Social distancing laws cause only small losses of economic activity during the COVID-19 pandemic in Scandinavia

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This paper uses real-time transaction data from a large bank in Scandinavia to estimate the effect of social distancing laws on consumer spending in the coronavirus 2019 (COVID-19) pandemic. The analysis exploits a natural experiment to disentangle the effects of the virus and the laws aiming to contain it: Denmark and Sweden were similarly exposed to the pandemic but only Denmark imposed significant restrictions on social and economic activities. We estimate that aggregate spending dropped by around 25% (95% CI: 24 to 26%) in Sweden and, as a result of the shutdown, by 4 additional percentage points (95% CI: 3 to 5 percentage points [p.p.]) in Denmark. This suggests that most of the economic contraction is caused by the virus itself and occurs regardless of social distancing laws. The age gradient in the estimates suggests that social distancing reinforces the virus-induced drop in spending for low-health-risk individuals but attenuates it for high-risk individuals by lowering the overall prevalence of the virus in the society.

COVID-19 | consumer spending | social distancing | shutdown |

One of the key policy choices facing governments in the coronavirus 2019 (COVID-19) pandemic is whether to shut down economic activity to slow the spread of the disease. Many types of consumption take place in settings with a high density of people (e.g., restaurants, public transit) or involve direct physical contact (e.g., hairdressers, dentists) and contribute to the disease spreading. Other types of consumer spending (e.g., retail shopping) involve proximity to shop assistants and other consumers, also posing risks. Most governments have implemented social distancing laws that restrict some of these economic activities. However, the severity of these restrictions varies considerably across countries and many governments are considering day by day whether to loosen, maintain, or tighten restrictions in response to new data on COVID-19 cases and mortality.

The policy choice facing governments is often portrayed as a simple trade-off between saving lives and saving the economy. By this logic, more severe restrictions help to contain the virus and reduce the ultimate death toll, but they cause more economic pain through lower consumer spending, increased business failures, and higher unemployment.* However, this view does not account for the possibility that the virus itself may inflict significant harm on the economy, with people cutting back on consumption and work due to personal health risks or because they internalize the externalities of their own economic activities for the health of others. If households cut back on spending voluntarily, a mandated shutdown may have only a small effect. Furthermore, this points to an indirect mechanism by which government-mandated shutdowns can actually improve economic outcomes: By reducing the spread of the virus, shutdowns can make people more comfortable with going outside, spending money, and working. If the virus deters economic activity due to personal health risks, then this indirect mechanism is most likely to improve economic outcomes for those who are most likely to suffer severe health consequences from contracting COVID-19, in particular the elderly. Indeed, shutting down sectors with high social proximity may constrain the choices of low-risk individuals (e.g., the young), who would otherwise contribute most to the spreading of the virus, and thus induce high-risk individuals (e.g., the elderly) to choose a higher level of economic activity as a result of the reduced probability of contracting the virus.

We study empirically how social distancing laws affect aggregate consumer spending in a pandemic. Our empirical design draws on a salient natural experiment in Scandinavia and individual-level bank account data from the second largest bank in the region for the period 1 January 2018 to 5 April 2020. Despite very similar early outbreaks of COVID-19, the neighboring countries Denmark and Sweden took very different policy responses. Starting 11 March 2020, Denmark introduced social distancing laws that included the full or partial shutdown of many venues of economic activity, such as restaurants, bars, cinemas, and personal care services (e.g., hairdressers and dentists). Unlike Denmark and almost every other Western country, Sweden adopted a light-touch approach that involved limited restrictions on the activities of private businesses. Consistent with adherence to social distancing laws helping to control the outbreak, mortality rates in Denmark and Sweden diverged from a common trend around 2 wk after the Danish shutdown, with excess deaths increasing much less rapidly in Denmark than

Significance

Social distancing laws that restrict the activities of private businesses are often seen as sacrificing the economy to save lives from COVID-19. Indeed, many countries have experienced massive reductions in consumer spending around the time they began to shut down. We show that these restrictions are, in fact, responsible for only a small portion of the drop in consumer spending. This suggests that the virus itself is responsible for the majority of the economic damage. We find that social distancing laws may provide an economic benefit: the laws reduce the economic activity of the low-risk population and can thus protect those with the greatest risk of mortality from also bearing the greatest burden in terms of reduced spending.

