

BANK NEGARA MALAYSIA
CENTRAL BANK OF MALAYSIA

Bank Negara Malaysia Working Papers
WP1/2023

An Application of the Forecast-Based Interrupted Time Series Methodology to Evaluate Policy Impact Using Daily Data: The Impact of COVID-19 Cash Handouts on Daily Spending

By Jing Lian Suah

13 January 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.

An Application of the Forecast-Based Interrupted Time Series Methodology to Evaluate Policy Impact Using Daily Data: The Impact of COVID-19 Cash Handouts on Daily Spending

Jing Lian Suah

Central Bank of Malaysia, Malaysia

January 13, 2023

Abstract

Slow-moving traditional macroeconomic data cannot adequately capture the dynamics of shocks in volatile environments, hence the increasing use of high frequency data, such as daily transactions, and mobility since the COVID-19 pandemic. While difference-in-difference (DiD) estimators are the standard of impact evaluation in non-trial settings, economic policies were typically implemented at the national-level, resulting in a lack of observed control groups. Fortunately, the interrupted time series (ITS) methodology, prevalent in the adjacent field of epidemiology, addresses this challenge using aggregated, high-frequency, time series data. This study applies the ITS methodology to evaluate the impact of three episodes of cash handouts in Malaysia (May 2020, July 2020, and September 2021) on daily spending. This forecast-based ITS setup constructs a post-intervention 'counterfactual' using forecast of daily transactions from an autoregression, with daily mobility as exogenous predictors (AR(n)-X) to account for changes in non-pharmaceutical interventions (NPIs). In all three episodes, the impact on spending is positive, precisely estimated, and dissipates within a month. The spending multipliers were lower when economic conditions were known to be more accommodative. Further research may help policymakers consider the interaction between timing, quantum, and precision (targeted versus broad) of both direct, and indirect stimulus measures on spending.

Keywords: Interrupted Time Series, Daily Data, Impact, Transactions, Cash Transfers
JEL Codes: C01, C32, E60, E65

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

1 Introduction

Slow-moving traditional macroeconomic data cannot adequately capture the dynamics of volatile environments, such as the COVID-19 pandemic. Consequently, high-frequency, and granular data found their way in several areas. The first is surveillance. Chetty et al. (2020), and Lewis et al. (2021) introduced the use of granular (high-frequency, and / or highly disaggregated) data, specifically real-time firm data aggregated at the ZIP code-level, and the Federal Reserve's Weekly Economic Index (WEI) to characterise the economic effects of the COVID-19 pandemic in the US. In terms of public health, non-pharmaceutical interventions (NPIs), including mask mandates, and movement restrictions, were similarly reflected in daily data — (i) tightening of de jure stringency (Hale et al. 2020) from March 2020 onwards, after the first round of lockdowns in China in January to February 2020, and (ii) the corresponding evolution of Google's daily mobility indicators (Google 2022). The second is policy evaluation. To address the economic fallout of the pandemic before the accessible mass distribution of first-generation vaccines, Suah (2020) argued that economic policy should be direct, timely, and adequate to ensure that incomes are supported, without compromising the then-goal of containing SARS-CoV-2 transmission. Casado et al. (2020), using high-frequency data on transactions and unemployment claims, found that the COVID-19 unemployment insurance supplement led to significantly higher consumer spending, even with increases in unemployment claims. The third is the evaluation of adverse shocks. Before the COVID-19 pandemic, Aladangady et al. (2019) illustrated the value of using high-frequency data to evaluate the impact of unanticipated weather shocks (hurricanes) on spending.

Broadly, policy evaluation in non-trial settings rests on the difference-in-difference (DiD) methodology, such as estimators proposed by Goodman-Bacon (2021), and Sun & Abraham (2021). However, episodes of economic policy may not necessarily come with observed control groups, a requisite of the DiD methodology. For instance, Sun & Abraham (2021) require at least one never-treated, or last-treated (last to receive the treatment chronologically) group in the data set to act as the control group. During the pandemic, both direct and indirect stimulus measures were implemented, primarily cash transfers, debt reliefs, credit guarantees, policy rate cuts, quantitative easing, and direct monetary financing, as documented over time by the International Monetary Fund (IMF) in IMF (2022). In most instances, these were implemented at the national-level, and concurrently, including the repeated cash transfers implemented in Malaysia over 2020-21. Fortunately, the interrupted time series (ITS) methodology, common in the adjacent field of epidemiology, e.g., (Bernal et al. 2017), (Lopez Bernal et al. 2013), and (Biglan et al. 2000), can be used to overcome this methodological challenge, which involves extending the pre-intervention trend as the post-intervention 'counterfactual' for impact evaluation, in the spirit of Lopez Bernal et al. (2019). One imple-

mentation in the context of single-entities involves using out-of-sample forecasts from a time series model estimated with pre-intervention data as the post-intervention 'counterfactual'. This was demonstrated in Linden (2018), and Schaffer et al. (2021), which differ from conventional ITS implementations that use binary pre- and post-intervention indicators similar to DiD. Depending on the choice of modelling, the post-intervention 'counterfactual' may incorporate not only dynamic out-of-sample forecasts, but also exogenous variables observed in the post-intervention period. This forecast-based ITS also differs from the single entity event study approach, which Gürkaynak et al. (2004) implemented to study the asset price responses to monetary policy shocks, in that impact is estimated through time dummies or a trend term in a single regression model, versus the use of out-of-sample forecasts in the post-event period.

In this study, the ITS is implemented to evaluate the impact of three episodes of cash transfers in Malaysia on daily spending. The first is cash transfers amounting to MYR11.5 billion based on reports from the Ministry of Finance (MOF) coinciding with the conditional movement control order (CMCO) on 4 May 2020, after the strict lockdown phase earlier in 18 March to 3 May 2020 (MOF 2020*b*). MYR11.5 billion here includes cash handouts disbursed between 6 to 15 April 2020. However, using 4 May 2020 as the onset date of the handouts has clear merits. During the strict lockdown phase in 18 March to 3 May 2020, most stimulus checks would not have been spent, as movement, and most economic sectors were prohibited in full. Hence, the effective start date in which spending can take place triggered by the handouts is when a new round of transfers were disbursed on 4 May 2020, which coincided with the relaxation of NPIs. As this coincided with the temporary withdrawals of statutory retirement savings in the Employers Provident Fund (EPF), dubbed i-Lestari from 4 May 2020 till April 2021, the study also includes an additional MYR1.9 billion, the amount of i-Lestari (replaced eventually by i-Citra and i-Sinar in 2021) withdrawals in May alone, on top of MYR11.5 billion from the cash transfers, totalling MYR13.4 billion. The second is the Bantuan Sara Hidup Fasa 3 (BSH3) amounting to MYR3 billion announced on 20 July 2020. The third is the Bantuan Prihatin Rakyat Fasa 3 (BPR3) amounting to MYR2.82 billion announced on 26 September 2021. Unlike the CMCO episode, the statutory retirement savings withdrawals do not affect the BSH 3 and BPR 3 studies. Specifically, the pre- and post-intervention periods are enveloped by the withdrawal periods, hence the withdrawals are likely absorbed in the respective counterfactuals. These correspond to 5.09%, 1.33%, and 1.25%, respectively, of the average quarterly nominal private consumption expenditure in 2019. The post-intervention 'counterfactual' is estimated from an autoregression of daily transactions, collected from financial institutions and payments system operators in Malaysia, with daily mobility indicators from Google (Google 2022) as exogenous predictors (AR(n)-X) to control for changes in NPIs. Specifically, the methodology estimates the excess daily transactions observed relative to what is implied by pre-intervention trends, and the

post-intervention relaxation of mobility restrictions, over the very near-term (within-month at a daily frequency). This gap is interpreted as the impact of the cash handouts on daily spending, which is subsequently used to compute the spending multipliers of each episode of cash handouts.

In short, this paper attempts to address the question 'what was the impact of the COVID-19 cash handouts implemented nationally on spending, accounting for the lack of an observed control group?' using daily transactions, and mobility data on Malaysia in an ITS setup. The descriptive analysis will discuss the data by episode of cash handouts, while the adjusted analysis will discuss by analytical steps (unit root tests, construction of counterfactuals, impact estimates, and spending multipliers) to best compare the findings.

2 Descriptive Analysis

2.1 Data

The first set of two data sources is the daily transactions at the national-level, which is collected from a representative share of financial institutions (about 60 to 70% of the banking sector) and payment system operator (monopoly firm) in Malaysia. While the transactions data set in this paper captures mainly transactions by individuals, those by businesses are also included. This data set is adjusted at a daily frequency at source, with a SARIMAX, adjusting for Malaysia-specific calendar effects. Routine spikes and troughs associated with salary disbursements typically at the end of each calendar month should be accounted for as a result.

Compared to quarterly transactions against nominal private consumption expenditure and services components of gross domestic product (GDP), minor discrepancies between transactions and the GDP components may arise from definitional differences in the national accounts, such as the exclusion of certain fringe items, and survey uncertainty in GDP estimates.

In its disaggregated form, this data set contains FPX online transactions (inter-bank transfers for Malaysian banks), MyDebit card transactions (Malaysia-registered debit cards), ATM withdrawals, and debit and credit card transactions, including a further physical-online breakdown. The cashless component is estimated from all categories other than ATM withdrawals, and the cash component is subsequently estimated from the dynamic ATM-cashless ratio. Differently, a physical versus online split is also possible. Conceptually, daily transactions at the national-level can be disaggregated into the following splits and combinations.

- **Exhaustive**: Cash (estimated based on the ATM-cashless ratio) versus cashless (sum of remaining components)
- **Exhaustive**: Physical (cash, and card transactions at brick-and-mortar locations) versus online (all other cashless components)
- **Non-exhaustive specific components**: FPX online transactions (inter-bank transfers), MyDebit card transactions, ATM withdrawals, all card transactions (physical, and online)

The second of which is the Google mobility index for retail & recreation, and grocery & pharmacy locations. Other indices, such as workplace, were excluded due to some movement restrictions that were not removed throughout 2020-21, e.g., enforced hybrid or remote working arrangements for some economic sectors. The mobility indicators were expressed as 7-day moving averages to address within-week seasonal fluctuations. As the seasonal adjustment methodology for the transactions data was implemented at source for this research, the mobility indices were not given the same treatment.

Both data sets are re-scaled in levels for ease of interpretation. Transactions data Y was originally available in MYR, but rescaled (100-indexed) to spending levels observed on 1 January 2020 for day t (equation 1). Mobility M were initially zero-indexed to levels in Jan-Feb 2020. To avoid numerical complications of negative real numbers, mobility M was also converted to 100-index, by simply adding 100 to all observations (equation 2).

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

$$M_{i,t}^{rescaled} = M_{i,t} + 100 \tag{2}$$

2.2 Time Series Plots of National-Level Data

2.2.1 CMCO (May 2020) Handouts

Figure 1 plots the time series of total transactions, the various disaggregation of transactions, and mobility, with the start of cash transfers disbursement, and the relaxation of lockdowns (4 May 2020) marked by a vertical line. Total spending, and most spending categories, increased visibly post-disbursement, before returning roughly to trend. These were visible for cash, physical, debit card (MyDebit), JomPay (a bills payment scheme by the national payments system operator), ATM withdrawals, and physical credit card spending.

However, online spending and subcategories (FPX, and online card spending) had a visibly persistent elevation above pre-disbursement trend. Mobility, in contrast, improved steadily, reflecting the relaxation in movement restrictions, but without the comparable observations for transactions post-disbursement.

Figure 1

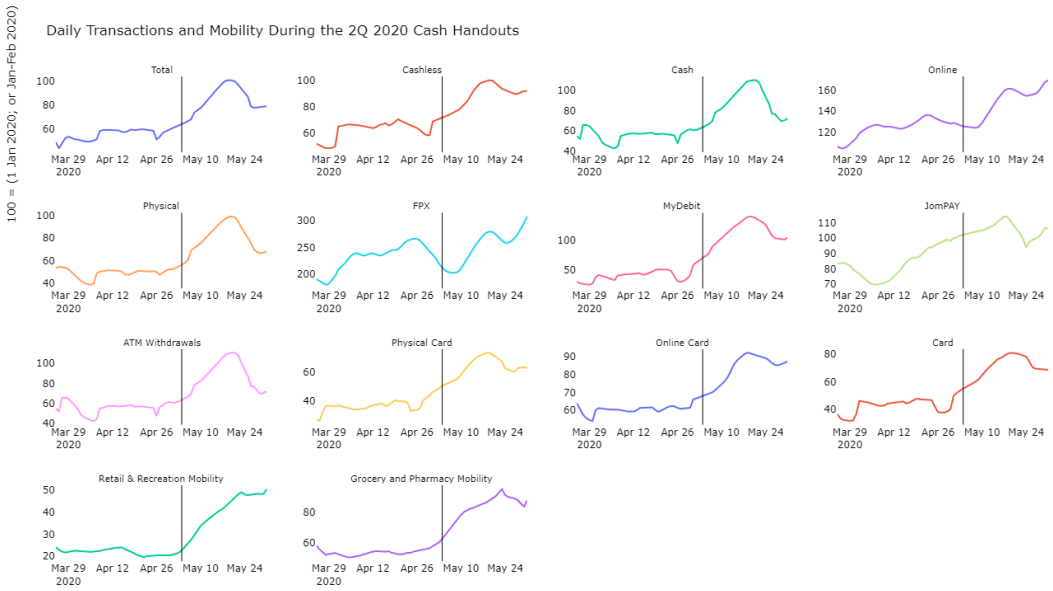
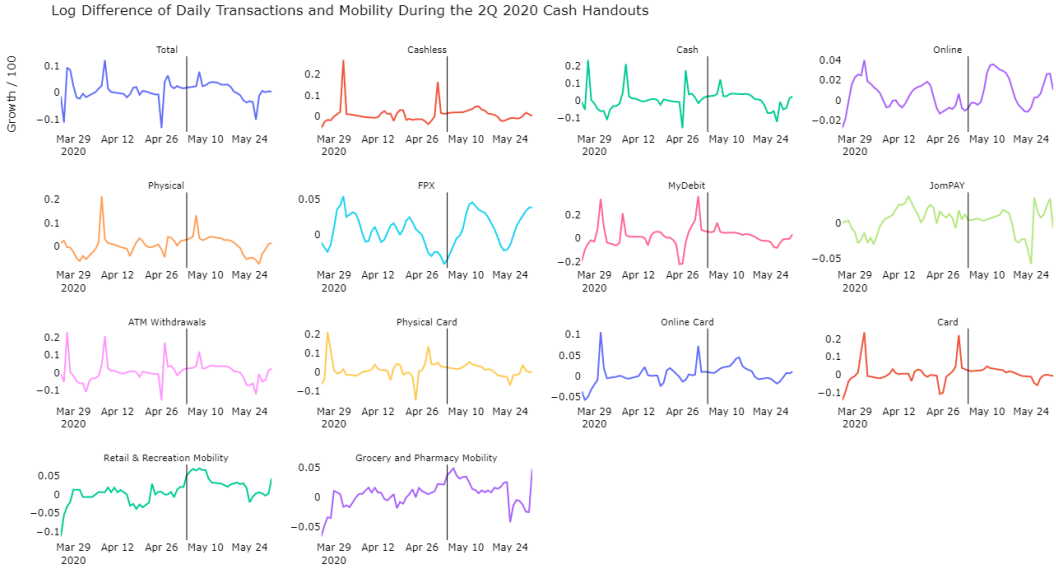


Figure 2 shows the log-difference version of the same indicators shown in figure 1, interpreted therefore as daily growth divided by 100 ($\frac{\%\Delta Y}{100}$). Similarly, total transactions, and physical spending categories registered large spikes initially, but corrected close to 0 growth, but online transactions and related subcategories registered sustained positive growth up until 20 May. The growth patterns of mobility indicators, as in levels, were not comparable to transactions, suggesting that they are capturing primarily the relaxation of NPIs, rather than consumer spending.

