



**BANK NEGARA MALAYSIA**  
CENTRAL BANK OF MALAYSIA

Bank Negara Malaysia Working Papers  
WP2/2023

Impact of 2021 Floods on Spending: An Event Study  
Using Daily State-Level Card Transactions and  
Mobility Data

By Jing Lian Suah

20 July 2023

Working papers describe research in progress by the author(s) and are published to elicit comments and to further debate. The views expressed are solely those of the author(s) and should not be taken to represent those of Bank Negara Malaysia.

# Impact of 2021 Floods on Spending: An Event Study Using Daily State-Level Card Transactions and Mobility Data

Jing Lian Suah

Central Bank of Malaysia, Malaysia

July 20, 2023

## Abstract

This paper studies the impact of unanticipated major floods in mid-December 2021 on consumer spending at the state-level. Visible breaks were observed in the trends and levels of spending, and mobility in the aftermath of floods. Duration of these dips varied. Taking the data to a panel event study setup, the average impact of floods on spending was short-lived, but substantial, at about 1-3% per day, before crossing, and exceeding the baseline (1 day before the floods) by day 7. Findings are broadly similar using both the naive two-way fixed effects (TWFE) and interaction-weighted (Sun & Abraham 2021) event study regressions. Policy-wise, direct cash assistance during climate disasters, such as the floods studied, should be disbursed to offset the spending shortfall, either as liquidity assistance, assistance to manage post-disaster recovery and repairs, or simply to compensate for aggregate spending losses. Further research could look at the distributional profile of policy multipliers from various angles, especially if complemented with more disaggregated data.

Keywords: Floods, Daily Transactions, Impact, Event Study

JEL Codes: E20, E65, Q54

† Any views expressed are solely mine and should not be taken to represent those of the Central Bank of Malaysia, or the Government of Malaysia.

# 1 Introduction

In mid-December 2021, several states in Malaysia were hit by major floods, including Klang Valley, which encompasses Malaysia’s largest state by GDP (Selangor), the financial capital (Kuala Lumpur), and the administrative capital (Putrajaya). This flood season was unique, as and when it happened, such that states that were rarely hit by floods, en masse, experienced major disruptive floods, all with devastating consequences to human life and infrastructure, owing in part to slower-than-necessary early warning systems. These floods affected each state on mostly different days. For economic policymakers, the policy question was simple — what was the damage, and what should be done to mitigate it? Valuation of infrastructure damage often takes a long time, and not all capital destruction necessarily have equal and direct implication to the household’s welfare. We therefore look to high-frequency data, specifically daily transactions, a measure of real-time consumer spending.

High-frequency (and granular) data has, since the onset of the COVID-19 pandemic, become a norm in economic surveillance and research for formulating policy. Pre-pandemic, Aladangady et al. (2019) used daily transactions data to implement an event study methodology to evaluate the impact of hurricanes on spending, and also argued that access to these data sets could alert policymakers of crises before they escalate into national proportions. During the pandemic, Chetty et al. (2020) used data from private firms to characterise the effects of the COVID-19 pandemic and subsequent macroeconomic interventions on consumer and businesses at the the ZIP code, industry, income group, and business size levels. Moreover, Lewis et al. (2021) described the Federal Reserve’s Weekly Economic Index (WEI), intended to track the evolution of the slower-reported GDP at a weekly frequency, which was launched for public use in the early days of the pandemic. In general, high-frequency (with some degree of granularity) data provide unique windows into the characterisation of macro shocks, and for policymakers to respond with adequate certainty (based on observed data, rather than conjectures), that traditional slow-reporting macroeconomic statistics could not.

This paper falls within close proximity both in methodology, and in data characteristic, to Aladangady et al. (2019) in asking the following question — how did the floods in Malaysia in mid-December 2021 impact consumer spending at the state-level?. However, we incorporate updated empirical methodology described in Sun & Abraham (2021) since the recent revamp of the difference-in-difference (DiD) literature to evaluate dynamic effects of unanticipated shocks in a staggered manner. Three things in this note — (i) a descriptive analysis of spending, and Google mobility indices (retail & recreation, grocery & pharmacy, and workplace) before and after the onset of the floods, (ii) an estimation of the impact of floods on spending using the event study methodology, and (iii) a discussion on policy.

## 2 Descriptive Analysis

### 2.1 Data

The first data set used in this paper comprises of card transactions at the state-level <sup>1</sup>, excluding online spending, which is collected from a representative share of private sector banks. These data are obtained from a representative sample of financial institutions (60% of market share), and Malaysia’s sole provider of payments system network.

In Malaysia, amid a policy-driven drive towards cashless payments since 2011, debit card transactions per capita rose from 0.86 in 2011 to 21.95 in 2021, and 35.37 in 2022, while credit card transactions per capita rose from 10.86 in 2011 to 16.56 in 2021, and 21.08 in 2022. E-money usage, on the other hand, also rose substantially, from 27.54 per capita in 2011 to 62.79 in 2021, and 93.81 in 2022. These data are published by the Central Bank of Malaysia. The aggressive jump between 2021 and 2022 may be additionally driven by a combination of preference for cashless mode of transactions adopted during the pandemic, and a full resumption of economic activity towards the end of 2021. However, credit cards generally have income thresholds (MYR24,000 per annum; median wage in 2021: MYR27,000). Debit cards, on the other hand, only require a bank account, of which 92.51% of adults aged 25 and above possess (88.37% for adults aged 15 and above), based on World Bank data. The corresponding data points at the state-level, however, is unavailable.

The second data set comprises of the Google mobility index at the state-level for retail & recreation, grocery & pharmacy, and workplace locations. Both data sets are available at the state-day level. The transactions data were seasonally adjusted at a daily frequency at source, using Malaysia-specific exogenous calendar effects. Google Mobility indicators were transformed into 7-day moving averages to smooth out within-week seasonal fluctuations.

Transactions data was originally available in MYR. For comparability between states, card spending  $Y$  was rescaled (100-indexed) to spending levels observed on 1 January 2020 for state  $i$ , and day  $t$  (equation 1). The mobility indicators were originally zero-indexed to levels observed in Jan-Feb 2020. For direct comparability, and to avoid numerical complications of negative real numbers, mobility  $M$  was converted to 100-index, by simply adding 100 to all observations (equation 2). Data from 14 December 2021 to 23 December 2021 are used. This time frame is chosen to avoid the 12-12 sales (12 December), Christmas Eve (24 December), and Christmas Day (25 December), whose inclusion may distort the analysis substantially.

---

<sup>1</sup>Johor, Perlis, Terengganu, Kelantan, Perak, Melaka, Kedah, Penang, Pahang, Negeri Sembilan, Selangor, Sarawak, Sabah, Labuan, Kuala Lumpur, Putrajaya

$$Y_{i,t}^{rescaled} = 100 * \left( \frac{Y_{i,t}}{Y_{i,t=1Jan2020}} \right) \quad (1)$$

