. The variable $NumRestrictions_{it}$ is the number of restrictions (up to five) that are in place in each municipality and week. We are interested in the coefficient β, which is useful for beginning to understand the basic relationship between restrictions and consumer spending. The term γ_i represents municipality fixed effects, while ϕ_q are year-quarter (e.g., 2021Q1) fixed effects. Note that because we have only four municipalities per year-week, we cannot rely on a large-sample justification to estimate year-week fixed effects. Therefore, we use year-quarter fixed effects ϕ_q instead to account for time trends.

Since Equation (1) assumes a linear effect of $NumRestrictions_{it}$, we also examine the possibility that the marginal effect of restrictions on spending varies. We hypothesize that restrictions may have a decreasing marginal effect especially in the short term, given that consumers' spending ability is fixed. To investigate this idea, we use ordinal categories for the number of restrictions, as follows:

$$y_{it} = \alpha + \theta_1 \mathbb{1}[\text{One restriction}]_{it} + \theta_2 \mathbb{1}[\text{Two or more restrictions}]_{it} + \gamma_i + \phi_q + \epsilon_{it}. \tag{2}$$

Here, $\mathbb{1}[\cdot]$ is an indicator function: $\mathbb{1}[\text{One restriction}]_{it}$ is a dummy variable that turns on for municipality-weeks for which only one of the five COVID-19 restrictions we study was implemented. The term $\mathbb{1}[\text{Two or more restrictions}]_{it}$ is defined similarly, and the omitted category is no restrictions.[3] If there are decreasing marginal returns to restrictions as we conjectured above (and assuming $\theta_2 < \theta_1 < 0$), then $|\theta_2 - \theta_1| < |\theta_1|$.

3.2 | Type of restrictions as the independent variable

While our first set of regressions focuses on the number of restrictions, our second analysis examines the *type* of restriction. In particular, we estimate the regression

$$\begin{aligned} y_{it} = \alpha &+ \rho_1 \mathbb{1}[\text{Closed shops}]_{it} + \rho_2 \mathbb{1}[\text{Closed restaurants}]_{it} \\ &+ \rho_3 \mathbb{1}[\text{Closed bars and pubs}]_{it} + \rho_4 \mathbb{1}[\text{Required home office}]_{it} \\ &+ \rho_5 \mathbb{1}[\text{No home guests}]_{it} + \gamma_i + \phi_q + \epsilon_{it}. \end{aligned} \tag{3}$$

As before, $\mathbb{1}[\cdot]$ is an indicator for whether shops, restaurants, and/or bars and pubs were shut down; whether home office was required; or whether households were barred from having guests at home. For example, holding all other restrictions constant, the coefficient ρ_1 shows the association between shop closures and spending outcomes, after accounting for municipality fixed effects and time trends over quarters.

3.3 | Difference-in-differences case study

For our third analysis, we use DiD to conduct a case study of a particular episode of restrictions in our sample. In comparison to the previous two correlational analyses, this case study sheds light on the potential *causal* effects of restrictions and the impact of *combinations* of restrictions. The case study we consider is the episode at the end of 2020 when Bergen required home office (from Week 45) and Oslo required home office and closed bars (beginning in Week 44 and 46, respectively). Our case study period is Week 23, the first week when the national lockdown ended, to Week 52 of 2020.

We concentrate on this period because it provides the "cleanest" comparison in our study setting. During this window: (1) Stavanger and Trondheim did not have any of the five restrictions we consider, thus serving as a pure (i.e., untreated) control group; (2) Prior to Week 44, there were no restrictions at all in our four study municipalities, apart from a nationwide lockdown in the earlier part of the year, which ended on Week 22 (see Figure 1). These points are critical for obtaining easily interpretable DiD estimates. As the recent econometric literature on DiD has shown, if we were to use all restriction events in our data and a staggered DiD design, we are likely to obtain biased estimates due to dynamic treatment effects (e.g., Borusyak & Jaravel, 2018; Goodman-Bacon, 2021; Callaway & Sant'Anna, 2021; Sun & Abraham, 2021; de Chaisemartin & D'Haultfoeuille, 2020; Baker et al., 2022; de Chaisemartin & D'Haultfoeuille, 2022; Roth et al., 2022).