Figure 2



2.2.2 BSH 3 (July 2020) Handouts

Figure 3 plots the same variables as in figure 1, with 20 July 2020, when the disbursement date was announced (4 days later on 24 July 2020), marked by a vertical line. The increase in total spending, and its disaggregation, is less prominent than in the CMCO handouts. This may reflect the smaller sized transfers than in May 2020. However, similar to the CMCO episode, the post-disbursement increase was more prominent for cash, physical, debit card (MyDebit), JomPay, ATM withdrawals, and physical credit card spending, than for online spending and subcategories (FPX, and online card spending) had a visibly persistent elevation above pre-disbursement trend. Mobility, in contrast, improved steadily, reflecting the relaxation in movement restrictions, but without the comparable observations for transactions post-disbursement.

Figure 3

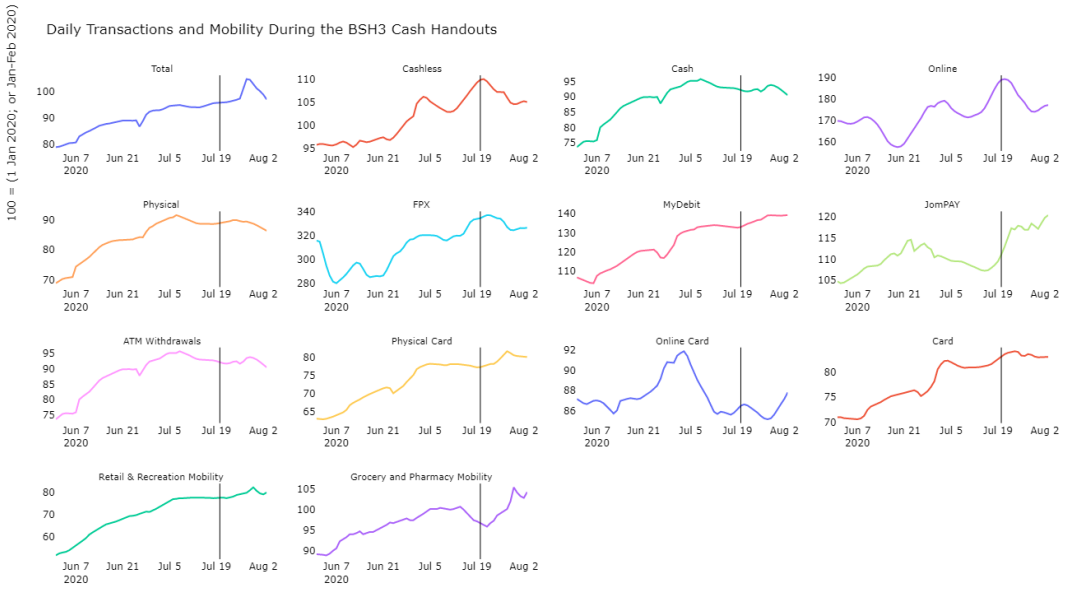
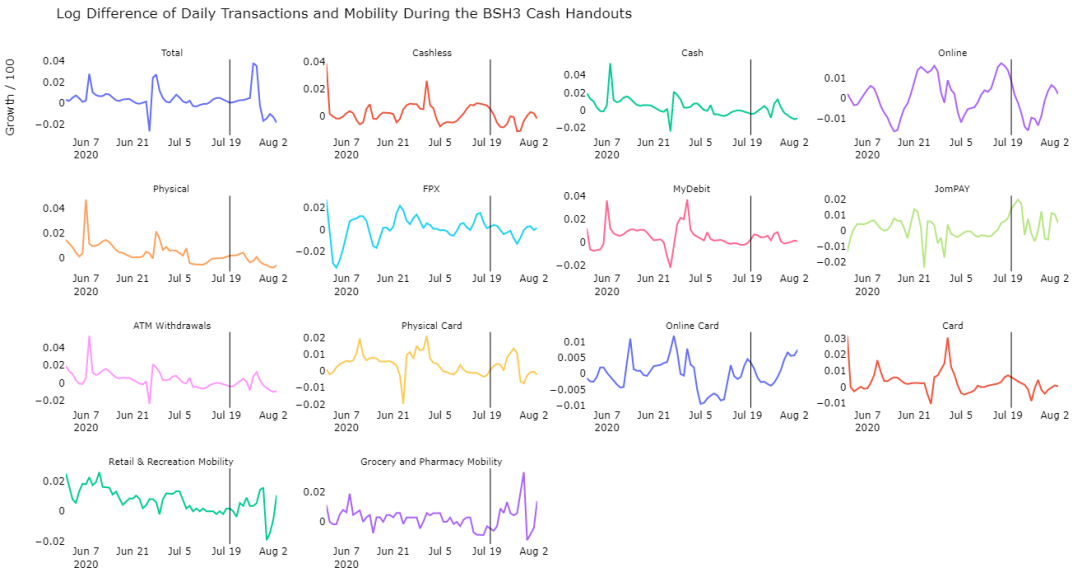


Figure 4 plots the log-difference version of figure 3. The daily growth of total spending surged briefly, before entering a correction phase, as did cash, ATM withdrawals debit card, and physical card spending. Cashless, and online spending, and its subcategories (FPX, and online card spending) exhibited different trends than the former categories.

Figure 4



2.2.3 BPR 3 (September 2021) Handouts

Figure 5 plots spending, and mobility, with 26 September 2021, the announcement date of the disbursement of the BPR 3 handouts (2 days later on 28 September 2021), marked by a vertical line. In contrast to the previous episodes of handouts, the descriptive statistics are unclear, with spending categories recording a mostly flat trend post-disbursement. Upticks post-26 September 2021 appear minor. Similar to the July 2020 episode, this likely reflects a smaller sized transfer, and that economic conditions were more accommodative amid a slew of loosening of NPIs, and ramped up COVID-19 vaccination efforts.

Figure 5

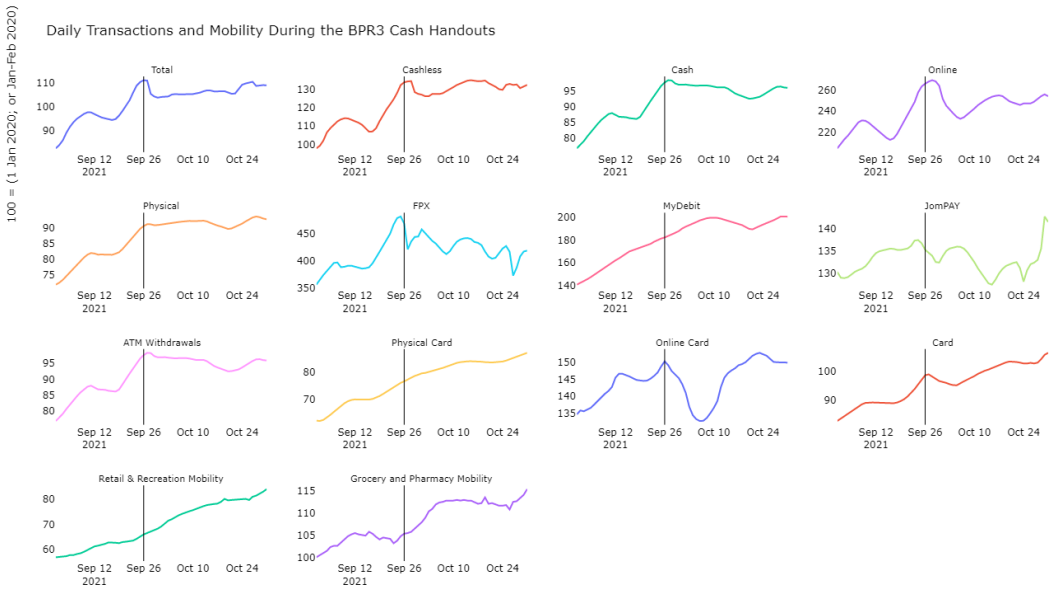
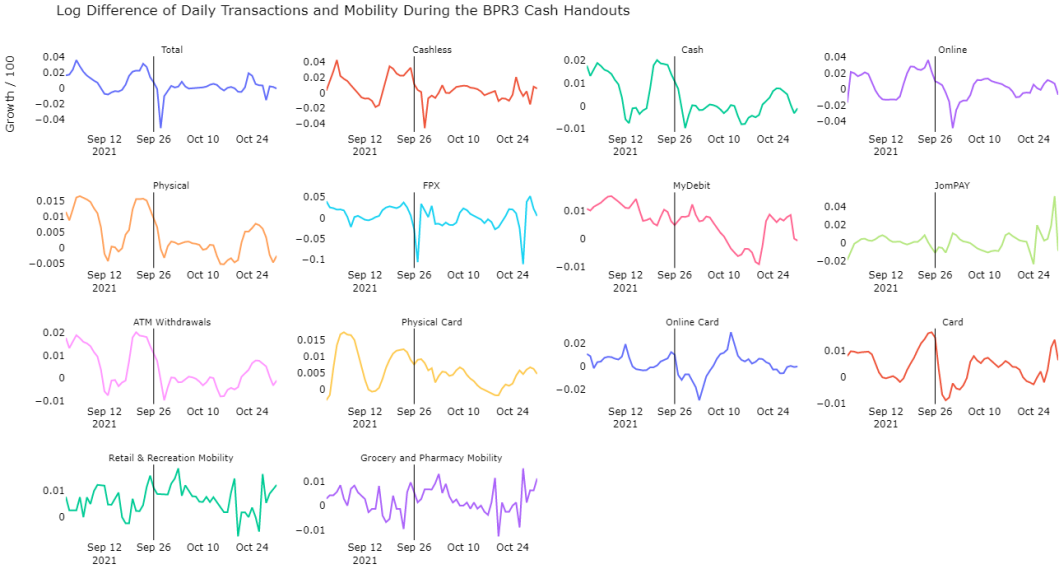


Figure 6 shows the log-difference transformation, with growth of most spending variables being steady at zero, reflecting the flat post-disbursement trends observed in figure 6.

Figure 6



3 Methodology

There are two methodological challenges in estimating specifically the impact of cash transfers on spending at a daily frequency here. Firstly, there is a no observed control group as (i) the transfers were concurrent nationally, and (ii) while primarily aimed at the lower income groups based on tax records, there is no disaggregation of transactions data along this dimension. This prevents the use of the event study methodology in Sun & Abraham (2021). This methodology requires explicitly an observed control group, either never received the intervention, or the last group that received the intervention chronologically. This includes its umbrella methodology — difference-in-difference (DiD), whose latest innovations are described in Goodman-Bacon (2021). Secondly, the disbursements of cash handouts, and the relaxation or tightening of NPIs may overlap, raising simultaneity concerns. Specifically, the effects of both policies are aggregated and not discernable in the observed data.

Hence, the interrupted time series (ITS) methodology, commonly used in epidemiology settings where DiD and randomised clinical trials are not feasible, was chosen. Specifically, this paper's implementation of the ITS implementation constructs a post-intervention 'counterfactual' using forecast of daily transactions from an autoregression, with daily mobility as exogenous predictors (AR(n)-X), where the order of lag is chosen by the Hannan-Quinn Information Criteria (Hannan & Quinn 1979) (HQIC). This time series model forecast-based implementation of the ITS was previously demonstrated in Linden (2018), and Schaffer et al.

(2021), and falls within the description of the ITS in Lopez Bernal et al. (2019). Conventionally, an ITS has a specification that more closely resembles a dynamic DiD regression, with pre- and post-intervention dummy interactions. Here, the augmented Dickey-Fuller test was also implemented to determine (1) if AR-X should be estimated with log-level or log-difference transformation, and (2) if the constant, trend, and / or quadratic trend terms should be included in the AR-X model. Irrespective of the transformation used in the estimation of the AR-X model, the forecast is expressed in log-levels before calculating the impact.

In essence, this paper estimates the excess (shortfall, or lack thereof) daily transactions observed relative to levels implied by pre-intervention trends, and the post-intervention relaxation of mobility restrictions. This is taken as the impact of cash transfers on daily spending.

3.1 Model Specification

This part describes the specification of the AR-X model used in this ITS implementation, and the equations used to calculate the impact estimate, and spending multiplier. Equations 3 and 4 the transformation of input variables through a function $f()$, which may be log-difference ($\Delta ln(Y)$) or log-levels ($ln(Y)$), based on findings from the unit root test. y_t is total transactions, and x_t the vector of mobility indices, either log-difference or log-levels. This may be determined through running a series of unit root tests, e.g., Augmented Dickey-Fuller tests, estimated below.

$$y_t = f(Y_t) \tag{3}$$

$$\mathbf{x_t} = f(\mathbf{X_t}) \tag{4}$$

Tables 1a-c show the estimates from the Augmented Dickey-Fuller test for all three episodes of the cash handouts. This test informs (i) if the AR(n)-X should be estimated in log-levels, or log-difference, (ii) if both mobility indicators for retail & recreation, and grocery & pharmacy locations should be included, and (iii) if the AR(n)-X should be estimated with a constant term, and / or a trend term, or without both terms. Table 1a and 1b suggest a log-difference transformation of both total transactions, the retail & recreation, and pharmacy & grocery mobility indicators, and without the constant term in the AR(n)-X specification for the CMCO and BSH 3 applications. Table 1c suggests the inclusion of a constant term, and only the log-difference of pharmacy & grocery mobility as the exogenous variable, for the BPR 3 application.

