

## Taxing the rich to finance redistribution – evidence from a permanent tax increase in Singapore

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### Abstract

Based on a representative sample of consumer financial transaction data, this paper studies the consumption and savings response to a 2015 permanent increase in the marginal income tax of the high-income taxpayers. Using difference-in-differences regressions, controlling for individual and time fixed effects, we show robust results that the affected consumers experienced little change in their spending. The pattern is prevalent across consumer demographics and is found even among consumers who are sophisticated, have high levels of debt, or register little income changes. Furthermore, the tax increase financed fiscal redistribution leads to a long-lasting increase in the consumption of the lower-income population.

*Keywords:* income tax; consumption; savings; redistributive policy; fiscal policy

JEL codes: H24, E21, H31, H53

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## 1. Introduction

As income disparity deepens social tension, effective redistribution from high- to low-income groups may be socially desirable and urgently needed. Often, the macro environment, like in 2022, constrains the deployment of expansionary monetary policy or government borrowing to finance redistributive spending. The governments may have to resort to direct fiscal transfer. To illustrate, the U.S. President signed a 1.9 trillion dollar coronavirus relief package on March 11<sup>th</sup>, 2021, which includes a \$1,400 direct payment to most households whose annual income is less than \$75,000 (Financial Times, Mar 11<sup>th</sup>, 2021 ). In this context, income tax is a promising policy tool to finance fiscal programs. Indeed, President Biden allegedly planned to raise taxes on wealthy Americans to finance new spending and tax credits to fight poverty, childcare, education, and paid leave programs in April 2021.<sup>2</sup> In 2022, in Capital Hill, politicians were juggling for political positions in pitching their stances on whether to raise taxes to finance various large government spending programs.<sup>3</sup>

A vast literature documents that the more affluent population has a lower marginal propensity to consume (MPC) (For example, Johnson, Parker, and Souleles 2006; Jappelli and Pistaferri 2010, 2014; Parker et al. 2013; Kaplan et al. 2014; Christelis et al. 2019; Kopczuk 2019; Holm et al., 2021). Thus, marginally raising taxes on the rich to finance redistribution towards the poor may have both the intended impact on different income classes' consumption patterns and a positive impact on aggregate demand.

This paper examines the multi-layered effects of income tax increases by utilizing a quasi-natural experimental setting in Singapore. Announced on February 23<sup>rd</sup>, 2015, the Singapore government raised personal income tax permanently on individuals making more than S\$160K per annum by 1 to 2% in a

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<sup>2</sup> See “*Biden prepares to announce string of tax rises for richest Americans,*” Financial Times, and “*Biden Will Seek Tax Increase on Rich to Fund Child Care and Education,*” *New York Times*, both on April 22 2021.

<sup>3</sup> See, e.g., Washington Post, “*The Senate bill and Biden’s pledge to not raise taxes on people making less than \$400,000,*” Aug 9, 2022.

progressive manner while leaving others' tax rates unchanged. The tax increase was to finance redistribution programs targeting Singaporeans in low-income brackets.

We use micro-level data to evaluate the income tax increase's impact on the affected taxpayers' consumption and savings and on those receiving redistributed benefits. The tax increase announced in February 2015 (2015M2) would only apply to income made in and after 2016. In Singapore, individuals are required to file taxes starting from March 1<sup>st</sup> every year, which introduced the additional tax levy in 2017M3. Thus, we set 2015M2 to 2017M2 as the announcement period, the earlier time as the pre-announcement period, and the later time as the implementation period. We form a control group comprising taxpayers whose annual income is between S\$100K and S\$160K, and a treatment group comprising taxpayers whose annual income is above S\$160K. We conduct a series of difference-in-differences (DID) regressions, controlling for individual fixed effects and month-year fixed effects to absorb individual invariant factors and macroeconomic factors that may affect consumption.

We use two data sets in our empirical work. The first data set is from a credit card company. It includes credit transaction records of more than 69,7000 card users from 2014M1 to 2017M8, spanning the pre-announcement period, the announcement period, and the implementation period. We also use a more refined proprietary dataset from Singapore's largest universal bank, DBS bank. It has more than 4.6 million customers, representing over 80 percent of the total population in Singapore. The dataset we obtained is a random and representative sample of more than 250,000 distinct customers in the bank from 2016M1 to 2017M12. It also contains more variables than the other data set, which allows us to conduct extra robustness tests. However, the data only let us examine taxpayers' post-announcement behavior stretching roughly from the middle of the announcement to the implementation period.

Our DID regressions reveal that the tax increase does not affect the tax targets' credit card consumption either in the announcement or implementation period. Using the more refined bank-provided data set, we find that the tax increase does not materially alter the affected taxpayers' total consumption trends.

Our main estimation is based on a matched sample constructed from propensity score matching. Our key result of a muted consumption response to the permanent tax increase remains robust when using the original sample before matching. In classifying taxpayers into income groups, we use the bank-reported income to proxy for taxable income. We verify that the bank-reported income is highly correlated with taxable income among the sub-sample of consumers for whom we have tax payment records. Furthermore, we obtain similar results when restricting the treatment and control group to those we observe their actual taxable income. Finally, the affected taxpayers, whether young or old, female or male, married or single, living in public or private housing, all exhibit no consumption response to the new tax policy.

We perform various robustness checks to examine whether consumer heterogeneity causes insignificant statistical results. First, consumers expecting less benefits from public spending may be more responsive to the negative income shock. Second, sophisticated consumers, such as those who set up auto-tax-payment, are more conscious in optimization over life cycles, and are more likely than others to cut consumption in response to the permanent income shock. Third, consumers with higher credit card debt face more liquidity constraints and are more likely to cut consumption after the negative income shock instead of smoothing consumption with borrowing. Finally, consumers who registered no income changes, unlike those making compensatory income changes, are more likely to cut consumption. We test these hypotheses but find no evidence that these consumers curtail their consumption in response to the tax increase. Such robust results are consistent with Baugh et al. (2021), which show that negative income shocks do not change consumption while positive income shocks increase consumption. The magnitude of the income tax increase is another possible reason why consumption does not fall in response, as the tax increase is very small relative to the consumer's income and wealth.

The no-consumption response begs the question of how Singaporeans finance their consumption amid the higher tax. The tax increase barely affects taxpayers' credit card debt indicating they are not borrowing from the credit card to smooth consumption. We find tentative suggestive evidence that the affected taxpayers

cut savings to absorb the permanent income shock: they reduce savings by \$0.20 the immediate year for each dollar of additional tax.

The objective of Singapore's 2015 tax increase is to finance redistribution programs. We explore whether Singapore's redistribution programs increase the consumption of the low-income group. To examine the effect of the redistribution programs, which start immediately after the 2015 budget announcement in 2015M2, we use the data set with the longer data window from 2014M1 to 2017M8. We cannot study the redistribution effect using the bank-provided data because many of the redistribution programs were already initiated before the starting month of the window of the bank-provided data set (2016M1).

We again use taxpayers in the income bracket from S\$100K to S\$160K as a control group because they are not focal targets of the redistribution program. We conduct DID regressions to compare lower-income consumers' consumption patterns to those in the control group, controlling for individual- and time-fixed effects. We find that lower-income bracket consumers increase their consumption from 2015M2 onward and beyond what would be caused by macroeconomic changes. In particular, those earning less than S\$40K per year increase their spending by S\$17 to S\$28 per month after the announcement of the tax increase, and by S\$15 to S\$30 after the implementation of the tax increase. Over 31 months after the announcement of the tax increase, not counting the macroeconomic multiplier effect, the low-income group in our sample increase their consumption by S\$3.8 million or S\$1.5 million per year, which accounts for 60% of the additional taxes raised from the higher income earners per year (S\$2.5 million).

This paper's contributions to the existing literature are twofold. First, we directly contribute to the literature on the effect of income tax (e.g., Eissa, Kleven, and Kreiner 2008; Zidar 2019). In evaluating the consumption and savings impact, previous studies focus on positive one-off tax rebates that apply to most taxpayers and find positive consumption responses (Souleles 1999; Johnson, Parker, and Souleles 2006;

Agarwal, Liu, and Souleles 2007; Parker et al. 2013).<sup>4</sup> Cloyne and Surico (2017) show that the consumption response to income tax change depends on the debt level. We add to this strand of literature by showing no consumption response of the rich population to a permanent income tax increase and provide suggestive evidence of a small decrease in savings among the higher-income population. The negative relation between income tax and savings is consistent with Tanzi and Zee (1998) and Baugh et al. (2021). It is possible that the taxed Singaporeans do not adjust their consumption downward because they believe the fiscal programs can indirectly benefit them, e.g., via bolstered future aggregate income.

Second, we show significant redistribution effects of fiscal policy. Our empirical evidence suggests that taxing the rich does not affect their consumption, but, the redistribution increases the poor's consumption beyond the usual macroeconomic effect of government spending programs. The result is in line with Jappelli and Pistaferri (2014) that redistribution is effective in increasing aggregate consumption. It also supports Kopczuk's (2019) argument that progressive income tax is an efficient way to raise revenues for redistributive policy. Our result also supports the optimal tax theory that redistribution should be achieved solely with the direct income tax (Atkinson and Stiglitz 1976).

In the next section, we describe Singapore's 2015 income tax changes. In Section 3, we describe our data and methodology. Section 4 presents the estimation results on consumption response and redistribution effects. Section 5 is the conclusion.

## **2. Singapore's institution setting and its 2015 government budget**

The Singapore government announced on 2015M2 a permanent income tax increase on taxpayers making more than S\$160K. The government explained that the additional progressive taxes strengthen future

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<sup>4</sup> More generally, our findings relate to a large literature that studies the consumption response to income shocks (see review by Browning and Collado, 2001 and Jappelli and Pistaferri 2010). A large effort focuses on the consumption responses of individuals to expected and unexpected income shocks in the presence of liquidity constraints and precautionary savings; for example, see Zeldes (1989a, 1989b), Carroll, Hall and Zeldes(1992) Carroll (1997), (Shapiro and Slemrod 2003a, 2003b), Souleles (1999, 2000, 2002), Parker (1999), Hsieh (2003), Stephens (2003; 2006; 2008), Stephens and Unayama (2011), Scholnick (2013), Agarwal and Qian (2014, 2017), Gelman et al. (2014), Di Maggio et al. (2017), Olafsson and Pagel (2018), and Agarwal, Pan, and Qian (2020).

revenues to finance redistribution programs and invest in Singaporeans' capabilities. It also announces a one-off tax rebate that goes mainly to the middle and upper-middle-income groups to mitigate their higher future income tax burdens. The rebate is half of a taxpayer's income tax on 2014 income, capped at S\$1000. Appendix Table 1 shows the Singapore tax schedules before and after the personal income tax change.

Singapore government releases to taxpayers their tax assessments in February and starts a maximum of twelve-month installment deduction from individuals' accounts in March. Thus, recipients will see the tax rebate starting 2015M3 in twelve monthly installments.

The Minister for Finance explained that his budget aimed to uplift small and medium-sized enterprises, build a "fair and inclusive" society and sustain a progressive system for the current and future generations. There were enhancements in spending programs on SMEs, retraining programs, support for seniors, retirement accounts, financial support for lower-income families in education going from pre-school to tertiary education, enhanced support for buying state-built accommodations, etc.<sup>5</sup> In other words, the fiscal program is classic redistribution from higher-income bracket taxpayers to retirees, to lower-income bracket Singaporeans, including investing in retraining programs.

This provides an opportunity to examine how a classic tax increase for redistribution affects consumption based on micro-level data and a clear-cut timeline, controlling for year-month effects that absorb away macroeconomic effects. Our setting has 2015M2 as the beginning of both the announcement and redistribution effects. We set 2017M3 as the beginning of the implementation of the tax increase on the targeted group because Singapore's practice is to collect tax on a person's previous year's deemed earned income in the current year starting in March in twelve monthly installments.

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<sup>5</sup> Examples of these programs include: an increase in "Good and Service Tax (GST) Rebate Voucher" for low income households by \$50, for low income seniors (65 or above) the voucher increases by another S\$150 to S\$300, a one-off additional Child Care Credit, a Silver Support Program starting in 2016 to distribution between \$300 to &750 quarter payout to about the bottom 20 per cent of senior Singaporeans.

### **3. Data and Methodology**

#### ***3.1 Data***

##### Data from a Credit Company

We use two proprietary datasets. The first is from an international credit company's Singapore branch; the data set covers all customers' credit card transaction records from 2014M1 to 2017M8. While credit card transaction is only a part of consumer spending, it is well correlated with consumption and has been used to examine consumption patterns, e.g., in Souleles (1999) and Agarwal et al. (2007). The data set includes customers' annual income, albeit historical and thus static over the years. The data set also reports customers' basic demographic characteristics. The data set's advantage is that it starts thirteen months before the announcement period and ends half a year after implementing the tax increase. The data window allows us to evaluate consumption patterns before and after the announcement and after the implementation of the tax changes.

##### Data from DBS

The second dataset is from DBS Bank, Singapore's largest bank with more than 4.6 million customers, representing over 80 percent of the total population in Singapore. The dataset we obtained includes a random representative sample of more than 250,000 distinct customers in the bank from 2016M1 to 2017M12. Thus, the data window starts about ten months after announcing the tax change and redistributional programs. The timing constrains our empirical investigation based on the data set. Because the data window overlaps with the tax rebate program for only one month, we cannot use the data to examine the distribution effect. However, the data set allows us to examine the tax increase's impact on the consumption of the taxed. The dataset's advantage is that it contains detailed transaction data on bank (including checking and savings) accounts, credit and debit cards, such as transaction date, amount, description, and merchant name for every individual in the sample. It also includes monthly statements that summarize a cardholder's aggregate banking and spending activities each month. Moreover, it records

monthly income uncovered from the certified income document, such as salary slip, which is constantly updated as income changes. Additionally, it covers a rich set of demographics such as age, gender, marital status, ethnicity, property type (public or private housing), etc.

Individuals could have multiple bank accounts, credit cards, and debit cards, even with the same bank. Following Agarwal and Qian (2014), we aggregate the data for every individual each month. "Cash spending" is the sum of all cash withdrawals from all bank accounts every month for every individual. "Bill" is the sum of all payments to utilities, hospitals, and credit cards issued by financial institutions other than DBS. Note that the payments to DBS' credit card are excluded from the calculation for "Bill" to avoid double counting for spending. Monthly "card spending" is the sum of each individual's credit and debit card spending. "Total spending" is the sum of cash withdrawals, bill payments, and card spending. We will trim the top 1 percent level in the final sample for all spending variables to eliminate the possible influence of outliers.

#### Sample selection, treatment, and control groups

To concentrate on individuals who have an active relationship with our data providers, we restrict the sample to individuals who have at least four-month active credit card transactions throughout the first half of the pre-treatment period. For the sample from the credit card company, the pre-treatment period is 2014M1-2015M1, before the announcement of the fiscal policy in 2015M2.<sup>6</sup> For the bank-provided sample, the pre-treatment period is 2016M1-2017M2, before implementing the higher tax collection on 2017M3. The first four months in each sample serve as the benchmark period that will be absorbed to identify the benchmark spending patterns.

The tax increase applies only to taxable income exceeding S\$160K from 2016 onward. Accordingly, our treatment group includes consumers with an annual income greater than S\$160K. Since the very low and

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<sup>6</sup> Singapore collects tax on a person's previous year's deemed earned income in the current year starting in March in up to twelve monthly installments. Thus, the higher tax collection starts in 2017M3 on income made in 2016.

high-income earners' consumption patterns may differ, we include as potential candidates in our control group individuals whose incomes are more than \$100K but less than S\$160K. To identify the treatment and control groups, we use the earliest possible annual income reported. The days are 2014M1 and 2016M1 for the credit card company-provided sample and the bank-provided sample, respectively. Using older income mitigates the concern that individuals adjust their income after the 2015 tax increase announcement. We shall conduct an explicit robustness check on whether our empirical results are affected by individuals who changed income brackets around the implementation of the higher tax.