Our estimating equation is given by the canonical DiD regression of the form

$$y_{it} = \alpha + \phi_1 HO_i + \phi_2 HOB_i + \phi_3 Post_t + \phi_4 HO_i * Post_t + \phi_5 HOB_i * Post_t + \epsilon_{it}, \tag{4}$$

where HO_i is a dummy for the municipality that implemented only the home office restriction (i.e., Bergen) and HOB_i is a dummy for the municipality that implemented both home office and bars restriction (i.e., Oslo). $Post_t$ is an indicator for weeks on and after Week 44 of 2022, the post-treatment period.[4] The variables HOB_i and HB_i absorb fixed differences between Oslo, Bergen, and the control municipalities of Stavanger and Trondheim. $Post_t$ captures time trends between the pre- and post-treatment periods.[5]

Our coefficients of interest are ϕ_4 and ϕ_5, which are the DiD estimates for the impact of the home office restriction and the combination of home office plus bar closure, respectively. The difference $\phi_5 - \phi_4$ then measures the marginal effect of the bars restriction.

3.4 | Estimation and identification

We estimate all regressions using Ordinary Least Squares, and in the case of Equations (1–3), we use the within estimator for fixed effects. Since our panel data has small N and relatively larger T (i.e., 4 municipalities, 80 weeks), our setting is closer to one with multiple time series rather than true panel data with large N and T. As a result, spurious regression may be a problem if the time series exhibit unit root processes. We conduct Augmented Dickey-Fuller tests to check for unit root in the time series of our outcome variables (see Appendix Table D2). For the vast majority of municipalities and outcomes, we reject the null hypothesis of a unit root.

Throughout all analyses, we use Driscoll-Kraay standard errors to account for the possible correlation of regression errors over time and between municipalities (Driscoll & Kraay, 1998). We set the lag length to 3 for the autocorrelation.[6] We implement the Driscoll-Kraay approach for several reasons. First, this technique does not have any restrictions on the limiting behavior of the size of the cross-sectional dimension. It is thus appropriate for settings with small N and large T (Driscoll & Kraay, 1998; Hoechle, 2007). Second, it has small sample properties that are substantially better than alternative covariance estimators (Driscoll & Kraay, 1998; Hoechle, 2007). Lastly, compared to clustering standard errors, which assumes errors are correlated within but not across clusters, Driscoll-Kray errors are robust to correlation both within and across municipalities. Moreover, clustering is not appropriate for our study since we only have four clusters. This small number results in standard errors that are downward biased, leading to over-rejection (Cameron et al., 2008).

In identifying the effects of COVID-19 policies on consumption, it is important to acknowledge the potential confounding from the COVID-19 infection rate. A high (expected) infection rate may have led policy makers to implement restrictions and closures; at the same time, it may have also induced behavioral changes in consumption in the population. For example, individuals fearful of contracting the virus may have chosen to self-quarantine, avoiding public places such as shops, bars, and restaurants. If these behavioral adjustments led to lower spending, then our estimates for the impact of COVID-19 policies would be larger in magnitude than the true direct effect.

Separating the effects of COVID-19 restrictions from those of the infection rate is methodologically difficult. Indeed, one cannot simply include the (expected) infection rate as a regressor because it is a "bad control" (i.e., the infection rate is itself an outcome of COVID-19 restrictions). This identification challenge is not unique to our research. In similar studies—such as Sheridan et al. (2020), which examines the effects of social distancing laws on economic activity in Scandinavia, as well as Coibion et al. (2020), which investigates the impact of lockdowns on consumer spending across counties in the US—confounding from the infection rate also cannot be ruled out. For these reasons, we prefer to consider our estimates as an upper bound, and this caveat should be borne in mind when interpreting the results.