Table 1: Augmented Dickey-Fuller Tests

(a) CMCO (May 2020)

	variable	cons_trend	pval	UnitRoot
0	ln(Total)	None	0.899	Yes
1	ln(Total)	Constant	0.479	Yes
2	ln(Total)	Constant & Trend	0.384	Yes
3	ln(Total)	Constant & Quad Trend	0.535	Yes
4	ln(Retail & Grocery Mobility)	None	0.897	Yes
5	ln(Retail & Grocery Mobility)	Constant	0.500	Yes
6	ln(Retail & Grocery Mobility)	Constant & Trend	0.738	Yes
7	ln(Retail & Grocery Mobility)	Constant & Quad Trend	0.698	Yes
8	ln(Retail Mobility)	None	0.758	Yes
9	ln(Retail Mobility)	Constant	0.273	Yes
10	ln(Retail Mobility)	Constant & Trend	0.160	Yes
11	ln(Retail Mobility)	Constant & Quad Trend	0.770	Yes
12	ln(Grocery Mobility)	None	0.941	Yes
13	ln(Grocery Mobility)	Constant	0.933	Yes
14	ln(Grocery Mobility)	Constant & Trend	0.856	Yes
15	ln(Grocery Mobility)	Constant & Quad Trend	0.650	Yes
16	Δln(Total)	None	0.000	No
17	Δln(Total)	Constant	0.000	No
18	Δln(Total)	Constant & Trend	0.000	No
19	Δln(Total)	Constant & Quad Trend	0.000	No
20	Δln(Retail & Grocery Mobility)	None	0.003	No
21	Δln(Retail & Grocery Mobility)	Constant	0.035	No
22	Δln(Retail & Grocery Mobility)	Constant & Trend	0.074	Yes
23	Δln(Retail & Grocery Mobility)	Constant & Quad Trend	0.740	Yes
24	Δln(Retail Mobility)	None	0.000	No
25	Δln(Retail Mobility)	Constant	0.000	No
26	Δln(Retail Mobility)	Constant & Trend	0.000	No
27	Δln(Retail Mobility)	Constant & Quad Trend	0.005	No
28	Δln(Grocery Mobility)	None	0.000	No
29	Δln(Grocery Mobility)	Constant	0.002	No
30	Δln(Grocery Mobility)	Constant & Trend	0.007	No
31	Δln(Grocery Mobility)	Constant & Quad Trend	0.100	Yes

(b) BSH 3 (July 2020)

	variable	cons_trend	pval	UnitRoot
0	ln(Total)	None	1.000	Yes
1	ln(Total)	Constant	0.273	Yes
2	ln(Total)	Constant & Trend	0.806	Yes
3	ln(Total)	Constant & Quad Trend	0.312	Yes
4	ln(Retail & Grocery Mobility)	None	0.978	Yes
5	ln(Retail & Grocery Mobility)	Constant	0.070	Yes
6	ln(Retail & Grocery Mobility)	Constant & Trend	0.993	Yes
7	ln(Retail & Grocery Mobility)	Constant & Quad Trend	0.525	Yes
8	ln(Retail Mobility)	None	0.946	Yes
9	ln(Retail Mobility)	Constant	0.115	Yes
10	ln(Retail Mobility)	Constant & Trend	0.967	Yes
11	ln(Retail Mobility)	Constant & Quad Trend	0.298	Yes
12	ln(Grocery Mobility)	None	0.945	Yes
13	ln(Grocery Mobility)	Constant	0.160	Yes
14	ln(Grocery Mobility)	Constant & Trend	1.000	Yes
15	ln(Grocery Mobility)	Constant & Quad Trend	0.985	Yes
16	Δln(Total)	None	0.000	No
17	Δln(Total)	Constant	0.000	No
18	Δln(Total)	Constant & Trend	0.000	No
19	Δln(Total)	Constant & Quad Trend	0.000	No
20	Δln(Retail & Grocery Mobility)	None	0.003	No
21	Δln(Retail & Grocery Mobility)	Constant	0.008	No
22	Δln(Retail & Grocery Mobility)	Constant & Trend	0.001	No
23	Δln(Retail & Grocery Mobility)	Constant & Quad Trend	0.002	No
24	Δln(Retail Mobility)	None	0.020	No
25	Δln(Retail Mobility)	Constant	0.075	Yes
26	Δln(Retail Mobility)	Constant & Trend	0.029	No
27	Δln(Retail Mobility)	Constant & Quad Trend	0.087	Yes
28	Δln(Grocery Mobility)	None	0.000	No
29	Δln(Grocery Mobility)	Constant	0.000	No
30	Δln(Grocery Mobility)	Constant & Trend	0.000	No
31	Δln(Grocery Mobility)	Constant & Quad Trend	0.000	No

(c) BPR 3 (September 2021)

	variable	cons_trend	pval	UnitRoot
0	ln(Total)	None	0.992	Yes
1	ln(Total)	Constant	0.000	No
2	ln(Total)	Constant & Trend	0.005	No
3	ln(Total)	Constant & Quad Trend	0.538	Yes
4	ln(Retail & Grocery Mobility)	None	1.000	Yes
5	ln(Retail & Grocery Mobility)	Constant	0.483	Yes
6	ln(Retail & Grocery Mobility)	Constant & Trend	0.339	Yes
7	ln(Retail & Grocery Mobility)	Constant & Quad Trend	0.607	Yes
8	ln(Retail Mobility)	None	1.000	Yes
9	ln(Retail Mobility)	Constant	0.971	Yes
10	ln(Retail Mobility)	Constant & Trend	0.006	No
11	ln(Retail Mobility)	Constant & Quad Trend	0.643	Yes
12	ln(Grocery Mobility)	None	0.983	Yes
13	ln(Grocery Mobility)	Constant	0.303	Yes
14	ln(Grocery Mobility)	Constant & Trend	0.795	Yes
15	ln(Grocery Mobility)	Constant & Quad Trend	0.999	Yes
16	Δln(Total)	None	0.039	No
17	Δln(Total)	Constant	0.014	No
18	Δln(Total)	Constant & Trend	0.073	Yes
19	Δln(Total)	Constant & Quad Trend	0.953	Yes
20	Δln(Retail & Grocery Mobility)	None	0.316	Yes
21	Δln(Retail & Grocery Mobility)	Constant	0.536	Yes
22	Δln(Retail & Grocery Mobility)	Constant & Trend	0.966	Yes
23	Δln(Retail & Grocery Mobility)	Constant & Quad Trend	1.000	Yes
24	Δln(Retail Mobility)	None	0.252	Yes
25	Δln(Retail Mobility)	Constant	0.122	Yes
26	Δln(Retail Mobility)	Constant & Trend	0.265	Yes
27	Δln(Retail Mobility)	Constant & Quad Trend	0.965	Yes
28	Δln(Grocery Mobility)	None	0.004	No
29	Δln(Grocery Mobility)	Constant	0.024	No
30	Δln(Grocery Mobility)	Constant & Trend	0.058	Yes
31	Δln(Grocery Mobility)	Constant & Quad Trend	0.156	Yes

An AR(n)-X is then estimated using pre-intervention data ($t \leq -1; t = [L, -1]$), where the order of lags for transactions (n) is determined by the HQIC, and mobility indicators are included as exogenous variables (contemporaneous terms). This is shown in equation 5.

$$y_{t,t\leq -1} = \mathbf{y_{t-n,t\leq -1}}\beta + \mathbf{x_{t,t\leq -1}}\gamma + \varepsilon_{t,t\leq -1} \tag{5}$$

Subsequently, using the estimated parameters ($\widehat{\beta}$ and $\widehat{\gamma}$), and data for the exogenous variables from the post-intervention period ($t \geq 0; t = [0, T]$), the AR(n)-X model is used to produce a dynamic forecast of transactions $\widehat{\mathbf{y_{t\geq 0}}}$ (equation 6). Specifically, this forecast series is used as the counterfactual as if only de facto changes in NPIs were implemented, pre-intervention trends were sustained, but without the cash transfers.

$$\widehat{\mathbf{y_{t\geq 0}}} = \mathbf{y_{t-n,t\geq 0}}\widehat{\beta} + \mathbf{x_{t,t\geq 0}}\widehat{\gamma} \tag{6}$$

Before calculating the impact, conversion back to log-levels is necessary, expressed as a function $g()$ (equation 7). Impact θ_t is then taken simply as the difference between the

natural logarithm of the observed transactions and the predicted counterfactual (equation 8), with the 95% lower and upper confidence bands calculated similarly using the corresponding forecast values (equations 9 and 10). The run in period for the post-intervention period can be informed by when impact converges towards 0, that is that the counterfactual and observed levels become indistinguishable, while allowing room for delayed responses, and the possibility of sign reversals in earlier / later periods.

$$Y_t = g(y_t) \tag{7}$$

$$\theta_t = Y_{t,t\geq 0} - \widehat{Y_{t,t\geq 0}} \tag{8}$$

$$\theta_t^{UB} = Y_{t,t\geq 0} - \widehat{Y_{t,t\geq 0}^{UB}} \tag{9}$$

$$\theta_t^{LB} = Y_{t,t\geq 0} - \widehat{Y_{t,t\geq 0}^{LB}} \tag{10}$$

The spending multiplier λ, analogous to the marginal propensity to consume (mpc) from an increase in income, is simply the ratio of the estimated cumulative impact to the quantum of handouts G in nominal terms (equation 11). The upper and lower confidence bands are calculated using the corresponding cumulative impact estimates (equations 12 and 13). Hence, λ is the estimated increase in spending associated with 1 MYR of handouts G.

$$\lambda = \frac{\sum_{t=0}^{T} \theta_t}{G} \tag{11}$$

$$\lambda^{UB} = \frac{\sum_{t=0}^{T} \theta_t^{UB}}{G} \tag{12}$$

$$\lambda^{LB} = \frac{\sum_{t=0}^{T} \theta_t^{LB}}{G} \tag{13}$$

3.2 Methodological Considerations

There are some considerations when pursuing this methodology.

Firstly, if multiple stimulus measures were being implemented at the same time as the handouts, this methodology would also capture the reaction in spending from these interventions. Throughout the study period, the primary fiscal policy measures being implemented in Malaysia were the aforementioned direct cash transfers. Other measures, such as withdrawals of statutory pension savings, debt guarantees, and subsidised loans were less substantial. The other major policy, the blanket debt moratorium, was sustained throughout

most of 2020, whose effect would likely be absorbed in the pre-intervention data, and hence the projected counterfactual. Nevertheless, absorption may be incomplete if one posits the effect of the debt moratorium is highly uneven across time, e.g., savings from forgone repayments early in the moratorium period are spent only in the later period coinciding with the time period covered by the ITS setup.

Secondly, while controlling for mobility, hence the de facto effect of NPIs, as an exogenous variable intends to account for epidemic control policies, sharp, frequent, or major changes during the post-intervention period may affect the reliability of the estimated counterfactual. In all three episodes of cash transfers, the evaluation period of about one month was characterised with relatively epidemic control policies.

Thirdly, while location-specific analysis is desirable when studying the crisis period dynamics, it is not feasible in this study due to the lack of location-disaggregated total transactions. Although physical card spending can be disaggregated at the state level, the resulting analysis could be misleading, as consumers may have systematically switched between online (e.g., bank transfers, card at online stores) and offline spending methods (e.g., cash, and card at brick-and-mortar stores) when NPIs are tightened or loosened. Nevertheless, the same analysis is repeated for all 16 states of Malaysia using state-level physical card spending, and the corresponding mobility indicators in the appendix. In short, the state-level analysis using alternate data suggests that while most estimates are aligned directionally, and trend-wise, with the main analysis in all three episodes, there could be state-level heterogeneity. Whether this reflects genuine sub-national heterogeneous policy impact, simply inappropriate data due to differential online-offline switching in spending mode, or a mixture of both, is unclear.

Fourthly, it should remain clear that the ITS is subsidiary to the DiD when evaluating impact of interventions of shocks using a 'natural experiment' construct, particularly when observed control groups are absent. However, it remains a functional tool to shed light for policymakers post-implementation of policy interventions when conditions are far from experimental or ideal, akin to Lopez Bernal et al. (2019)'s case. Accompanying this crucial caveat, attention ought to be given in the study design to minimise design-driven biases. Baicker & Svoronos (2019) discussed the validity of the classic implementation of the single entity ITS. While Baicker & Svoronos (2019) focused on the segmented linear regression version of the ITS, rather than the time series model forecast-based version in Linden (2018), and indeed in this study. Nevertheless, where extendable to the time series model forecast-based implementation, Baicker & Svoronos (2019)'s survey of best practices is considered. For instance, all three episodes used pre- and post-intervention windows of well longer than 12 days / period, each day's observation is constructed from nationally-representative data, both seasonal adjustments and unit root tests were conducted, and that confounding from mobility

is controlled for as an exogenous variable. However, the recommendation to include nonlinear terms may be problematic for time series modelling, as stationarity in these alternative specifications may not be guaranteed within the context of AR(n)-X models.

4 Findings

4.1 Counterfactual Spending

Figure 7 shows the observed time series of daily total transactions throughout the pre- and post-disbursement periods, and the counterfactual constructed from the AR(n)-X model using the dynamic forecasts of transactions, and post-disbursement observations of mobility. The run-in period is determined, as described earlier, by convergence between counterfactual and observed spending in the post-disbursement period, while allowing for the possibility of temporary sign reversal. n is selected by the HQIC ($n = 1$ for all three episodes) [1] for the post-disbursement period. The pre-disbursement fitted values of transactions from the estimated AR(n)-X model are also shown to illustrate model fit, which closely resemble the observed spending levels in all three episodes, up until the post-disbursement period.

