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MOBILITY BEFORE GOVERNMENT RESTRICTIONS IN THE WAKE OF COVID-19

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People's retail and recreation mobility, a proxy for consumption, correlated negatively with the increase in the number of COVID-19 cases and mortality rate even before the implementation of national stay-at-home orders. This provides suggestive evidence that epidemiological developments per se may affect household spending and economic activity, even in the absence of mandatory mobility restrictions.

To curb the COVID-19 epidemic and mitigate the pressure on national health systems, most countries imposed restrictions like school closures, sheltering recommendations, the shutdown of non-essential economic activities, and stay-at-home orders. Those measures have been successful in drastically reducing the circulation of the virus and the number of deaths (see for example Vinceti et al., July 2020 for the Italian case), but many have questioned their economic costs.

In this note, we provide suggestive evidence that mobility (and hence economic activity) was falling rapidly before governments took action to mitigate the epidemic. The reduction appears correlated to the arrival of bad epidemiological news. In particular, an increase in the number of reported COVID-19 cases and mortality consistently correlates with reduced leisure mobility, i.e. mobility around restaurants, cafes, shopping centers, theme parks, museums, libraries, and movie theatres (places where people spend their free time and consume). Hence, epidemiological developments *per se* may have contributed to reducing household consumption and, therefore, economic activity, even before the implementation of nationwide stay-at-home orders.[2] In the same vein, the October IMF WEO Chapter 2 notably concludes: "although easing lockdowns can lead to a partial recovery, economic activity is likely to remain subdued until health risks abate".[3] This prediction is also consistent with

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[1] Bank of Italy. The views expressed herein represent those of the authors and do not necessarily reflect those of the Bank of Italy. The authors are in debt to Alessandro Borin, Andrea Brandolini, Pietro Antonio Catte, Andrea Finicelli, and Giovanni Veronese for the insightful discussions and valuable comments. All remaining errors are the sole responsibility of the authors. This note replaces a previous version released on November 3, 2020 with the title "Mobility and Government restrictions in the wake of Covid-19".

[2] On this line, the Special Survey of Italian Households reported in the Bank of Italy Economic Bulletin 4/2020 displays that a relevant share of households (half of those that had planned to go on vacation) revised their plans because of the fear of contracting the virus.

[3] For similar findings, see Andersen et al. (2020) for Sweden and Denmark, and Goolsbee and Syverson (2020) for the US. Focusing on re-openings, Franks et al. (2020), find that reopening policies explained the bulk of subsequent mobility activity with a limited role played by voluntary social distancing. The asymmetric role of

the idea that individuals increase their precautionary savings to weather individual health risks (Ercolani, 2020). A caveat is in order: as a reduction of mobility negatively affects the virus spread by reducing the interactions among households (and, as a consequence, the infection risk), our exercise may suffer from a problem of endogeneity. We discuss more extensively in the text our approach to mitigate this issue.

Contagion and leisure-mobility aside from the lockdown

In order to document the relationship between mobility and the evolution on the epidemic, we rely on three data sources. The European Centre for Disease Prevention and Control (ECDC) provides daily data on epidemiological variables as cases and deaths, together with population, for most territories around the world starting from December 31, 2019. The Google COVID-19 Community Mobility Reports contain daily information at the country level on several types of mobility (for example, indicators associated with retail & recreation – our indicator for leisure mobility –, workplaces, and transit stations).[4] The Oxford Coronavirus Government Response Tracker (OxCGRT) summarises information on restrictive policies enforced by governments of more than 180 countries on a daily basis. Every single measure adopted by a government (as stay-at-home order, school closures, closure of workplace, restrictions on international travel, and public gatherings) is coded as an ordinal variable based on its intensity, with a flag indicating whether a policy is adopted nationwide or only locally. Some of these variables enter into the synthetic indicator of the level of restrictions in a country on a given date, which ranges between 0 (no restrictions) and 100 (highest level of restrictions; Hale et al. 2020).