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*For instance, Gourinchas (ref. 1, p. 2) writes "flattening the infection curve inevitably steepens the macroeconomic recession curve."

in Sweden. Constructing measures of consumer spending from bank account transactions, we compare the evolution of spending around the Danish shutdown among highly comparable individuals who live in the two countries. This comparison isolates the causal effect of social distancing laws on consumer spending and captures both the direct effects of the restrictions through the reduced availability of goods and services and the indirect effects of the restrictions on spending through the reduced spreading of the virus.

Bank account data have several advantages for studying the impact of policy responses to the COVID-19 pandemic on economic activity, relative to publicly available aggregate measures such as statistics from national accounts. First, bank account data enable real-time analysis at a high frequency, allowing researchers to capture sharp changes in behavior through the pandemic and around the exact point of public policy changes. Second, linked sociodemographic data on bank customers enable comparisons of changes in behavior across areas that are potentially exposed to different policies while adjusting for any compositional differences. The strength of these adjustments is aided by the large and diverse samples of customers that many banks cater to. Third, bank account data enable researchers to study heterogeneity in behavior change both by customer characteristics and by type of spending.

Applying our research design to the bank account data, we find that the Danish shutdown had a modest effect on aggregate spending. The causal shutdown effect is estimated as the 4-percentage-point (95% CI: 3 to 5 percentage points [p.p.]) difference between the 29% drop in spending in Denmark and the 25% drop in spending in Sweden through the COVID-19 pandemic. Our second finding is that the small shutdown effect conceals considerable heterogeneity across groups that vary in their personal risks from contracting COVID-19. Specifically, we find that the shutdown reduces the spending of the young (low risk, 18 to 29 y old) and increases the spending of the elderly (high risk, 70+ y old), relative to a counterfactual with fewer restrictions and where the outbreak is worse.

Data and Research Design

We study the effect of COVID-19 shutdown policies on consumer spending by using bank account data from Danske Bank, the second largest bank in Scandinavia. We focus our analysis on 860,000 active customers of the bank across Denmark and Sweden. The Danish subsample is broadly representative of the population. The Swedish subsample is similar to that of the Danes in terms of key sociodemographics, geographic concentration in urban areas and the capital of each country, and local exposure to affected industries. Moreover, the subsamples have similar spending behavior prior to the pandemic, both in the level of spending and in expenditure shares on different categories of goods and services. We expect spending levels and category shares to capture a range of factors related to economic resources, preferences, and expectations. Hence, similarity in this dimension is encouraging for our empirical design. Crucially, although the subsamples do not match perfectly along all observables, our sample contains large numbers of individuals across all ranges of sociodemographics, allowing us to weight estimates to adjust for remaining imbalances. Moreover, existing evidence on the individual-level correlates of spending reductions through the COVID-19 pandemic suggests reductions are very similar across a range of household characteristics (2, 3), from income levels to income risk. This suggests that small compositional differences across countries are unlikely to impact our findings. *SI Appendix*, Table S1 contains summary statistics.

Our key outcome variable is daily total consumer spending. We measure total spending as the sum of credit and debit card transactions, mobile wallet payments, cash withdrawals, and electronic invoice payments associated with online shopping. We also construct four categories of spending in venues of varying social and physical proximity to others. These measures include social spending at bars, restaurants, cinemas, and the like; personal care spending at hairdressers, beauticians, and dentists; spending on public transport; and spending on the high street and in malls. Details of the construction of the spending measures are included in *SI Appendix*, Table S2.

Using these data, we exploit an exogenous difference in policy response to COVID-19 to identify the effect of social distancing laws on consumer spending. Starting from 11 March 2020, the Danish government introduced a range of social distancing laws, including banning congregations of more than 10 people; closing schools, universities, and nonessential parts of the public sector; and fully or partially shutting down the activities of a range of private businesses. Affected businesses included restaurants, bars, and cafes, which were forced to offer only take-away service, and a range of other high-proximity establishments, from cinemas, nightclubs, and shopping malls to dental practices and hairdressers, which were forced to close down completely. In contrast, Sweden responded to the COVID-19 outbreak with a light-touch approach that relied, in the main, on voluntary measures to limit the spread of the virus. This starkly different response is likely a result of historical differences in constitutions: unlike Denmark, features of the Swedish constitution make it difficult to quickly pass laws affecting individual liberties (4). We provide further details on policy responses in *SI Appendix*. Exploiting the different policy response to the outbreak, we can isolate the effect of the Danish shutdown on economic activity by comparing spending changes in Denmark and Sweden around the Danish shutdown date. In constructing spending changes, we weight the Swedish sample to match the Danish sample on key economic and demographic variables that might otherwise explain differences in spending growth.