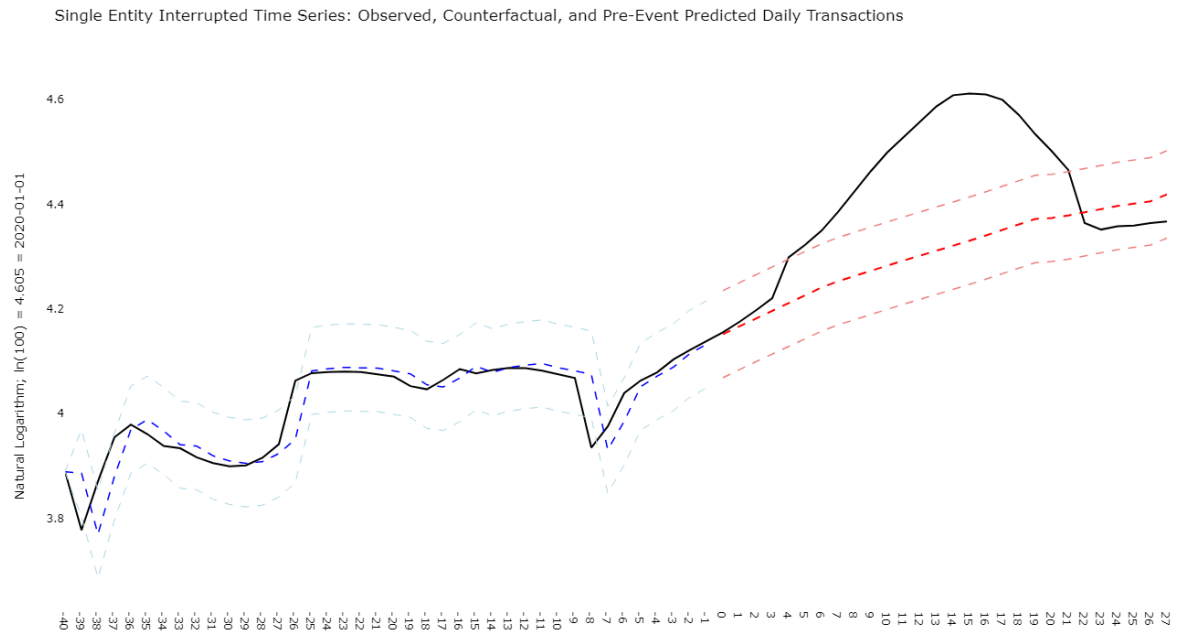
In the CMCO, and BSH 3 episodes, observed spending briefly overshoots the constructed counterfactual, but quickly converges back to the counterfactual within about 14 to 21 days. The BPR 3 episode differs. The counterfactual first overshoots, then falls below the observed spending for about 20 days, before the two series converged. These differences, specifically the prominence of the observed-counterfactual gap, and the presence of a sign reversal, likely reflects two matters. First, the quantum of the BSH 3, and BPR 3 handouts were vastly smaller than the CMCO handouts. Consumers may have simply been more responsive to the more generous package, reflecting a large enough positive income shock that moved them to a more favourable equilibrium. This is in of a similar line of thinking as the poverty trap literature, such as in Balboni et al. (2022). Second, broader economic conditions during the CMCO episode were better than during the BSH 3, and in turn the BPR 3 episodes. The CMCO handouts immediately followed a period of strict lockdowns, with full closures of most economic sectors, and prohibition of movement of people, hence the boost in spending from the handouts may have been compounded, considering the income, and wealth losses accumulated throughout the lockdown. The BSH 3 episode, however, was implemented when COVID-19 transmission was near-zero in Malaysia, and the economy was fully opened, hence a lack of both economy-constricting NPIs, and risk aversion towards COVID-19. The BPR 3 episode, followed a period of aggressive loosening of NPIs amid achieving near-

[1] The Akaike Information Criterion (AIC), and the Bayesian Information Criterion (BIC) both yield the same selected number of lags.

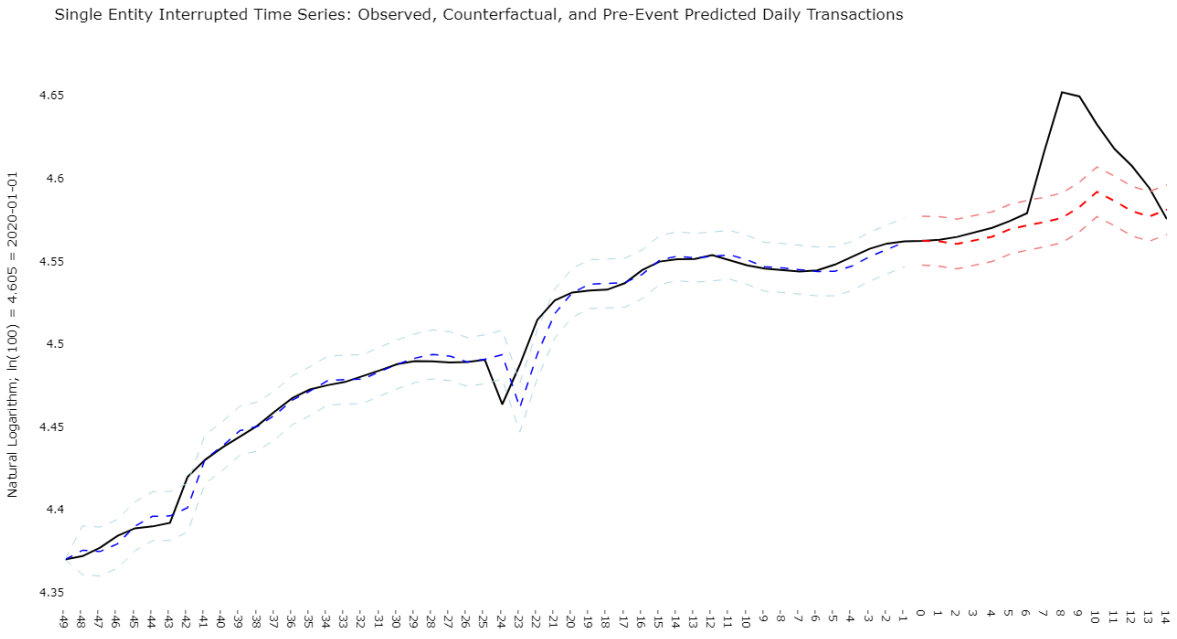
universal COVID-19 vaccine coverage. Hence, the dividends from additional income shocks may have been less prominent. In the BPR 3 case, consumers may have smoothed out the additional income shock by saving in the first weeks, which may explain the sign reversal at the beginning, and the generally flat observed spending levels in the post-disbursement period. This may also reflect delayed disbursement of transfers, a mechanism documented in Bazzi et al. (2015) to have been associated with reduced near-term expenditure, using survey data in the context of unconditional cash transfers in Indonesia albeit over a longer horizon and with individual data. However, these plausibilities cannot be ascertained within the context of this study, and require further research through other data and study designs.

Figure 7: Observed, Counterfactual, and Pre-Disbursement Predicted Daily Transactions
Dotted lines indicate the 95% confidence bands for the pre-disbursement fitted values, and the post-disbursement out-of-sample forecasted counterfactual

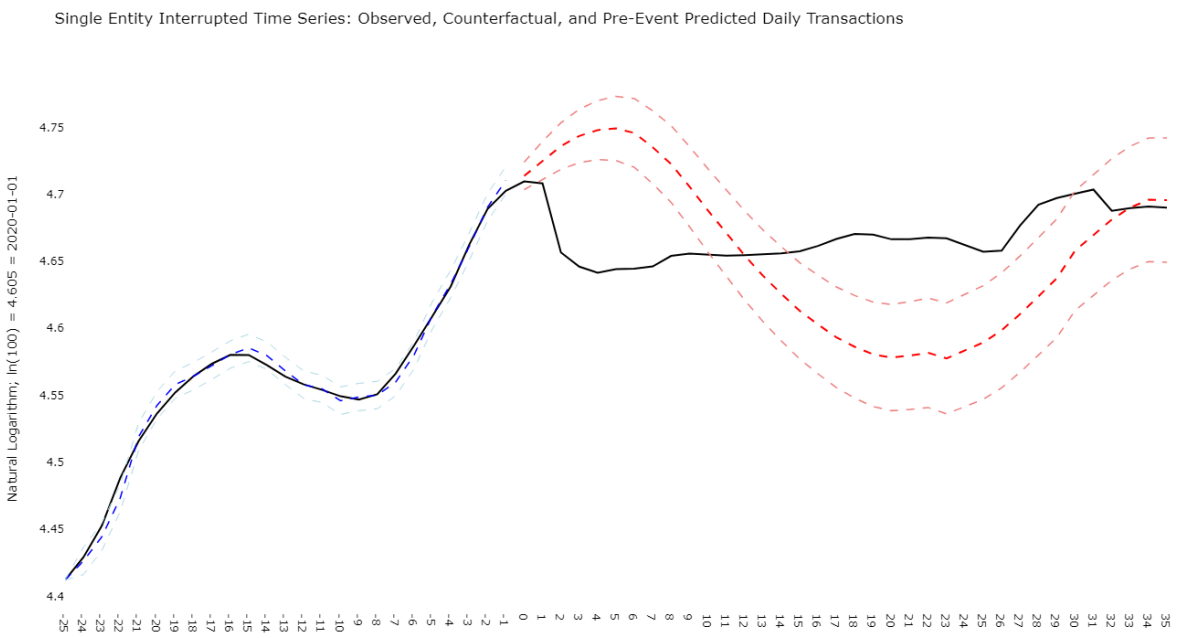
(a) CMCO (May 2020)



(b) BSH 3 (July 2020)



(c) BPR 3 (September 2021)

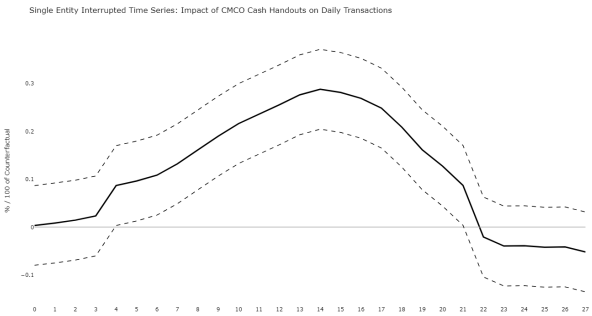


4.2 Impact Estimates

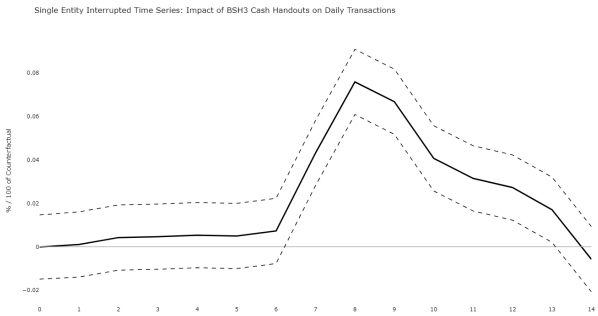
Computing the impact estimates is a mechanical exercise. Figure 8 takes the difference between observed and counterfactual transactions previously for impact estimates of the cash handouts on spending at a daily frequency, together with the 95% CIs. These are the same as the observed-counterfactual gaps discussed earlier.

Figure 8: Impact of Cash Handouts on Daily Transactions
Dotted lines indicate the 95% confidence bands

(a) CMCO (May 2020)



(b) BSH 3 (July 2020)



(c) BPR 3 (September 2021)

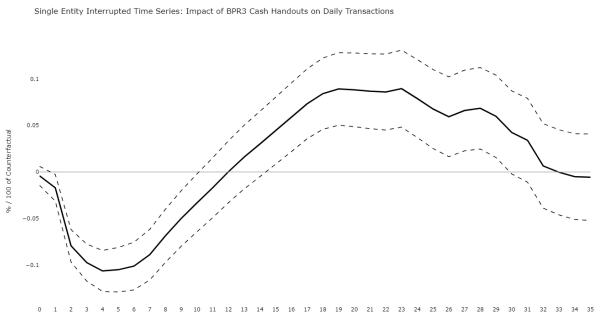
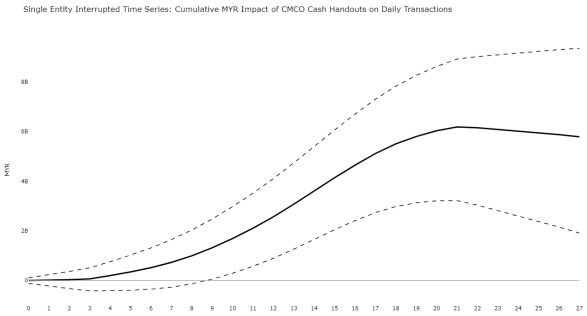


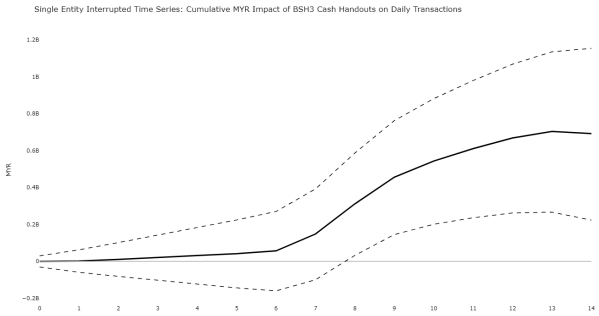
Figure 9 converts the daily impact estimates back into MYR from log-level indices (with the reference being the MYR value of transactions on 1 January 2020), and takes the cumulative sum. In the CMCO episode, the cumulative impact of cash transfers stabilised after from days 21 onwards at about MYR6 billion. Cumulative impact of the BSH 3 cash transfers was minimal up to day 6, which is 2 days after the actual date of the disbursement (day 0 refers to the announcement of the transfers to account for anticipatory effects) at well below MYR0.1 billion. This figure peaks quickly at MYR0.7 billion by day 13. In the BPR 3 episode, the cumulative impact dips below zero up until day 23, before peaking up to MYR0.9 billion by day 32. The daily impact of all three episodes were precisely estimated for a majority of the post-disbursement period, as is the cumulative impact of the CMCO, and BSH 3 episodes, the cumulative impact of the BPR 3 episode is not statistically significant. This, again, may stem from (i) the smaller quantum, (ii) more accommodative overall economic conditions during the BPR 3 episode, and (iii) delayed disbursement of transfers, as discussed earlier.

Figure 9: Cumulative Impact of Cash Handouts on Daily Transactions in MYR
Dotted lines indicate the 95% confidence bands

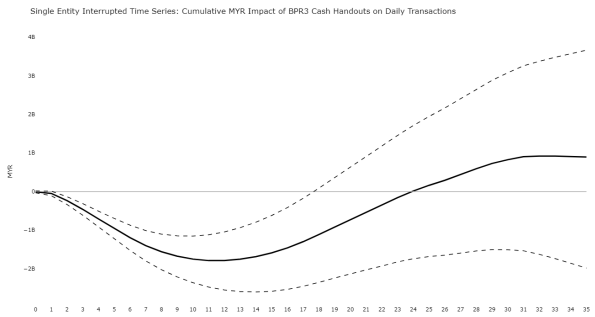
(a) CMCO (May 2020)



(b) BSH 3 (July 2020)



(c) BPR 3 (September 2021)



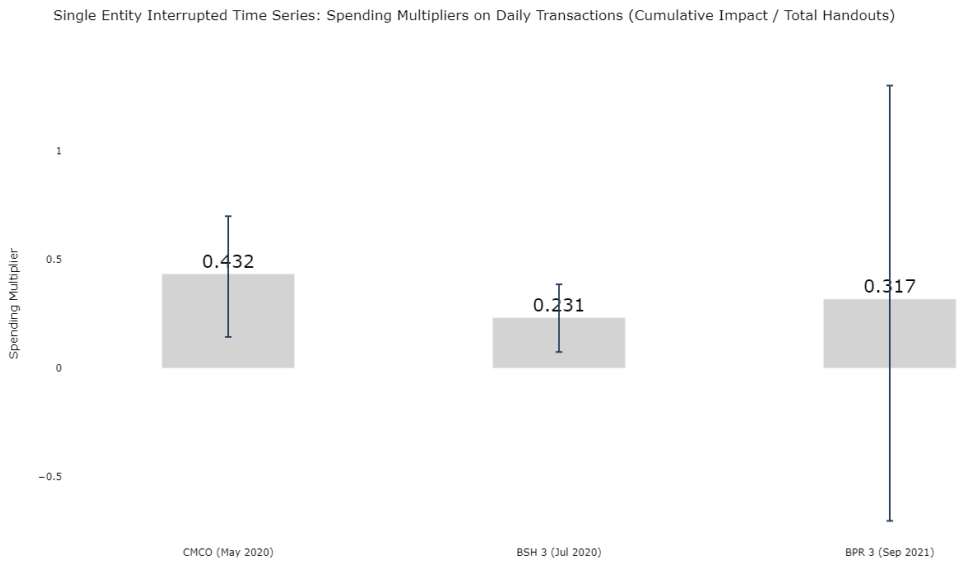
Taken together, the ITS methodology estimated a statistically significant positive impact on spending when viewed at a daily frequency. Suppose cash transfers were not implemented, spending likely would have trended upwards, as per the counterfactual, reflecting broad easing of NPIs, and improving economic conditions in each episode. Taken together, the cash transfers accounted for a substantial part of the observed increase in daily transactions, which went beyond what is implied by the relaxation of NPIs.