4 | RESULTS

4.1 | Effects of number of restrictions

Table 1 reports estimates with the number of restrictions as the independent variable. In this linear model, we find that restrictions are negatively correlated with consumption across the board; this is true for all sectors of spending, and whether we measure expenditure in terms of value (Panel A) or number of card transactions (Panel B).

Panel A, column 1 shows that on average, restrictions are associated with a 3.9% point decline in total expenditure, after accounting for municipality characteristics that are constant over time as well as time trends in spending that are common to all municipalities. Columns 2 to 6 show the impact of restrictions on each spending category; the effect sizes are all negative, ranging from −2.6 (Shopping) to −19.5 (Nightlife) percentage points. In Panel B, we use the percentage

TABLE 1 Effects of number of restrictions.

	Total	By spending category				
	All categories (1)	Shopping (2)	Dining (3)	Nightlife (4)	Travel & personal services (5)	All other categories (6)
Panel A: Outcome is percentage change in value of transactions						
No. of restrictions	−3.865***	−2.642***	−14.149***	−19.461***	−7.044**	−2.873**
	(0.973)	(0.847)	(2.734)	(4.294)	(2.747)	(1.287)
Municipality FEs	Yes	Yes	Yes	Yes	Yes	Yes
Year-quarter FEs	Yes	Yes	Yes	Yes	Yes	Yes
N	320	320	320	320	320	320
Panel B: Outcome is percentage change in number of transactions						
No. of restrictions	−5.809***	−4.421***	−10.559***	−17.090***	−8.728***	−5.805***
	(1.435)	(1.156)	(2.632)	(4.096)	(2.814)	(1.523)
Municipality FEs	Yes	Yes	Yes	Yes	Yes	Yes
Year-quarter FEs	Yes	Yes	Yes	Yes	Yes	Yes
N	320	320	320	320	320	320

Note: OLS regression, Driscoll-Kraay standard errors with 3 lags. Data from bank card transactions of DNB. The data are at the municipality-week level, covering the period 2020w2–2021w30 and four municipalities (Oslo, Bergen, Stavanger, and Trondheim). In Panel A, the dependent variable is the percentage change in the value of spending, while in Panel B, it is the percentage change in the number of card transactions, both relative to the same week in 2019. The independent variable is the number of COVID-19 restrictions in place in a given municipality-week (maximum of five in our study).

***$p < 0.01$, **$p < 0.05$, *$p < 0.10$.

change in the number of transactions relative to 2019 as the dependent variable. The results follow a similar pattern, although the effect on total spending is somewhat larger at a 5.8% point decline. The smallest magnitude in the point estimate is again for spending in the Shopping category and the largest is for Nightlife (−4.4 and −17.1% points, respectively).

These results are consistent with those found for other countries, including from studies that have used similar bank card transaction data. For instance, in China, Chen et al. (2021) find that dining, entertainment, and travel were the hardest-hit sectors during the first 3 months of 2020. Additionally, Cox et al. (2020) and Baker et al. (2020) report that food delivery and groceries are the only exception to the decline in consumption in the US during the early phase of the pandemic. Meanwhile, Chronopoulos et al. (2021) show that in the UK, dining expenditure declined by almost 50%, and grocery shopping increased by 30%. Shopping, which includes groceries, tends to be less affected as people replaced dining out with home-cooked meals.

In Table 2, we present results where the regressors are a dummy for one restriction and a dummy for two to five restrictions (with coefficients θ_1 and θ_2, respectively, as described in Equation (2)). It does not seem to matter much for expenditure values whether there is exactly one restriction (overall effect −9.2% points) or more than one restriction (overall effect −9.6% points). The differences between θ_1 and θ_2 for spending outcomes by category are also small: the point estimates are very similar in almost all cases, and their 95% confidence intervals are overlapping. Hence, every additional restriction does not seem to necessarily lead to lower spending. This supports our earlier hypothesis about the decreasing marginal effect of restrictions, because consumer's spending ability is fixed (see Section 3.1). It is also possible that the marginal impact of restrictions declines because of spillovers in the effect of spending. For example, the closure of bars may impact spending not only in nightlife, but also in the shopping and dining sectors. Our analysis in the next sub-sections speak to this idea.