The recorded income may differ from the actual taxable income because it excludes additional income, e.g., rental revenue, and tax exemptions such as childcare reliefs. We check if the gap is systematic. From the bank-provided sample, we can recover the taxable income in 2015 and 2016 for a subgroup of individuals who authorize Singapore's tax authority to directly collect their tax payments from their banking accounts (see Appendix Section A1 for detail). This yields a subsample of 5,080 individuals whose income and taxable income are both available in 2016. The correlation between the bank-recorded income and taxable income in the subsample is as high as 71%. The two variables are similarly distributed (see Appendix Figure 1). This suggests that the recorded income is a reasonable proxy for the taxable income. Based on the tax payment for the income in 2015 and 2016, we also show in Appendix Table 2 that high-income earners, on average, pay S\$1060 in response to the new tax policy. Those in the top income bracket with annual income greater than S\$280K increase tax payments more substantially in absolute and relative change. We shall conduct a robustness check using sub-samples less possibly affected by errors in reported income. From here onwards, the income variable refers to the recorded income unless otherwise specified.

The final sample derived from the credit card company's provision includes 6,656 distinct individuals (2,205 in the treatment group and 4,451 in the potential control group), corresponding to 256,540 individual-month observations (86,156 in the treatment group and 170,384 in the control group). To ensure that the treatment and adopted control groups are comparable, we use propensity score matching to identify a matched sample for the treatment group from the potential control group. In particular, we run a logit

regression of the treatment dummy on a set of demographic information, including age, gender, marital status, ethnicity, property type, education, and occupation, to compute propensity scores, based on which we perform the nearest-neighbor matching. Appendix Table 3 Column 1 reports the estimation result of the propensity score matching logistic regression that yields the matching group. We use the matching group as the control group for the main estimation. There are 2,165 distinct individuals in the treatment and matched control group, respectively, corresponding to 83,436 and 84,569 individual-month observations.

The final sample derived from the bank's provision includes 23,467 distinct individuals (10,882 in the treatment group and 12,585 in the potential control group), corresponding to 563,178 individual-month observations (261,154 in the treatment group and 302,024 in the potential control group). Again, we use propensity score matching to identify a matched sample for the treatment group from the potential control group. Appendix Table 3 Column 7 reports the estimation result of the propensity score matching logistic regression that yields the matching group. We adopt that as the control group in our subsequence analysis. After matching, there are 10,431 distinct individuals in both treatment and control groups, corresponding to 25,0343 and 25,0341 month-observations. Throughout the paper, we focus on the matched sample unless otherwise specified.

### ***3.2 Summary Statistics***

We start by examining whether the treatment and control groups are comparable along dimensions other than spending. Appendix Table 4 shows the summary statistics of demographic characteristics for the treatment group and the control group in the matched and original sample<sup>7</sup>. By selection, the treatment group is wealthier than the (matched) control group and the raw control group. The demographics of the treatment group and the raw control group in terms of age, ethnicity, gender, and marital status, are quite different. For example, the average age of the treatment group is 52, which is statistically higher than that

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<sup>7</sup> Appendix Table 3 provides the estimation results of propensity score matching that is applied to identify the matched sample.

of the raw control group (49) at the 1% significance level. The difference between the treatment and the control groups disappears in the matched sample. Thus, the matching exercise allows us to compare the treatment and control groups' consumption patterns relatively free of demographic differences. Still, we shall control for time-invariant individual effects in our subsequent regression analyses.

We next check whether the treatment and control groups share the same time trends in spending before the policy change. Figure 1 illustrates the unconditional mean of the total spending for the treatment and control groups from both data samples and across the samples' time horizons. While the wealthier treatment group has consistently higher average spending than the control group, the two group's time trends are observationally parallel throughout the sample periods, including before and after the announcement (in the credit card company sample) and before and after the implementation of the tax increase (in both the credit card company provided sample and the bank provided sample). This suggests the treatment group does not react much to the tax increase. Similar parallel trends are found for the original sample shown in Appendix Figure 2. The parallel trends before the announcement and implementation of the tax policy justify using the DID method. In the subsequent analysis, we further test the pre-trends following Freyaldenhoven et al. (2021) before performing key regressions (see Figure 2 and Appendix Figures 4-6).

[Insert Figure 1 about here]

Although our sample size is quite large, the estimation may still be subject to attrition bias if many individuals cross the income threshold (S\$160K), above which the income tax increase applies. We cannot access the transition probability based on the credit card provided sample because its income variable is mostly static. However, the bank-provided sample does offer reliable time-varying income records that allow us to examine cross-over in income classes. Recall that we identify the treatment and control groups based on taxpayers' 2015 income recorded in 2016M1. Appendix Table 5 reports the probability of transition between the treated and non-treated groups using taxpayers' 2015 and 2016 income. As shown in Panel A, out of 10,882 individuals in the treatment group, 94.6% are treated in 2016; and out of 12,585

individuals in the control group, 95.8% remained to be untreated in 2016. The probability of crossing over the threshold income is rather small, which mitigates attrition bias. Almost identical patterns are observed for the matched samples. We also compute the transition probability between treated and non-treated from 2016 to 2017. The annual income in 2016 and 2017 are both subject to the new tax rates. Panel B shows that the transition probability matrix from 2016 to 2017 is similar to that from 2015 to 2016, for both original and matched samples. These observations suggest that the crossover of the income threshold is unlikely to be systematically related to the policy change. Note that our subsequent main baseline results are qualitatively identical if we suppress the cross-income bracket observations.

Appendix Table 6 presents the unconditional mean and standard error of spending for the treatment and control groups in pre- and post-treatment periods. Panel A presents the result based on the credit card company sample. We focus on the comparison between the treatment and the matched control groups. Before the announcement of the new fiscal policy (2014M1-2015M1), the average monthly credit card spending is S\$526 for the treatment group and S\$381 for the control group, the difference of S\$145 is statistically significant at the 1% level.<sup>8</sup> After the announcement (2015M2-2017M8), the average monthly credit card spending for both the treatment and control groups declines by a very similar amount. The difference in the respective declines is an economically trivial and statistically insignificant S\$1.97.

Panel B of Appendix Table 6 presents similar summary statistics based on the bank sample. The average spending is significantly higher for the treatment group than the control group, both before and after the implementation of the tax increase. However, the difference in the spending gap between the treatment and control groups before and after the implementation of the tax increase (DID) is statistically insignificant and economically trivial. These preliminary findings of no spending response to the tax increase are

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<sup>8</sup> These numbers are on the low side because the credit card is not the most popular in Singapore. Within our sample, in the income class of S\$160,000, the average credit card spending in the DBS sample is larger by about four times (S\$2642). That is one of the reasons that we also use the DBS data in this study.

consistent with Figure 1. Similar results are found for cash and card spending, as well as credit card spending.

### 3.3 Methodology

We formally examine the average monthly response of consumption to the tax increase using difference-in-differences regressions. We here repeat the timeline: the permanent tax increase was announced in 2015M2, the collection started in 2017M3 on the previous year's income (i.e., 2016 income), and there was a once-off tax rebate from 2015M3 to 2016M2.

We conduct the DID regression using our credit card company provided data as follows:

$$Y_{it} = \gamma \times ExtraTax_i \times D_{preAnn} + a \times ExtraTax_i \times D_{Ann} + \beta \times ExtraTax_i \times D_{Imp} + c_i + m_t + \varepsilon_{it} \quad (1)$$

The dependent variable  $Y_{it}$  is consumer  $i$ 's credit card spending in Singapore dollar in time  $t$ . We focus on the dollar amount of spending to facilitate interpretation and comparison. We also repeat the analysis using the logarithmic-transformed and income-weighted consumption measures to check the robustness of our results.  $ExtraTax_i$  is consumer  $i$ 's dollar amount of extra tax charged against her annual income in 2016 after the implementation of the new tax policy; it is calculated based on the income reported to the credit card company in 2016 multiplied by the corresponding increment in the tax rate for each income bracket. The value of  $ExtraTax_i$  is 0 for the control group. As in Agarwal and Qian (2014), a subset of the pre-treatment period from 2014M1 to 2014M6, which is the first half of the pre-treatment period, is used to identify the benchmark consumption patterns and absorbed in the regression. We show in the robustness analysis that our results are not sensitive to the choice of benchmark period. The dummy variable  $D_{preAnn}$  equals to 1 for the pre-announcement period from 2014M7 to 2015M1. The coefficient of  $D_{preAnn}$  further verifies the parallel pre-treatment trends by capturing whether the treatment group exhibits any pre-announcement change in consumption relative to monthly consumption in the first six months (2014M1 to 2014M6). We subsequently describe the adoption of the Freyaldenhoven et al. (2021) approach to test for the parallel pre-treatment trends in the pre-announcement periods.  $D_{Ann}$  equals to 1 for the post-

announcement but pre-implementation period (2015M2 – 2017M2), and  $D_{Imp}$  equals to 1 after the implementation (2017M3 to 2017M8). Individual fixed effects ( $C_i$ ) and year-month fixed effects ( $m_i$ ) are included to account for consumers' idiosyncratic spending habits, seasonality in consumption, and the time variations in macroeconomic factors.

In such a setup, we identify the differential announcement and implementation effects of the tax increase on the treatment and control group taxpayers' consumption patterns, which are captured by the coefficients  $\alpha$  and  $\beta$ . The coefficient  $\gamma$  needs to be statistically insignificant to support the assumption of parallel pre-treatment trends in the treatment and control groups' spending, the prerequisite of using DID analysis.

We supplement our analyses using the more detailed bank-provided data set which starts in the post-announcement period. While the data set's short window is a constraint, it has useful variables that allow us to conduct detailed empirical comparisons of consumption patterns before and after the implementation of the tax increase. The basic specification is as follows:

$$Y_{it} = \alpha \times ExtraTax_i \times D_{preImp} + \beta \times ExtraTax_i \times D_{Imp} + c_i + m_t + \varepsilon_{it} \quad (2)$$

The dependent variable  $Y_{it}$  is consumer  $i$ 's spending in Singapore dollar that comprises cash, bills, and card spending, the latter of which can be further decomposed into credit and debit card spending.  $ExtraTax_i$  is the extra tax paid in Singapore dollar since 2017M3 for the 2016 annual income after the implementation of the tax increase.<sup>9</sup> The value of  $ExtraTax_i$  is 0 for the control group. Similarly, a subset of the pre-treatment period from 2016M1 to 2016M7 is used to identify the benchmark consumption patterns and absorbed in the regression.  $D_{preImp}$  is defined as a dummy that equals 1 for the pre-implementation period (which is post-announcement) from 2016M8 to 2017M2.  $D_{Imp}$  is a dummy that equals 1 for the post-treatment period from 2017M3 to 2017M12, the period that the government started collecting the additional

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<sup>9</sup> The extra tax is calculated based on the reported annual income. Our key results remain consistent when using monthly extra tax, calculated as the average extra tax burden over 12 months.

tax. Individual and year-month fixed effects are included to account for consumers' idiosyncratic spending habits, seasonality in consumption, and the time variations in macroeconomic factors.

The key coefficient of interest is  $\beta$ , which reflects the direct effect of every dollar of extra tax paid on average monthly consumption for individuals in the treatment group after the implementation of the new tax policy, relative to the spending difference during the benchmark period from 2016M1 to 2016M7. The coefficient  $\alpha$  captures the differential change in spending difference between the treatment and control group in the pre-treatment period from 2016M8 to 2017M2 relative to that in the benchmark period, in response to every dollar of expected extra tax. The coefficient  $\alpha$  needs to be statistically insignificantly different from zero to support the assumption of parallel pre-treatment trends in the treatment and control groups' spending, the prerequisite of using DID analysis. Again, we adopt the Freyaldenhoven et al. (2021) approach to test for parallel trends in the pre-implementation periods.

Specifically, to explore the dynamic spending response per dollar of extra tax, we follow Freyaldenhoven et al. (2021) to estimate the dynamic spending response to the new tax policy:

$$Y_{it} = \sum_{s=-5}^{s=5} \theta_s \text{ExtraTax}_i \times D_s + c_i + m_t + \varepsilon_{it}. \quad (3)$$

The variable  $D_s$  (for  $s \in [-7,9]$  in the bank provided sample) is a dummy that equals one if the year-month is  $s$  month(s) after (before) the policy implementation if  $s > 0$  ( $s < 0$ ). The coefficients  $\theta_1, \dots, \theta_5$  measure the marginal response in spending per dollar extra tax incurred in the month of policy implementation (2017M3), 1 month, ..., and five months after the policy implementation, respectively. Similarly, the coefficients  $\theta_{-5}, \dots, \theta_{-1}$  captures the change in the spending difference between the treatment and control groups in each of the seven pre-treatment months. If parallel trends are present in the pre-treatment period,  $\theta_{-5}, \dots, \theta_{-1}$  should be statistically insignificantly different from zero. For the credit card company provided sample, we focus on five months before and after the policy announcement (2015M2).

To understand the heterogeneous spending response across consumers and spending categories, we expand Eq. (2) to include a triple interaction term that captures cross-group differences:

$$Y_{it} = a \times ExtraTax_i \times D_{preImp} + \beta \times ExtraTax_i \times D_{Imp} + \gamma \times ExtraTax_i \times D_{Imp} \times Ind_{it} + c_i + m_t + \varepsilon_{it}. \quad (4)$$

The dummy variable  $Ind_{it}$  equals 1 for consumers (or spending items) falling in the specified categories, e.g., 1 if married, and 0 otherwise. The coefficient for the triple interaction term thus captures the difference in spending response between individuals in and out of the specified category (e.g., married and single). The coefficient for  $ExtraTax_i \times D_{Imp}$  reflects the spending response of the individuals not in the specified category. Adding up the coefficients for  $ExtraTax_i \times D_{Imp}$  and  $ExtraTax_i \times D_{Imp} \times Ind_{it}$  yields the spending response of individuals in the specified categories to the policy shock.

## 4. Results

### 4.1 Baseline Results

Table 1 reports the regression estimates on the average monthly credit card spending response to every dollar of extra tax as specified in Eq. (1). The coefficient for  $ExtraTax_i \times D_{preAnn}$  is both economically trivial and statistically insignificant, which suggests a good match of pre-treatment spending patterns. Later in Section 4.2, we further test the parallel trends formally following Freyaldenhoven et al. (2021). The coefficient for  $ExtraTax_i \times D_{Ann}$  is statistically and economically insignificant, suggesting that credit card spending is not affected by the announcement of a higher future tax. For \$1 extra tax, after the announcement and before the implementation of the tax increase, consumers in the treatment group increase their monthly credit card spending by 0.8 cents (with a 95% confidence interval of -0.5 cents to 2.1 cents, and an associated  $p$ -value of 0.26), which is indistinguishable from zero.<sup>10</sup> Thus, we can rule out the possibility that monthly consumption drops by more than 0.5 cents per dollar of extra taxes with 95%

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<sup>10</sup> See also Panel A of Figure 2 for the cumulative response and the corresponding confidence interval. Both the upper and lower bound of the confidence interval are economically small.

confidence. There is also no evidence that they respond to the extra tax after the implementation of the new tax policy as implied by the statistically and economically insignificant coefficient for  $ExtraTax_i \times D_{Imp}$ .

Replacing  $ExtraTax_i$  with  $Top_i$ , a dummy indicating individual  $i$  is the treatment group, in Eq. (1) yields similar results. In particular, consumers who are expected to face higher income tax reduce their monthly credit card spending by S\$1.15 (with a 95% confidence interval of -S\$22 to S\$19, and an associated  $p$ -value of 0.91) after the announcement of the tax policy and before its implementation, and increase their monthly credit card spending by S\$0.69 (with a 95% confidence interval of -S\$27 to S\$29, and an associated  $p$ -value of 0.96) after the implementation of the new tax policy. These changes are statistically insignificant and economically trivial—they are less than 1% of the average monthly spending of the treatment group. Our estimation rules out the possibility that monthly consumption declines by more than S\$27 after the policy announcement with 95% confidence.