However, the cumulative benefits are only clear for the CMCO, and BSH 3 episodes. In cumulative MYR terms, these estimates suggest that (i) the CMCO cash handouts boosted spending by MYR5.789 billion (95% CI: MYR1.913 billion to MYR9.355 billion), (ii) the BSH 3 handouts MYR0.692 billion (95% CI: MYR0.223 billion to MYR1.154 billion), and (iii) the BPR 3 handouts MYR0.893 billion (95% CI: MYR-1.986 billion to MYR3.666 billion).

4.3 Spending Multiplier

Figure 10 shows the spending multiplier of the three episodes. This is computed by offsetting the cumulative impact estimates in figure 10 against the amount disbursed reported by the Ministry of Finance (MOF (2020*b*), and MOF (2020*a*)), and the Prime Minister's Office (PMO (2021)). The estimated spending multiplier of (i) the CMCO cash handouts is 0.432 (95% CI: 0.143 to 0.698), (ii) the BSH 3 handouts at 0.231 (95% CI: 0.074 to 0.385), and (iii) the BPR 3 handouts 0.317 (95% CI: -0.704 to 1.300).

Figure 10



These spending multiplier estimates are similar to the pre-pandemic estimates, such as Murugasu et al. (2013), which used a cross-section of individual-level data from a household survey, and Murugasu et al. (2015), which used an autocorrection model (ECM) with macroeconomic time series. Suppose the methodological corrections from past studies from Malaysia (Murugasu et al. (2013), and Murugasu et al. (2015)) to address endogeneity concerns are sufficient (instrumenting income with education level of the head of household, and an ECM specification), they are likely a pooled measure of various types of income shocks, e.g., indirect subsidies, conditional transfers, income effects from commodity price movements.

However, the multiplier estimates for the CMCO episode is noticeably higher than the later BSH 3 and BPR 3 episodes. This reflects three things. Firstly, this CMCO handouts were stacked with voluntary and capped withdrawals of statutory retirement savings. Secondly, the CMCO handouts followed immediately the worst of the economic crisis of the COVID-19 pandemic. Some individuals may have been closer to subsistence income than pre-crisis, hence triggering a more elaborate spending response to income assistance. Thirdly, the quantum in the CMCO episode, even excluding the i-Lestari withdrawals, is substantially larger than the BSH 3 and BPR 3 handouts, which have been more effective at moving households to a more favourable consumption equlibrium. Notably, the methodology does not permit disentangling the impact of the statutory retirement savings withdrawals (MYR1.9 billion) from the cash transfers (MYR11.5 billion).

The spending multiplier for the BSH 3 and BPR 3 episodes were close to, but lower than,

Murugasu et al. (2013), and Murugasu et al. (2015). This may be due to the difference in quantum, such that the amount disbursed were insufficient to move households to more favourable consumption equilibria. As the broader economic conditions were more accommodative during these episodes than in the CMCO episode, marginal gains of additional income shocks may have been minimal. It is also possible for both multipliers to be underestimated for two reasons — (i) if reaction of credit card spending, and ATM withdrawals, are stronger amongst the excluded banks than amongst the sample banks, and (ii) the lack of disaggregated data to uncover the multiplier of target income groups away from the upper end of the income distribution. Moreover, the central bank's loan moratorium, described in IMF (2022) was scaled down progressively towards the BSH 3, and BPR 3 episodes, hence additional income offered to individuals have have been diverted to loan repayments, including principal payments deferred during the initial moratorium, in lieu of spending on goods and services.

Addressing the relative performance of income support interventions of different degrees of directness during a crisis is beyond the scope of this study. Replication studies in other countries that relied more heavily on indirect income support measures may find this feasible.

5 Conclusion

By leveraging on daily national-level data on transactions, and mobility, this paper analysed the impact of cash transfers in May 2020 (CMCO), July 2020 (BSH 3), and September 2021 (BPR 3) on spending. Methodological adjustments, namely using the ITS approach via an AR(n)-X, were implemented to overcome (1) the absence of an observed control group as in traditional DiD and event study setups, e.g., Goodman-Bacon (2021), and Sun & Abraham (2021). The choice of using ITS here squares with applications in the neighbouring field of observational studies in epidemiology, where DiD cannot be implemented due to either the lack of disaggregation, or an outright absence of a control group, but where time series data is available.

The main strength of this study lies in the data sets used, especially the transactions data. The rich disaggregation by type, even at the national-level, allows a detailed descriptive analysis of heterogeneous trends in transaction subtypes. For instance, the data illustrated a systematic difference in the persistence of surge in spending between online spending, e.g., FPX transfers, JomPay bill payments, versus physical spending, e.g., brick-and-mortar store card transactions, ATM withdrawals, total cash spending. Moreover, the use of daily mobility data further allowed a creative isolation of the observed effect of the relaxation of mobility restrictions, separately from the spending trends adjusted in a time series linear regression model.

However, there are some limitations, firstly in the lack of highly disaggregated data, such as transaction-level or postcode-level data available in the US, e.g., Aladangady et al. (2019), Chetty et al. (2020), and Casado et al. (2020). Further data work in Malaysia to build these data sets, and, importantly, ensure that the anonymised, or sufficiently aggregated versions, are openly accessible is necessary to fuel research in EMEs. While there is growing research in the US deploying granular data using both surveys and administrative sources, reproducibility of interventions in EMEs is crucial to maximise policy gains globally. In this study, for instance, location-specific analysis using total transactions is not feasible due to the lack of location-disaggregated data, even when mobility data is available at the state-level. Alternative analysis using state-level physical card transactions suggests potential heterogeneity in sub-national policy impact, but could be convoluted by potential systematic switching between online and offline spending methods correlated with the tightness of NPIs. Secondly, the use of time series methods, specifically the ITS is the best compromise, without losing scope of interpretation of results. While the ideal remains a DiD, or a controlled ITS, described in Lopez Bernal et al. (2018), this study's 'counterfactual' estimate uses observed mobility directly, as well as a lagged dependent variable to reflect continuity in pre-intervention trends, to single out as best the effect of the only other major policy intervention in the study period — the cash transfers. Moreover, Lopez Bernal et al. (2019) notes that DiD and ITS methodologies are complementary, and the latter bears merit in settings such as this, where the family of DiD methods, including synthetic controls, are inapplicable. Thirdly, without access to the actual line items of the Government's expenditure, this paper could not identify the actual dynamics of disbursement, and could only identify announcement dates, and static quanta from government reports (MOF (2020*b*), MOF (2020*a*), and PMO (2021)). Fourthly, this ITS implementation does not allow for disentangling possible long-run effects of cash handouts lingering into subsequent episodes of cash transfers. While in principle, one may simply extend the 'counterfactual' estimated from the pre-CMCO handouts period (the first episode in this study) far beyond the post-CMCO handouts period, the resultant observed-'counterfactual' difference may inadvertently be convoluted by other shocks, policy measures in the interim. Hence, short pre-intervention periods are chosen across all three analyses. Nevertheless, this concern warrants further studies focused on long-run effects of cash transfers, along with interactions with sequentiality or other policy measures.

The ITS methodology with an AR(n)-X (with n selected using a Hannan-Quinn Information Criterion) found precisely estimated daily impact on daily spending from all three episodes of cash transfers. However, the cumulative impact for the BPR 3 (September 2021) episode is not precisely estimated. The adjusted estimates affirm the following interpretation of the descriptive analysis that the cash transfers triggered a short-lived, but of varying degree of prominence, increase in spending across various classes. Some classes, primarily online-type transactions had signs of persistence post-disbursement. Importantly, this is potentially

separate from the effects of relaxation of NPIs. Offsetting the estimated impact against the Government's reported disbursement quantum (MOF (2020*b*), MOF (2020*a*), and PMO (2021)) suggests that the spending multiplier, including its confidence band, to be similar to pre-pandemic estimates for the CMCO cash transfers and statutory retirement savings withdrawals at 0.432 (95% CI: 0.143 to 0.698). However, reflecting (i) lower quantum, (ii) more accommodative broader economic conditions, and (iii) the progressive scaling back of a loan moratorium, hence an increase in debt service burdens, the spending multipliers of the BSH 3, and BPR 3 cash transfers were close to, but lower than, pre-pandemic estimates at 0.231 (95% CI: 0.074 to 0.385), and 0.317 (95% CI: -0.704 to 1.300), respectively. Hence, this study opens the possibility that direct cash transfers may be have a higher multiplier when compounded with other income support tools, e.g., temporary debt relief, and that the spending multiplier may be innately higher during times of crisis. Finally, while the blanket debt moratorium was in place for a considerably long time, it is unclear if its impact is fully absorbed in both the pre-event and post-event periods in this study, especially if impact from the moratorium is highly uneven across time, e.g., when repayment savings are substantially higher in early days of the moratorium, or if repayment savings are spent only towards the end of the moratorium period.

For policymakers, this strengthens pre-pandemic priors that direct, unconditional, cash transfers are high-powered interventions to boost spending. In the wake of crises, in particular, cash transfers may be placed on the top of the policy toolkit, especially if they fit the criteria outlined in Suah (2020) — direct, adequate, and timely. In subsequent research, there is scope to dive into the interaction between timing, quantum, and how targeted transfers are and its impact on spending. Specifically, questions remain as to if the lifting of the NPIs compounded the impact from cash transfers, suppose the methodological adjustments made were inadequate, and if the impact may have been different had the transfers were implemented later. For instance, one may consider the plausible dynamic effects of some households defaulting, or descending into subsistence with delayed transfers. Overall, there is ample scope for continued policy research using high-frequency and granular data in EME settings.

Data and replication statement

Data sharing will be subject to the Information Security Management Policy (ISMP), due to the confidential nature of the data used in the study. However, all codes used in this study, and synthetic data are available at github.com/suahjl/impact-single-its.

Acknowledgements

This paper has benefited from the following individuals for their critical review and feedback, in particular colleagues from the Institute for Clinical Research, National Institutes of Health, Ministry of Health, Malaysia for feedback related to the methodology.

- Boon Hwa Tng, Central Bank of Malaysia, Malaysia
- Masliyana Husin, Institute for Clinical Research, National Institutes of Health, Ministry of Health, Malaysia
- Xin Rou Teh, Institute for Clinical Research, National Institutes of Health, Ministry of Health, Malaysia
- Sheamini Sivasampu, Institute for Clinical Research, National Institutes of Health, Ministry of Health, Malaysia
- Sui-Jade Ho, Central Bank of Malaysia, Malaysia
- Eilyn Yee Lin Chong, Central Bank of Malaysia, 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'.

Baicker, K. & Svoronos, T. (2019), Testing the validity of the single interrupted time series design, Technical report, National Bureau of Economic Research.

Balboni, C., Bandiera, O., Burgess, R., Ghatak, M. & Heil, A. (2022), 'Why do people stay poor?', *The Quarterly Journal of Economics* **137**(2), 785–844.

Bazzi, S., Sumarto, S. & Suryahadi, A. (2015), 'It's all in the timing: Cash transfers and consumption smoothing in a developing country', *Journal of Economic Behavior & Organization* **119**, 267–288.

Bernal, J. L., Cummins, S. & Gasparrini, A. (2017), 'Interrupted time series regression for the evaluation of public health interventions: a tutorial', *International journal of epidemiology* **46**(1), 348–355.

Biglan, A., Ary, D. & Wagenaar, A. C. (2000), 'The value of interrupted time-series experiments for community intervention research', *Prevention Science* **1**(1), 31–49.

Casado, M. G., Glennon, B., Lane, J., McQuown, D., Rich, D. & Weinberg, B. A. (2020), 'The aggregate effects of fiscal stimulus: Evidence from the covid-19 unemployment supplement'.

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'.

Goodman-Bacon, A. (2021), 'Difference-in-differences with variation in treatment timing', *Journal of Econometrics* **225**(2), 254–277.

Google (2022), 'Google COVID-19 community mobility reports'.
URL: *https://www.google.com/covid19/mobility/*

Gürkaynak, R. S., Sack, B. P. & Swanson, E. T. (2004), 'Do actions speak louder than words? the response of asset prices to monetary policy actions and statements', *The Response of Asset Prices to Monetary Policy Actions and Statements (November 2004)* .

Hale, T., Angrist, N., Kira, B., Petherick, A., Phillips, T. & Webster, S. (2020), 'Variation in government responses to covid-19'.

Hannan, E. J. & Quinn, B. G. (1979), 'The determination of the order of an autoregression', *Journal of the Royal Statistical Society: Series B (Methodological)* **41**(2), 190–195.

IMF (2022), 'Policy responses to COVID-19'.
URL: *https://www.imf.org/en/Topics/imf-and-covid19/Policy-Responses-to-COVID-19*

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.

Linden, A. (2018), 'Using forecast modelling to evaluate treatment effects in single-group interrupted time series analysis', *Journal of evaluation in clinical practice* **24**(4), 695–700.

Lopez Bernal, J. A., Gasparrini, A., Artundo, C. M. & McKee, M. (2013), 'The effect of the late 2000s financial crisis on suicides in spain: an interrupted time-series analysis', *The European Journal of Public Health* **23**(5), 732–736.

Lopez Bernal, J., Cummins, S. & Gasparrini, A. (2018), 'The use of controls in interrupted time series studies of public health interventions', *International journal of epidemiology* **47**(6), 2082–2093.

Lopez Bernal, J., Cummins, S. & Gasparrini, A. (2019), 'Difference in difference, controlled interrupted time series and synthetic controls', *International journal of epidemiology* **48**(6), 2062–2063.

MOF (2020*a*), 'Media statement by the ministry of finance, 20 july 2020'.
URL: *https://www.mof.gov.my/portal/ms/berita/siaran-media/pembayaran-fasa-ketiga-bantuan-sara-hidup-bsh-2020-dan-tarikh-permohonan-rayuan*

MOF (2020*b*), 'Perkongsian laporan ke-7 pencapaian PRIHATIN — LAKSANA'.