TABLE 2 Effects of number of restrictions (ordinal categories).

	Total	By spending category				
	All categories (1)	Shopping (2)	Dining (3)	Nightlife (4)	Travel & personal services (5)	All other categories (6)
Panel A: Outcome is percentage change in value of transactions						
𝟙[# Restrictions = 1]	−9.245***	−3.433	−33.957***	−49.693***	−26.098***	−9.903**
	(3.440)	(2.494)	(7.618)	(8.568)	(8.768)	(4.138)
𝟙[# Restrictions ≥2]	−9.560***	−4.989	−40.789***	−59.785***	−19.792***	−8.099**
	(3.019)	(3.088)	(5.673)	(7.636)	(6.594)	(3.113)
Municipality FEs	Yes	Yes	Yes	Yes	Yes	Yes
Year-quarter FEs	Yes	Yes	Yes	Yes	Yes	Yes
N	320	320	320	320	320	320
Panel B: Outcome is percentage change in number of transactions						
𝟙[# Restrictions = 1]	−16.162***	−11.321***	−30.252***	−46.341***	−29.039***	−17.683***
	(4.271)	(3.378)	(7.314)	(8.300)	(8.536)	(4.555)
𝟙[# Restrictions ≥2]	−15.059***	−10.622***	−29.473***	−53.078***	−24.399***	−15.888***
	(3.510)	(3.216)	(5.490)	(7.154)	(6.510)	(3.571)
Municipality FEs	Yes	Yes	Yes	Yes	Yes	Yes
Year-quarter FEs	Yes	Yes	Yes	Yes	Yes	Yes
N	320	320	320	320	320	320

Note: OLS regression, Driscoll-Kraay standard errors with 3 lags. Data from bank card transactions of DNB. The data are at the municipality-week level, covering the period 2020w2–2021w30 and four municipalities (Oslo, Bergen, Stavanger, and Trondheim). In Panel A, the dependent variable is the percentage change in the value of spending, while in Panel B, it is the percentage change in the number of card transactions, both relative to the same week in 2019. The independent variables are indicators. 𝟙[# Restrictions = 1] is a dummy variable that turns on for municipality-weeks for which only one of the five COVID-19 restrictions we study was implemented. 𝟙[# Restrictions ≥2] is defined similarly. The omitted category is no restrictions.

***$p < 0.01$, **$p < 0.05$, *$p < 0.10$.

4.2 | Effects of type of restrictions

Table 3 shows results on the effects of the *type* of restrictions on consumption value (Panel A) and the number of card transactions (Panel B). The table reveals three findings. First, the estimates show that, as expected, restrictions tend to have larger impacts on the sector in which it is targeted. For instance, the largest effect of the closure of shops is on shopping expenditure, both in spending amounts and the number of card transactions (−14.0 and −10.9% points,

TABLE 3 Effects of types of restrictions.