To support our causal interpretation, Fig. 1 provides evidence that these different policy choices were not motivated by differences in early exposure to COVID-19. In Denmark and Sweden, trends in mortality (Fig. 1, *Top*) and Google searches for symptoms (Fig. 1, *Bottom*) were highly similar in the runup to the Danish shutdown. In both countries, all-cause mortality was somewhat below normal for the time of year, while searches for symptoms followed a similar upward trend from mid-February, likely reflecting awareness of early cases (5) and increased incidence of sickness. The effect of the Danish shutdown is also clear: Excess mortality diverges upward in Sweden around 2 wk after the Danish shutdown and searches for symptoms remain elevated only in Sweden. Health outcomes in Italy further highlight the similar early experiences of Sweden and Denmark: In Italy, excess mortality was already increasing from early March, and searches for symptoms were elevated from January. In *SI Appendix*, we provide further details regarding the background to the COVID-19 outbreak in Denmark and Sweden and discuss one economic difference concerning their exchange rate policies.

Results

Small Effect of Shutdown on Aggregate Consumer Spending. Fig. 2 shows that in both Sweden and Denmark, daily aggregate spending in January and February 2020 was evolving similarly to the same period in 2019: Cyclical patterns were the same as those in the previous year, and there was some growth in spending levels in both countries. As the COVID-19 outbreak took off, and around the date of the Danish shutdown, spending drops sharply in both countries and remains below 2019 levels throughout the analysis window.

We present our estimate of the causal effect of the Danish shutdown in Fig. 3, estimated as the 4-percentage-point (95%

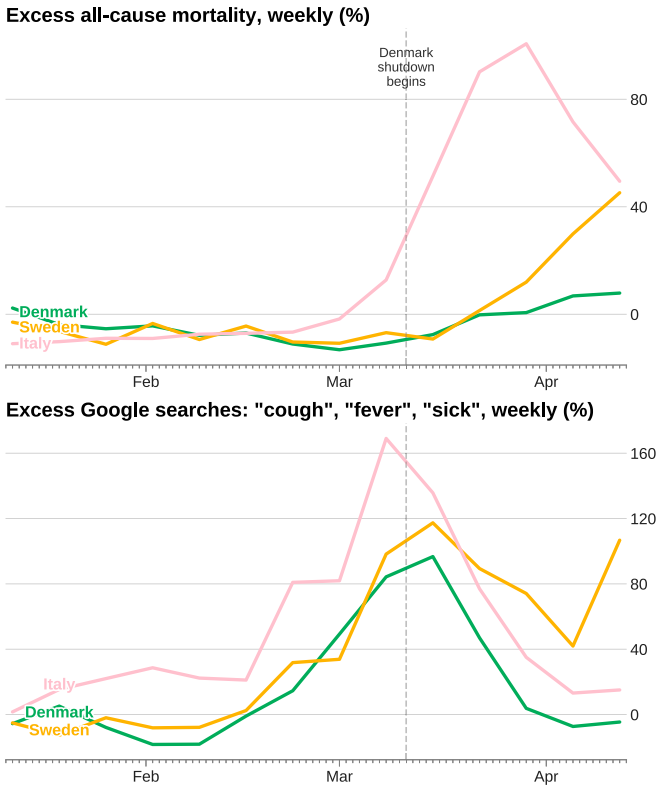

Fig. 1. Impact of COVID-19 outbreak on mortality and health. Shown is weekly excess mortality in Denmark, Sweden, and Italy for weeks 2 through 15 in 2020 (*Top*) and excess Google searches for symptoms of illness and sickness (*Bottom*) for the same period. Excess mortality is calculated based on daily deaths data from Statistics Denmark (Denmark), Statistics Sweden (Sweden), and the Italian National Institute of Statistics (Italy). Deaths data for Denmark and Sweden cover the whole population, whereas the Italian data are available for approximately half of all communes. Excess mortality is calculated as the percentage difference between the total number of deaths from all causes in a week in 2020 and the average number of total deaths in that week, 2015 to 2019. Excess Google searches are calculated based on data from Google Trends of weekly indexes of search intensity for the terms "cough" (Denmark, "hoste"; Sweden, "hosta"; Italy, "tosse"), "fever" (Denmark, "feber"; Sweden, "feber"; Italy, "febbre"), and "sick" (Denmark, "syg"; Sweden, "sjuk"; Italy, "malato"). Individual indexes for each search term are summed to create a composite index of all terms. Excess Google searches are then calculated in the same way as for excess mortality.