[Insert Table 1 about here]

Table 2 reports the estimation results, using the more comprehensive bank supplied dataset, on the average monthly spending response to every Singapore dollar of extra tax as specified in Eq. (2). The coefficients for  $ExtraTax_i \times D_{preImp}$  measure the difference in spending change per dollar of extra tax between the treatment and control group in the post-announcement but pre-implementation period, relative to the benchmark period (2016M1 to 2016M7). We further support the parallel trends assumption in Section 4.2. Across the different spending measures, the coefficients are economically trivial (not more than 0.002) and statistically insignificant. For example, in the regression of total spending, for every Singapore dollar of extra tax, the spending difference between the treatment and control group in the pre-treatment period (2016M8 to 2017M2) increases by S\$0.002 or 0.2 cents ( $p$ -value=0.42), compared to that in the benchmark period. This suggests that there is little variation in the difference in the spending patterns between the treatment and control groups before the implementation of the new tax policy. The results support the usage

of the difference-in-differences method and enable us to ascribe any change in spending difference after the policy implementation to the policy.

The coefficient for the interaction between  $ExtraTax_i$  and  $D_{Imp}$  captures the spending response of the treatment group to every dollar of extra tax, relative to the benchmark period (2016M1 to 2016M7), after the policy implementation. Column 1 in Table 2 shows the average response of total monthly spending to the implemented tax increase. The estimated coefficient for  $ExtraTax_i \times D_{Imp}$  is -0.001, which means that, for every dollar of extra tax incurred, a consumer in the treatment group (a top earner with more than S\$160K annual income) reduces her total monthly spending minimally by 0.1 cents, which is economically trivial and statistically insignificant (the 95% confidence interval is -0.5 cents to 0.4 cents, and the associated p-value is 0.81). This means the probability of monthly consumption dropping by more than 0.5 cents is only 2.5%. We cannot reject the null hypothesis of no spending response to extra income tax.

Decomposing the total spending into cash, bills, and card spending, we find similar results of no response (see Columns 2 – 4 of Table 2). The estimated coefficients all remain statistically insignificant and economically trivial. The same results hold when we further break the card spending into credit and debit card spending (see Columns 5 and 6). The 95% confidence intervals for these coefficients through the various consumption measures are fairly narrow around zero: they range from -0.6 cents to 0.4 cents in response to every dollar of extra tax, depending on which spending categories we look into. We can rule out the possibility that different components of monthly spending drop by more than 0.6 cents per dollar of extra tax. Overall, there is no evidence that consumers facing the tax increase change their consumption in cash, bill, credit card, and debit card after the actual collection of the higher tax. The treatment group's lack of change in consumption in the implementation period, relative to the earlier announcement period, is also consistent with the results in Table 1, obtained in the credit card company provided data set. All these results are obtained after controlling for individual fixed effects and year-month fixed effects.

[Insert Table 2 about here]

We consistently find no evidence of consumption response after the policy implementation using (1) the original sample instead of the matched sample; (2) matched sample based on the taxable income instead of the bank reported income; (3) a sample that includes the outliers without winsorization; (4) the logarithmic transformation of consumption; (5) spending normalized by income; (6) alternative benchmark period from 2016M1-2016M2, and (7) only the treatment group in a simple multivariate regression instead of in a DID specification (see Appendix Table 7).

Worrying about errors in recorded income, we excluded individuals whose income is very close to the cut-off value, i.e., those earning between (155K, 165K), (150K,170K), (140K,180K), (130K, 190K). Doing so mitigates the possibility of inadvertently misclassifying consumers into the treatment and control groups. The results in Appendix Table 8 show that our no-consumption response results remain robust. The results are also robust across various spending categories (transportation, supermarket, entertainment, apparel, dining, travel, durable, service, others and online), for both visible and non-visible goods identified by either Charles, Hurst, and Roussanov (2009) or Heffetz (2011), and for loosely and strictly defined discretionary goods (see Appendix Table 9).

The high-income earners may not respond to the higher tax because the additional tax is too trivial relative to income. To explore this possibility, we evaluate the consumption response to the extra tax normalized by the 2015 income using the same DID specification in Eq.(2).<sup>11</sup> We find no evidence in Column 1 of Appendix Table 10 that total spending responds to the change in tax-income ratio. We further decompose the tax-income ratio into five quintiles but find no evidence that they have differential impacts on total spending (column 2). Note that even in the top quintile, individuals' extra tax accounts for only 1% of their income in 2015, which may be too small to trigger optimal consumption adjustments as a result of bounded rationality (Browning and Collado 2001; Hsieh 2003) It is also possible that high-income earners are also

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<sup>11</sup> We verify no pre-trend effects in Appendix Figure 4.

super wealthy, which leads to a rather low marginal propensity to consume (Holm, Paul, and Tischbirek 2021; Kaplan, Violante, and Weidner 2014).<sup>12</sup>

#### ***4.2 Dynamic Spending Response***

To check whether the spending response is smoothened out over time, we explore the dynamic evolution of spending response based on Eq.(3), following Freyaldenhoven et al. (2021). The top panel of Figure 2 plots, based on the credit card company provided data, the time paths of credit card spending response from five months before to five months after the policy announcement in diamond and the 95% confidence interval in vertical lines. Consistent with the regression results in Tables 1 and 2, the spending response is statistically and economically insignificant both before and after the policy announcement. These results first support the pre-treatment parallel trends. Furthermore, they support our previous finding that even after the implementation of the tax increase, the spending response remains economically and statistically trivial.

The bottom panel of Figure 2 shows similar evidence of parallel trends in the pre-treatment periods and of a lack in consumption response to the tax increase, based on the spending data from the bank-provided sample. Decomposing the total spending into cash, bills, card, credit, and debit spending yields similar results, which are not reported. These findings suggest that consumers directly affected by the tax increase do not change their consumption patterns.

[Insert Figure 2 about here]

#### ***4.3 Heterogeneous Spending Response***

The insignificant consumption response to income shock could be driven by heterogeneity—while some spend less, others spend more, which net out each other. Furthermore, the proportion of consumers changing spending is possibly so small that the significance of any estimated change is overwhelmed in the

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<sup>12</sup> It would be an interesting to further explore how the extra tax as a ratio of wealth affect consumption should wealth related data become available.

error term. In this sub-section, we first explore the heterogeneity across various demographic characteristics which may affect individuals' perceptions of benefits from rising government spending originated from the higher tax. We next delve into the sub-sets of individuals in the treated group that are more likely to cut their consumption in response to the permanent income shock. Not finding consumption responses in these sub-groups adds credence to the baseline results.

We hypothesize that more sophisticated consumers are more rational and thus more likely to cut consumption in response to income shocks; likewise, the more indebted consumers would have similar responses, for they are more credit-constrained. Consumers whose gross income had stayed unchanged or even declined are also more likely than others to cut consumption than those whose gross income had increased after the new tax policy. We now turn to explore potential heterogeneity across individuals.

#### **4.3.1 Different Perspectives on the Benefits**

The gist of this test is that different people see different benefits in transferring the current private budget to the government budget and therefore exhibit heterogeneous consumption responses to the income tax change. Hence, we check variations in consumption responses among sub-groups: young versus old, female versus male,<sup>13</sup> married versus single, and those enjoying government-supported homeownership versus those who are not.<sup>14</sup> These sub-groups should receive different incremental benefits from enhanced government spending, which affect their consumption responses. For example, the younger will possibly benefit more than the older, the married more than the single because the Singapore government is pro-

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<sup>13</sup> Female are likely to benefit more because they may get additional subsidy from the government; for example, working mother may claim a tax relief of \$3,000. Likewise, working mothers are eligible to claim the Grandparent Caregiver Relief of \$3,000 if their parent or grandparent is looking after any of their Singapore Citizen children aged 12 years and below.

<sup>14</sup> Singapore government offers "affordable housing" via its Housing Development Board (HDB). These HDB flats are less expensive than housing properties on the private open markets. A first-time buyer of a HDB flat directly from HDB gets a subsidized rate (below the market). Or, a first-time buyer of a resale HDB unit gets a cash grant. These subsidies and grants are not available to higher income people, e.g., those making more \$168K annual.

marriage, etc.<sup>15</sup> Mixing very different consumers in the treatment group could weaken the reliability of our regression estimates and thus contribute to the insignificant coefficient estimates. We estimate the heterogeneous response based on Eq. (4) and report the results in Table 3. We find that young people, defined as those below 45-year old,<sup>16</sup> spend more than the old people after the policy shock, but the result is not significant. Similarly, there is no statistical evidence that the spending responses are different across gender, or between those enjoying and not enjoying government-supported homeownership. There is a difference between married and single, although not statistically significant. Single taxpayers in the treatment group drop their annual consumption by 24 cents (= S\$0.02 per month  $\times$  12 months) per extra tax dollar paid while the married raise their annual consumption by a trivial 1.2 cents (=S\$(0.02-0.021) per month  $\times$  12 months) per extra-tax dollar paid.

[Insert Table 3 about here]

#### 4.3.2 Sophisticated Consumers

In Singapore, taxpayers can choose to participate in Provisional Instalment Plan (PIP), allowing the tax authority to automatically deduct taxes from their bank account based on the previous year's tax record. PIP participants can pay their income tax in interest-free monthly installments of up to 12 months. We identify this group of individuals based on their banking transactions and refer them as "sophisticated" consumer. These consumers are sophisticated from the perspective of opportunity costs of time and cash flows—it

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<sup>15</sup> There is an additional concern that justifies paying attention to the married vs single status. In Singapore, people file income tax individually. However, some could be from a joint household. Thus, an individual in the treatment group could have her spouse/partner in the control group, and their consumption patterns are based on their joint after tax income. This means the treatment group person faces an effective tax increase less than the government's stipulation while her spouse/partner in the control group faces a non-zero effective tax increase. This could taint our regression analysis. This concern applies mostly to those who are married. In Singapore, the cohabitant arrangement is not popular, partly because of its social norms and partly because of its social policies. Therefore, to mitigate the concern, we rerun our DID regressions keeping only the single in the control group. Our results in Table 2 and 3 are not qualitatively affected. Worrying about children in the control group living with parents, we further restrict the matched control to include only the single older than 40. Again, we obtained results qualitatively similar to those in Table 2 and 3.

<sup>16</sup> Age 45 is a threshold used by government to determine eligibility of certain reward program according to Agarwal and Qian (2014). Our result remains qualitatively identical if we move up the aged threshold to 50, 55, and 60,

costs little to participate in PIP yet saves the taxpayer transaction time and potential interest costs stemming from paying taxes in advanced. These sophisticated consumers are very rational and, therefore, more likely to cut consumption after the income shock as predicted by the permanent income hypothesis. We identify this sub-group of treated consumers in our matched sample (1347 out of 10431, or 13%).<sup>17</sup>

We repeat the regression in Eq. (3), replacing  $Ind_i$  with a dummy variable  $Sophisticated_i$  that equals 1 if individual  $i$  participates in PIP and has not revised the payment manually throughout the dataset's time window.<sup>18</sup> These sophisticated taxpayers' monthly consumption response to each extra dollar taxed in the implementation period is the sum of the coefficients for  $ExtraTax_i \times D_{Imp}$  and  $ExtraTax_i \times D_{Imp} \times Sophisticated_i$  while the others' response is just the coefficient for  $ExtraTax_i \times D_{Imp}$ . Column 1 in Table 4 shows that the coefficients for  $ExtraTax_i \times D_{Imp}$  ( $\beta < 0.001$ , p-value=0.966),  $ExtraTax_i \times D_{Imp} \times Sophisticated_i$  ( $\beta = -0.034$ , p-value=0.198), and their sum ( $\beta = -0.034$ , p-value=0.197) are all statistically insignificant. Thus, neither the sophisticated consumers nor the rest of the treatment group adjusts their consumption significantly after actually paying for the additional tax. These results add credence to our baseline results.

[Insert Table 4 about here]

### 4.3.3 Indebted Consumers

Consumers who are already heavily indebted before the implementation of the new tax policy may face more liquidity constraints; they are more likely to cut their consumption to cope with the negative income shock. We define a dummy variable  $HighDebt_i$ , which equals 1 for consumers whose credit card debt is above the 75th percentile (S\$500) in the benchmark period (2016M1 to 2016M7), and 0 otherwise. We then

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<sup>17</sup> The installment deduction will be reflected with a transaction code "ITX" in the bank transaction records. See Appendix A.1 for a detailed description of PIP and tax collection.

<sup>18</sup> Admittedly, it is possible that some individuals who do not participate in PIP in the bank that provides us the data may enrol in it from other bank. The probability should be small given that individuals in our sample have active transaction records in our sample.

estimate Eq. (3), replacing the dummy variable  $Ind_i$  with  $HighDebt_i$ .<sup>19</sup> By this classification, 1790 out of 10431 (17%) of the treated consumers in our matched sample have  $HighDebt = 1$ . In Column 2 of Table 4, the coefficient of  $ExtraTax_i \times D_{Imp}$  is statistically and economically insignificant ( $\beta = 0.001$ , p-value=0.709), which suggests that consumers not so much indebted do not respond to the income shock. The coefficient of  $ExtraTax_i \times D_{Imp} \times HighDebt_i$  is also statistically insignificant ( $\beta = 0.041$ , p-value=0.288), which suggests that even very indebted consumers do not behave differently from the rest in their spending responses to the income shock. The coefficients sum of  $ExtraTax_i \times D_{Imp}$  and  $ExtraTax_i \times D_{Imp} \times Indebted_i$  share similar insignificance ( $\beta = -0.040$ , p-value=0.300), pointing to the lack of consumption response by consumers with relatively high debt. Thus, both those credit-constrained with high levels of debt and the rest do not respond to the income shock; this evidence adds credence to our baseline results.

#### 4.3.4 Compensatory Income Adjustment

Consumers may adjust their labor supply to an income tax increase. A tax increase reduces the effective wage rate. This may induce a worker to choose to have more leisure and less income or to choose to have less leisure and more gross income. In the former case, consumption also drops, which drives towards a negative consumption response to income tax increase. That is not consistent with our baseline results in Table 2. In the latter case, her after-tax income may be less or more than before, and so is the change in consumption. In practice, firms may also increase salaries to retain talents. These compensatory adjustments may render the tax increase only a trivial impact on consumption. One may therefore conjecture that our baseline result of no consumption response is driven by the dominance of consumers whose gross income had increased after the new tax policy.

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<sup>19</sup> Redefining the indebted consumers as those whose credit card debt is higher than the median value yields similar results (not reported).

To test whether compensatory income adjustments drive our baseline results, we define the dummy variable  $HigherIncome_i$  as 1 for consumers whose gross income in 2016 is more than 2% higher than that in 2015.<sup>20</sup> Out of our matched sample, 1306 out of 10431 (13%) of the treated consumers' income increased by more than 2% after the new tax policy. To test whether these individuals' consumption differs from the rest, we estimate Eq. (3) by replacing the dummy variable  $Ind_i$  in Eq.(3) with  $HigherIncome_i$  and report the results in Column 3 of Table 4. The coefficient for  $ExtraTax_i \times D_{Imp}$  remains statistically insignificant and economically trivial ( $\beta=0.006$ ,  $p\text{-value}=0.381$ ), suggesting that even those experiencing *no income increase* do not curtail their consumption as in our baseline results. Note further that the coefficient of  $ExtraTax_i \times D_{Imp} \times HigherIncome_i$  is statistically insignificant ( $\beta=-0.009$ ,  $p\text{-value}=0.173$ ), suggesting that consumers who adjust their income upward do not respond differently to the extra income tax paid compared to their peers. Thus, compensatory income adjustments are unlikely to drive our results.

#### 4.3.5 Summary and Discussion

We have documented robust evidence of no consumption response to anticipated negative income shock, even among those very likely to respond: consumers who are more sophisticated, more in debt, and who do not make compensatory income changes. Our finding is consistent with Baugh et al. (2021), which document similar evidence using the US data that consumers do not change consumption when faced with anticipated *negative* income shock. Nevertheless, our result differs from the large number of studies that document positive consumption responses to anticipated *positive* income shocks such as tax refunds (see, for example, Agarwal, Liu, and Souleles 2007; Shapiro and Slemrod 2003b).