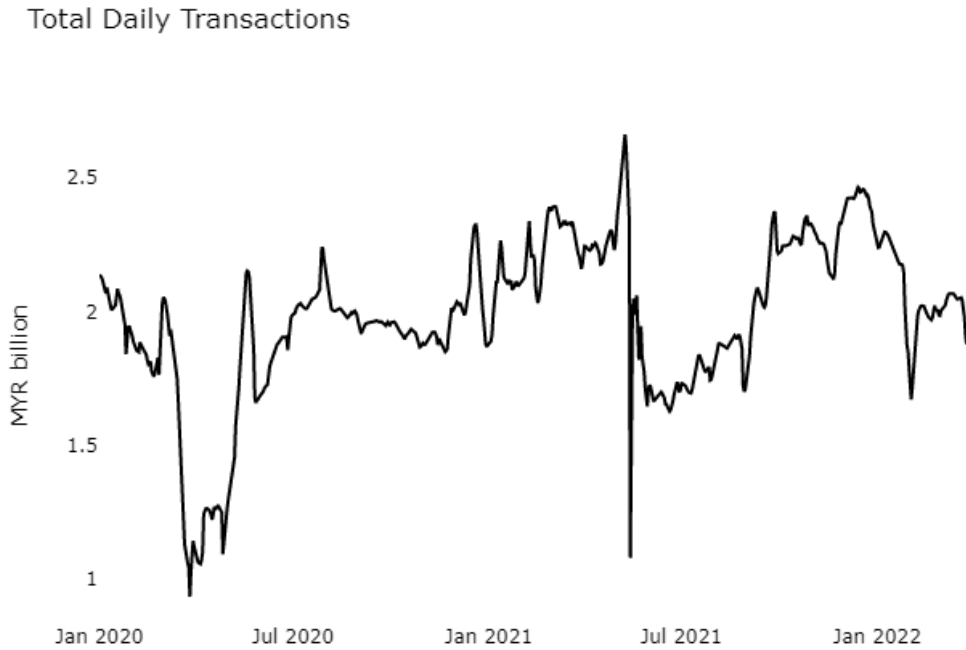
$$M_{i,t}^{rescaled} = M_{i,t} + 100 \quad (2)$$

In addition to levels, both series were also further transformed into log-differences, which can be interpreted directly as the daily growth rate divided by 100. In the event study regression, the levels series were also transformed into log-levels, so that the coefficients can be interpreted as the percentage association of spending to the relative time of flood onset.

## 2.2 Time Series Plots of National-Level Data

To introduce the transactions data used in subsequent analysis, figure 1 shows the full time series of total transactions, and figure 2 physical (the state-disaggregated version is used in the subsequent sections) and online card spending. Figure 3 further breaks down total transaction (in figure 1) into online and physical spending, and in figure 4 the cash, and cashless components. The cashless component is estimated at source using a dynamic cash-cashless ratio based on the cashless components, and ATM withdrawals, both of which are measured deterministically.

Figure 1



Zooming into card spending specifically in figure 2, physical spending appears to be more cyclical, with clear downturns and upturns corresponding to COVID-19 lockdowns, cash transfers, reopening of the economy as COVID-19 subsided, and episodes of significant outbreaks even in the absence of lockdowns, e.g., the initial Omicron wave in Jan - Feb 2022. Daily online card spending, in contrast, appears steadier, and permanently increased in scale since the start of the pandemic, having expanded by about 50% between Jan 2020 and March 2022. The breakdown of total transactions into online and physical modes in figure 3 show a similar trend as the breakdown of only card spending in figure 2. When broken down by cash and cashless components in figure 4, both components bear the fluctuations visible in physical transactions, suggesting that cash, and cashless spending cut across both online, and physical breakdowns. Moreover, both cash, and cashless transactions trace each other closely throughout 2020-21. Right before the pandemic in Jan-Feb 2020, cashless transactions are almost half in value that of cash transactions. However, in 2022, after the full resumption of economic activity, cash transactions are barely half that of cashless transactions.