CI: 3 to 5 p.p.) difference between the 29% drop in spending in Denmark and the 25% drop in spending in Sweden through the COVID-19 pandemic. The results in Fig. 3 weight the Swedish sample to match the economic and demographic characteristics of the Danish sample; we show that the estimates are virtually unchanged in an unweighted specification in *SI Appendix*, Fig. S1). In *SI Appendix*, Table S3 we show that the results are robust to alternative weighting specifications. *SI Appendix*, Fig. S2 decomposes the country-specific estimates into spending changes relative to 2019 before and after the Danish shutdown date.

To assess the interpretation of these findings as an estimate of the effect of social distancing laws, we address three possible confounders. First, *SI Appendix*, Fig. S3 shows that these results cannot be explained by different economic exposure to the COVID-19 pandemic: The major stock market indexes in Denmark and Sweden followed almost exactly the same trajectory through the pandemic, suggesting that firms in each country were equally affected by the global contraction in trade and economic activity. Moreover, *SI Appendix*, Fig. S4 shows that, if anything, the rise in unemployment claims was slightly sharper in Denmark than in Sweden, implying that greater exposure to job losses in Sweden cannot explain the similar drop in spending in the absence of a shutdown. Second, in *SI Appendix* we compare the government economic policy responses to the pandemic and show that they were highly similar across countries. This suggests that it is not the case that particularly generous subsidies to firms and workers in Denmark have attenuated the spending drop there, relative to Sweden. Finally, we note that before the pandemic less than 1% of total spending among the Swedish sample took place in Denmark and that only around 10% of the Swedish sample live in Skåne, the Swedish region most integrated with Denmark. Thus, there is no meaningful direct effect of the Danish shutdown on Swedish aggregate consumer spending.

Large Differences in Shutdown Effect by Health Risks. Fig. 4*A* shows how the effect of the shutdown, the differential spending drop in Denmark relative to Sweden, varies across age groups and hence health risks from COVID-19 exposure. The effect is negative for the youngest group (age 18 to 29 y): Spending by this age group dropped around 10 percentage points more in Denmark than in Sweden. By contrast, the effect of the shutdown is positive for the oldest group (age 70+ y): Spending by this age group dropped around 5 percentage points less in Denmark than in Sweden. In the intermediate age groups, the effect of the shutdown is generally moderately negative, sometimes statistically indistinguishable from zero. We report the absolute magnitude of the effect in each country by age group with standard errors in *SI Appendix*, Fig. S5.