Murugasu, D., Huei, C. & Hwa, T. (2015), 'Implications of evolving household balance sheets for private consumption in malaysia', *Bank Negara Malaysia Working Paper Series WP7/2015* .

Murugasu, D., Wei, A. J. & Hwa, T. B. (2013), 'The marginal propensity to consume across household income groups', *Bank Negara Malaysia Working Paper Series WP2/2013* .

PMO (2021), 'Press statement by the prime minister of malaysia, 26 september 2021'.
URL: *https://www.pmo.gov.my/wp-content/uploads/2021/09/KENYATAAN-MEDIA-YAB-PERDANA-MENTERI-PEMBAYARAN-BPR-FASA-3-DAN-BPR-RAYUAN.pdf*

Schaffer, A. L., Dobbins, T. A. & Pearson, S.-A. (2021), 'Interrupted time series analysis using autoregressive integrated moving average (arima) models: a guide for evaluating large-scale health interventions', *BMC medical research methodology* **21**(1), 1–12.

Suah, J. L. (2020), 'Crises and policy: Covid-19 truly is different', *Quarterly Bulletin 1Q 2020* .

Sun, L. & Abraham, S. (2021), 'Estimating dynamic treatment effects in event studies with heterogeneous treatment effects', *Journal of Econometrics* **225**(2), 175–199.

A Time Series Plots of Non-Seasonally Adjusted Data

A.0.1 CMCO (May 2020) Handouts

Figure A.1

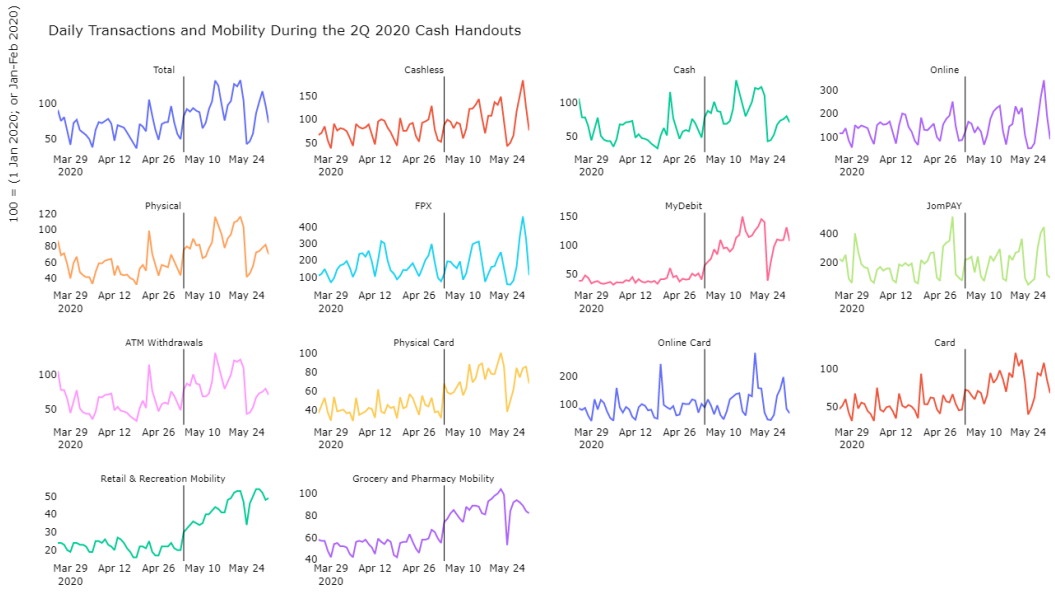
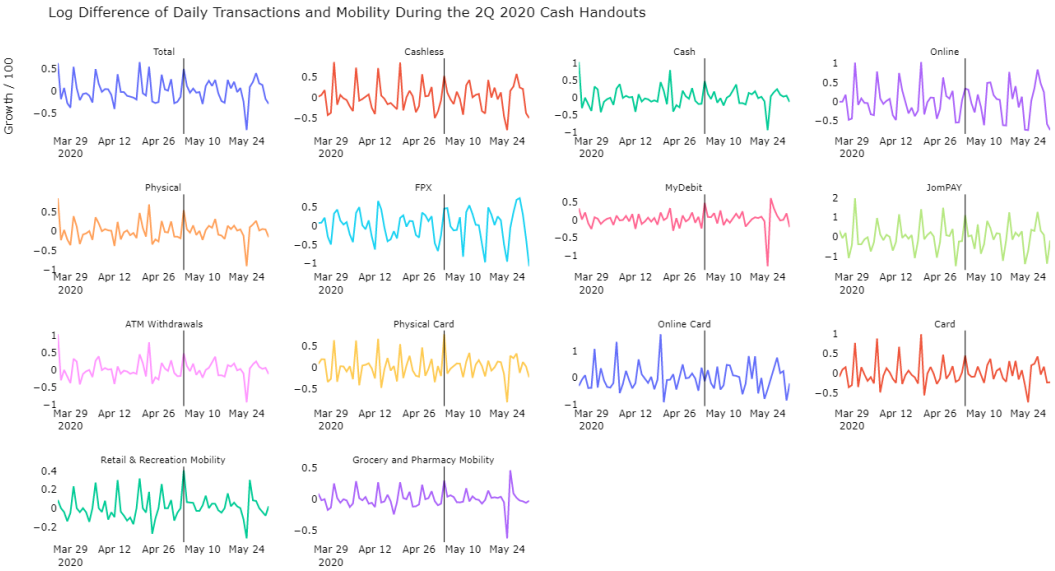


Figure A.2



A.0.2 BSH 3 (July 2020) Handouts

Figure A.3

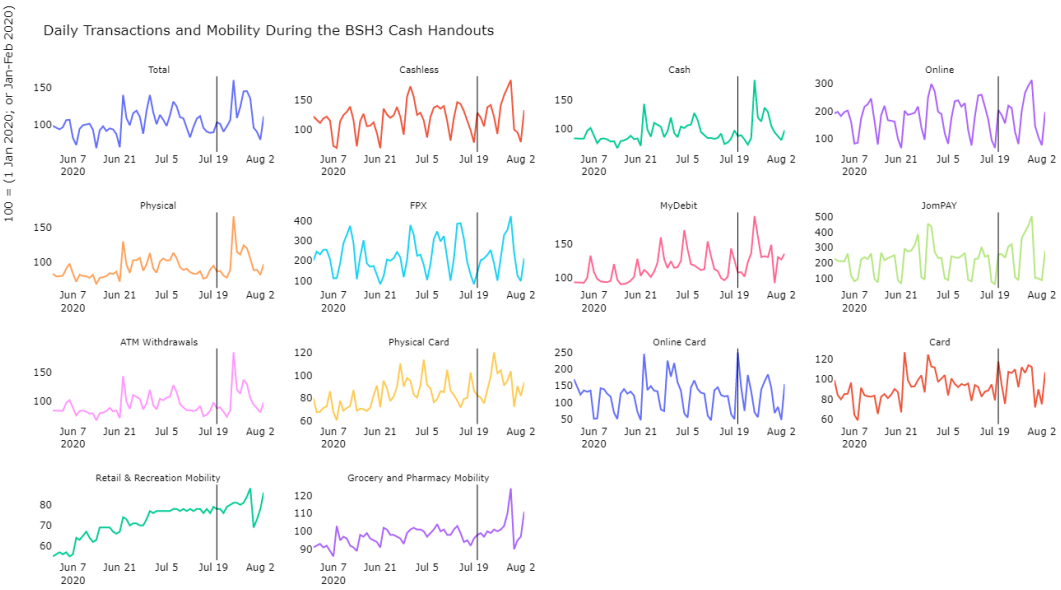
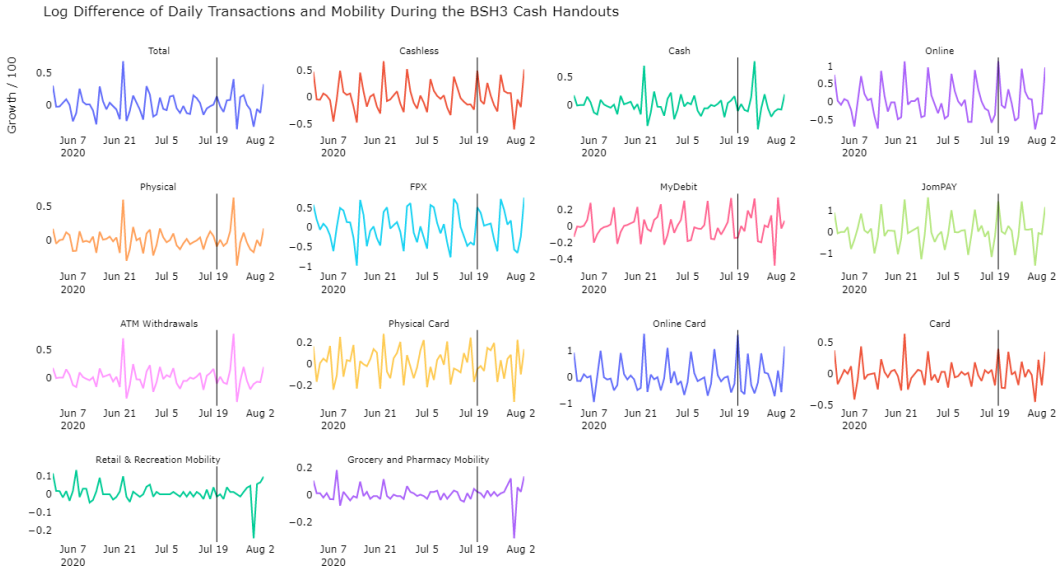


Figure A.4



A.0.3 BPR 3 (September 2021) Handouts

Figure A.5

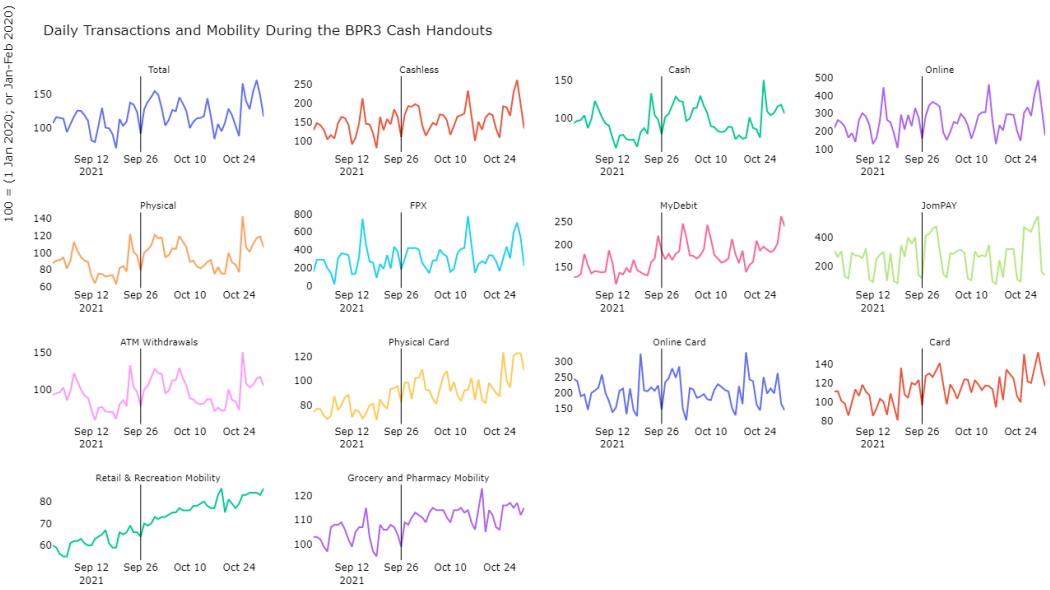
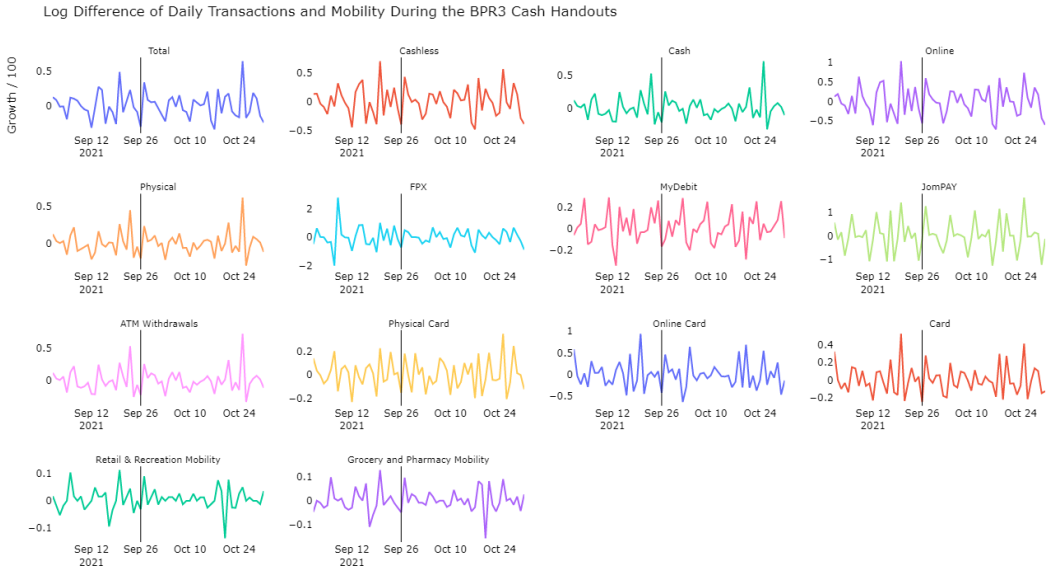


Figure A.6

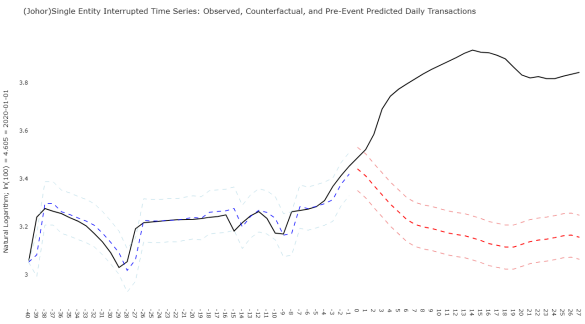


B Additional Analysis With State-Level Physical Card Transactions and Mobility

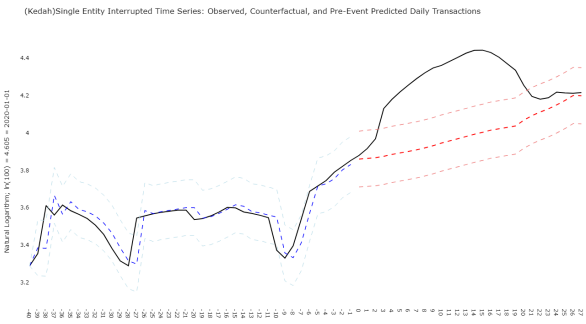
B.1 CMCO (May 2020) Handouts

Figure B.1: Impact of Cash Handouts on State-Level Daily Physical Card Transactions
Dotted lines indicate the 95% confidence bands