There are several reasons underlying the difference. First, while existing literature focuses on positive income shock, we explore the effects of a negative income shock. As documented by Baugh et al. (2021), positive and negative income shocks have asymmetric impacts on consumption. Second, the increase in

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<sup>20</sup>We adjust the income by 2%, which is about the average inflation rate in Singapore. Redefining the dummy variable by the reported income without adjustment yields similar result.

taxes was rather small relative to the rich population's income. Figure 3 shows the population density in every income bracket and their average extra income tax. After the progressive tax rate increase of 1%–2%, the high-income earners pay an extra income tax of S\$174–S\$3,206 per year, which accounts for 0.1%–1% of their annual income, depending on their income bracket. This segment of taxpayers may also have considerable unearned income (e.g., dividends, interest earnings), which is not taxed in Singapore. The extra tax payment is possibly negligible relative to their actual earnings and even more trivial relative to their wealth. As a result of bounded rationality, individuals do not adjust their consumption in response to small changes in income (Browning and Collado 2001; Hsieh 2003). Third, high-income earners studied in our paper have a relatively lower marginal propensity to consume or MPC than the low-income group that receives tax subsidies (Jappelli and Pistaferri, 2014).

[Insert Figure 3 about here]

#### ***4.4 Financing the income shock***

Regardless of the underlying mechanisms, no consumption adjustments imply that individuals have to finance the income shock with alternative sources. We explore whether consumers increase their income, borrow more, or dig into their savings to meet the extra income tax in this sub-section. We first utilize the income data from the credit company, which spans a relatively long horizon, to explore the income response to the new tax policy. We find no evidence in Panel A of Figure 4 that the treated consumers, who are subject to the higher tax rate, have experienced significant income changes relative to the control group. One may argue that consumers only need to increase their income after bearing the extra tax burden (since 2017M3) or that income adjustments lead consumption responses. However, this is unlikely given that all dynamic effects after the policy change in Panel A of Figure 4 are statistically insignificant.

[Insert Figure 4 about here]

We further verify the lack of income responses to the actual tax burden based on the monthly income recorded by the DBS bank. Again, we find no evidence in Panel B of Figure 4 that high-income earners

who are subject to the tax increment generate more monthly income than their peers. These results support the parallel trends before the implementation of the tax policy, which justifies our application of DID to evaluate the income response based on Eq.(2). We replace the dependent variable with the logarithmic transformation of one plus monthly income and report the result in Column 1 of Table 5.<sup>21</sup> To facilitate reading, we multiply the estimated coefficients by 10,000 so the numbers reflect the relative change in income in basis point (bp). We find that the estimated coefficients of  $ExtraTax_i \times D_{Imp}$  is statistically insignificant ( $\beta = -0.012$  bps, p-value = 0.154), which provides no evidence that consumers earn more to offset the income shock.

[Insert Table 5 about here]

We repeat the same DID method to investigate whether individuals increase outstanding debt to finance the income shock by replacing the dependent variable in Eq.(2) with  $\ln(1+debt)$ , where debt is the total credit card debt outstanding. The parallel trends in panel C of Figure 4 support our application of DID to evaluate the debt response to the new tax policy. The estimation result in Column 2 of Table 5 shows little debt response to the higher tax. For every \$1 extra tax payment, the debt increases by 0.007 bps, which is economically trivial and statistically insignificant (p-value=0.613).

Column 3 of Table 5 summarizes the response in savings, measured by the logarithm of one plus savings account balance, to every dollar of extra tax payment. We find that, on average, the savings of treated individuals drop significantly by 0.025 bps for every dollar of extra tax payment (p-value=0.042). This translates to a reduction of S\$0.2 in savings, given an average balance of S\$80367 for the treated group.

Panel D of Figure 4 shows that the savings balance started to decline two months before the implementation

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<sup>21</sup> Our results remain robust when using the untransformed dollar amount of income. Note also from Column 7 of Appendix Table 7 that our baseline result of no consumption response remains robust when using the logarithmic spending as the dependent variable. Subsequent regressions in this sub-section involve using savings and 'debt' as dependent variable. Savings is a stock variable, some savings amount can be huge. We take natural log to mitigate the influence of very large value. To be consistent, we apply the natural log transformation of the other dependent variables – debt and income. For some consumers, 'Debt' may attain the value of zero. We therefore add 1 to the raw value. We apply the same logarithmic transformation of one plus the raw value to all three dependent variables – income, debt, and savings – so that the dependent variables in Table 5 are in a consistent format.

of the new tax policy, possibly because individuals prepare for the increased tax payment, i.e., by transferring funds from their savings accounts to other accounts for direct tax debit. This finding is similar to Baugh et al. (2021). Given the violation of the pre-trend assumption in this case, we should be careful in deriving the causal impact of the tax income shock on saving reductions. Nonetheless, the magnitude of saving reductions provides suggestive evidence of how consumers may finance their tax increases.

The decline in savings is clearly not enough to offset the extra tax payment. There are several conjectures. First, the savings account balance is only a partial proxy for savings. Thus, our estimate could be only a lower bound of the treated taxpayers' dip into their savings. Second, some treated taxpayers may reallocate their income or labor supply among family members to deal with the negative income shock. Nevertheless, the results provide a suggestive direction.

#### ***4.5 Redistribution***

In this sub-section, we explore the consumption response of other income groups to the tax increase for the rich. Fiscal policy programs often have redistribution effects. In the current case, the Singapore 2015 budget announcement explicitly states that the extra tax raised from the rich would be used to enhance various spending programs aimed at the lower-income groups, many of which are immediately initiated. There are currently two major redistribution programs in Singapore. The first is the GST Voucher scheme, which supports low-income households in the forms of cash (for immediate needs), MediSave (for medical needs), U-Save (to offset utility bills), and S&CC Rebate (to offset Service and Conservancy). The second is the Workfare Income Supplement (WIS) scheme, which tops up low-income workers' salaries through direct cash payments and CPF (Central Provident Fund, Singapore's pension fund) contributions, which bolsters their retirement savings. The magnitude of these subsidies varies over the years depending on the need for support and the fiscal budget. Singapore's constitution stipulates that every term of government needs to

keep its budget balanced. Thus, raising fiscal revenue indicates greater fiscal capacity to provide financial support to low-income groups and less need to raise fiscal revenues in the remaining government term.<sup>22</sup>

Therefore, we examine if other income groups' consumption changes after the budget announcement, controlling for individual fixed effects and economy-wide year-month fixed effects. We cannot use the bank provided sample because many of the redistribution programs are initiated before the starting month of the data window. However, the credible card company supplied data set is appropriate for the investigation because its data window begins in 2014M1, which is well ahead of 2015M2, the announcement month of the tax increases and the initiation of the redistribution programs as well as the tax rebate program.

We conduct DID regressions to compare lower-income bracket consumers with the control group, whose 2014 income is between S\$100K to S\$160K. The lower income brackets are, respectively, (0, 20K], (20,30K], (30, 40K], (40, 80K], and (80, 100K]; these are our treatment groups. As reported earlier in Section 3.1 (sample selection), we use propensity score matching to construct a matched sample so that every low-income group (treated) is comparable to the matched control group in their demographic characteristics such as age, gender, ethnicity, property type, occupation, and education. Compared with the full sample, the differences between the treatment and control groups by design shrink substantially in the matched sample (see Appendix Table 4).

We first test for parallel trends in Appendix Figure 5 following Freyaldenhov et al. (2021), which justifies the application of DID approach in evaluating the redistribution effects. The estimation results are reported in Table 6. The coefficient for  $Treat_i \times D_{preAnn}$  is both economically small and statistically insignificant across different treated income brackets, save the (40, 80K] treatment group. It suggests that all but the (40, 80K] treatment groups and the control group have a consistent gap in their credit card spending before the

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<sup>22</sup> A government can have a surplus at the end of its term; the surplus will be transferred to Singapore's sovereign wealth funds.

announcement of the fiscal programs. We, therefore, acknowledge that we need to be cautious about the validity of the DID regression for the (40, 80K] treatment group.

[Insert Table 6 about here]

The results in Table 6 show that individuals in the annual income range of (0, 20K], (20,30K], (30, 40K], and (40, 80K] increase their monthly consumption by S\$25, \$28, \$17, and \$15 per month from 2015M2 to 2017M2, respectively, relative to the first half of 2014. Note that these estimates should add extra credence to successful fiscal redistribution: the (0, 20K] group received no tax rebate from 2015 M3 to 2016 M2. Likewise, the (20, 30K] and (30, 40K] received less tax rebate than the control group. The changes are both statistically and economically significant. After the actual implementation of the higher tax collection, the lower-income subgroups, (20, 30K] and (30, 40K], continue to increase their credit card spending by \$30 and \$15 per month, respectively, again relative to the first half of 2014. These results mean that the positive redistribution impact on consumption in the (20, 30K] and (30, 40K] groups are rather long-lasting and consistent for thirty-one months. We note, however, the impact on card spending by the income groups (0, 20K] and (40, 80K] has become statistically insignificant after the implementation of the higher tax.

We further note that the (80, 100K] group has not changed their card spending pattern throughout. Moreover, the results for the (40, 80K] group need to be interpreted with caution as the estimated coefficient of  $Treat_i \times D_{preAnn}$  is significant at the 10% level, which challenges the parallel trend prerequisite.

Nevertheless, the above results suggest that while the tax increase does not directly affect the rich population, it has a positive redistribution effect, directly raising the lower-income groups' consumption. Using our credit card company provided matched sample, we also note that the extra tax directly raised amounts to S\$2.5 million per year according to the income in 2016 while the extra-consumption by the (0, 20K], (20,30K] and (30, 40K) groups from 2015M2 to 2017M8 amounts to S\$3.8 million, which is about

S\$1.5 million per year.<sup>23</sup> Such effective redistribution echoes Agarwal and Qian (2014), which shows that consumers in Singapore spend S\$0.8 for every S\$1 they receive from the government. It is also consistent with Jappelli and Pistaferri (2010) that redistribution is effective in increasing aggregate consumption because of the relatively high marginal propensity to consume for the low-income groups (Jappelli and Pistaferri 2014).

There are several reasons why Singapore's redistribution programs are effective. First, the redistribution programs are highly targeted, ensuring that support is provided to the most needed with few errors. For example, to be eligible for the WIS benefit, one's gross annual income cannot exceed S\$24K as of 2018, which policymakers can directly verify through tax records and income reports. Second, the redistribution programs are progressive even among legitimate recipients. For example, GST cash payout is higher for those with lower income and/or lower home value, and GST MediSave is higher for the older population. Third, the deliveries are effective and user-friendly. Most of the financial supports are automatically credited to eligible citizens' accounts, and the recipients can utilize them with great ease. For example, the utility company can directly debit from GST U-Save to offset households' utility bills. We believe these features – effective targeting, progressivity, and user-friendly utilization – are characteristics of effective redistribution programs.

## 5. Conclusions

Governments worldwide now face challenges from rising inflation concerns, slowing economic growth, growing government deficits, and rising income inequality. Fiscal redistribution has to play a more

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<sup>23</sup> For those in the income bracket of (0, 20K), the average consumption increases by S\$24.808 per month per individual after the announcement and before the implementation of the tax policy (2015M2-2017M2, a total of 25 months) and by S\$14.515 after the implementation of tax policy (2017M3-2017M8, a total of 6 month), which adds up to an additional consumption of  $24.808 \times 25 + 14.515 \times 6 = S\$707.290$  per person. Similarly, the average consumption per month per individual increases by  $27.865 \times 25 + 29.762 \times 6 = S\$875.197$  and  $17.086 \times 25 + 15.053 \times 6 = S\$517.468$  respectively for those in the income bracket of (20,30K) and (30, 40K). Note from Appendix Table 4 that, in the matched sample, the number of individuals in the income bracket of (0, 20K), (20,30K) and (30, 40K) is 966, 1522 and 3371 respectively. The total consumption incremental for those in the income bracket of (0,40K) sums up to  $707.290 \times 966 + 875.197 \times 1522 + 517.468 \times 3371 = S\$3.76$  million. The average extra tax for the 2165 treated individuals in 2016 in the matched sample is S\$1170, which amounts to a total of S\$2.5 million per year.

significant role than ever to smoothen out the population's economic stress. This is evident from the two trillion dollar stimulus package that Senate leaders and the White House agreed to on March 25<sup>th</sup>, 2020 (Washington Post, 2020) and then another \$1.9 trillion on Mar 11, 2021 (Financial Times, Mar 11, 2021). Economists, e.g., Krugman (2020), openly advocate raising long-term government expenditures financed by government borrowing. Yet, many worry that government debts across many countries are already very high, e.g., the US, Japan, the EU, and China. Hence, there is a reservation on raising government debt to finance stimulus packages.

Singapore's experience sheds light on the alternative of raising taxes to finance fiscal spending; such a program may not be as politically unappealing as some may imagine. In 2015, Singapore raised personal income tax on those in higher income brackets to strengthen the government's spending power on redistribution programs that could also benefit the economy. The government explicitly stated that the fiscal policy aimed for "progressivity" and "fairness." The government's enhanced resources would be spent on SMEs, retraining programs, support for seniors, retirement accounts, financial support for a lower-income family in education, and enhanced support for buying state-built accommodations.

We first find that the very marginal tax increase has minimal impact on the taxed Singaporeans' consumption, which is consistent with the literature (e.g., Johnson, Parker, and Souleles 2006; Jappelli and Pistaferri 2010, 2014; Parker et al. 2013; Christelis et al. 2019). However, we also find the promised redistribution effect materializes: the lower-income classes individually raise their consumption in an economically significant and long-lasting manner. In aggregate, the redistribution immediately raises aggregate consumption above and beyond the change in macroeconomic conditions, which are absorbed away by year-month time dummies.

We believe these results deserve attention and must be scrutinized, especially in the era when the importance of fiscal policy is rising. We welcome alternative explanations for our empirical findings.

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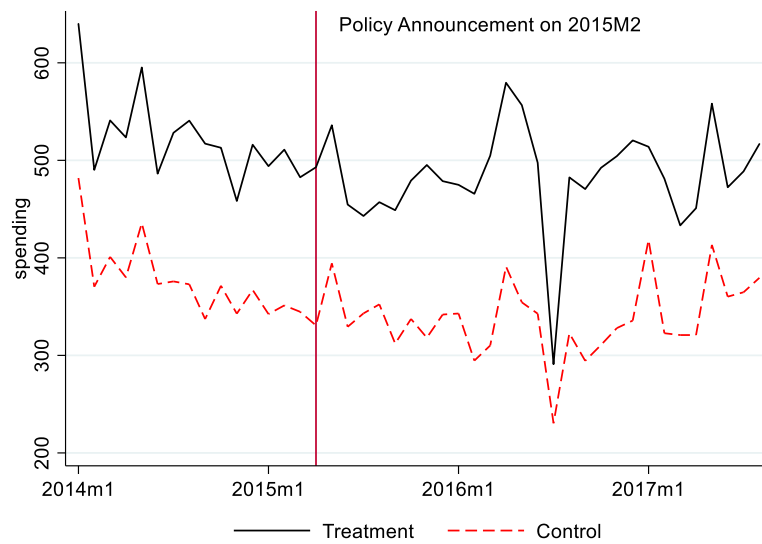
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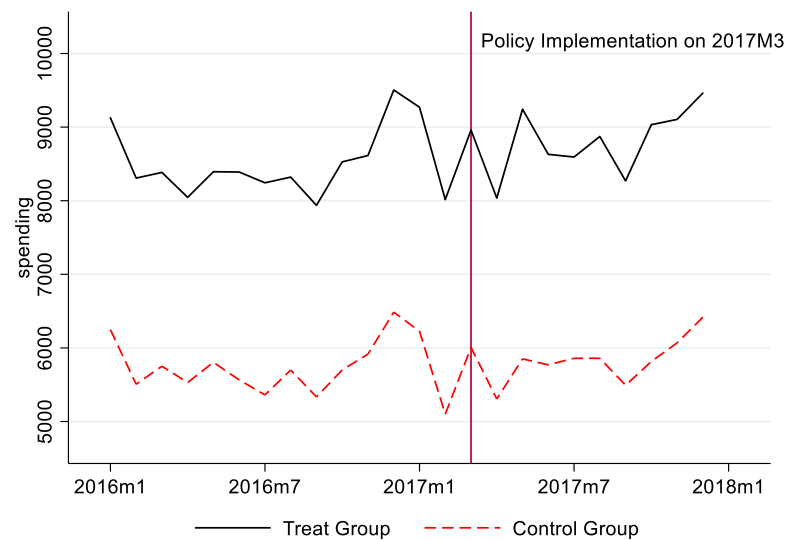
### Figure 1. Unconditional Spending Response: Matched Sample

The chart plots the spending (in S\$) for treatment and control groups in solid and dashed lines, respectively. In panels A and B, spending refers to, respectively, credit card spending and total spending (the sum of cash, bills, and card spending). The treatment group includes individuals whose annual income is more than S\$160K. The control group covers individuals whose annual income is more than S\$100K but not higher than S\$160K. The vertical line highlights the cut-off month between the pre- and post-treatment period, in 2015M2, the policy announcement month for panel A, and 2017M3, the policy implementation month for panel B.