We assume the following relationship, for country i and day t:

$$y_{i,t} = \beta_0 + f(epidemic_{i,t-1}) + \gamma X_{i,t} + \epsilon_{i,t}, \qquad (1)$$

where $y_{i,t}$ represents the leisure-mobility reduction, $epidemic_{i,t-1}$ is the lagged value of epidemiological variables (the log of reported total cases and mortality in the previous day), $X_{i,t}$ is a set of controls, and $\epsilon_{i,t}$ is the error term. The sample includes daily data for all EU countries (except Cyprus for which mobility data are not available) and the UK.

Our objective is to assess the effect of epidemic diffusion on voluntary mobility reductions before the implementation of governments' nationwide stay-at-home orders. To this aim, we exploit two empirical strategies. In the first exercise (Table 1), we consider only the countries that eventually introduced a nationwide stay-at-home order, but we restrict the sample only to the days before the order was actually implemented. Then, we test the results including also countries that did not enforce a nationwide stay-at-home order (like Sweden), but we limit the time-span to a few weeks after the first reported case; hence, we consider all countries in the sample up to 14, 21, 28 days after the occurrence of the first case of COVID-19 in each country (Table 2). A limited time-span allows us to compare countries adopting different policies. Lastly, as a robustness check, we replicate the above exercise considering new cases instead of cumulated cases as the main dependent variable (Table 3).

It is important to highlight that a specification that links epidemiological variables to mobility may suffer from reverse causality. On the one hand, the virus spread induces people to limit their mobility; on the other hand, the reduction of mobility negatively affects the reported epidemiological variables, although with some lags. The two effects move in the opposite direction. To mitigate the aforementioned reverse causality issue, we use the lagged value of each epidemiological variable. In

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voluntary social distancing in shaping the mobility before lockdown and after re-openings may be attributable to several reasons. First, the outbreak of the first wave was largely an unknown phenomenon; second, the reaction of people to the epidemic may exhibit non-linear behavior; third *lockdown fatigue* may kick in when restrictions are in place for a long period.

[4] Starting from February 15, each indicator is computed with respect to a benchmark level set to the period January 3 – February 6.

addition, as we are using reports on cases and mortality rates, these variables would reflect the infections contracted days before rather than those generated at time *t*, because of the existing lags associated with incubation, testing, and reporting. Finally, as our sample spans a short period of time following the reporting of the first COVID-19 case, it is likely that the effects of mobility on the epidemic had not yet fully unfolded at the time, suggesting that they were limitedly observable in our sample.

The main result of the exercises is that the reduction of leisure-mobility is positively and significantly associated with the (log) number of cases, with a remarkably stable coefficient across different specifications.

In particular, in Table 1, we also provide results controlling for the mortality rate (number of deaths per 1 million inhabitants; column 2); for the lagged number of cases in neighboring countries[5] (column 3); for the GDP per capita in 2018 (column 4). We observe that mortality is positively correlated to the reduction in mobility though the estimated coefficient is not always statistically different from zero. This may derive from the fact that at the initial stage of the epidemic, i.e. the period considered in our sample, the death toll was still too low to influence people's decisions and their mobility. Additionally, the number of cases is positively correlated with the mortality rate. When we introduce the epidemic in neighboring countries as a control, we see that the coefficient of the number of cases halves: this happens because the control likely contains some time-trend common to log cases; in fact, its coefficient is no longer significant when we introduce month and country fixed effects.

Table 1. Regression of mobility reduction on *total* cases
(sample includes only countries implementing nationwide stay-at-home orders, but in days before their implementation)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
log cases (lag)	0.062***	0.062***	0.038***	0.035***	0.067***	0.060***	0.066***
	(0.004)	(0.005)	(0.005)	(0.005)	(0.013)	(0.015)	(0.020)
mortality rate (lag)		0.002	0.003	0.023**		0.037*	0.023*
		(0.012)	(0.024)	(0.010)		(0.020)	(0.013)
log subregion cases (lag)			0.032***	0.031***			0.034
			(0.003)	(0.004)			(0.024)
log GDP p.c. 2018				-0.104***			
				(0.015)			
constant	-0.073***	-0.073***	-0.184***	0.900***	-0.092*	-0.077	-0.534***
	(0.011)	(0.012)	(0.016)	(0.158)	(0.050)	(0.056)	(0.142)
Country FE					Yes	Yes	Yes
Weekend FE							Yes
Month FE							Yes
Observations	426	426	426	426	426	426	426
Adjusted R^2	0.319	0.324	0.388	0.438	0.466	0.490	0.607