Figure 2

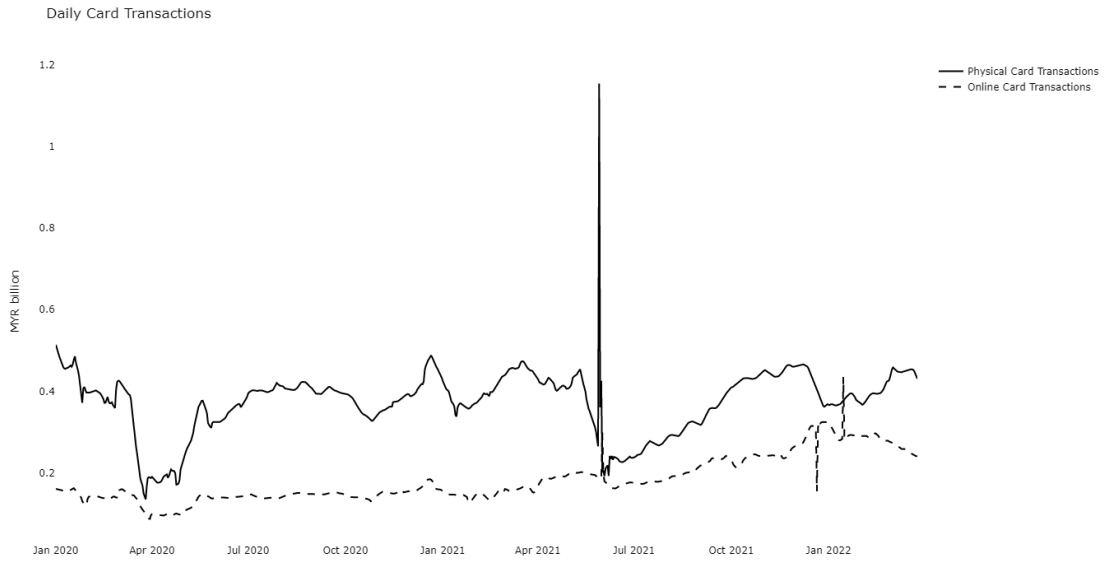


Figure 3

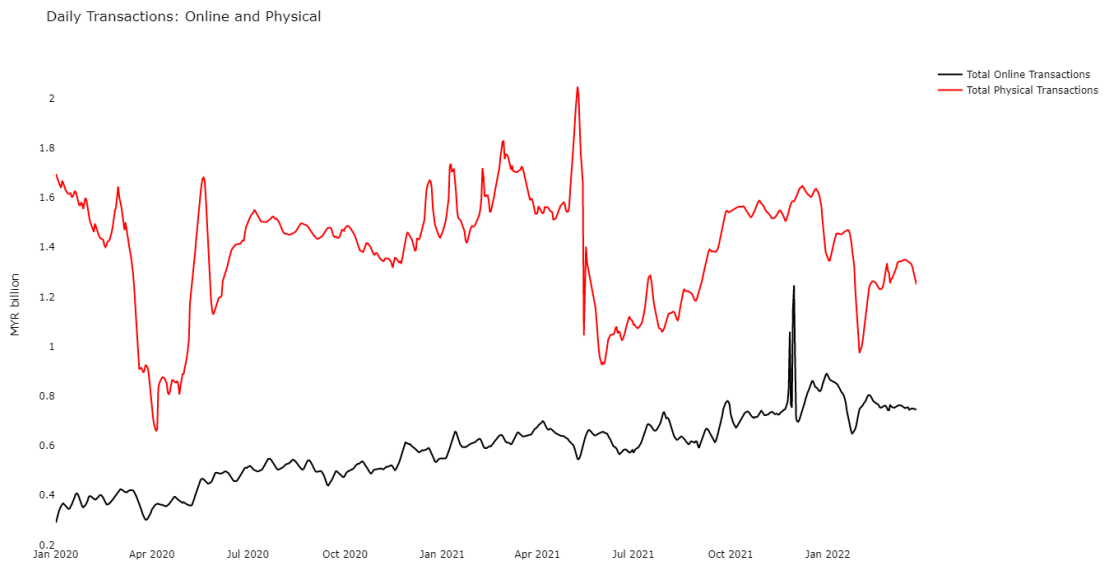


Figure 4

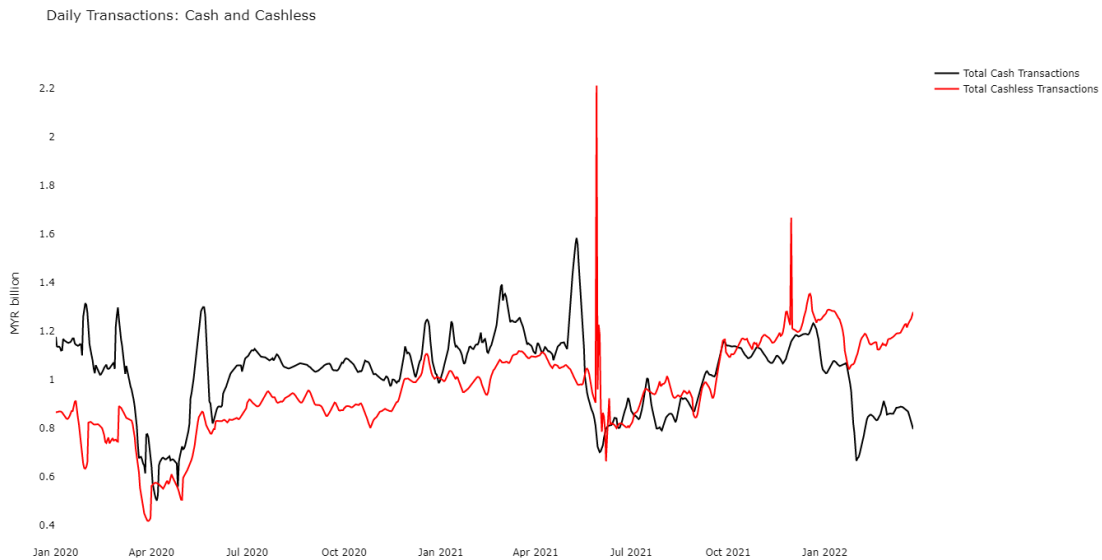
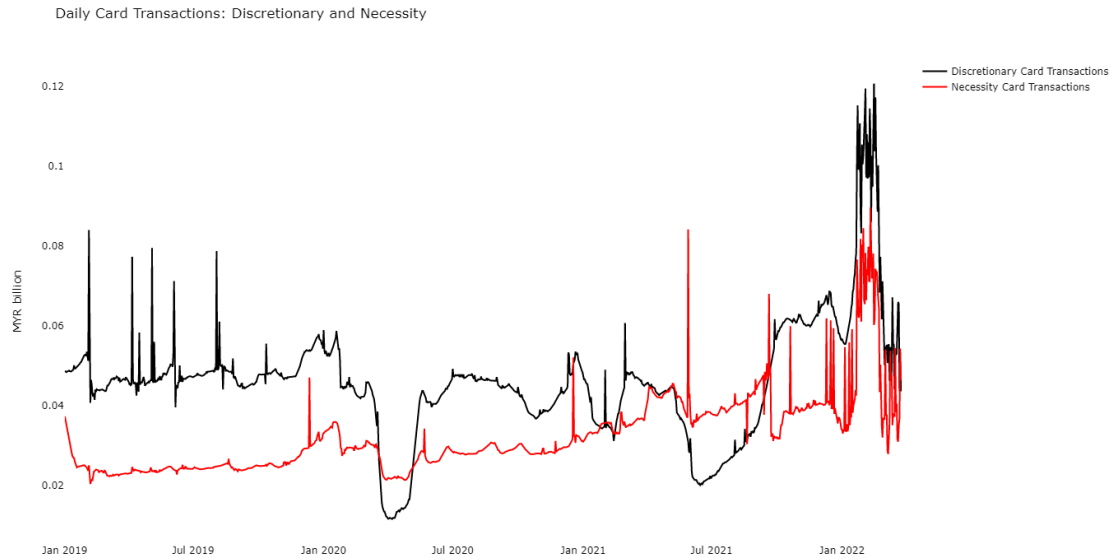


Figure 5 plots card transactions, grouped by merchant category codes deemed as necessity (e.g., grocery, food and beverages), and discretionary spending (recreational activities). In general, except for periods corresponding to major COVID-19 lockdowns (Mar-May 2020, Jan-Mar 2021, and Jun-Aug 2021), necessity card transactions constitute only about half that of discretionary card transactions, including during the December 2021 floods analysed in this paper. Moreover, other than the lockdowns, both necessity, and discretionary card transactions trended similarly, but fluctuations are more visible in discretionary transactions.

Figure 5



Figures 6 and 7 compare directly the quarterly aggregated total transactions (in figure 1) against private consumption expenditure and services GDP. Aggregating into quarterly sums using the non-seasonally adjusted version, total transactions fall short slightly compared to nominal PCE and services expenditure from the national accounts in levels (figure 6), but the trends are mostly aligned. While transactions is a direct measure of consumer spending, discrepancies definitions may differ in the national accounts due to the exclusion of certain fringe items, such as expenditure categories classified as investment, or survey uncertainty. Figure 7 shows that, once converted into log-difference (growth divided by 100), trends are almost identical directionally, and in rates.

Figure 6

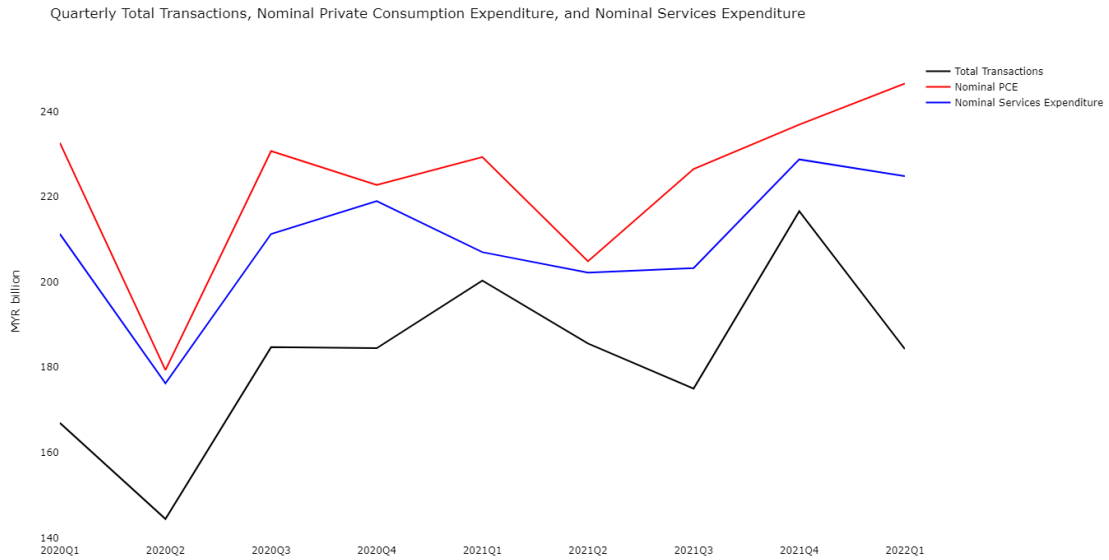
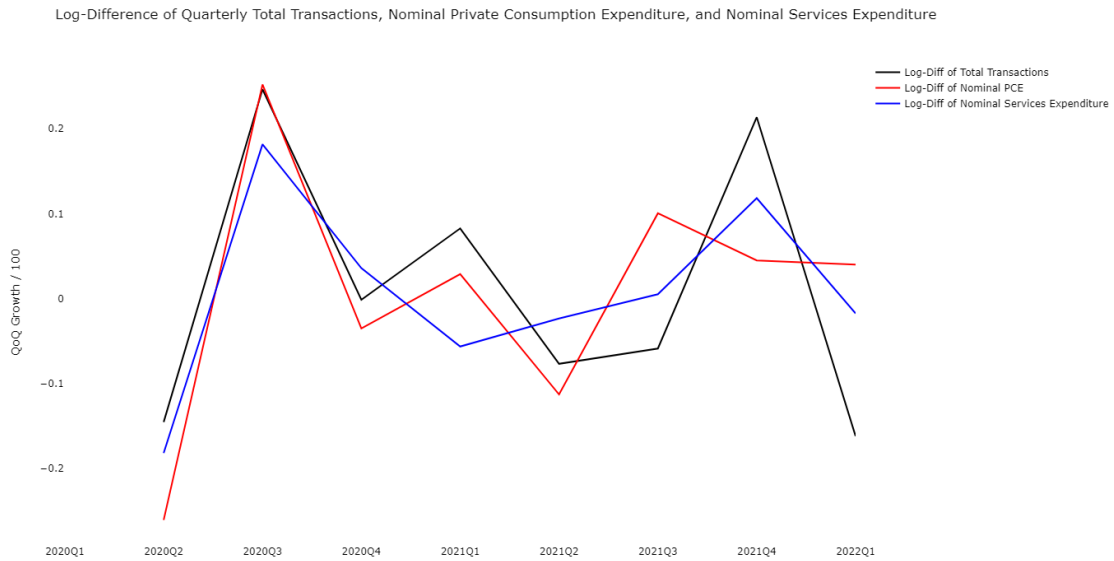


Figure 7



Overall, this data set appears to capture key developments throughout 2020-22 on a daily basis, covering evolution in consumer spending that would otherwise be missed by quarterly, or even monthly, survey-based macroeconomic statistics. Yet, when benchmarked directly against traditional national accounts indicators, daily transactions data held up comparably as a ‘hard’ measure of spending.