	Total	**By spending category**				
	All categories (1)	**Shopping (2)**	**Dining (3)**	**Nightlife (4)**	**Travel & personal services (5)**	**All other categories (6)**
Panel A: Outcome is percentage change in value of transactions						
𝟙[Closed shops]	−10.565***	−14.002***	−12.521***	−5.506	−3.562	−5.606*
	(2.969)	(4.425)	(4.263)	(4.893)	(4.122)	(3.308)
𝟙[Closed restaurants]	2.110	3.384	−5.969	−15.241**	−0.535	2.694
	(2.877)	(4.669)	(5.204)	(6.596)	(3.830)	(3.072)
𝟙[Closed bars/pubs]	−10.302***	−3.956	−37.039***	−54.486***	−28.152***	−10.698**
	(3.584)	(2.624)	(7.686)	(8.371)	(9.330)	(4.498)
𝟙[Required home office]	1.079	0.921	−5.779*	−8.910***	5.020	1.331
	(1.737)	(1.640)	(3.208)	(3.351)	(3.566)	(1.736)
𝟙[No home guests]	−1.293	−2.091	0.003	3.945	−1.480	−0.018
	(2.666)	(2.121)	(4.890)	(4.793)	(4.952)	(2.205)
Municipality FEs	Yes	Yes	Yes	Yes	Yes	Yes
Year-quarter FEs	Yes	Yes	Yes	Yes	Yes	Yes
N	320	320	320	320	320	320
Panel B: Outcome is percentage change in number of transactions						
𝟙[Closed shops]	−10.213***	−10.873***	−8.660***	−3.549	−4.778	−9.767***
	(2.588)	(3.091)	(2.978)	(3.277)	(4.096)	(2.318)
𝟙[Closed restaurants]	−1.927	−1.457	−5.902*	−11.900**	−3.688	0.755
	(2.283)	(3.070)	(3.516)	(4.965)	(3.554)	(2.159)
𝟙[Closed bars/pubs]	−16.716***	−11.434***	−31.013***	−49.644***	−30.832***	−18.725***
	(4.675)	(3.784)	(8.005)	(8.483)	(9.164)	(4.859)
𝟙[Required home office]	−0.393	−0.537	−2.985	−8.553***	3.536	0.350
	(1.847)	(1.616)	(2.944)	(2.735)	(3.145)	(2.161)
𝟙[No home guests]	3.338	3.584	4.246	4.843	−0.788	2.317
	(2.976)	(3.014)	(4.184)	(3.186)	(4.277)	(2.257)
Municipality FEs	Yes	Yes	Yes	Yes	Yes	Yes
Year-quarter FEs	Yes	Yes	Yes	Yes	Yes	Yes
N	320	320	320	320	320	320

Note: OLS regression, Driscoll-Kraay standard errors with 3 lags. Data from bank card transactions of DNB. The data are at the municipality-week level, covering the period 2020w2–2021w30 and four municipalities (Oslo, Bergen, Stavanger, and Trondheim). In Panel A, the dependent variable is the percentage change in the value of spending, while in Panel B, it is the percentage change in the number of card transactions, both relative to the same week in 2019. The independent variables are indicators for the closure of shops, restaurants, bars/pubs and the like, home office requirement, and prohibition of guests at home.
***$p < 0.01$, **$p < 0.05$, *$p < 0.10$.

respectively). In a similar vein, the largest impact of the closure of bars is in the nightlife category: holding all other restrictions constant, the closure of bars is associated with a 54.5 and 49.6% point decline in the spending and number of transactions, respectively. Consumption reductions of this magnitude are also observed in other countries with such restrictions (see Appendix A).

Second, we see spillover effects: restrictions in one expenditure category also impacts spending in other categories. In particular, the closure of shops affects not only shopping expenditure, but also dining out (−12.5% points). We find the same pattern of results for the closure of bars, which is the restriction that has the largest effect on spending across all outcomes. The closure of bars has large negative effects not only on nightlife spending, but also on shopping, dinning, travel and other personal services, and other categories (−4.0 to −37.0% points).

Third, not all types of restrictions are equal: the closure of bars/pubs, shops, and to some extent restaurants, individually have a stronger correlation with total spending than the home office requirement and the ban on home guests. Indeed, Table 3 shows that the point estimates for the effects of the closure of shops, restaurants, and bars tend to be larger in magnitude and more statistically significant than the point estimates for the home office and home guests restrictions. This is intuitive because the restrictions on home office and home guests have a less clear-cut effect on spending. Moreover, in our study setting, both were not strictly enforced.

4.3 | Difference-in-differences estimates

The regression estimates in the previous two subsections are mostly correlational. To better understand the potential causal effects, we conduct a case study using DiD. Table 4 presents the DiD estimates.[7] The table shows two patterns that are suggestive of a causal interpretation of the correlations we found in Tables 2 and 3.