Panel A: Credit Card Company Data (2014M1-2017M8)



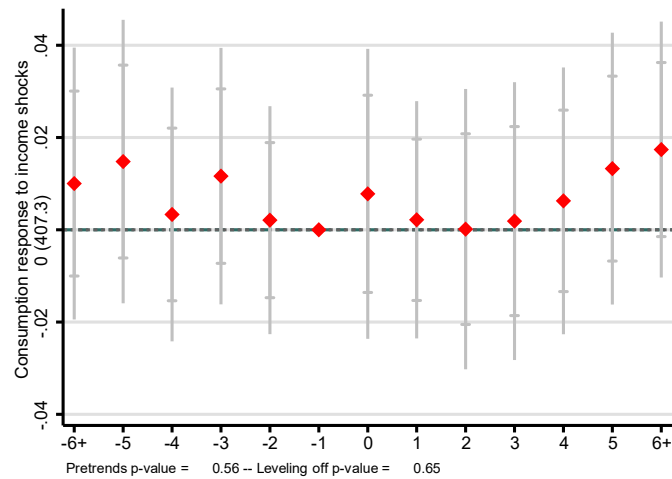
Panel B: DBS Bank Data (2016M1-2017M12)



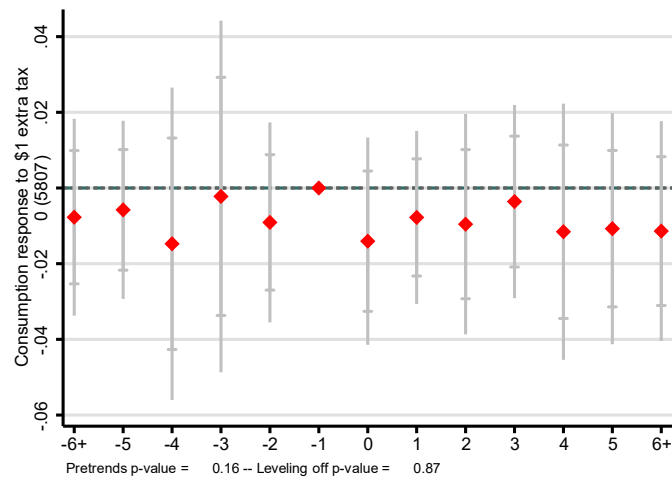
## Figure 2. Dynamic Spending Response

This figure plots the matched sample results of the cumulative spending response to every dollar of extra tax. The dashed lines represent the 95% confidence intervals. The vertical line is the month when the new tax policy was announced (implemented) in panel A (B). The x-axis denotes the  $s$ th month after the announcement (implementation) of the higher tax rate in panel A (B). The y-axis represents the cumulative response in the Singapore dollar term. Panel A reports the cumulative credit card spending response from seven months before the announcement of higher tax in 2015M2 to thirty months after it. Panel B illustrates the cumulative total spending response from seven months before to nine months after the implementation of the new tax policy in 2017M3.

Panel A: Credit card company data

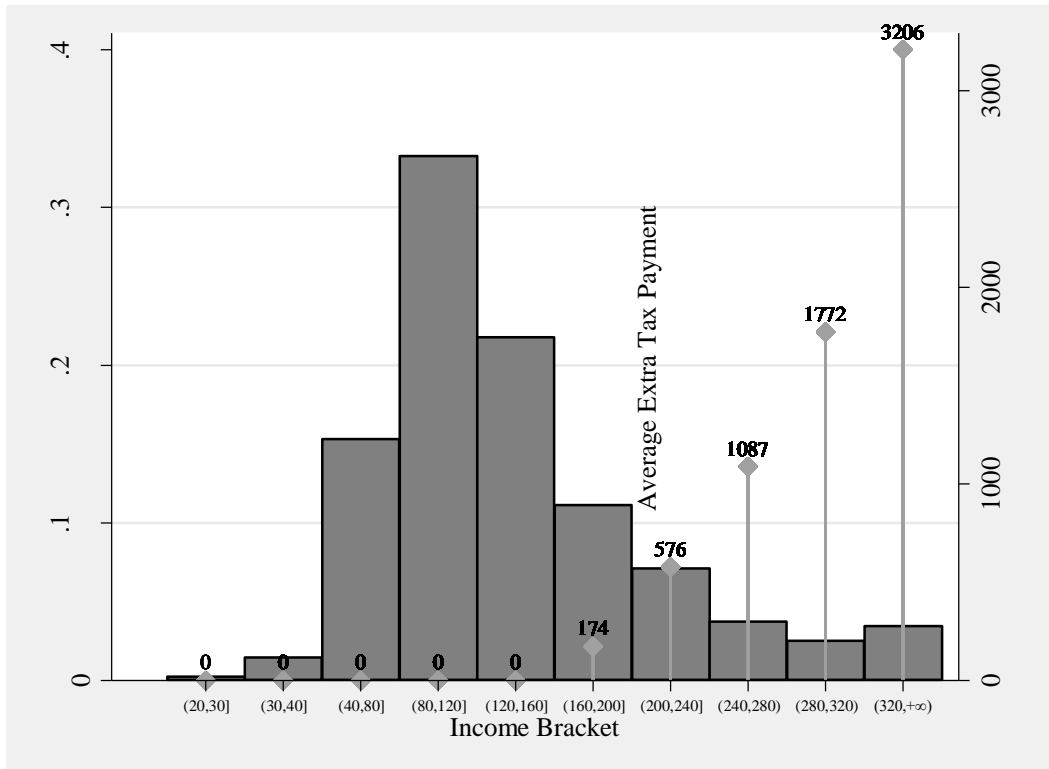


Panel B: DBS Bank Data



**Figure 3. Distribution of Income and Tax Increase**

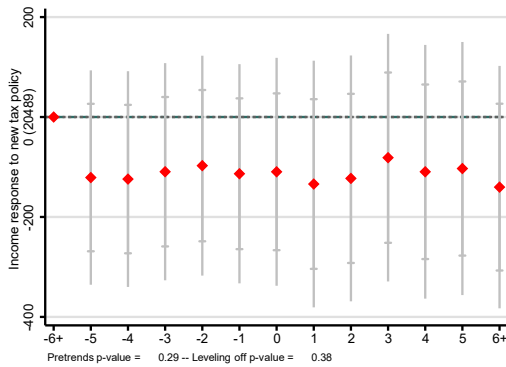
This figure plots the density of consumers in bars (left y-axis) and the average extra tax caused by the new tax policy in diamond (right y-axis) against the income brackets that correspond to different marginal tax rates. The income brackets are identified based on annual income in 2016 based on DBS bank data.



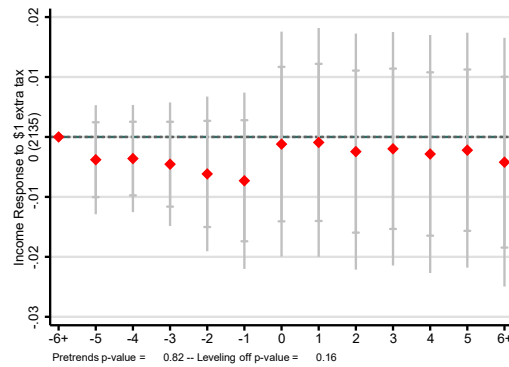
**Figure 4. Dynamic response of income, debt, and savings to the new tax policy.**

This figure plots the dynamic response of income, debt, and savings to the new tax policy. Panel A illustrates the monthly income in Singapore dollar obtained from the credit card company five months around January 2016. The other panels demonstrate the dynamic response of monthly income (panel B), debt—the logarithm of one plus total credit card debt outstanding (panel C), and saving—the logarithm of one plus saving account balance (panel D) to every dollar of extra tax five months around the implementation of the new tax policy in 2017M3, based on the matched sample from DBS bank. The diamonds mark the estimated dynamic effects, the vertical lines crossing the diamonds represent the 95% confidence intervals, and the outer lines reflect the uniform sup-t confidence bands.

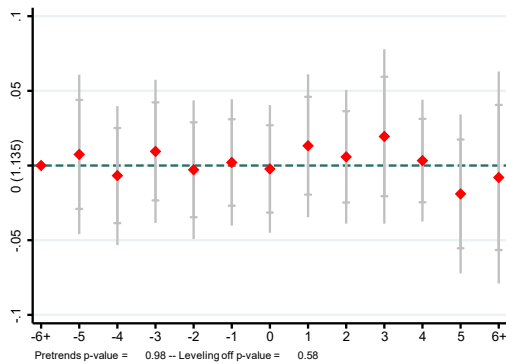
Panel A: Income from the credit card company



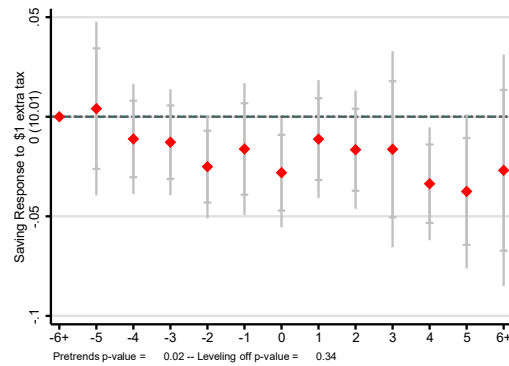
Panel B: Income from DBS bank



Panel C: Debt



Panel D: Saving



**Table 1. Average Consumption Response to Tax Increase**

This table reports the matched sample results of the average credit card spending response to the announcement of tax rate increment for the top income earners on 2015M2 based on the credit card company provided sample from 2014M1 to 2017M8. The estimation model for the left panel is:

$$Y_{it} = \gamma \times ExtraTax_i \times D_{preAnn} + a \times ExtraTax_i \times D_{Ann} + \beta \times ExtraTax_i \times D_{Imp} + c_i + m_t + \varepsilon_{it}$$

The dummy variable  $D_{preAnn}$  equals to 1 for pre-announcement period (2014M7 – 2015M1),  $D_{Ann}$  equals to 1 for the post-treatment period; that is, after the announcement but before the implementation of new tax policy (2015M2 – 2017M2), and  $D_{Imp}$  equals to 1 after the implementation of tax policy (2017M3 to 2017M8). Observations in the first six months (2014M1 to 2014M6) are absorbed to identify the spending patterns. The treatment includes individuals whose annual income in 2014 is more than S\$160K, while the control group includes those whose annual income in 2014 is more than S\$100K but less than S\$160K.  $ExtraTax_i$  is calculated based on the actual income in 2016. The left panel is estimated by replacing  $ExtraTax_i$  with  $Top_i$ , a dummy that equals 1 for individuals in the treatment group. Consumer and year-month fixed effects are included, and standard errors reported in the parenthesis are clustered by consumers. \*, \*\* and \*\*\* denote significant at 10%, 5% and 1% level respectively.

	<u>Credit Card Company Data</u>		
	Credit		Credit
$ExtraTax_i \times D_{pre}$	0.003 (0.005)	$Top_i \times D_{pre}$	8.365 (9.560)
$ExtraTax_i \times D_{Ann}$	0.008 (0.007)	$Top_i \times D_{Ann}$	-1.151 (10.650)
$ExtraTax_i \times D_{Imp}$	0.005 (0.009)	$Top_i \times D_{Imp}$	0.686 (14.570)
Constant	338.618*** (3.171)	Constant	345.096*** (4.178)
Observations	158,246	Observations	166,328
R-squared	0.502	R-squared	0.506

**Table 2. Average Consumption Response: Additional Evidence**

This table reports the matched sample results of the average spending response to every dollar of extra tax using DBS bank's data, based on the following model:

$$Y_{it} = a \times ExtraTax_i \times D_{preImp} + \beta \times ExtraTax_i \times D_{Imp} + c_i + m_t + \varepsilon_{it}.$$

The dependent variable  $Y_{it}$  is the dollar amount of total spending, which is decomposed into cash (column 2), bill (column 3), and card spending (column 4) that comprises of credit card (column 5) and debit card (column 6) spending.  $ExtraTax_i$  is extra tax dollar incurred by the treated individuals, whose annual income exceeds S\$160K, after the implementation of the new tax policy.  $D_{preImp}$  is a dummy that equals 1 for the pre-treatment period from 2016M8 to 2017M2, which is after the announcement but before the implementation of the new tax policy.  $D_{Imp}$  is a dummy that equals 1 for the treatment period from 2017M3 to 2017M12, which is after the implementation of the new tax policy. Observations from 2016M1 to 2016M7 are absorbed to identify the benchmark spending pattern. Consumer and year-month fixed effects are included, and standard errors reported in the parenthesis are clustered by consumers. \*, \*\* and \*\*\* denote significant at 10%, 5% and 1% level respectively.

	(1) Total spending	(2) Cash	(3) Bill	(4) Card (credit + debit)	(5) Credit card	(6) Debit card
$ExtraTax_i \times D_{preImp}$	0.002 (0.002)	-0.001 (0.001)	0.002 (0.002)	0.001 (0.001)	0.001 (0.001)	-0.000 (0.000)
$ExtraTax_i \times D_{Imp}$	-0.001 (0.003)	0.002 (0.001)	0.000 (0.002)	-0.001 (0.002)	-0.001 (0.002)	-0.000 (0.000)
Constant	5,641.894*** (1.527)	1,478.874*** (0.481)	2,542.204*** (1.192)	1,664.244*** (1.070)	1,631.454*** (1.149)	43.194*** (0.088)
Observations	481,713	495,678	495,672	490,793	495,673	495,669
R-squared	0.619	0.572	0.602	0.523	0.516	0.590

**Table 3. Heterogeneous Spending Response: By Demographic Characteristics**

This table reports the matched sample results of heterogeneous spending across various consumer groups in response to every dollar of extra tax using DBS bank data, based on the following model:

$$Y_{it} = a \times ExtraTax_i \times D_{preImp} + \beta \times ExtraTax_i \times D_{Imp} + \gamma \times ExtraTax_i \times D_{Imp} \times Group_i + c_i + m_t + \varepsilon_{it}.$$

The dependent variable  $Y_{it}$  is the dollar amount of total spending.  $ExtraTax_i$  is the dollar amount of extra tax incurred for the treated individuals, whose annual income exceeds S\$160K, after the implementation of the new tax policy.  $Group_i$  is a dummy that equals to 1 if the consumer falls into the group specified in the second row (young people (age<45), Female, Married, and HDB residence).  $D_{preImp}$  is a dummy that equals 1 for the pre-treatment period from 2016M8 to 2017M2, which is after the announcement but before the implementation of the new tax policy.  $D_{Imp}$  is a dummy that equals 1 for the treatment period from 2017M3 to 2017M12, which is after the implementation of the new tax policy. Observations from 2016M1 to 2016M7 are absorbed to identify the benchmark spending pattern. Consumer and year-month fixed effects are included, and standard errors reported in the parenthesis are clustered by consumers. \*, \*\* and \*\*\* denote significant at 10%, 5% and 1% level respectively.