## 2.3 Time Series Plots of State-Level Data During the December 2021 Flood Period

This section plots the time series of state-level spending, and mobility, at the state-level between 14 December and 23 December 2021. A short window is used, as extending the event study horizon will overlap with the 12-12 (12 December) sales, Christmas Eve (24 December), and Christmas Day (25 December).

Responses of spending to the onset of floods, marked by vertical lines, were diverse (figure 8). Kuala Lumpur, Kelantan, Putrajaya, Negeri Sembilan, for instance, observed brief declines. Some states, such as Terengganu, and Selangor observed trend breaks. The equivalent is observed in log-differences (growth rates divided by 100), e.g., spending in Kuala Lumpur contracted briefly, before rebounding, and correcting back to nil (figure 7).

Figure 8

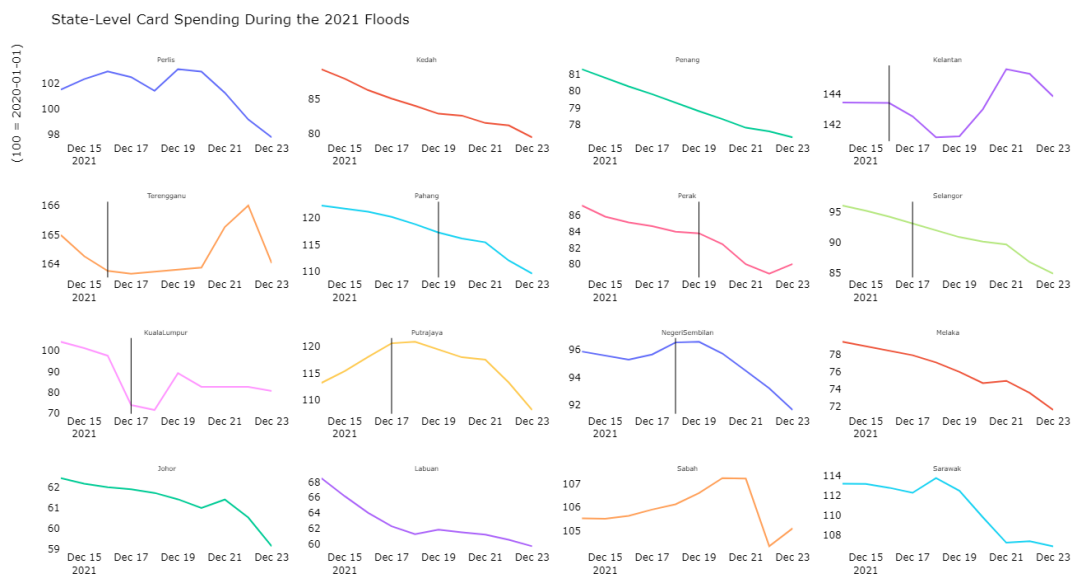
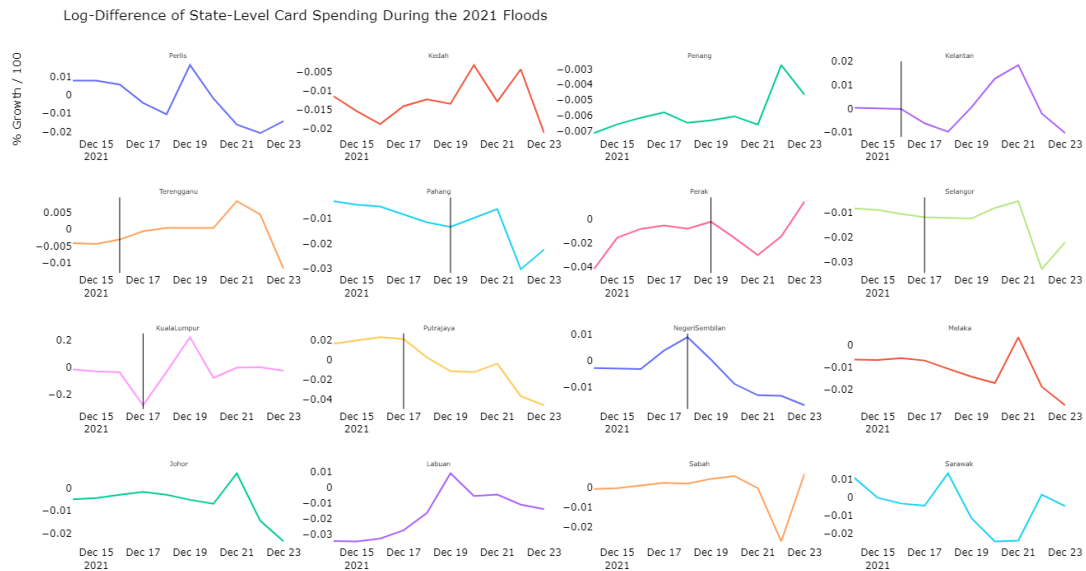


Figure 9



Mobility at retail locations (figures 10 and 11) recorded similar observations, where affected states experienced a brief decline, and then a reversion towards pre-flood levels. However, the onset did not always coincide with a sharp break in trend, which may either be reflecting other seasonal factors, a mix of delayed and premature response due to observation of other states, or pre-flood bad weather. The same trends could be observed for grocery & pharmacy (figures 12 and 13), and workplace locations (figures 14 and 15).