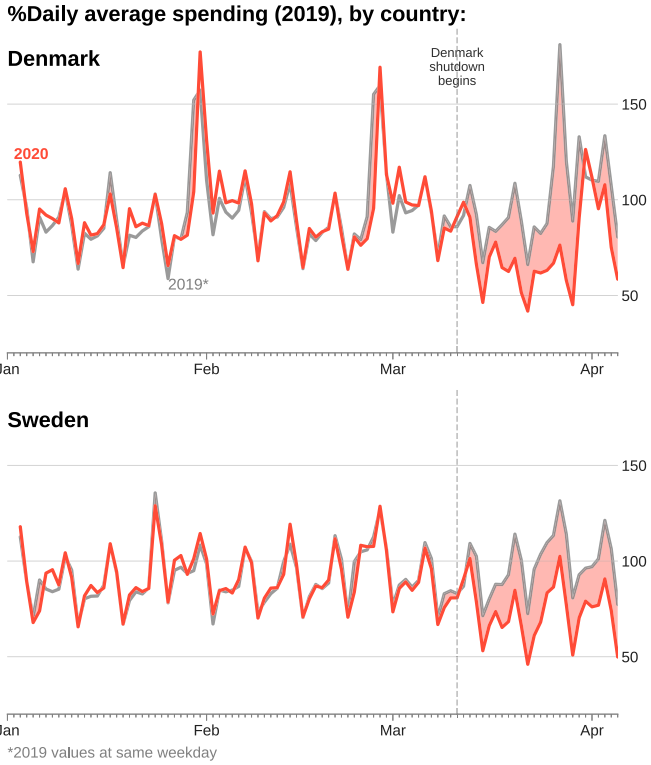

Fig. 2. High-frequency evolution of aggregate spending in Denmark and Sweden. Shown is the evolution of aggregate spending in Denmark and Sweden from 2 January to 5 April 2020. Red lines show the evolution of spending in 2020 as a percentage of daily average spending in 2019. Gray lines show the same series for the same weekday in 2019, i.e., 364 d earlier. The dashed vertical line denotes 11 March, when the Danish government announced the shutdown. Shaded red regions highlight the drop in spending in both countries at this point in time.

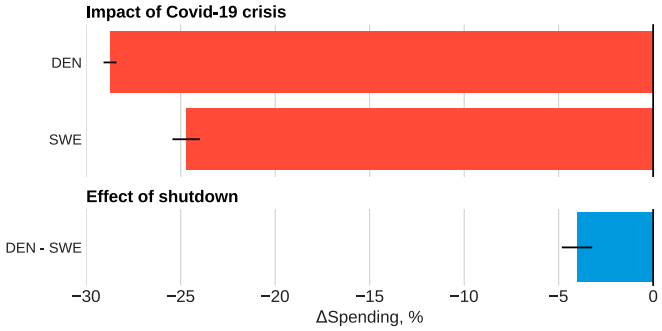

Fig. 3. Effect of the shutdown on total spending. The plot shows the impact of the COVID-19 crisis on consumer spending in Denmark (DEN) and Sweden (SWE) and isolates the effect of the Danish shutdown as the difference (DEN – SWE). The estimates are based on weighting of the Swedish observations to match the sociodemographic composition of the Danish sample, as described in *Materials and Methods*. Confidence bounds at the 95% level (black horizontal lines) are based on robust standard errors.

The positive effect of the shutdown on the economic activity of the elderly suggests that the Danish shutdown served to contain the spread of the virus and hence reduced the need for extreme self-isolation among those most at risk for severe health consequences. This interpretation is supported by the large negative effect of the shutdown on the spending of the young: The shutdown massively constrains the economic activity of the young, a group who are least at risk for severe health consequences and hence who, in the absence of a shutdown, would likely contribute most to the spreading of the virus. Alternative explanations for the age gradient, such as cross-country differences in age-specific government advice, are less likely to be responsible for this finding. In both Denmark and Sweden, high-risk groups, including those over 70 y old, received very similar recommendations from the government concerning limiting their social interactions. But it was only in Denmark that they faced sanctions for breaking social distancing laws and experienced the mandated closure, full or partial, of merchants they frequented. Despite these legal restrictions on their economic activity, the elderly in Denmark still reduced their spending by less than the elderly in Sweden. Further information on government advice and policies is included in *SI Appendix*.

Fig. 4*B* shows the age gradient in the effect of the shutdown for categories of spending involving high or moderate social proximity: retail (i.e., high-street shops and malls), social spending (i.e., food, drink, and entertainment away from home), personal care (e.g., hairdressers and dentists), and public transport (i.e., trains and busses). Across all four categories, we observe a clear age gradient: The effect of the shutdown is strongly negative for the young (low health risk) and less negative, sometimes even positive, for the elderly (high health risk). We report the absolute magnitude of the effect in each country by age group with standard errors in *SI Appendix*, Fig. S6.

Discussion

In many countries, the introduction of social distancing laws in response to COVID-19 has occurred at the same time as a sharp contraction in economic activity, including consumer spending (2, 3, 6–12). This coincidence in time may suggest that the laws themselves are responsible for the economic harm. We present evidence that this is not the case: Only Denmark mandated a shutdown but aggregate spending fell sharply around the Danish shutdown date in both Denmark and Sweden (Fig. 2), suggesting the cause of the sudden drop in spending was the similarly escalating COVID-19 outbreak (Fig. 1).

The fact that the shutdown has only a small effect on consumer spending suggests that most of the economic contraction occurs regardless of whether governments mandate social distancing or not. Moreover, while these results confirm the popular notion that the lives saved by a shutdown come at an economic cost, the trade-off is much less stark than suggested by the large drops in economic activity around shutdowns. Governments should weigh the benefits of the public health interventions in terms of reduced mortality and serious illness (13) against the small differential cost in terms of economic activity.