Group category	Dependent variable = Total spending			
	(1) age<45	(2) Female	(3) Married	(4) HDB Residence
$ExtraTax_i \times D_{preImp}$	0.002 (0.002)	0.002 (0.002)	0.002 (0.002)	0.002 (0.002)
$ExtraTax_i \times D_{Imp}$	-0.001 (0.002)	-0.000 (0.003)	-0.020 (0.014)	-0.001 (0.003)
$ExtraTax_i \times D_{Imp} \times Young$	0.002 (0.018)			
$ExtraTax_i \times D_{Imp} \times Female$		-0.002 (0.011)		
$ExtraTax_i \times D_{Imp} \times Married$			0.021 (0.014)	
$ExtraTax_i \times D_{Imp} \times HDB$				0.001 (0.013)
Observations	481,713	481,713	481,713	481,713
R-squared	0.619	0.619	0.619	0.619

**Table 4. Heterogeneous Spending Response: By Consumer’s Financial Characteristics**

This table reports the matched sample results of the spending differences between different groups of individuals in response to every dollar of extra tax using DBS bank’s data, based on the following model:

$$Y_{it} = a \times ExtraTax_i \times D_{preImp} + \beta \times ExtraTax_i \times D_{Imp} + \gamma \times ExtraTax_i \times D_{Imp} \times Ind_i + c_i + m_t + \varepsilon_{it}$$

The dependent variable  $Y_{it}$  is the dollar amount of total spending.  $ExtraTax_i$  is the dollar amount of extra tax incurred for the treated individuals, whose annual income exceeds S\$160K, after the implementation of the new tax policy.  $Ind_i$  is a set of dummy including (i)  $Sophisticated_i$ , a dummy that equals 1 for consumers who participate in the Provisional Instalment Plan so that future installment of tax payments is automatically deducted from the bank account; (ii)  $HighDebt_i$ , a dummy that equals 1 for consumers whose credit card debt is in above the 75<sup>th</sup> percentile (S\$500) in the benchmark period (2016M1-2016M7); and (iii)  $HigherIncome_i$ , a dummy that equals 1 for consumers whose income in 2016 had increased relative to 2015.  $D_{preImp}$  is a dummy that equals 1 for the pre-treatment period from 2016M8 to 2017M2, which is after the announcement but before the implementation of the new tax policy.  $D_{Imp}$  is a dummy that equals to 1 for the treatment period from 2017M3 to 2017M12, which is after the implementation of the new tax policy. Observations from 2016M1 to 2016M7 are absorbed to identify the benchmark spending pattern. Consumer and year-month fixed effects are included, and standard errors reported in the parenthesis are clustered by consumers. \*, \*\* and \*\*\* denote significant at 10%, 5% and 1% level respectively.

Consumer category	Dependent variable = Total spending		
	(1) Sophisticated	(2) High Debt	(3) Higher Income
$ExtraTax_i \times D_{preImp}$	0.002 (0.002)	0.002 (0.002)	0.002 (0.002)
$ExtraTax_i \times D_{Imp}$	0.000 (0.003)	-0.001 (0.003)	0.006 (0.006)
$ExtraTax_i \times D_{Imp} \times Sophisticated$	-0.034 (0.026)		
$ExtraTax_i \times D_{Imp} \times HighDebt$		0.041 (0.039)	
$ExtraTax_i \times D_{Imp} \times HigherIncome$			-0.009 (0.006)
Observations	481,713	481,713	481,713
R-squared	0.619	0.619	0.619
No of Specific Consumers	1347	1790	1306

**Table 5. Financing the tax increase**

This table summarizes how monthly income, credit card debt, and savings account balance respond to every dollar of extra tax using the matched sample from DBS bank’s data, based on the following model:

$$\ln(1 + Y_{it}) = a \times ExtraTax_i \times D_{preImp} + \beta \times ExtraTax_i \times D_{Imp} + c_i + m_t + \varepsilon_{it}.$$

The dependent variable is the logarithm of one plus  $Y_{it}$ , is monthly income in Singapore dollar (column 1); total credit card debt outstanding (column 2), and savings account balance at the end of each month (column 3).  $ExtraTax_i$  is the dollar amount of extra tax incurred for the treated individuals, whose annual income exceeds S\$160K, after the implementation of the new tax policy.  $D_{preImp}$  is a dummy that equals 1 for the pre-treatment period from 2016M8 to 2017M2, which is after the announcement but before the implementation of the new tax policy.  $D_{Imp}$  is a dummy that equals 1 for the treatment period from 2017M3 to 2017M12, which is after the implementation of the new tax policy. We have multiplied all coefficients by 10,000 ( $10^4$ ) to improve readability, which means that the estimated coefficients reflect the relative change in  $Y_{it}$ , in basis point. Observations from 2016M1 to 2016M7 are absorbed to identify the benchmark spending pattern. Consumer and year-month fixed effects are included, and standard errors reported in the parenthesis are clustered by consumers. \*, \*\* and \*\*\* denote significant at 10%, 5% and 1% level respectively.

	(1) ln(1+ Monthly income)	(2) ln(1+Debt)	(3) ln(1+Saving)
$ExtraTax_i \times D_{preImp}$	0.000 (0.002)	-0.006 (0.009)	-0.008 (0.007)
$ExtraTax_i \times D_{Imp} \times 10^4$	-0.012 (0.009)	-0.007 (0.013)	-0.024** (0.012)
Constant	7.103*** (0.000)	1.273*** (0.001)	9.669*** (0.001)
Observations	500,681	437,051	476,353
R-squared	0.892	0.532	0.810

**Table 6. Redistribution**

This table reports the matched sample result of the average credit card spending response of the relatively low-income groups to the announcement of tax rate increment for the top income earners on 2015M2 using the credit card company data, based on the following model:

$$Y_{it} = \gamma \times Treat_i \times D_{preAnn} + a \times Treat_i \times D_{Ann} + \beta \times Treat_i \times D_{Imp} + c_i + m_t + \varepsilon_{it}$$

The outcome variable  $Y_{it}$  is credit card spending. The dummy variable  $D_{preAnn}$  equals to 1 for pre-announcement period (2014M7 – 2015M1),  $D_{Ann}$  equals to 1 for the post-treatment period that is after the announcement and before the implementation of new tax policy (2015M2 – 2017M2), and  $D_{Imp}$  equals 1 after the implementation of tax policy (2017M3 to 2017M8). Observations in the first six months (2014M1 to 2014M6) are absorbed to identify the spending patterns.  $Treat_i$  is a dummy that equals 1 for individuals in the treatment group whose annual income in 2014 falls in the range specified in the first row of the table and 0 for those whose annual income in 2014 is more than S\$100K but less than S\$160K. Consumer and year-month fixed effects are included, and standard errors reported in the parenthesis are clustered by consumers. \*, \*\* and \*\*\* denote significant at 10%, 5% and 1% level respectively.

Treated Group	Income Range					
	(160K,+∞]	(80K,100K]	(40K,80K]	(30K,40K]	(20K,30K]	(0, 20K]
Treat×D <sub>preAnn</sub>	8.365 (9.560)	6.112 (5.054)	7.958* (4.583)	-1.058 (4.575)	6.326 (6.444)	5.277 (7.807)
Treat×D <sub>Ann</sub>	-1.151 (10.650)	7.945 (5.605)	14.689*** (5.050)	17.086*** (5.126)	27.865*** (7.223)	24.808*** (8.631)
Treat×D <sub>Imp</sub>	0.686 (14.570)	4.489 (7.979)	11.621 (7.273)	15.053** (7.132)	29.762*** (10.384)	14.515 (12.588)
Observations	166,328	292,716	313,621	257,538	115,001	72,899
R-squared	0.506	0.443	0.441	0.460	0.492	0.519

## **Appendix**

### ***A.1 Calculation of Taxable Income***

Every year in Singapore, taxpayers are required to file taxes from March 1<sup>st</sup> to April 18. During this period, taxpayers verify the taxable income reported by their employers, reported additional income and claim for tax reliefs. (Thus, theoretically, taxpayers are cognizant of their tax liability for year t-1 in March of year t.) After that, the tax authority will process the Notice of Assessment (NOA) and send them out between May 1st and September 30. The first income tax payment is due within 30 days after the NOA letter date. However, some may start to pay their income tax in March every year after reporting their income.

Income tax can be paid manually or automatically through GIRO, an auto-direct debit mechanism authorized by the account holders to allow creditors (tax authorities in our context) to collect payment directly from their accounts. Those using GIRO can pay tax in full or by installment of up to 12 months only. For customers in our bank, the GIRO installment cycle starts from the month after the GIRO application is approved or May, whichever is the latest, and always ends in April of the following year. To spread the payment into 12-month installments, the GIRO application must be approved before May. The installment deduction will be made on the 6<sup>th</sup> of each month, or the next working day closest to 6<sup>th</sup> if it falls on a weekend or public holiday. The installment deduction will be reflected with a transaction code "ITX" in the bank transaction records.

Based on the above information, we can recover the taxable income for those paying taxes through GIRO. We first identify individuals who pay their tax in full as those having a unique tax payment record before November 6<sup>th</sup> (or the next working day) 2016 and 2017, respectively. Requiring them to have a unique tax transaction record is for excluding those who pay tax for others. We further verify that the tax payment is reasonable given the bank-recorded income. The number of individuals who pay their taxes in full is quite small.

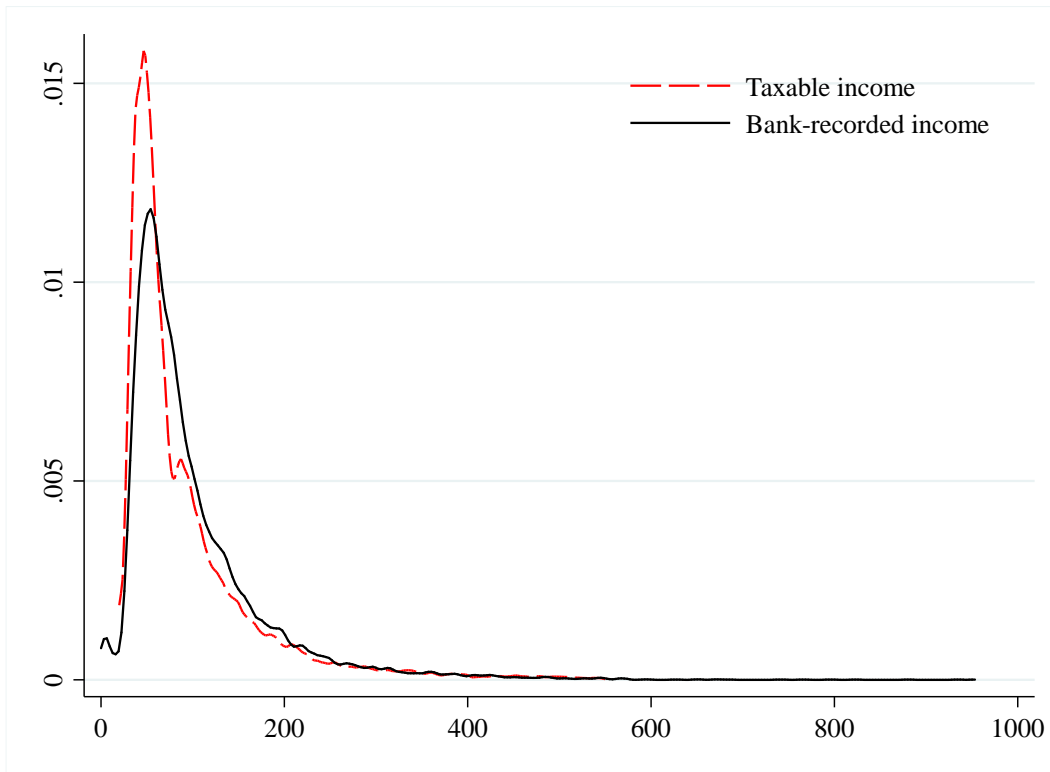
We then identify those who pay their tax in installments. Note installment payment for the 2015 tax is from 2016M5 to 2017M4. To calculate taxable income in 2015, we first identify those who have a unique tax

payment record on March 6<sup>th</sup> (or March 20<sup>th</sup> in the case of late payment due to insufficient funds) and April 6<sup>th</sup> (or April 20<sup>th</sup>) 2017. To ensure taxpayers do not switch between banks for the installment, we also require them to have a tax payment record on November 6<sup>th</sup> (or November 21) and December 6<sup>th</sup> (December 20) 2016. We then sum up the total tax payment from May 2016 to April 2017, based on which we uncover the taxable income in 2015.

We calculate the taxable income in 2016 in a similar manner. The installment payment for the 2015 tax is from 2017M5 to 2018M4. We proxy the tax payment in 2018 using that in 2017M12. By December 2017, taxpayers shall have updated their tax in 2016 (recall that taxpayers receive the NOA on September 30 the latest) and the installment shall have been updated accordingly. Therefore, the tax payment in the four months in 2018 shall be equal to that in 2017:12.

### Appendix Figure 1. Kernel Density Plots

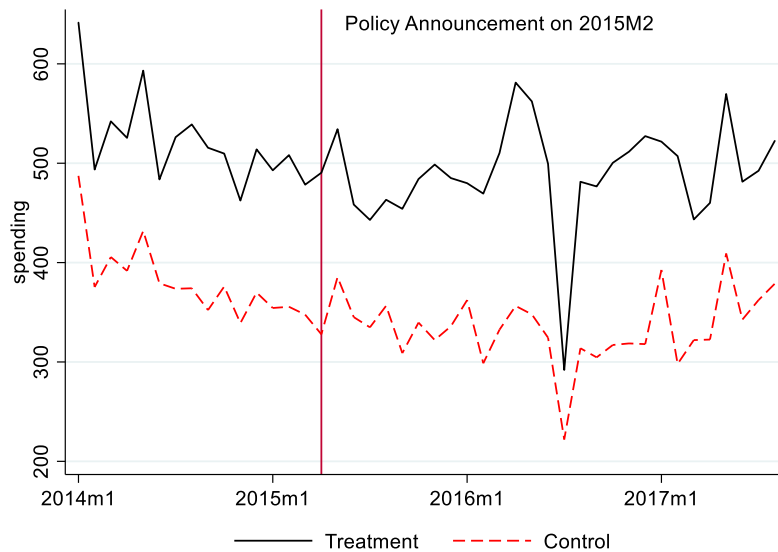
The kernel density of bank-recorded and taxable income in 2016 based on DBS bank data are plotted in dotted and solid lines, respectively. The x-axis is the annual income in thousand S\$, while the y-axis represents the estimated kernel density function for individuals in each income category.



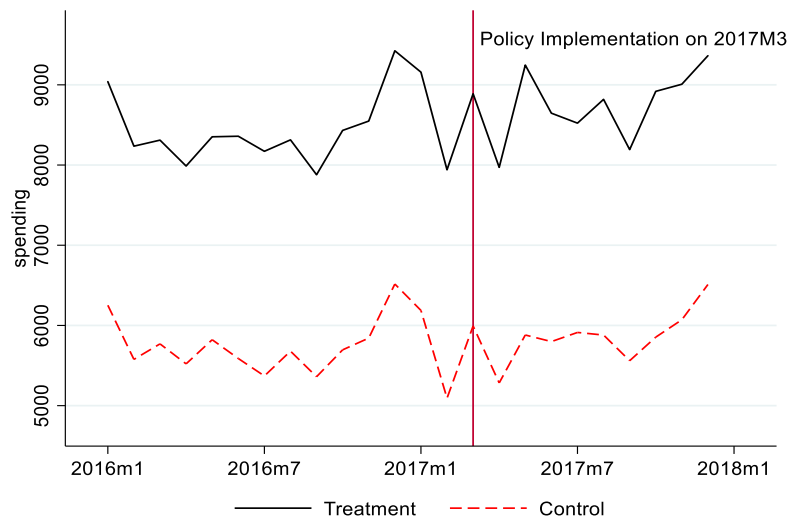
## Appendix Figure 2. Unconditional Spending Response: Full Sample

The chart plots the average monthly spending in the full sample for the treatment and control groups, respectively. The spending refers to credit card spending in panel A and total spending (sum of cash, bill and card spending) in panel B. The treatment group includes individuals whose annual income is more than S\$160K. The control group covers individuals whose annual income is more than 100K but not higher than S\$160K. The vertical line highlights the cut-off month between pre- and post-treatment period, which is in 2015M2 the policy announcement month for panel A and 2017M3 the policy implementation month for panel B.