Figure 10

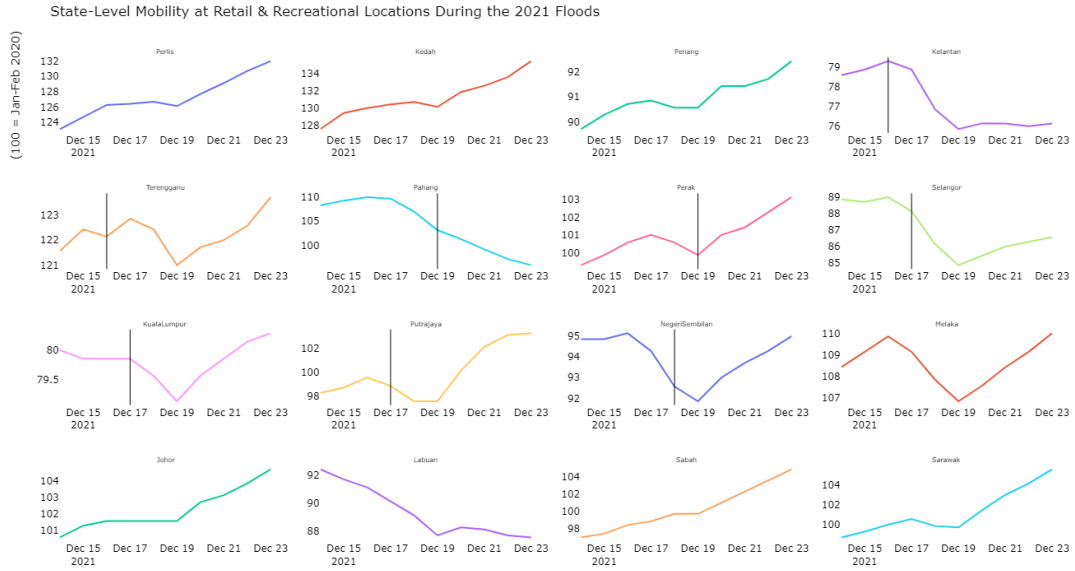


Figure 11

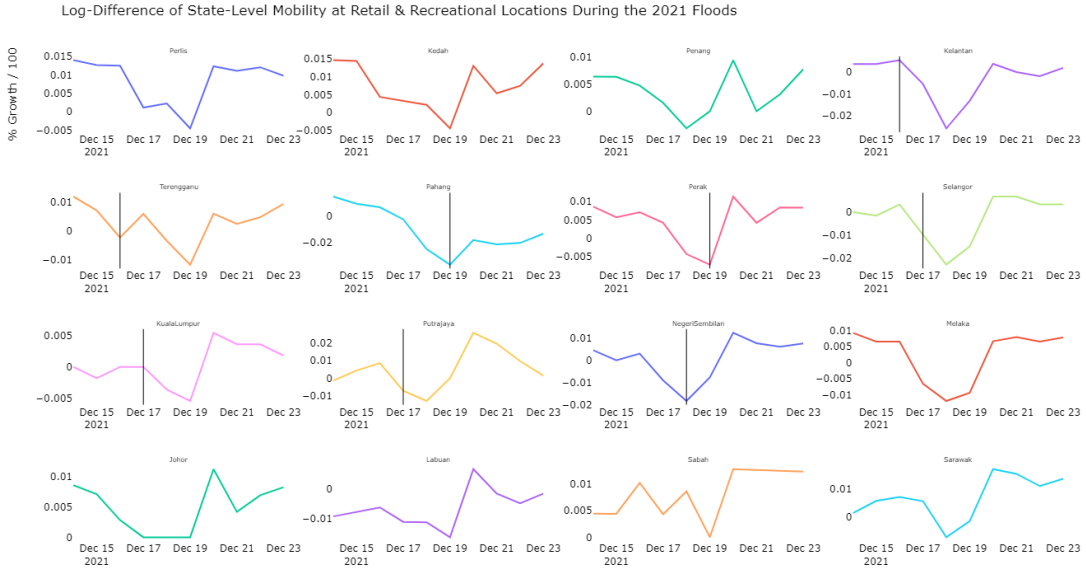


Figure 12

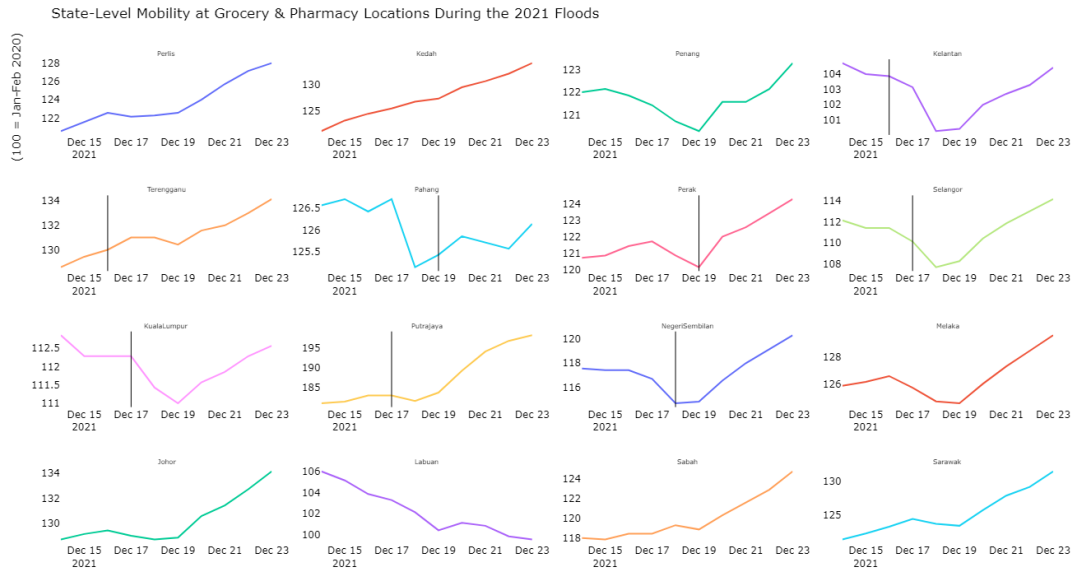


Figure 13

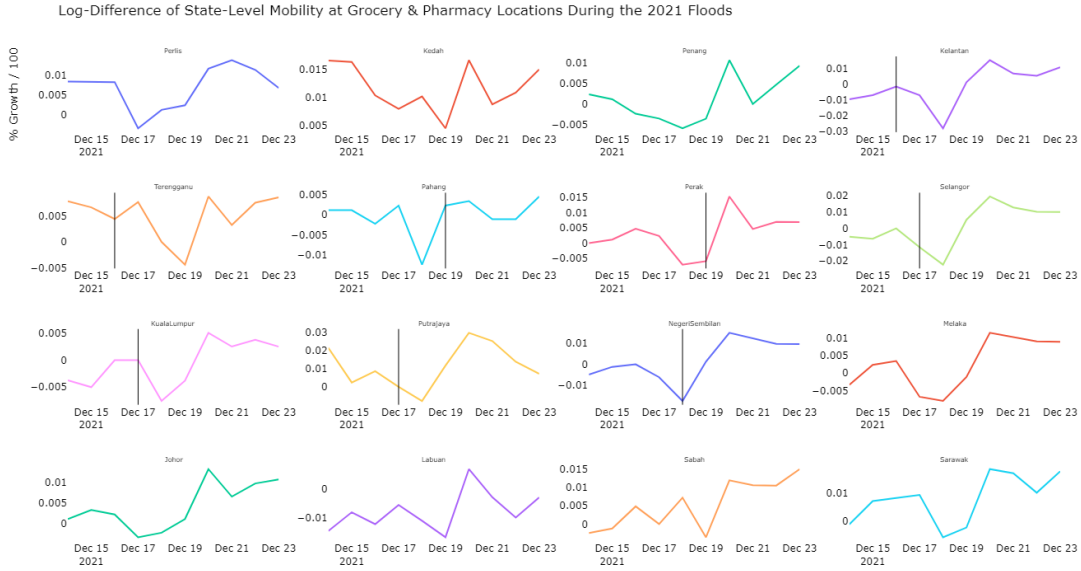


Figure 14

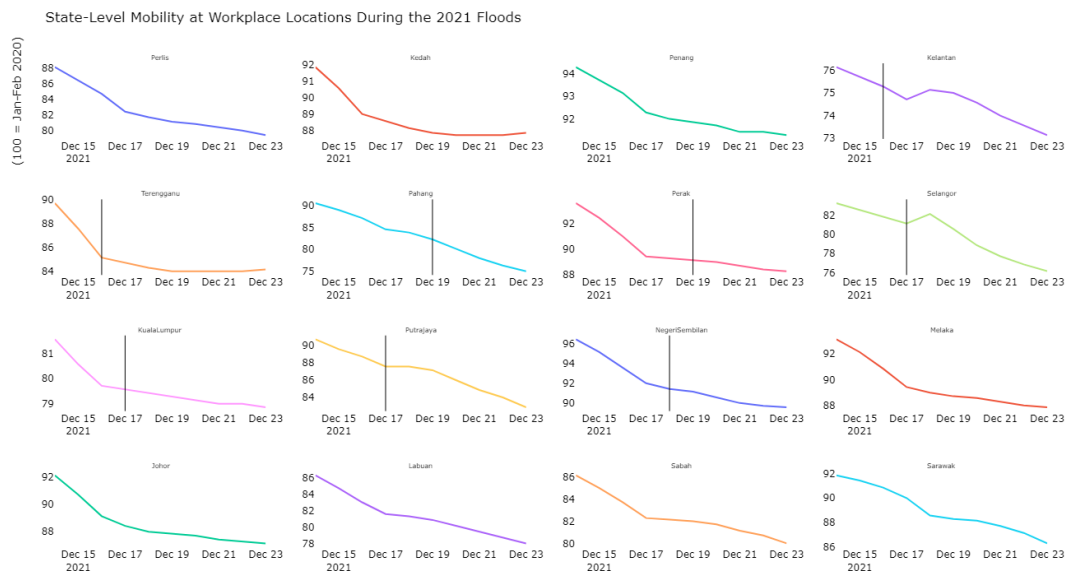
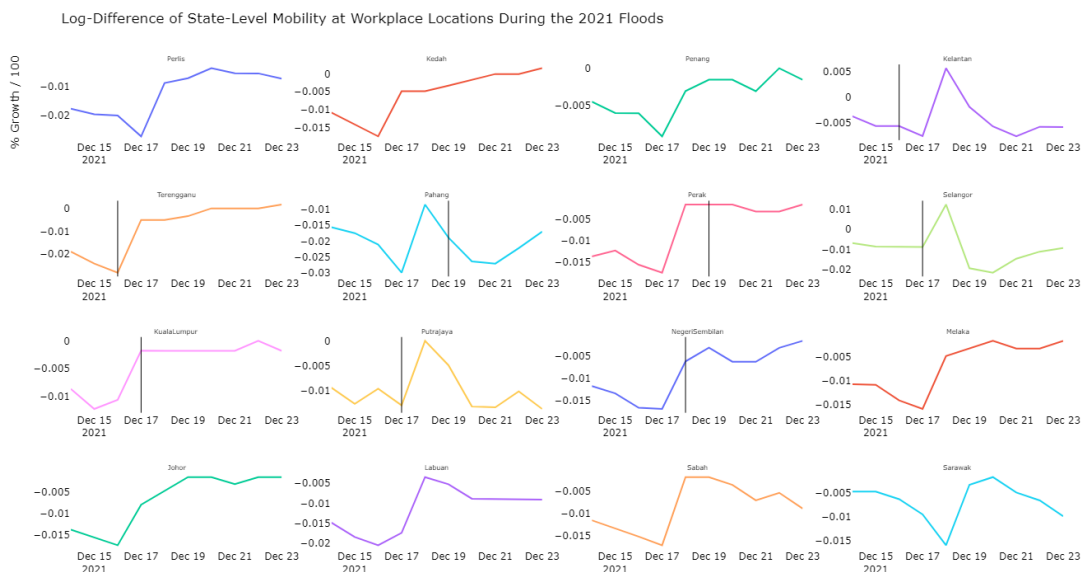


Figure 15



### 3 Adjusted Analysis

#### 3.1 Model Specification

Before pumping the data through an event study regression, we should check for stationarity in the respective series. This will determine if the analysis is best done on levels ( $\beta = \Delta Y$  due to floods at time  $t + k$ ) or log-difference ( $\beta = \frac{\Delta \% Y}{100}$  due to floods at time  $t + k$ ).

The data frame shown below indicates the p-values from the augmented Dickey-Fuller test for the null hypothesis of that the series for the respective state-variable combination has a unit root, conditional on the model / test assumptions, against the alternative that the time series is stationary.

Figure 16

	spending	mob_retl	mob_groc	mob_tran	mob_work	mob_resi	mob_retgro	mob_retgrotran	mob_retgrowork	mob_retgrotranwork
Perlis	0.976	0.989	0.974	0.848	0.998	0.242	0.989	0.971	0.685	0.951
Kedah	0.130	0.997	0.998	0.000	0.000	0.003	0.998	0.981	0.999	0.996
Penang	0.171	0.997	0.000	0.320	0.001	0.519	0.694	0.934	0.079	0.001
Kelantan	0.100	0.000	0.013	0.977	1.000	0.841	0.000	0.000	0.000	0.000
Terengganu	0.960	0.000	0.982	0.759	0.981	0.539	0.924	0.013	0.273	0.324
Pahang	0.992	0.991	0.435	0.999	0.997	0.536	0.000	0.998	0.001	0.999
Perak	0.955	0.950	0.996	0.218	0.451	0.000	0.996	0.279	0.388	0.441
Selangor	0.614	0.283	0.085	0.892	0.969	0.782	0.000	0.002	0.350	0.416