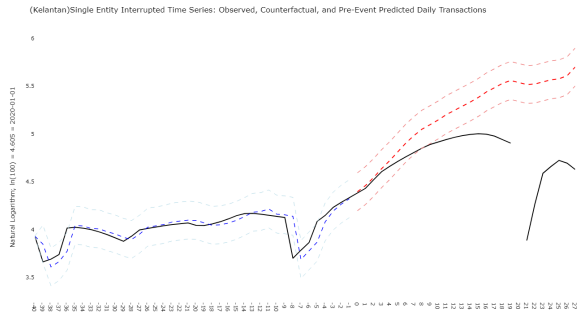
(a) Johor



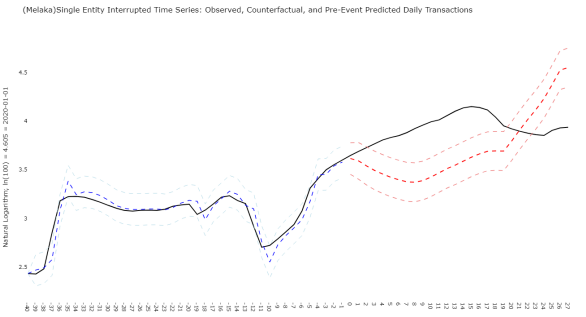
(b) Kedah



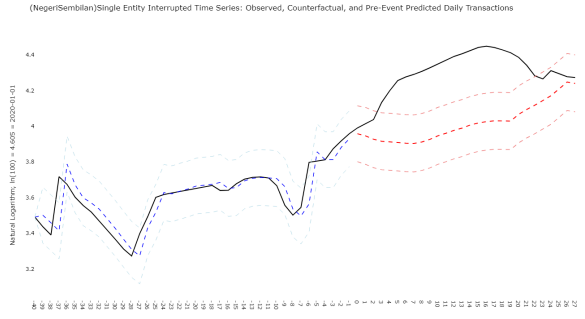
(c) Kelantan



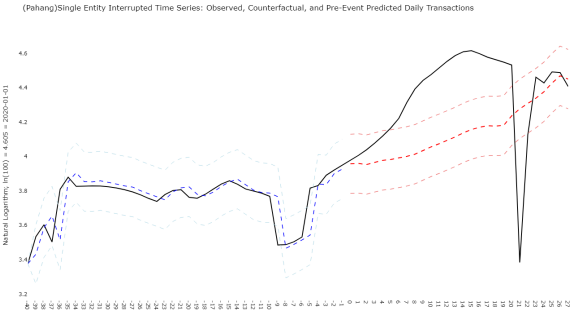
(d) Melaka



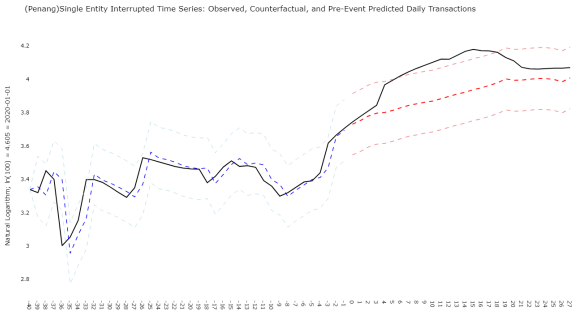
(e) Negeri Sembilan



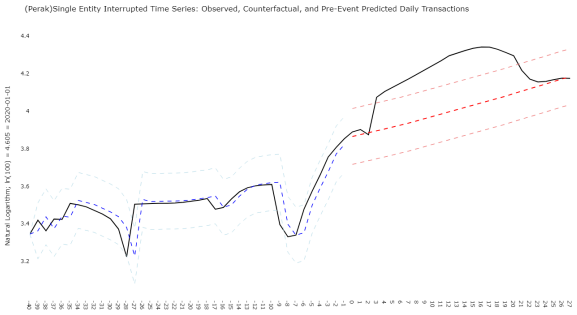
(f) Pahang



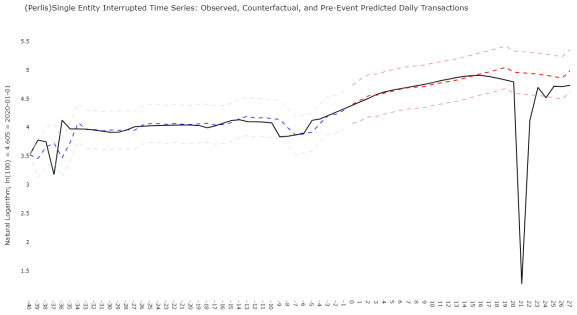
(g) Penang



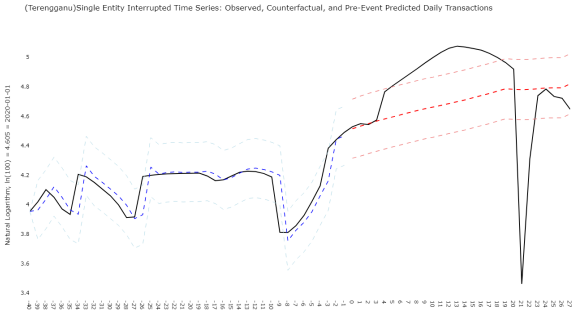
(h) Perak



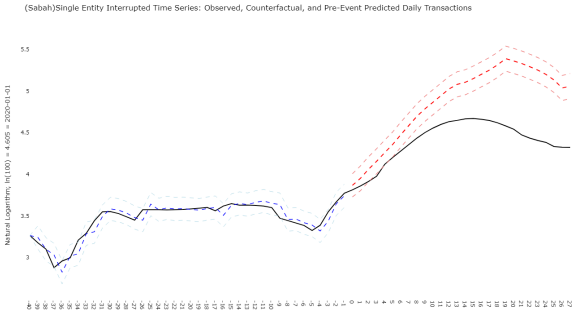
(i) Perlis



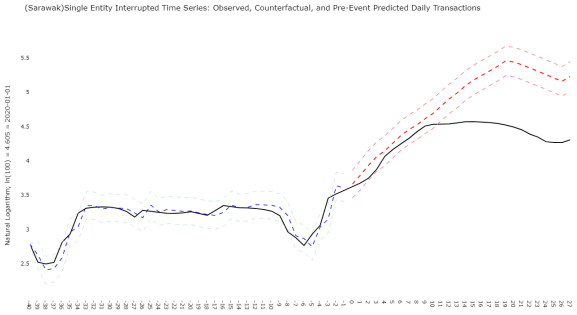
(j) Terengganu



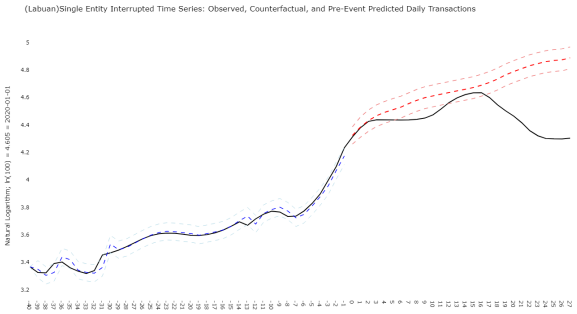
(k) Sabah



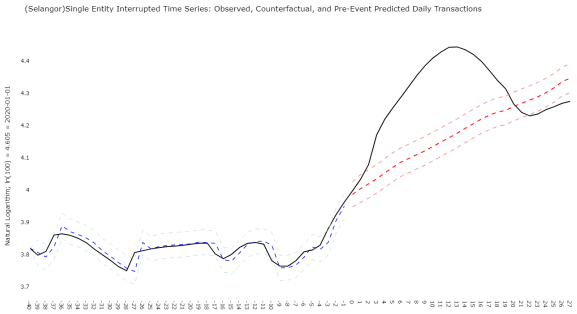
(l) Sarawak



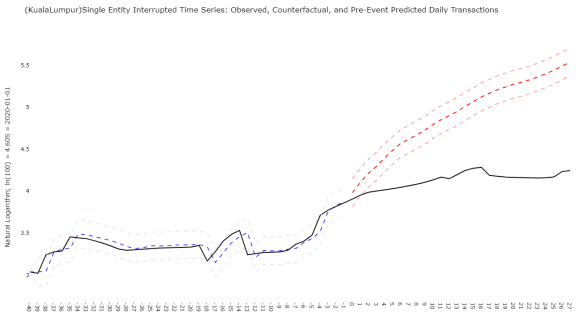
(m) Labuan



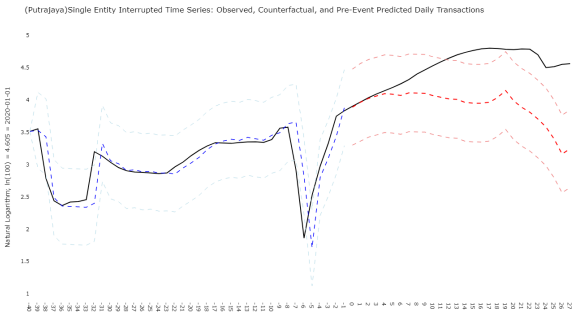
(n) Selangor



(o) Kuala Lumpur



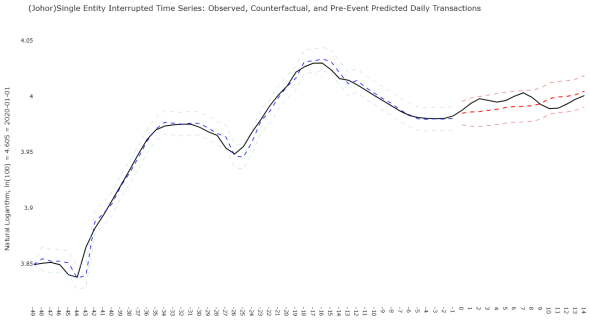
(p) Putrajaya



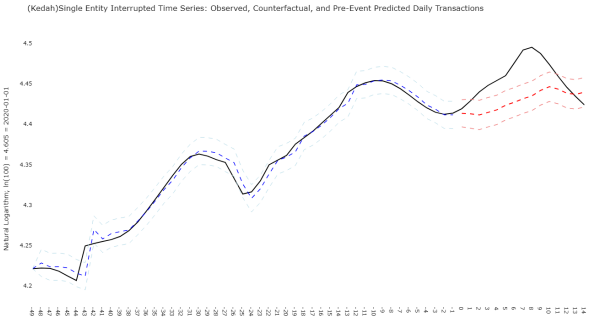
B.2 BSH 3 (July 2020) Handouts

Figure B.2: Impact of Cash Handouts on State-Level Daily Physical Card Transactions
Dotted lines indicate the 95% confidence bands