Panel A: Credit Card Company Data (2014M1-2017M8)

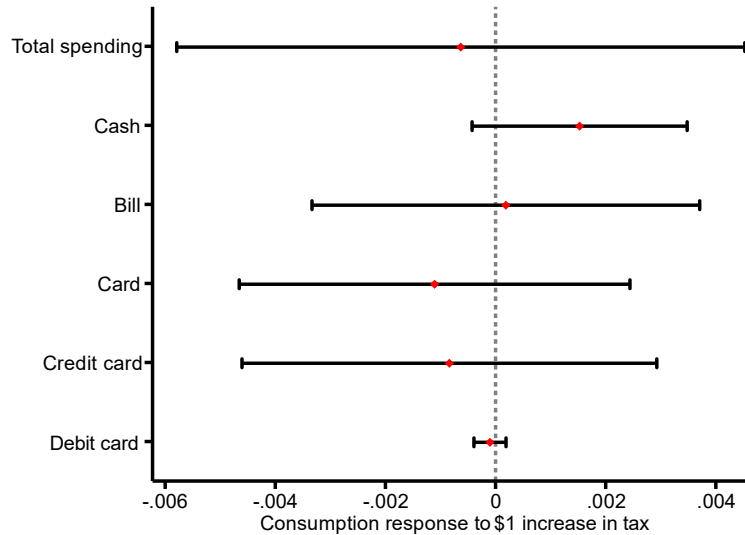


Panel B: DBS Bank Data (2016M1-2017M12)



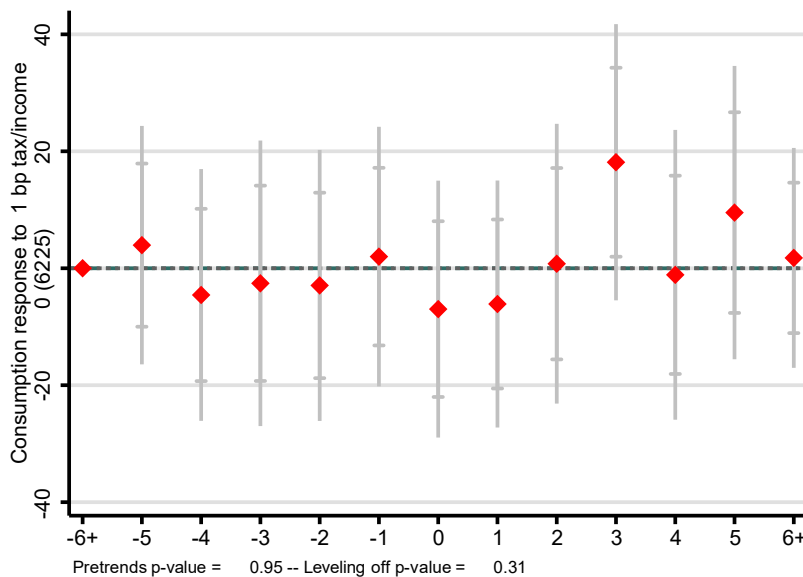
### Appendix Figure 3. Confidence intervals for consumption response

This figure plots in diamond the response of each consumption variable on the y-axis to \$1 increase in the income tax, and the corresponding confidence intervals in horizontal lines.



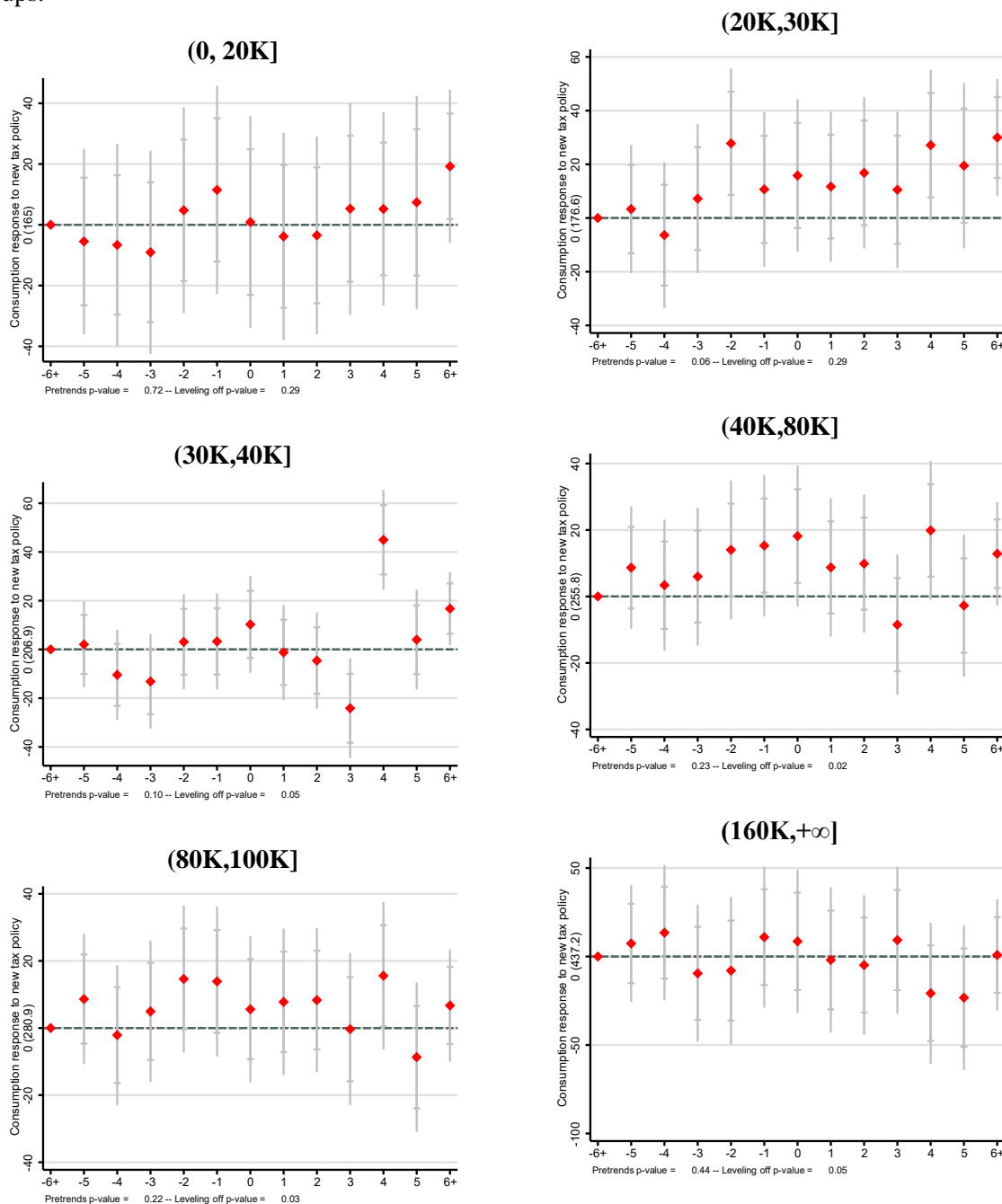
### Appendix Figure 4. Dynamic Spending Response to Normalized Extra Tax

This figure plots the dynamic spending response to extra tax normalized by income. The diamonds mark the estimated dynamic effects, the vertical lines crossing the diamonds represent the 95% confidence intervals, and the outer lines reflect the uniform sup-t confidence bands. The x-axis denotes the *s*th month before/after the implementation of the new tax policy in 2017M3. The y-axis represents the dynamic consumption response to every base point (bp) increase in income-normalized extra tax.



## Appendix Figure 5. Dynamic Consumption Response of Low-income Groups

This figure illustrates the dynamic response of total spending of low-income groups in various income brackets to the new tax policy five months around its announcement in 2015M3, based on the matched sample from the credit card company. The diamonds mark the estimated dynamic effects, the vertical lines crossing the diamonds represent the 95% confidence intervals, and the outer lines reflect the uniform sup-t confidence bands. The title of each panel highlights the income bracket of the low-income groups.



**Appendix Table 1. Marginal Tax Rates Before and After the Policy Shock.**

This table reports the marginal tax rate in percentage for each taxable income bracket specified in thousand Singapore dollars (S\$) for annual income earned before and after the calendar year of 2016. The rate change is the change in the marginal income tax rate for each income bracket. Extra tax in S\$ is the total additional tax caused by the policy change for those whose income falls in the corresponding bracket. It is calculated as the sum of the products of the tax rate change and the taxable income in exceedance of the lower band of its income bracket.

2013-2015		2016-			
Income Bracket (Thousand S\$)	Tax Rate (%)	Income Bracket (Thousand S\$)	Tax Rate (%)	Tax Rate Change (%)	Extra Tax (S\$)
[0,20]	0	[0,20]	0	0	
[20,30]	2	[20,30]	2	0	
[30,40]	3.5	[30,40]	3.5	0	
[40,80]	7	[40,80]	7	0	
[80,120]	11.5	[80,120]	11.5	0	
[120,160]	15	[120,160]	15	0	
[160,200]	17	[160,200]	18	1	[0, 400]
		[200,240]	19	1	[400,800]
[200,320]	18	[240,280]	19.5	1.5	[800,1400]
		[280,320]	20	2	[1400,2200]
[320,∞)	20	[320,∞)	22	2	[2200,+∞)

## Appendix Table 2. Tax payment

This table reports the difference in actual tax payment between the treatment and control group in 2016—after the implementation of the new tax policy, relative to that in 2015 before the new tax policy, based on DBS bank data in 2015 and 2016. The dependent variable is  $TaxPayment_{i,t}$ , the annual tax payment in Singapore dollar (S\$). The treatment dummy,  $Treat_i$ , equals 1 for individuals whose 2015 annual income is specified in the top row. The post-treatment dummy,  $D_{2016}$ , equals 1 in 2016 and 0 otherwise. Consumer and year fixed effects are included, and standard errors reported in the parenthesis are clustered by consumers. \*, \*\* and \*\*\* denote significant at 10%, 5% and 1% level respectively.

	Treated group income range				
	(160K,+∞) (1)	(160K,200K] (2)	(200K,240K] (3)	(240K,280K] (4)	(320K,∞) (5)
Treat×D <sub>2016</sub>	1,059.859*** (331.819)	346.361 (282.694)	598.303 (538.929)	1,689.344* (982.810)	2,198.332** (929.250)
Observations	9,578	6,766	6,056	5,550	6,236
R-squared	0.934	0.906	0.895	0.907	0.949

### Appendix Table 3. Propensity Score Matching.

This table reports the estimation results of propensity score matching logistic regressions. Columns 1-6 use the credit card company sample from 2014M1 to 2017M8, and Column 7 is based on DBS bank data from 2016M1 to 2017M12. The dependent variable is  $Treat_i$ , a dummy that equals 1 for a consumer whose income falls in the range specified in the second row of the table. The control group includes those whose annual income is more than S\$100K but less than S\$160K. Homeownership, Female, Married, Singapore citizen, Chinese, Malay, and Indian are all dummy variable that equals 1 for the specified group and 0 otherwise. Occupation and education fixed effects are included in all regressions. Standard errors reported in the parenthesis are clustered by consumers. \*, \*\* and \*\*\* denote significant at 10%, 5% and 1% level respectively.

	Credit Card Company Data						DBS bank Data
	(1) (160K,+∞)	(2) (80,100K]	(3) (40,80K]	(4) (30,40K]	(5) (20,30K]	(6) (0,20K]	(7) (160K,+∞)
log(age)	2.024*** (0.175)	-1.047*** (0.129)	-1.814*** (0.101)	-3.126*** (0.121)	-1.461*** (0.157)	-2.950*** (0.199)	1.557*** (0.079)
Female	-0.015 (0.073)	0.058 (0.055)	0.250*** (0.043)	0.487*** (0.051)	0.663*** (0.069)	1.129*** (0.082)	-0.168*** (0.031)
Married	0.138 (0.088)	0.077 (0.064)	-0.210*** (0.052)	-0.418*** (0.058)	-0.612*** (0.078)	-0.489*** (0.094)	0.261*** (0.030)
Singapore citizen	0.011 (0.119)	0.136 (0.087)	0.886*** (0.073)	0.896*** (0.090)	1.381*** (0.171)	1.664*** (0.226)	-0.285*** (0.040)
Home ownership	-0.138** (0.059)	0.047 (0.046)	0.107*** (0.037)	0.031 (0.044)	-0.539*** (0.063)	-0.824*** (0.080)	0.042 (0.029)
Chinese	-0.205* (0.123)	0.124 (0.106)	-0.198** (0.086)	-0.230** (0.099)	-0.405*** (0.142)	0.306 (0.203)	-0.356*** (0.051)
Malay	-0.734*** (0.238)	0.317* (0.164)	0.344*** (0.134)	0.680*** (0.145)	0.911*** (0.189)	2.107*** (0.245)	-1.222*** (0.133)
Indian	-0.193 (0.132)	0.035 (0.112)	-0.266*** (0.090)	-0.550*** (0.105)	-0.713*** (0.156)	-0.269 (0.223)	-0.392*** (0.059)
Constant	-9.761*** (0.863)	3.864*** (0.585)	9.056*** (0.450)	13.351*** (0.525)	5.556*** (0.673)	7.888*** (0.887)	9.160 (683.061)
Occupation FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Education FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	6,601	9,274	32,111	19,814	8,653	12,950	23,465

**Appendix Table 4. Summary Statistics: Demographic Characteristics**

This table reports the summary statistics for the demographic characteristics for the treatment and control group. Mean and SD refer to the mean and standard error. Difference is the average difference between the treatment and control groups and the p-value is the significance level that the difference is not 0. In panel A, the treatment group includes individuals whose income in 2014 is in the range of (160K,+∞), (80K,100K], (40K,80K], (30K,40K], (20K,30K] or (0,20K] respectively. The control group is always individuals whose income in 2014 falls in (100K,160K]. In Panel B, the treatment group includes individuals whose income in 2015 is more than S\$160K and the control group includes individuals whose income in 2015 falls in the range of (100K,160K]. Homeownership, Female, Married, Singapore citizen, Chinese, Malay, and Indian are all dummy variable that equals 1 for the specified group and 0 otherwise.