First, our DiD estimates indicate that the “home office only” and “home office and bar closure” treatments had similar effects. Table 4 shows that the coefficient estimates on these two variables tend to have similar magnitudes, whether we measure spending by consumption value (Panel A) or number of transactions (Panel B). Moreover, the difference between the two coefficients is generally not statistically significant at the 10% level. We interpret this pattern as evidence consistent with the correlations we find in Section 4.1 and Table 2, that is, that additional restrictions have a decreasing marginal effect on consumption, and that one restriction has similar impacts as two to five restrictions. In DiD estimates in Table 4, we find that one restriction (home office only) has a negative impact on spending, and having two restrictions (home office and bar closure) yields an effect of analogous size. Thus, according to our DiD estimates, the marginal impact of the additional restriction (bar closure) appears to be very small or close to zero.

Second, the DiD results in Table 4 is in line with our main finding in Table 3 that the type of restriction matters for spending. In the correlations in Table 3, we find that restrictions have larger effects on the sector in which it is targeted —i.e., the closure of bars and pubs had the largest effect on nightlife spending. This pattern is also evident our DiD results in Table 4, where we find that it is only the combination of home office and bar closure (and not home office alone) that has an impact on nightlife expenditure. Indeed, although the marginal effect of bars beyond the home office restriction is generally small, it is the largest for nightlife spending.[8]

5 | CONCLUSION

Bank card data from one of the largest banks in Norway, together with hand-collected data on municipal COVID-19 restrictions, allow us to quantify the economic effects of non-pharmaceutical pandemic policies. Our results show that restrictions have decreasing marginal effects, indicating that policy makers should be aware that there may be a threshold beyond which additional measures no longer have any impact. Moreover, the *type* of policies is important, and policies do not affect all types of spending equally. We find that the specific sectors targeted by restrictions are hit hardest both in spending value and number of card transactions. Hence, in future pandemic situations, policymakers may also consider directing support interventions to the same sectors targeted by restrictions.

This paper joins a growing literature that has used bank card transactions and other private sector data to examine the economic implications of COVID-19 (e.g., Chetty et al., 2023). While our study in particular is retrospective, it underscores private sector data as a critical resource for real-time policy guidance during health or economic crises. The global community continues to face the threat of subsequent, deadly infectious disease outbreaks. Therefore,

TABLE 4 Difference-in-difference estimates.

	Total	By spending category				
	All categories (1)	Shopping (2)	Dining (3)	Nightlife (4)	Travel & personal services (5)	All other categories (6)
Panel A: Outcome is percentage change in value of transactions						
Home office × post	−1.052	−0.839	−10.211**	0.058	0.127	−0.523
	(1.257)	(0.610)	(3.933)	(4.518)	(1.362)	(2.372)
Home office & bars × post	−2.963***	−0.091	−20.337***	−24.520***	3.120***	−3.824*
	(0.910)	(0.865)	(5.132)	(6.223)	(0.830)	(1.936)
Post	1.560	1.550	−10.339***	−21.677***	−8.648	0.978
	(1.884)	(2.293)	(3.410)	(3.346)	(5.181)	(1.936)
Home office	−1.014	0.136	−5.166*	−6.485*	0.266	−2.374*
	(0.670)	(0.392)	(2.865)	(3.437)	(0.797)	(1.200)
Home office & bars	−0.358	−0.515	7.613***	6.439***	0.832	−1.265
	(0.583)	(0.644)	(0.876)	(1.852)	(0.526)	(1.320)
Constant	−2.691***	11.828***	−13.590***	−43.534***	−53.182***	−10.384***
	(0.624)	(0.821)	(2.174)	(2.541)	(4.276)	(1.132)
H_0: $\phi_4 = \phi_5$, p-val	0.284	0.283	0.194	0.004	0.007	0.306
Municipality FEs	No	No	No	No	No	No
Year-quarter FEs	No	No	No	No	No	No
N	120	120	120	120	120	120
Panel B: Outcome is percentage change in number of transactions						
Home office × post	−3.732*	−3.715**	−11.815***	−3.398	−2.853*	0.035
	(1.833)	(1.423)	(3.778)	(3.258)	(1.570)	(2.318)
Home office & bars × post	−4.116***	−2.367***	−11.943***	−19.306***	0.518	−2.847***
	(0.749)	(0.636)	(1.929)	(5.398)	(1.186)	(0.857)
Post	−4.646***	−3.574**	−7.889***	−19.147***	−9.652*	−6.799**
	(1.595)	(1.382)	(1.995)	(2.700)	(4.837)	(2.715)
Home office	−0.746	−1.227*	−2.573	−3.982	1.860**	−1.144
	(1.027)	(0.711)	(2.817)	(3.054)	(0.817)	(1.007)
Home office & bars	1.434**	−0.680	7.588***	2.403	−0.527	3.004***
	(0.575)	(0.599)	(0.618)	(1.652)	(0.934)	(0.872)
Constant	−4.792***	1.402*	−24.206***	−50.100***	−40.214***	−2.292*
	(0.817)	(0.719)	(1.471)	(2.198)	(3.584)	(1.253)
H_0: $\phi_4 = \phi_5$, p-val	0.849	0.379	0.976	0.003	0.113	0.258
Municipality FEs	No	No	No	No	No	No
Year-quarter FEs	No	No	No	No	No	No
N	120	120	120	120	120	120