KualaLumpur	0.000	0.390	0.002	0.220	0.415	0.304	0.107	0.095	0.180	0.464
Putrajaya	0.959	0.988	0.881	0.000	1.000	0.719	0.001	0.997	0.841	0.948
Negeri Sembilan	0.994	0.105	0.301	0.146	0.000	0.816	0.000	0.519	0.000	0.461
Melaka	0.999	0.476	0.943	0.858	0.252	0.076	0.284	0.368	0.643	0.000
Johor	0.999	0.992	1.000	0.714	0.736	0.194	0.998	0.904	0.000	0.987
Labuan	0.001	0.432	0.429	0.000	1.000	0.911	0.477	0.156	0.104	0.972
Sabah	0.975	0.998	0.998	0.664	0.106	1.000	0.999	0.998	0.812	0.998
Sarawak	0.951	0.999	1.000	1.000	0.486	0.998	1.000	1.000	0.996	1.000

However, the model assumptions of linear regression-type tests such as the ADF are rather strong. Importantly, spending levels over a short horizon may be more important than trends, which may be more relevant over a longer response horizon. For completeness, and policy-relevance, we will look at use the **log-levels** regression (so that  $\beta = \Delta \% Y$ ).

#### 3.2 Naive TWFE

We are interested in estimating  $\beta_l$ , the coefficients on leads and lags of treatment dummies, where  $l$  is relative time as in Sun & Abraham (2021), i.e., the time period relative to treatment onset.  $l = 0$  refers to when the treatment was applied to entity  $i$ .

$D_{i,t}^l$  are dummies switching on if entity  $i$  is in calendar time  $t$ , and is  $l$  periods relative to the treatment onset. That is also to say that  $D_{i,t}^l \forall t, l$  never-treated entities will take values 0.

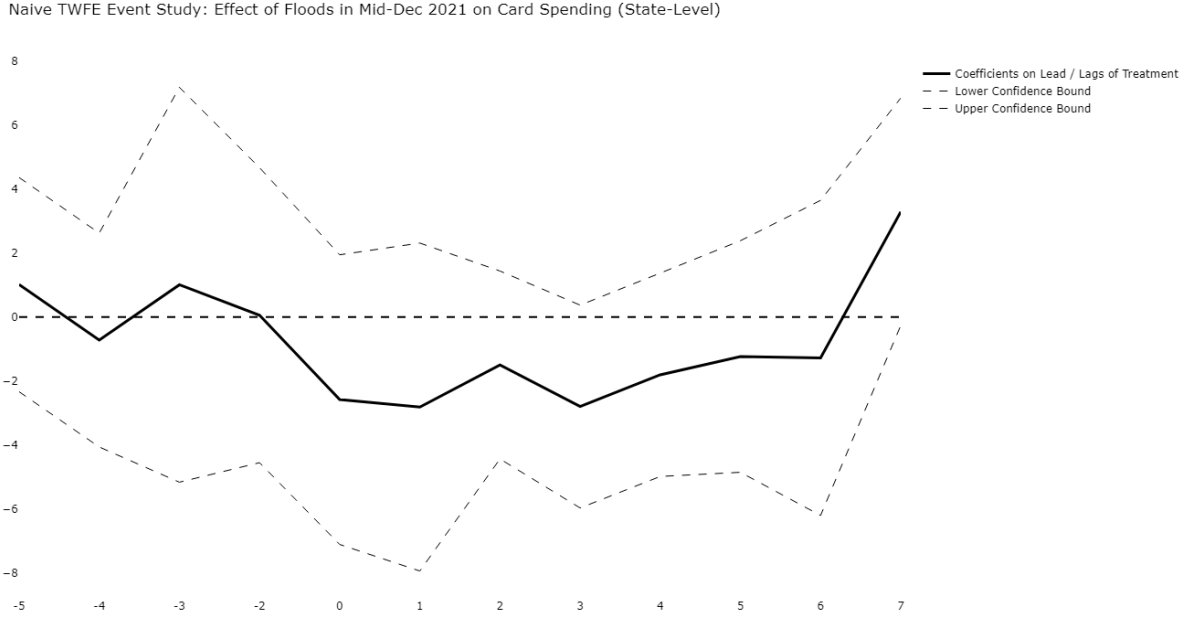
A TWFE regression model includes entity fixed effects ( $\alpha_i$ ), and time fixed effects ( $\alpha_t$ ).  $\mathbf{X}_{i,t}$  is an optional vector of time-varying (within-entity) controls.  $\epsilon_{i,t}$  are the errors.