Standard errors in parentheses. Regressions (1)-(4): robust standard errors. Regressions (5)-(7): clustered standard errors. The sample consists of data for countries that issued a nationwide stay-at-home order before its starting date. The set of countries is Austria, Belgium, Croatia, Czechia, France, Greece, Hungary, Ireland, Italy, Luxembourg, the Netherlands, Poland, Portugal, Romania, Slovakia, Spain, the UK.
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

In columns (5) and (6), we include country fixed effects, while in column (7) we introduce monthly and weekend fixed effects. Those effects take into account unobserved heterogeneity at the country level (including social norms, the structural use of on-line shopping as opposed to physical mobility,

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[5] We use United Nations geoscheme (available at https://unstats.un.org/unsd/methodology/m49/) to identify regions. According to this classification, there are four regions in Europe: Eastern Europe, Northern Europe, Southern Europe, and Western Europe.

the health system situation). Results are remarkably stable, with the correlation between cases and mobility being 0.066 in our favored specification (column 7). This coefficient means that a one percentage point increase in the number of cases is associated with a 0.066% reduction in mobility.

In Table 2, we show the results of our main specification (column 7 in Table 1) considering the sample of all EU countries and the UK and cutting the sample for each country up to 14, 21, and 28 days after the first case.[6] The coefficient of the number of cases increases to 0.09-0.1, depending on the specification. In the specifications, we observe that the coefficient for the mortality rate is positive and not statistically significant in columns (1) and (2), which correspond to the 14- and 21-day time-span, whereas it becomes significant in column (3), where the time-span is 28 days, with a magnitude similar to that of column (7) in **Errore. L'origine riferimento non è stata trovata.**. The fact that mortality becomes significant as we increase the time-dimension of the panel seems to suggest that reported deaths play a role in mobility reduction as they become large enough.

Finally, Table 3 shows that the relation holds also if we consider new daily cases instead of total cases though the magnitude is lower. We observe that the epidemic evolution in the neighboring countries matters, unlike what happens in **Errore. L'origine riferimento non è stata trovata.** and Table 2.

Table 2. Regressions of mobility reduction on *total* cases
(sample includes all EU countries; only days before nationwide stay-at-home orders are included)

	(1)	(2)	(3)
log cases (lag)	0.098***	0.098***	0.088***
	(0.025)	(0.020)	(0.022)
mortality rate (lag)	0.130	0.061	0.020***
	(0.111)	(0.48)	(0.007)
log subregion cases (lag)	-0.013	0.017	0.028
	(0.022)	(0.021)	(0.025)
constant	-0.095	-0.291***	-0.451***
	(0.111)	(0.095)	(0.121)
Country FE	Yes	Yes	Yes
Weekend FE	Yes	Yes	Yes
Month FE	Yes	Yes	Yes
Observations	254	402	538
Adjusted R^2	0.442	0.686	0.725

Standard errors in parentheses. Regressions (1)-(3): clustered standard errors. The sample consists of all EU countries (except Cyprus) and the UK before a nationwide stay-at-home order is implemented, if any. Column (1) refers to mobility data 14 days after the first case is reported in each country, column (2) to mobility 21 days after, and column (3) to mobility 28 days after.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 3. Regressions of mobility on *new* cases
(sample includes all EU countries; only days before nationwide lockdowns are included)

	(1)	(2)	(3)
log newcases (lag)	0.065**	0.068***	0.069***
	(0.023)	(0.017)	(0.014)
mortality rate (lag)	0.149	0.079	0.021*
	(0.111)	(0.067)	(0.011)
log subregion new cases (lag)	0.061***	0.086***	0.093***
	(0.017)	(0.019)	(0.015)
constant	-0.345***	-0.466***	-0.626***
	(0.097)	(0.098)	(0.102)
Country FE	Yes	Yes	Yes

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[6] Note that we drop all the subsequent observations for that country if a nationwide stay-at-home order is enforced in a given country at date *t* (before the cutting date).