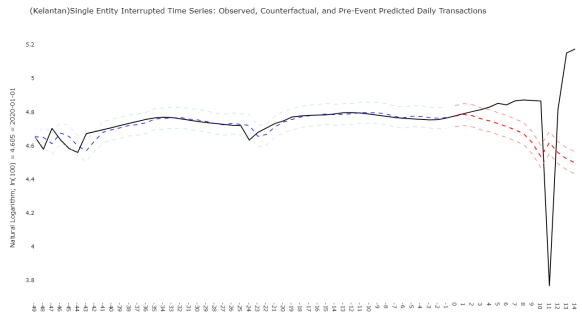
(a) Johor



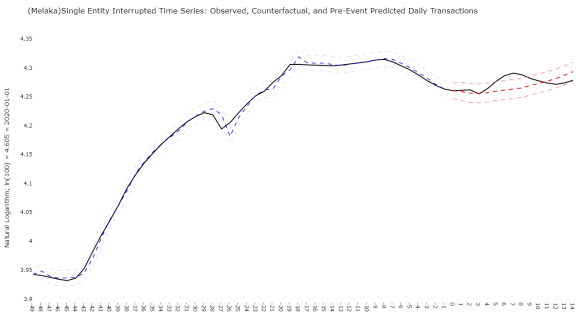
(b) Kedah



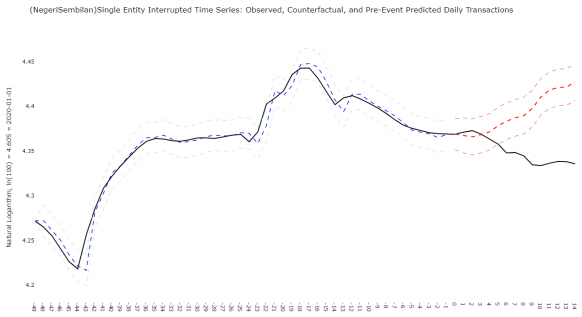
(c) Kelantan



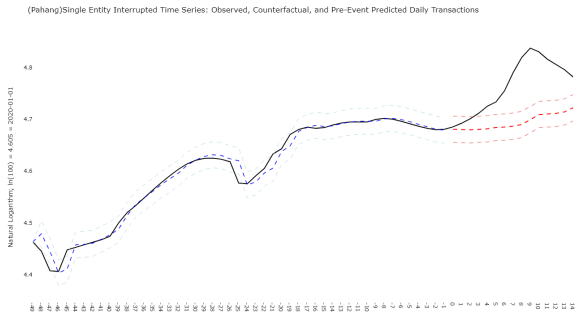
(d) Melaka



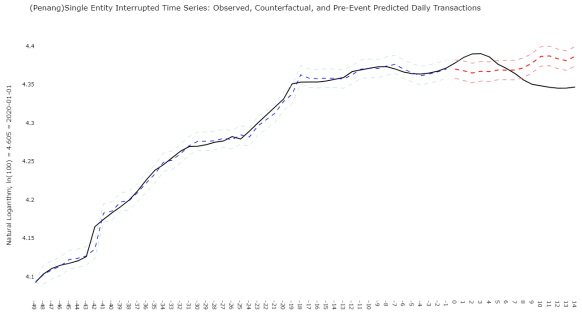
(e) Negeri Sembilan



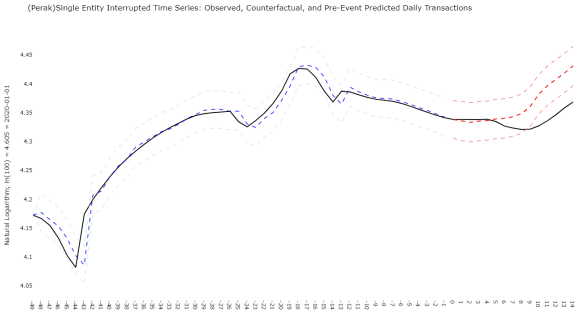
(f) Pahang



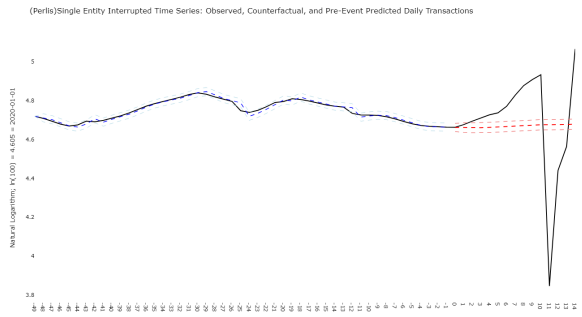
(g) Penang



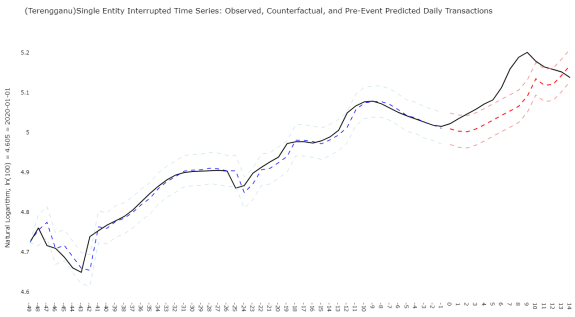
(h) Perak



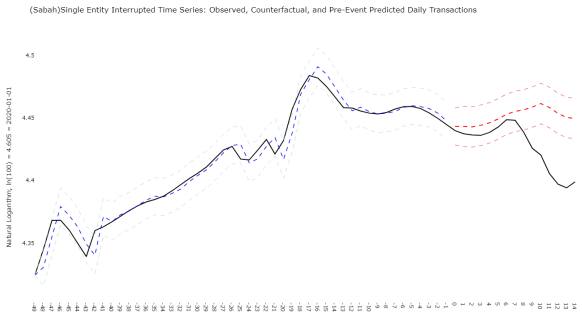
(i) Perlis



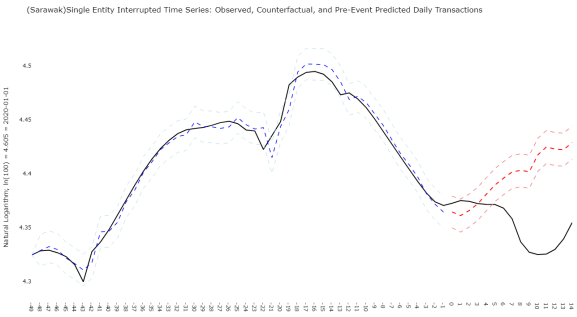
(j) Terengganu



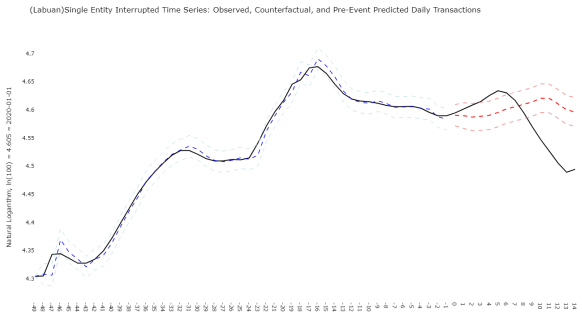
(k) Sabah



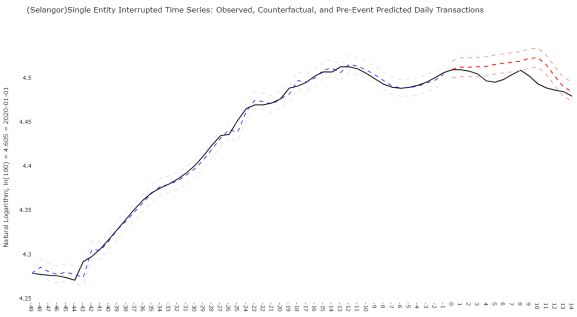
(l) Sarawak



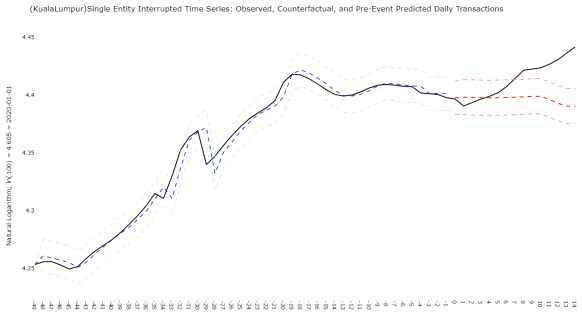
(m) Labuan



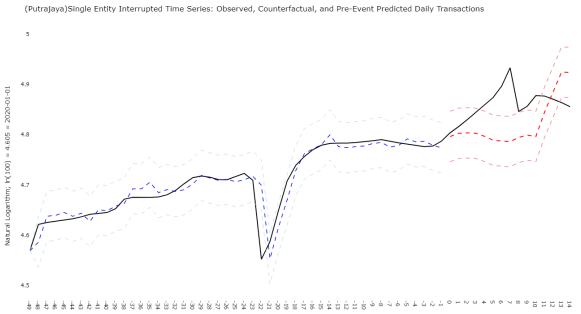
(n) Selangor



(o) Kuala Lumpur



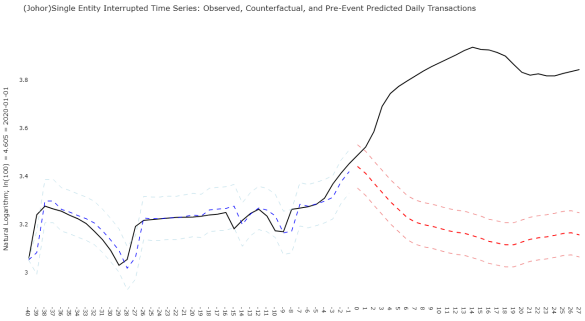
(p) Putrajaya



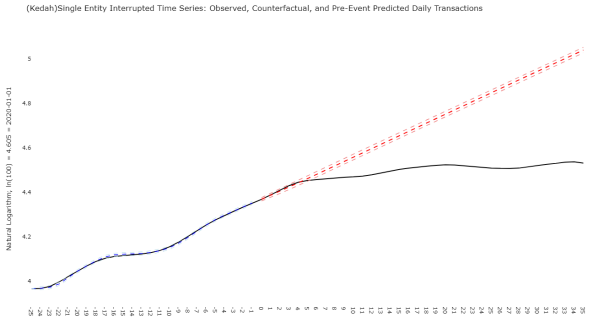
B.3 BPR 3 (September 2021) Handouts

Figure B.3: Impact of Cash Handouts on State-Level Daily Physical Card Transactions
Dotted lines indicate the 95% confidence bands

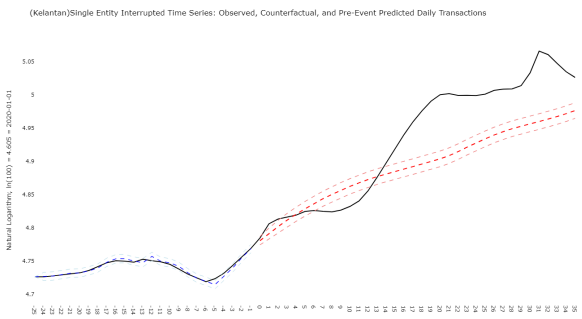
(a) Johor



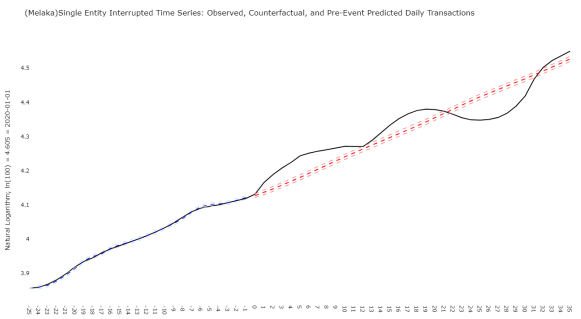
(b) Kedah



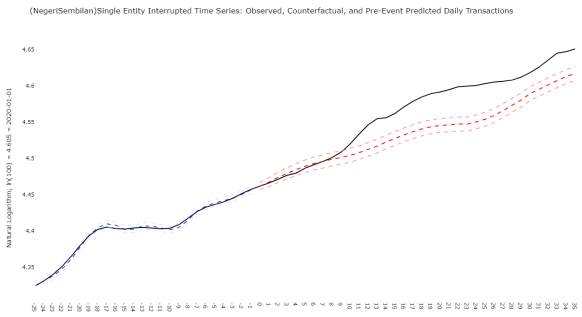
(c) Kelantan



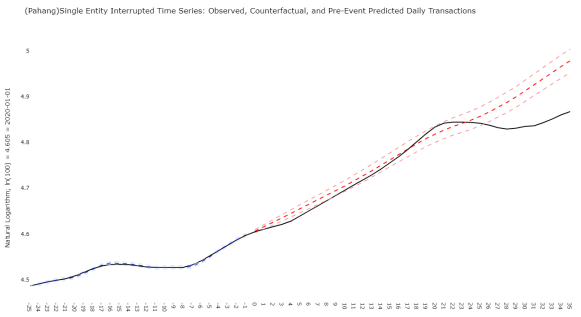
(d) Melaka



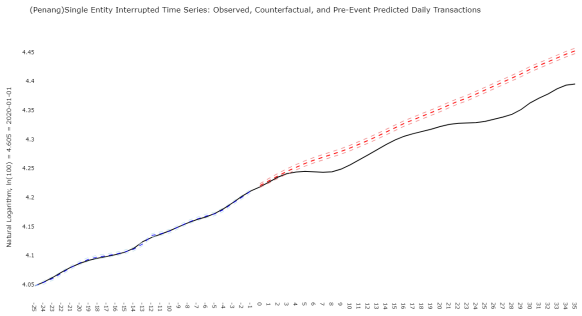
(e) Negeri Sembilan



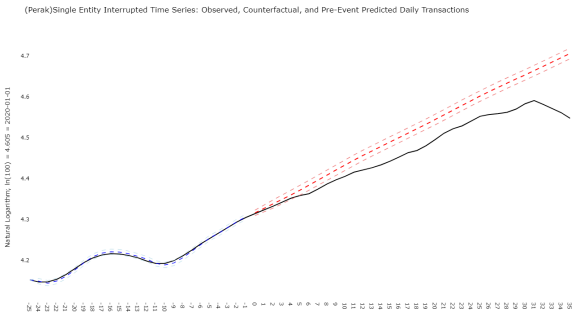
(f) Pahang



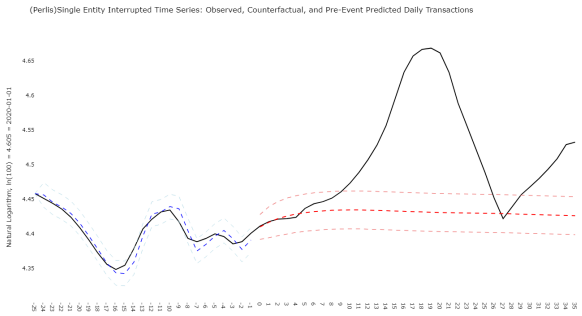
(g) Penang



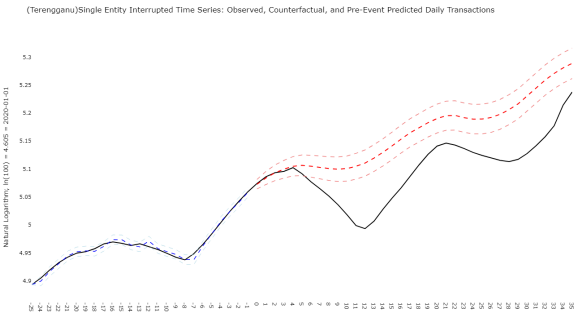
(h) Perak



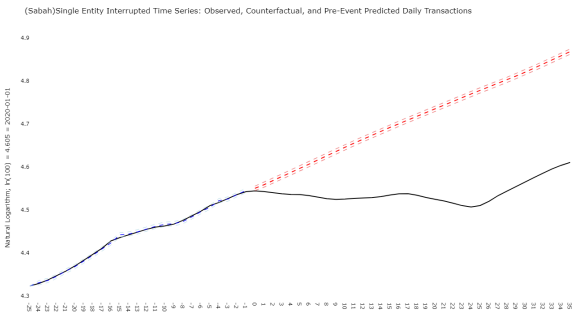
(i) Perlis



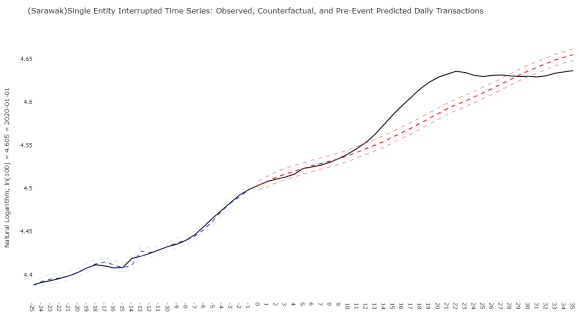
(j) Terengganu



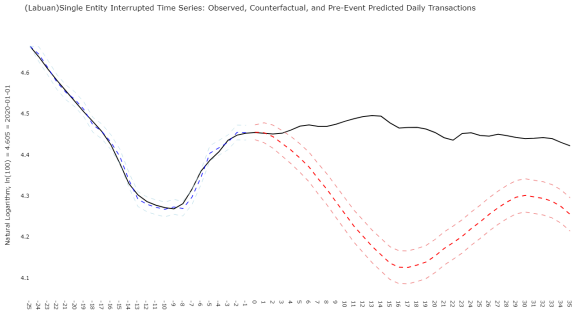
(k) Sabah



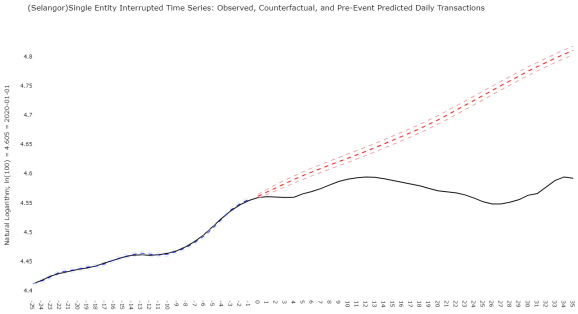
(l) Sarawak



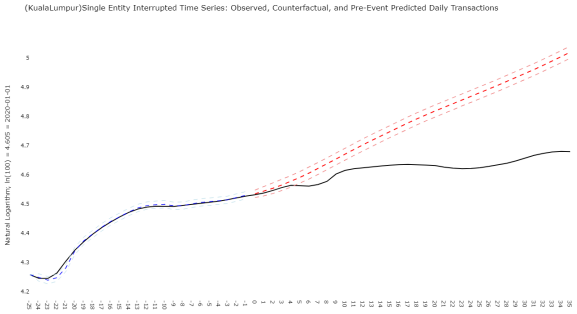
(m) Labuan



(n) Selangor



(o) Kuala Lumpur



(p) Putrajaya