Our finding that the effect of the shutdown amplifies spending cuts among young adults and attenuates spending cuts among the elderly suggests that social distancing mandates can reduce the economic activity of the young, mitigating their role in spreading the virus and thus allowing for higher activity among the

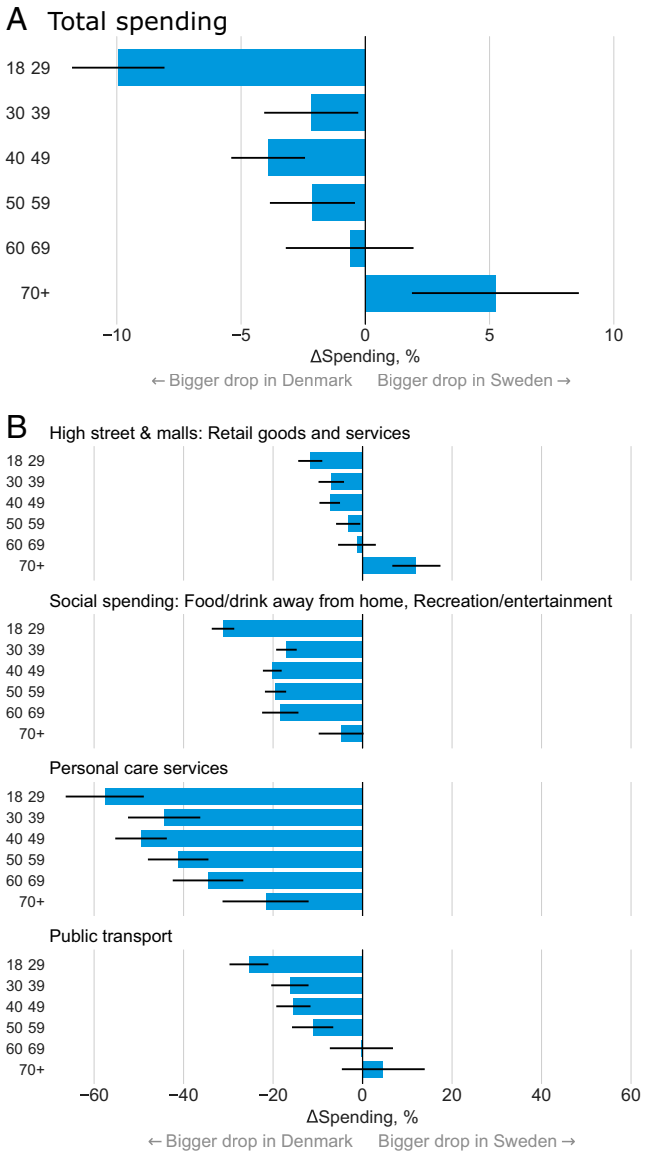

Fig. 4. Effect of the shutdown by age group. (*A* and *B*) The effect of the shutdown on total spending (*A*) and spending in venues of differing social proximity (*B*) by age group, a measure of COVID-19 disease risk. Age-specific estimates of the shutdown effect are calculated as the difference between the estimated drop in consumer spending among that age group in Denmark and in Sweden, with the Swedish sample weighted to match the sociodemographic composition of the Danish sample, as described in *Materials and Methods*. Confidence bounds at the 95% level (black horizontal lines) are based on robust standard errors.

most at risk (the elderly). That is, social distancing mandates may protect those with the greatest risk of mortality from also bearing the greatest burden in terms of reduced spending. In other contexts, it would be highly surprising that constraints on the availability of certain goods and services cause some individuals to choose a higher overall level of consumption. In the context of the pandemic, however, shutting down sectors with high social proximity reduces the prevalence of the virus in society at large. The evidence on specific spending categories shows, in a very concrete way, a potential mechanism by which a shutdown can improve aggregate economic outcomes in a pandemic: By reducing the prevalence of the virus a shutdown lowers the risk of contracting it on public transit and on the high street, which stimulates the spending of individuals with a high underlying health risk.