Weekend FE	Yes	Yes	Yes
Month FE	Yes	Yes	Yes
Observations	190	295	383
Adjusted R^2	0.438	0.651	0.703

Standard errors in parentheses. Regressions (1)-(3): clustered standard errors. The sample consists of all EU countries (except Cyprus) and the UK before a nationwide stay-at-home order is implemented, if any. Column (1) refers to mobility data 14 days after the first case is reported in each country, column (2) to mobility 21 days after, and column (3) to mobility 28 days after.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

All in all, the exercises point at the fact that epidemic bad news (an increase of reported cases) decreases people's mobility even before national governments implemented stay-at-home orders in their respective countries.

Implications for consumption and GDP

The existence of a positive relation between mobility reduction and COVID-19 cases and deaths, even before the implementation of nationwide stay-at-home orders, raises an important question. How much of the contraction of economic activity is due to people voluntarily choosing to stay at home to avoid infection as opposed to formal restrictive measures introduced by governments?

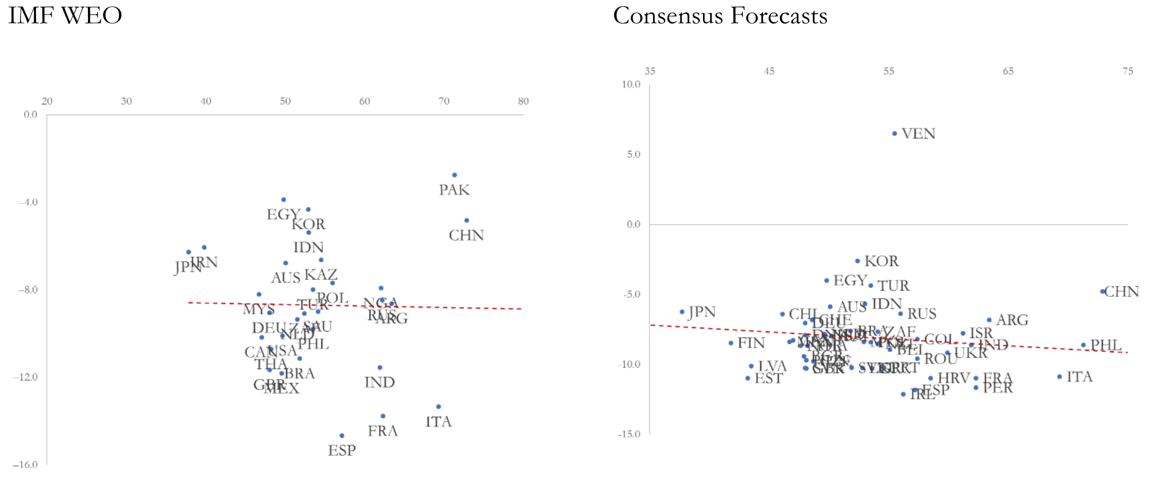
Disentangling these two factors is a non-trivial task. First, there is a measurement problem for both government restrictions measures and behavioral choices. As far as it concerns government measures, the OxCGRT indicators provide us with a qualitative and quantitative assessment of the different policies adopted at the country level. However, it does not contain detailed information either on the share of the population targeted (when measures do not have a nationwide scope) or on the number of economic sectors involved by a restriction (Conteduca et al, 2020). Concerning people's choices, no direct measure of the voluntary reduction of consumption is available at the data frequency required (daily). Some authors (Andersen et al., 2020) use financial transaction data to gauge the reduction of private consumption. Second, it is crucial to define a proper identification strategy to retrieve the causal effect on the economic outcome stemming from the above channels. Since it is not possible to observe what would have happened in the same country (both in terms of epidemiologic dynamic and economic outcome) with and without restrictive measures, an accurate definition of the counterfactual is essential to compare results with or without legal restrictions. In particular, when one introduces both restrictions and epidemiological variables in the same regression to evaluate which share of economic contraction stems from each channel, one has to consider that the observed variables are equilibrium outcomes. On the one hand, the adoption of measures aiming at reducing physical proximity among the population reflects in the number of new cases and deaths, changing the underlying data generating process for the epidemic variables. On the other hand, the evolution of the epidemic calls for different restrictions depending on its severity. Hence, because of this simultaneity and the fact that the relation between the two sets of variables is unknown, it is hard to pin down neatly the contribution of each channel.