$$Y_{i,t} = \alpha_i + \alpha_t + \sum_{l=-K}^{-2} \beta_l D_{i,t}^l + \sum_{l=0}^M \beta_l D_{i,t}^l + \mathbf{X}_{i,t} \gamma + \epsilon_{i,t} \quad (3)$$

In this implementation, we regress the log-level of state-level spending on the lead and lag dummies of the flood onset, state fixed effects, day fixed effects, and the average of mobility at retail, pharmacy and grocery, and work locations. The coefficient on the lead and lags are interpreted as the state-day average % change in spending  $Y$  at relative time  $l$  relative to the baseline period (one day before the onset of the flood,  $l = -1$ ), adjusting for changes in mobility. The inclusion of only mobility in  $X$  is on two grounds. Firstly, there is a shortage of state-level daily time series for Malaysia relevant to an analysis of consumer spending. Secondly, the floods were largely unanticipated as they were described as ‘unprecedented in 50 years’, the public flood warnings generally arrived at the same time, or after the actual onset of floods, hence arguably as good as random.

The estimated confidence bands encompass zero across all time periods, hence we cannot ascertain with precision the impact of spending. However, the point estimate of the TWFE nonetheless suggests that spending fell in association with the onset of floods on days 0 through 4 by about 2-3% that of the baseline ( $l = -1$ ), before beginning to return to the baseline from day 5 onwards. Adjusted spending overshoots by about 3-4% on day 7. The wide confidence band is likely a by-product of small numbers of entities in the analysis (16 states). More granular disaggregation in future builds of the data set, and analysis may resolve this issue.

Figure 17



### 3.3 Interaction-Weighted

Estimates dynamic treatment effects using the interaction-weighted estimator described in Sun & Abraham (2021).

For the following structural equation, we are interested in estimating  $\beta_l$ , the coefficients on leads and lags of treatment dummies, where  $l$  is relative time.

$$Y_{i,t} = \alpha_i + \alpha_t + \sum_{l=-K}^{-2} \beta_l D_{i,t}^l + \sum_{l=0}^M \beta_l D_{i,t}^l + \mathbf{X}_{i,t} \gamma + \eta_{i,t} \quad (4)$$

This implementation has 3 broad steps.

Firstly, calculate the cohort shares by relative time,  $\mathbb{E}(E_i = e | E_i \in g)$  where  $g$  is the set of relative times included in the analysis. This package uses a no-constant linear regression model with an OLS estimator as per the Sun & Abraham (2021)'s original Stata package here. Using a linear regression approach, instead of simple tabulation, allows for calculation of standard errors of the cohort share estimates.

$$1\{E_i = e | E_i \in g\} = w_{e,l} D_{i,t}^l + e_i \quad (5)$$

Secondly, estimate the cohort-specific average treatment effects,  $CATT_{e,l}$ , by interacting the cohort dummy with the treatment / relative time dummy,  $1(E_i = e)D_{i,t}^l$ .

$$Y_{i,t} = \alpha_i + \alpha_t + \sum_{l=-K}^{-2} \delta_l 1(E_i = e) D_{i,t}^l + \sum_{l=0}^M \delta_l 1(E_i = e) D_{i,t}^l + \mathbf{X}_{i,t} \gamma + \varepsilon_{i,t} \quad (6)$$

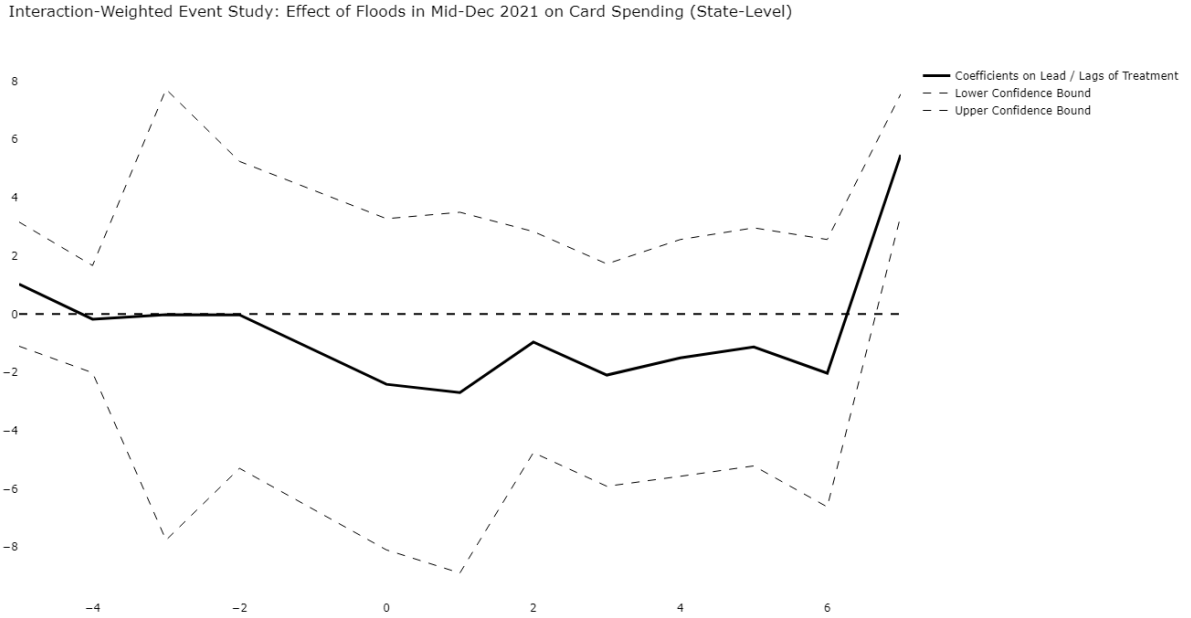
Thirdly, calculate the interaction-weighted average treatment effects using output from the previous steps for every relative time  $l$ . In this analysis, the estimated confidence bands are scaled the same way.

$$\hat{\beta}_l = \sum_e \hat{\delta}_l \hat{w}_{e,l} \quad \forall l \quad (7)$$

Again, similar to the TWFE setup,  $Y$  is card spending, and  $X$  is an empty vector (justified earlier in the TWFE section). The coefficients on leads and lags are interpreted as the state-day average % change in spending  $Y$  at relative time  $l$  relative to the baseline period (one day before the onset of the flood,  $l = -1$ ). However, the difference is that Sun & Abraham (2021)'s methodology avoids the negative weights problem encountered in the naive TWFE arising from staggered event onsets, or the absence of a groups that never experienced the event. It does so specifically by pre-specifying the control groups (either never-treated or last-treated groups, in order of preference), and then interacting the lead and lag terms with the shares of cohorts (entities / groups that experienced the event at the same calendar time). This was previously flagged in Goodman-Bacon (2021) for DiD implementations.