Our findings have implications for the rapidly developing macroeconomic literature on pandemics. The models start from different assumptions about the nature of the shock to the economic system. Some assume that the COVID-19 pandemic is essentially a shock to the supply side with possible spillovers to the demand side (14), while others emphasize that the pandemic affects demand directly because it introduces a health cost of consumption (15). Our findings suggest that the direct demand shock is important: Spending drops massively even when supply is unconstrained and the drop correlates strongly with health risk.[†]

These findings contribute to an emerging literature studying the effects of the social distancing laws imposed by most governments in the world in response to the COVID-19 pandemic. Most of the literature focuses on the effectiveness of these policies in limiting personal mobility (16, 17) and containing the virus (18). While several papers use quantitative economic models to evaluate social distancing policies (19, 20), and there is some evidence on the economic impact of nonpharmaceutical interventions in past pandemics (21), we are not aware of other causal evidence on the effect of such policies on spending or other dimensions of economic activity through the COVID-19 pandemic.

The extent to which our findings from Scandinavia are generalizable to other countries, and can help to inform policy decisions more broadly, depends on at least three things. First, our results are more likely to extend to countries that introduced similar income-support schemes, such as furlough and social insurance policies, in response to the pandemic. Many countries across Europe and North America introduced government programs that were roughly similar in scope and scale to those launched in Denmark and Sweden (22). Second, our findings are more likely to hold in countries with similar population age structures to those in Scandinavia, as is the case in much of Europe (23). While we cannot test this directly, our findings may suggest that the economic costs of a shutdown are larger than we estimate in younger countries, for example, in many developing countries, and smaller than we estimate in older countries. Finally, to the extent that our main finding, the small shutdown effect, is driven by the virus causing people to reduce their economic activity to minimize their role in spreading the disease, rather than personal health concerns, then similarity to Scandinavia along a collection of factors related to the strength of social norms and sense of civic duty might be relevant for generalizability. Greater incidence of these factors may correlate with larger self-initiated reductions in economic activity and hence result in a smaller impact of government-mandated shutdowns.

[†]We cannot exclude that some of the drop in spending that we estimate in Sweden and Denmark is due to merchants closing down in response to concerns about the health risks for workers. This could be considered a supply shock.

Materials and Methods

Sample and Data. We focus our analysis on approximately 760,000 Danish and 100,000 Swedish active customers of Danske Bank, the second largest bank in Scandinavia. We define active customers as individuals who have made at least one card payment in each month for the period January 2018 to December 2019 and, when information is available, who have declared their Danske Bank account as their primary account to the government tax authority. We impose only the minimum monthly spending requirement until December 2019 as we want to allow for individual spending to fall to zero in response to the crisis. Since individual spending is often partly on behalf of other household members, we use the bank's household identifiers—constructed from information on joint accounts and coresidence—to split all expenditures of couples on their personal and joint accounts across each spouse equally. For instance, when one member of a household spends $50 on groceries, we consider that each spouse has spent $25.

We link the spending data with key demographics for each customer, sourced from the bank's customer records. These include age, gender, municipality of residence, and permanent income. Permanent income is measured as average monthly total spending over a long period (2 y, 2018 to 2019). This measure is designed to capture access to economic resources for individuals at different life stages more accurately than income measured at a point in time. We create two measures of geographic location of customers using municipality of residence: residence in urban, high-density areas and residence in localities with an above median share of affected (high social proximity) workers. We use these sociodemographics to assess the comparability of our samples in *SI Appendix*, Table S1, and to adjust our estimates of spending changes for the Swedish sample such that they can be interpreted as measuring the spending change in a counterfactual Denmark absent the shutdown policy. Crucially, the Swedish sample is similar, on average, to the Danish sample and contains large numbers of individuals across all ranges of demographics, allowing us to effectively control for possible confounding differences in response to the crisis. We provide more details on the data in *SI Appendix*.

The data used in this paper are from Danske Bank, a private bank. All individual data used in this analysis have been anonymized and no single customer can be traced in the data. All data processing has been conducted by authorized Danske Bank personnel, following the bank's strict data privacy guidelines. The data use is subject to the European Union's General Data Protection Regulation (GDPR). Danske Bank controls data access. Researchers interested in obtaining access to Danske Bank's data are required to submit a written application to the bank. Applications should include a detailed research proposal consisting of a research question and motivation, information on the researchers who wish to gain access, and a detailed explanation of the data at Danske Bank that are needed for the project. Applications can be submitted to tule@danskebank.dk. After submitting the application and following communication between the applicants and Danske Bank, the bank will inform the applicants as to whether the project is suitable for initiating a data access agreement. If a researcher wishes to analyze our data for replication purposes, we can provide assistance in the process of acquiring access.