Ongoing economic research is striving to tackle these different issues and provide some preliminary answers, using microdata that helps design *ad hoc* identification strategies. Coibion et al. (2020) analyze how consumption and expectations of economic outcomes have been affected by COVID-19 using panel data on US households. The data allow the comparison at the county level between spending patterns and expectations in January 2020 (before the outbreak of COVID-19) and April 2020 (after the outbreak). By comparing counties with and without lockdowns, they find that the lockdown accounts for most of the economic costs of the pandemic. Their identification strategy relies on the observation that mobility dropped much more after a lockdown than after the occurrence of the first reported case of COVID-19. However, this strategy may fail if individual behavior responds more to the number of cases and deaths than to the occurrence of a single reported case.[7] Other studies, by contrast, find a negligible effect of government restrictions on consumption. Andersen et al. (2020) use transactional data for Denmark and Sweden, two countries with similar institutions and characteristics that were similarly exposed to the pandemic, at least in the very few weeks, but adopted different restrictive measures. Their findings suggest that the epidemic itself rather than government restrictions caused the observed economic contraction. In particular, they find that aggregate spending dropped by around 25 percentage points in Sweden, and only by 4 additional points in Denmark because of the shutdown.

Goolsbee and Syverson (2020) estimate the effect of the lockdown on the economy during the initial spread of COVID-19 in the US using as dependent variable foot traffic at individual businesses. Their empirical strategy separates the effects of voluntary distancing from that of government restrictions by comparing differences across jurisdictions facing differing legal restrictions. The empirical specification leverages two sources of variation: different timing of shelter-in-place (SIP) orders and different choices concerning the SIP orders. They show that the high coefficient found in a regression of foot traffic around businesses on county-level SIP order (-0.714) becomes ten times lower (-0.076) when fixed effects at the commuting-zone-by-week level are introduced. In other words, once comparing similar establishments within a commuting zone but adopting different SIP policies, then the lockdown accounts for a small fraction of the overall change in consumption behavior. More precisely, foot traffic fell by 60 percentage points, only 7 points of which is attributable to legal restrictions.

Macroeconomic forecast revisions may provide suggestive, if indirect, evidence on the impact of government restrictions. The IMF forecast revisions of GDP growth in June 2020, relative to the October 2019 estimates, are negative for all countries but show virtually no correlation with the average OxCGRT restrictiveness index computed between February and May (Figure 1a). Findings are similar by considering forecast revisions of 2020 GDP growth of Consensus Forecasts in June with respect to the pre-COVID-19 estimates as collected in December 2019 (Figure 1b).

Figure 1. Revisions of GDP Growth Forecasts and Restrictions


IMF WEO

Consensus Forecasts

Source: based on Consensus Forecasts, IMF WEO October 2019 and WEO-update June 2020, and Oxford Coronavirus Government Response Tracker.

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[7] For comparison, using their framework, one would consider similarly the voluntary reductions in mobility induced by the cases of two Chinese tourists in Rome, who tested positive on January 30, and by those reported in the area of Codogno starting from February 20.

Conclusion

An epidemic may reduce households' willingness to consume because individuals respond to bad news by limiting activities that can be risky for their health (see also Gans, 2020). Research is trying to assess the relative contribution of this precautionary behavior relative to mandatory restrictive measures in the drop of worldwide GDP following COVID-19, but consensus has yet to be reached. Disentangling the economic effects of voluntary social distancing from those of government restrictions would help policy design as the second wave hits Europe. When evaluating public health effects, we should consider that restrictive measures may facilitate effective behavioral responses, as they mitigate coordination costs and contain negative externalities from risky individual behavior, especially if combined with additional measures like testing and tracing or mandatory protective equipment.

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