Similar to the TWFE implementation, and likely a by-product of small count of entities (16 states), the estimated CIs span zero across all relative times. The estimated impact on spending on days 0 to 6 from the onset of floods also peaked at about 2-3%, and overshoots on day 7 by about 6% of the baseline ( $l = -1$ ).

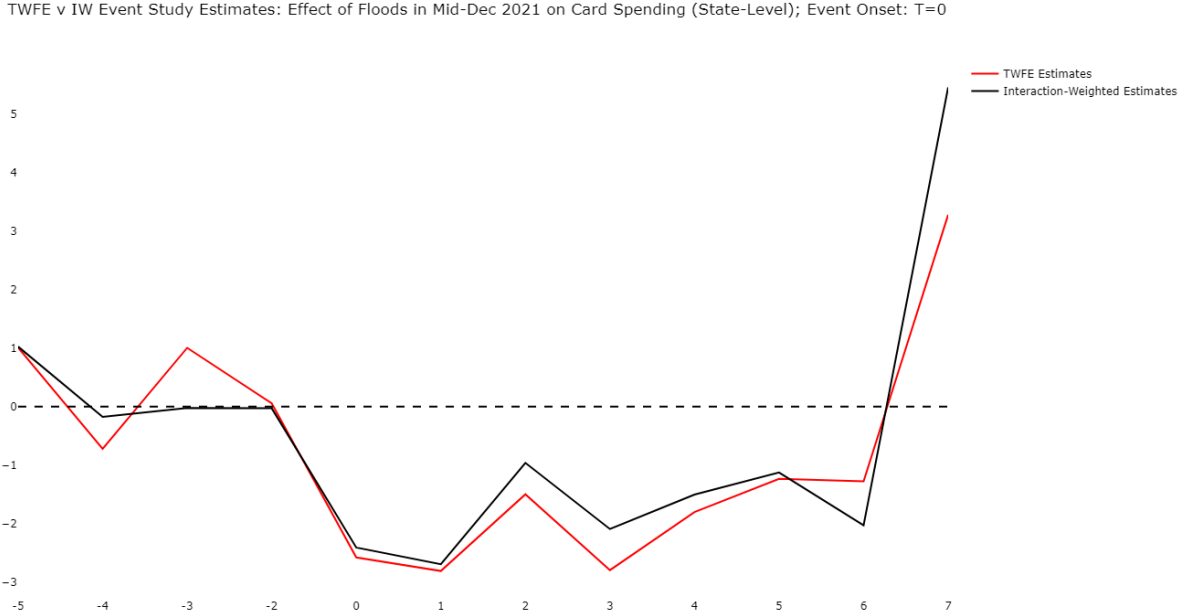
Figure 18



### 3.4 Comparison of TWFE and IW

A direct comparison of the two methodologies show minor differences. On days 0 to 3, both point estimates are similar, peaking at about 3% of spending in the baseline period ( $l = -1$ ). However, in the pre-flood relative times (2 to 5 days before the floods), the TWFE estimate captured some fluctuating trends, but the IW only did so for day 5 before the flood ( $l = -5$ ) at 1% above the baseline. Moreover, the TWFE estimated a smaller overshoot on day 7 post-flood (about 3-4%) than the IW estimator (about 6%). Overall, these are minor, and both estimates are generally similar. This reflects that there were a handful of ‘never flooded’ states, and that the timing of floods, although staggered, were not too far apart, hence limiting the influence of the negative weights problem in the TWFE.

Figure 19



## 4 Conclusion

This paper analysed the trends in daily card spending (dis)aggregated at the state-level to characterise the impact of the mid-December 2021 floods on spending, through (1) a descriptive analysis on levels and growth rates (log-differences), and (2) an adjusted analysis using the latest panel event study methodology, specifically as in Sun & Abraham (2021). Additionally, descriptive analysis on mobility was also analysed.

The core strength of this study is in the use of high-frequency, observed, transactions data from financial institutions that is sufficiently disaggregated in a way that corresponds to the nature of an unanticipated shock (state-level). The main limitation of this study is the event interval (14 December to 23 December; up to 5 days pre-shock, and 7 days post-shock). Extending the event study horizon will overlap with the 12-12 / early December sales, Christmas Eve, and Christmas Day causing distortions to both the time series plots, and adjusted estimates.

In the descriptive plots of spending and mobility levels, there were a visible breaks either in trend or values as and when floods hit. For instance, KL had a visible, but short-lived, dip in spending. Some states experienced longer dips, such as in Kelantan, which may reflect poorer infrastructure, and possibly slower recovery response. Beyond the scope of this study,

the data here also highlights the use case of using high-frequency, (dis)aggregated data to signpost policy inefficiencies, or other frictions for policymakers to address.

The adjusted estimates using **log-spending** as the endogenous variable, and average mobility in retail, grocery and pharmacy, and workplace locations as the exogenous variable, adjusting for cohort-specific effects and distortions due to staggered flood onsets through Sun & Abraham (2021)'s IW estimator affirm the earlier interpretation of the descriptive plots. Estimates from both the TWFE and IW methods were largely the same different, with only minor differences. This reflects that the 'treatment' (flood onsets) were not too staggered, that is they are rather close in calendar time (date) to each other across entities (state). Overall, the average impact of the floods on spending, while short-lived, is substantial. The estimated coefficients indicate that the daily impact on spending was about 2-3%. Spending fully normalises by day 7, with a visible 'correction'. Specifically, the rebound is less than the cumulative negative impact in the IW approach (6%), and the TWFE approach (3-4%).

In terms of policy, handouts should have been released in the aftermath of the flood crisis at a quantum comparable to the estimated spending impact, while overweighting groups that experienced direct flood damages. Further study could be conducted to refine distributional elements. One may study if and how, even at the state-level, groups more directly affected by adverse shocks have a different multiplier to direct expansionary policy, e.g., handouts. For instance, even if consumption is temporarily affected, salaried workers may not experience any loss of income throughout the flood, while businesses employing salaried workers may absorb additional losses. Moreover, another follow-up may be to study the influence of timing of direct economic assistance on the multiplier. Using more disaggregated data, studies may also map the distributional effects of both adverse shocks, and policy interventions, specifically to identify if there are groups more excessively harmed by these shocks, target groups not benefiting from interventions, or any unintended adverse consequences.

### **Acknowledgements and Independent Expert Review**

This paper has benefited from critical review and feedback given by Boon Hwa Tng, Central Bank of Malaysia, Malaysia, and has further received review and appraisal from an external policy and research economist outside of the Central Bank of Malaysia, Malaysia.

### **Replication Codes and Data**

Replication codes are available on [github.com/suahjl/impact-2021-floods](https://github.com/suahjl/impact-2021-floods). The transactions data used in this study is confidential, and is subject to the ISMP, hence will require separate, and additional permission from the data owners. All other data used in the study can be sourced directly, open access, from Google, and the Department of Statistics, Malaysia.

## References

- Aladangady, A., Aron-Dine, S., Dunn, W., Feiveson, L., Lengermann, P. & Sahm, C. (2019), From transactions data to economic statistics: constructing real-time, high-frequency, geographic measures of consumer spending, Technical report, National Bureau of Economic Research.
- Chetty, R., Friedman, J. N., Hendren, N., Stepner, M. et al. (2020), The economic impacts of covid-19: Evidence from a new public database built using private sector data, Technical report, national Bureau of economic research.
- Goodman-Bacon, A. (2021), ‘Difference-in-differences with variation in treatment timing’, *Journal of Econometrics* **225**(2), 254–277.
- Lewis, D. J., Mertens, K., Stock, J. H. & Trivedi, M. (2021), High-frequency data and a weekly economic index during the pandemic, *in* ‘AEA Papers and Proceedings’, Vol. 111, pp. 326–30.
- Sun, L. & Abraham, S. (2021), ‘Estimating dynamic treatment effects in event studies with heterogeneous treatment effects’, *Journal of Econometrics* **225**(2), 175–199.