Note: OLS regression, Driscoll-Kraay standard errors with 3 lags. Data from bank card transactions of DNB. The data are at the municipality-week level, covering the period 2020w23–2020w52 and four municipalities (Oslo, Bergen, Stavanger, and Trondheim). In Panel A, the dependent variable is the percentage change in the value of spending, while in Panel B, it is the percentage change in the number of card transactions, both relative to the same week in 2019. *Home Office* is a dummy for the municipality that implemented only the home office restriction (i.e., Bergen) and *Home Office & Bars* is a dummy for the municipality that implemented both home office and bars restriction (i.e., Oslo). *Post* is an indicator for weeks on and after Week 44 of 2022. The parameter ϕ_4 corresponds to the coefficient on *Home Office* × *Post*, and the parameter ϕ_5 corresponds to the coefficient on *Home Office & Bars* × *Post*.

$^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.10$.

policymakers should contemplate establishing systems and infrastructure that facilitates swift access to such data for research and policy analysis, as part of the strategy for pandemic preparedness and response.

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DATA AVAILABILITY STATEMENT

The data that supports the findings of this study are available from DNB. Restrictions apply to the availability of these data, which were used with permission for this study. Please contact DNB to obtain access to the data. The replication package is available online; see Carpena et al. (2023).

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ENDNOTES

[1] Appendix C provides more details on our coding.

[2] In particular, Week 1 of 2019 is from December 31, 2018 to January 6, 2019, but spending for December 31, 2018 was not included when aggregating the data to weeks, as DNB provided data only from 2019 onwards.

[3] Conditional on having any restriction, 74 percent of observations have exactly one restriction, so we opt for the three categories: no restriction, only one restriction, and two or more restrictions.

[4] Note that even though the "treatment" was implemented in weeks 44–46, for simplicity, we choose week 44 to define the cutoff date for the start of the "post" period.

[5] Because of the small sample size in this case study, we do not include separate fixed effects for each year-quarter and for Stavanger and Trondheim, as we do in Equations (1–3).

[6] The lag of 3 is based on the heuristic in Newey and West (1994), which suggests setting the lag length to floor $[4(T/100)^{2/9}]$. To assess robustness, we also used 2 or 4 and found qualitatively similar results.

[7] Plots of the raw data before and after the treatment (Week 44 of 2020) are shown in Appendix E.

[8] In our correlational analysis of the effects of different *types* of restriction (Section 4.2 and Table 3), we also found evidence suggesting spillover effects and that the closure of bars, shops, and to some extent restaurants, individually have a stronger correlation with total spending than the home office requirement and the ban on home guests. To test these ideas using DiD, we would need episodes where exactly one restriction was in place at a time, so that we can compare each restriction individually with each other (e.g., bars closure only vs. home office only). In our study, such a comparison is not available.

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SUPPORTING INFORMATION

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