Method. Our unit of observation is individual by day. For each person in our sample and for each day of our analysis window (2 January to 5 April 2020), we compute an individual-level measure of year-on-year spending changes as

$$s_{ict} = \frac{spending_{ict} - spending_{ict-364}}{average\ spending_c},$$

where $spending_{ict}$ is spending on day t for individual i living in country c, and $average\ spending_c$ is average daily spending per person in that country taken over all days in 2019. We measure the individual-level spending change over the COVID-19 crisis as

$$\Delta s_{ic} = E_t[s_{ict}|t \in post] - E_t[s_{ict}|t \in pre],$$

where $t \in post$ is the postshutdown period, 11 March to 5 April, and $t \in pre$ is the preshutdown period, 2 January to 15 February, before any restrictions (e.g., early travel bans) or anticipation of the COVID-19 crisis are likely to have been affecting spending behaviors. This measure of the change in year-to-year spending changes effectively controls for high-frequency cyclicality in spending. Since spending changes are scaled relative to average daily 2019 spending in each country, the simple average of Δs_{ic} across individuals in country c is equal to the change in aggregate spending relative

to this baseline. We estimate the following regression model to isolate the effect of the variation in social distancing laws, removing the influence of differences in the sociodemographic composition of the sample of bank customers across Denmark and Sweden,

$$\Delta s_{ic} = \alpha_c + \mathbf{X}_i \delta_c + \epsilon_{ic}, \quad [1]$$

where α_c and δ_c are country-specific parameter vectors and $\mathbf{X}_i$ is a vector of covariates capturing the age, sex, geographic location, exposure to affected industries, and permanent income of individual *i*. Permanent income is measured as the individual-level average of monthly total spending over the period January 2018 to December 2019. Each variable is represented discretely by a set of one or more dummies: six age groups (ages 18 to 29 y, 30 to 39 y, 40 to 49 y, 50 to 59 y, 60 to 69 y, and 70+ y), sex, residence in an urban location, residence in a municipality with above median (high) exposure to affected industries, and quartiles of purchasing power parity adjusted permanent income. The model is fully saturated: We include all interactions between these dummies, as well as allowing for different coefficients in Denmark and Sweden.

We use the estimates from Eq. **1** to produce weighted estimates of the aggregate spending drop in each country: For each value of *c*, we compute the predicted value from the model when all covariates are evaluated at their averages in the Danish sample. For Denmark, this is just equivalent to taking the Danish sample average of Δs_{ic}. Under the identifying assumption that year-to-year spending growth between 2019 and 2020 would have been the same after 11 March as before, absent the epidemic and the shutdown, this captures the impact of the COVID-19 crisis on aggregate spending in Denmark. For Sweden, the estimate is essentially a weighted average of individual spending changes where the weights are constructed to make the Swedish sample match the joint distribution of the variables in $\mathbf{X}_i$ in the Danish sample. One can think of this as an estimate of the counterfactual aggregate spending drop in a Denmark that had followed the Swedish light-touch approach, holding sociodemographic variables constant.

To estimate how the shutdown affects people differently depending on their health risk, we also construct measures of the change in spending within age groups in each country. These measures are constructed exactly like the country-wide measures, except that year-to-year spending changes for an individual in age group *a* in country *c* are now scaled relative to average daily spending in 2019 for individuals in that age group and country. Letting Δs_{iac} denote the change in year-to-year spending changes for person *i* belonging to age group *a* in country *c*, we estimate the equation

$$\Delta s_{iac} = \alpha_{ac} + \mathbf{X}_i \delta_{ac} + \epsilon_{iac}, \quad [2]$$

where α_{ac} and δ_{ac} are age-group–country-specific parameters and $\mathbf{X}_i$ is the same vector of controls as in Eq. **1**, except age. For each age group, we then compute predicted values for both values of *c* and evaluate them for the average individual within that age group in the Danish subsample. As for the full sample, the weighted measures are equal to simple subsample averages for Denmark, whereas the weighted measures for Sweden can be thought of as providing age-specific counterfactual estimates for Denmark, holding the demographic composition of each age group fixed.

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