Panel A: Credit card company data

	Full Sample						Matched Sample					
	Treatment: Income∈(160K,+ ∞)		Control		Difference	p- value	Treatment: Income∈(160K,+ ∞)		Control		Difference	p- value
	Mean	SD	Mean	SD			Mean	SD	Mean	SD		
Age	52.44	8.34	49.12	9.10	3.33	0.00	52.24	8.17	52.36	8.75	-0.13	0.63
Annual Income (K)	241.75	86.73	124.1 4	17.5 5	117.61	0.00	241.59	86.78	125.3 9	17.8 6	116.20	0.00
Chinese Ethnicity	0.76	0.43	0.72	0.45	0.04	0.00	0.76	0.42	0.77	0.42	0.00	0.89
Indian Ethnicity	0.15	0.35	0.20	0.40	-0.05	0.00	0.15	0.35	0.15	0.36	0.00	0.83
Malay Ethnicity	0.02	0.12	0.03	0.16	-0.01	0.01	0.02	0.12	0.01	0.11	0.00	0.29
Female	0.20	0.40	0.23	0.42	-0.03	0.01	0.20	0.40	0.21	0.40	-0.01	0.65
Married	0.89	0.31	0.86	0.35	0.03	0.00	0.89	0.31	0.90	0.30	0.00	0.62
Home ownership	0.58	0.49	0.60	0.49	-0.02	0.18	0.58	0.49	0.58	0.49	0.00	1.00
Singapore PR	0.93	0.25	0.93	0.26	0.00	0.54	0.93	0.25	0.94	0.24	-0.01	0.38
Singapore citizen	0.92	0.27	0.91	0.29	0.01	0.05	0.92	0.27	0.93	0.26	0.00	0.56
Observations	2205		4451				2165		2165			

	Treatment:				Difference	p-value	Treatment:				Difference	P-value
	Income€(80,100 K]		Control				Income€(80,100 K]		Control			
	Mean	SD	Mean	SD			Mean	SD	Mean	SD		
Age	47.33	9.20	49.12	9.10	-1.78	0.00	48.82	9.04	48.52	8.95	0.30	0.14
Annual Income	89.18	6.89	124.14	17.55	-34.96	0.00	89.47	7.02	123.63	17.65	-34.16	0.00
Chinese Ethnicity	0.70	0.46	0.72	0.45	-0.03	0.00	0.73	0.44	0.73	0.44	0.00	0.94
Indian Ethnicity	0.22	0.42	0.20	0.40	0.03	0.00	0.20	0.40	0.20	0.40	0.00	0.89
Malay Ethnicity	0.03	0.18	0.03	0.16	0.01	0.01	0.02	0.16	0.03	0.16	0.00	0.72
Female	0.26	0.44	0.23	0.42	0.03	0.00	0.25	0.43	0.25	0.43	0.00	0.75
Married	0.86	0.35	0.86	0.35	0.00	0.93	0.86	0.34	0.86	0.34	0.00	0.87
Homeownership	0.60	0.49	0.60	0.49	0.01	0.49	0.61	0.49	0.60	0.49	0.01	0.31
Singapore PR	0.94	0.24	0.93	0.26	0.01	0.09	0.95	0.23	0.93	0.25	0.01	0.01
Singapore citizen	0.90	0.30	0.91	0.29	-0.01	0.24	0.91	0.28	0.91	0.28	0.00	0.78
Observations	4873		4451				3883		3883			

	Treatment:				Difference	p-value	Treatment:				Difference	P-value
	Income€(40,80K]		Control				Income€(40,80K]		Control			
	Mean	SD	Mean	SD			Mean	SD	Mean	SD		
Age	45.91	9.31	49.12	9.10	-3.21	0.00	52.55	9.95	49.02	9.19	3.53	0.00
Annual Income	56.53	11.52	124.14	17.55	-67.61	0.00	60.25	11.55	123.84	17.63	-63.58	0.00
Chinese Ethnicity	0.69	0.46	0.72	0.45	-0.04	0.00	0.76	0.43	0.73	0.44	0.03	0.00
Indian Ethnicity	0.20	0.40	0.20	0.40	0.01	0.29	0.19	0.39	0.20	0.40	-0.01	0.47

Malay Ethnicity	0.06	0.23	0.03	0.16	0.03	0.00	0.01	0.11	0.03	0.16	-0.01	0.00
Female	0.33	0.47	0.23	0.42	0.10	0.00	0.18	0.39	0.24	0.43	-0.06	0.00
Married	0.82	0.38	0.86	0.35	-0.04	0.00	0.89	0.32	0.85	0.35	0.03	0.00
Homeownershi p	0.61	0.49	0.60	0.49	0.01	0.11	0.61	0.49	0.60	0.49	0.00	0.72
Singapore PR	0.98	0.14	0.93	0.26	0.05	0.00	0.96	0.19	0.94	0.24	0.03	0.00
Singapore citizen	0.95	0.23	0.91	0.29	0.04	0.00	0.91	0.29	0.91	0.28	0.00	0.51
Observations	28014		4451				4123		4123			

	Treatment: Income€(30,40K]		Control		Difference	p-value	Treatment: Income€(30,40K]		Control		Difference	P- value
	Mean	SD	Mean	SD			Mean	SD	Mean	SD		
Age	44.35	9.30	49.12	9.10	-4.77	0.00	48.70	9.79	48.28	8.82	0.42	0.06
Annual Income	35.21	3.29	124.1 4	17.5 5	-88.94	0.00	35.28	3.04	123.7 5	17.5 2	-88.47	0.00
Chinese Ethnicity	0.69	0.46	0.72	0.45	-0.03	0.00	0.76	0.43	0.75	0.43	0.01	0.59
Indian Ethnicity	0.15	0.35	0.20	0.40	-0.05	0.00	0.18	0.38	0.17	0.38	0.01	0.28
Malay Ethnicity	0.11	0.31	0.03	0.16	0.08	0.00	0.02	0.13	0.03	0.17	-0.01	0.00
Female	0.41	0.49	0.23	0.42	0.18	0.00	0.24	0.43	0.27	0.44	-0.03	0.01
Married	0.75	0.43	0.86	0.35	-0.11	0.00	0.84	0.37	0.83	0.37	0.00	0.64
Homeownershi p	0.57	0.49	0.60	0.49	-0.02	0.01	0.62	0.48	0.62	0.49	0.01	0.47
Singapore PR	0.98	0.15	0.93	0.26	0.05	0.00	0.97	0.18	0.96	0.20	0.01	0.06
Singapore citizen	0.95	0.21	0.91	0.29	0.05	0.00	0.93	0.26	0.94	0.24	-0.01	0.09
Observations	15777		4451				3371		3371			

	Treatment: Income€(20,30K]		Control		Difference	p-value	Treatment: Income€(20,30K]		Control		Difference	P- value
	Mean	SD	Mean	SD			Mean	SD	Mean	SD		

	Mean	SD	Mean	SD			Mean	SD	Mean	SD		
Age	44.83	11.73	49.12	9.10	-4.29	0.00	49.67	11.42	49.16	9.14	0.51	0.18
Annual Income	26.07	3.80	124.14	17.55	-98.07	0.00	27.50	3.39	123.20	17.68	-95.70	0.00
Chinese Ethnicity	0.62	0.48	0.72	0.45	-0.10	0.00	0.77	0.42	0.79	0.41	-0.02	0.25
Indian Ethnicity	0.11	0.31	0.20	0.40	-0.09	0.00	0.14	0.35	0.12	0.32	0.02	0.10
Malay Ethnicity	0.21	0.41	0.03	0.16	0.19	0.00	0.03	0.17	0.05	0.21	-0.02	0.00
Female	0.51	0.50	0.23	0.42	0.28	0.00	0.34	0.47	0.36	0.48	-0.02	0.17
Married	0.68	0.47	0.86	0.35	-0.18	0.00	0.79	0.41	0.79	0.41	0.00	0.82
Homeownership	0.49	0.50	0.60	0.49	-0.11	0.00	0.55	0.50	0.56	0.50	-0.01	0.49
Singapore PR	0.99	0.11	0.93	0.26	0.06	0.00	0.97	0.16	0.98	0.15	-0.01	0.35
Singapore citizen	0.98	0.14	0.91	0.29	0.07	0.00	0.96	0.20	0.97	0.18	-0.01	0.29
Observations	4674		4451				1522		1522			

	Treatment: Income€(0,20K]		Control		Difference	p-value	Treatment: Income€(0,20K]		Control		Difference	P-value
	Mean	SD	Mean	SD			Mean	SD	Mean	SD		
Age	44.19	11.91	49.12	9.10	-4.93	0.00	48.69	11.25	48.50	9.12	0.18	0.70
Annual Income	14.34	4.76	124.14	17.55	-109.81	0.00	15.72	5.30	123.20	18.76	-107.47	0.00
Chinese Ethnicity	0.65	0.48	0.72	0.45	-0.07	0.00	0.77	0.42	0.80	0.40	-0.03	0.17
Indian Ethnicity	0.09	0.29	0.20	0.40	-0.10	0.00	0.13	0.34	0.10	0.30	0.03	0.03
Malay Ethnicity	0.22	0.41	0.03	0.16	0.19	0.00	0.05	0.22	0.05	0.23	-0.01	0.61
Female	0.71	0.46	0.23	0.42	0.48	0.00	0.47	0.50	0.44	0.50	0.03	0.24
Married	0.72	0.45	0.86	0.35	-0.14	0.00	0.78	0.42	0.78	0.41	-0.01	0.74

Homeownershi p	0.48	0.50	0.60	0.49	-0.11	0.00	0.54	0.50	0.52	0.50	0.02	0.36
Singapore PR	0.98	0.14	0.93	0.26	0.05	0.00	0.97	0.16	0.98	0.15	-0.01	0.46
Singapore citizen	0.97	0.17	0.91	0.29	0.07	0.00	0.97	0.17	0.97	0.17	0.00	1.00
Observations	9743		4451				966		966			

Panel B: DBS bank data

	Full Sample						Matched Sample					
	Treatment: Income $\in$ (160K,+ $\infty$ )		Control		Difference	p- value	Treatment: Income $\in$ (160K,+ $\infty$ )		Control		Difference	p- value
	Mean	SD	Mean	SD			Mean	SD	Mean	SD		
Age	49.70	9.13	47.21	9.51	2.50	0.00	49.25	8.88	48.67	9.35	0.58	0.00
Annual Income	298.17	389.35	127.2 9	61.2 3	170.88	0.00	295.68	390.30	127.8 9	65.7 1	167.78	0.00
Chinese Ethnicity	0.76	0.43	0.79	0.41	-0.03	0.00	0.78	0.41	0.78	0.41	0.00	0.76
Indian Ethnicity	0.10	0.30	0.10	0.30	0.00	0.94	0.10	0.30	0.10	0.30	0.00	0.55
Malay Ethnicity	0.01	0.09	0.02	0.15	-0.01	0.00	0.01	0.09	0.01	0.10	0.00	0.61
Female	0.26	0.44	0.32	0.47	-0.06	0.00	0.27	0.45	0.30	0.46	-0.02	0.00
Married	0.73	0.45	0.64	0.48	0.09	0.00	0.72	0.45	0.70	0.46	0.01	0.02
Homeownershi p	0.39	0.49	0.37	0.48	0.02	0.00	0.38	0.49	0.39	0.49	0.00	0.88
Singapore PR	0.89	0.32	0.92	0.28	-0.03	0.00	0.90	0.31	0.91	0.29	-0.01	0.00
Singapore citizen	0.72	0.45	0.77	0.42	-0.05	0.00	0.74	0.44	0.75	0.44	-0.01	0.34
Observations	10882		12585				10431		10431			

**Appendix Table 5. Transition Probability**

This table reports the probability of transiting between treated (annual income exceeds S\$160K) and non-treated (annual income not more than S\$160K) groups based on income data recorded by DBS bank. The diagonal cells report the probability of remaining treated or non-treated while the off-diagonal cells report the probability of crossing over the income threshold (S\$160K) over time. Panel A shows the transition probability from 2015, before the new tax policy becomes effective, to 2016, when the new tax policy applies. Panel B shows transition probability from 2016 to 2017, when the new tax policy applies.

**Panel A: Transition probability from 2015 to 2016.**

		Original Sample			Matched Sample		
		<u>2016 Income</u>			<u>2016 Income</u>		
		Treatment	Control	No of individuals	Treatment	Control	No of individuals
<i>2015</i> <i>Income</i>	Treatment	0.946	0.054	10,882	0.944	0.056	10,431
	Control	0.042	0.958	12,585	0.043	0.957	10,431
	No of individuals	10822	12645	23,467	10295	10567	20,862

**Panel B: Transition probability from 2016 to 2017**

		Original Sample			Matched Sample		
		<u>2017 Income</u>			<u>2017 Income</u>		
		Treatment	Control	No of individuals	Treatment	Control	No of individuals
<i>2016</i> <i>Income</i>	Treatment	0.939	0.061	10,822	0.938	0.062	10,295
	Control	0.070	0.930	12,645	0.071	0.929	10,567
	No of individual	11049	12418	23,467	10406	10456	20,862

**Appendix Table 6. Summary Statistics: Spending, Debt, and Saving**

This table reports the summary statistics of the outcome variables for the treatment and control group in the original and matched sample, respectively. Mean and SD refer to the mean and standard error. Difference is the average difference between the treatment and control groups. In panel A (B), *Before* refers to the subsample before the announcement (implementation) of the new tax policy from 2014M1 to 2015M1 (2016M1 to 2017M2), *After* refers to the subsample from 2015M2 to 2017M8 (2017M3 to 2017M12), and DID is the difference in the credit card spending (total spending) difference between treatment and control group before and after the announcement (implementation) of the new tax policy. In panel A, the treatment group includes individuals whose income in 2014 is in the range of (160K,+∞), (80K,100K], (40K,80K], (30K,40K], (20K,30K] or (0,20K] respectively. The control group is always individuals whose income in 2014 falls in (100K,160K]. In Panel B, the treatment group includes individuals whose income in 2015 is more than S\$160K and the control group includes individuals whose income in 2015 is in the range of (100K,160K]. Saving balance is measured in thousand.

Panel A: Summary statistics on credit card spending based on credit card company data

Treatment Income Bracket	Full Sample						Matched Sample					
	Treatment		Control		Difference	p-value	Treatment		Control		Difference	p-value
	Mean	SD	Mean	SD			Mean	SD	Mean	SD		
(160K,+∞)												
Before	525.65	6.32	384.96	4.45	140.69	0.00	525.97	6.69	380.53	6.69	145.44	0.00
After	492.21	4.44	337.13	3.18	155.08	0.00	487.59	4.70	340.18	4.75	147.41	0.00
DID					14.38	0.13					1.97	0.87
(80K,100K]												
Before	322.75	3.12	384.96	3.26	-62.21	0.00	328.40	3.75	387.36	3.75	-58.96	0.00
After	269.26	2.24	337.13	2.32	-67.87	0.00	275.76	2.69	339.87	2.68	-64.11	0.00
DID					-5.66	0.31					-5.15	0.43
(40K,80K]												
Before	263.49	0.93	384.96	2.33	-121.47	0.00	281.12	4.61	400.33	4.61	-119.21	0.00
After	227.45	0.67	337.13	1.67	-109.68	0.00	238.18	3.25	347.41	3.29	-109.22	0.00
DID					11.79	0.00					9.99	0.21
(30K,40K]												
Before	207.36	1.18	384.96	2.22	-177.60	0.00	224.46	3.91	394.08	3.91	-169.62	0.00
After	180.29	0.85	337.13	1.58	-156.84	0.00	193.35	2.74	338.15	2.78	-144.80	0.00
DID					20.75	0.00					24.82	0.00

<hr/>												
(20K,30K]												
Before	152.91	2.73	384.96	2.79	-232.05	0.00	192.93	6.17	396.62	6.16	-203.70	0.00
After	139.66	1.98	337.13	1.99	-197.47	0.00	174.66	4.40	352.91	4.38	-178.25	0.00
DID					34.58	0.00					25.45	0.02
<hr/>												
(0,20K]												
Before	158.56	1.59	384.96	2.34	-226.40	0.00	186.98	7.86	392.72	7.86	-205.74	0.00
After	142.96	1.14	337.13	1.67	-194.17	0.00	161.11	5.63	357.30	5.61	-196.19	0.00
DID					32.22	0.00					9.55	0.48

Panel B: Summary statistics on total spending, debt and total saving based on DBS bank data

Treatment Income Bracket (160K, +∞)	Treatment		Full Sample Control		Difference	p-value	Treatment		Matched Sample Control		Difference	p-value
	Mean	SD	Mean	SD			Mean	SD	Mean	SD		
	Total Spending											
Before	8439.56	37.85	5733.86	35.19	2705.70	0.00	8505.77	39.86	5730.93	39.86	2774.84	0.00
After	8757.67	44.78	5874.59	41.64	2883.08	0.00	8820.69	47.16	5844.69	47.16	2976.00	0.00
DID					177.38	0.03					201.16	0.02
Debt												
Before	14.18	11.34	7.20	10.55	6.98	0.63	14.07	11.96	5.43	11.96	8.64	0.58
After	47.66	12.93	17.65	12.02	30.01	0.12	46.71	13.64	18.36	13.64	28.35	0.18
DID					23.03	0.33					19.71	0.44
Saving												
Before	288	1	126	1	162	0.000	288	1	131	1	158	0.000
After	304	2	134	2	171	0.000	306	2	139	2	167	0.000
DID					9	0.004					9	0.004

**Appendix Table 7. Robustness Checks**

This table reports various robustness checks on the average spending response to every dollar of extra tax based on the bank-provided sample using the following model:

$$Y_{it} = a \times ExtraTax_i \times D_{preImp} + \beta \times ExtraTax_i \times D_{Imp} + c_i + m_t + \varepsilon_{it}.$$

The dependent variable  $Y_{it}$  is the dollar amount of total spending.  $ExtraTax_i$  is extra tax in dollar for the treated individuals, whose annual income exceeds S\$160K, after the implementation of the new tax policy.  $D_{preImp}$  is a dummy that equals 1 for the pre-treatment period from 2016M8 to 2017M2, which is after the announcement but before the implementation of the new tax policy.  $D_{Imp}$  is a dummy that equals 1 for the treatment period from 2017M3 to 2017M12, which is after the implementation of the new tax policy. Columns 1-8 repeat the analysis using (1) original sample instead of the matched sample; (2) matched sample based on the taxable income instead of bank reported income; (3) all individuals whose income is below 160K as the control group; (4) only treatment group in the sample; (5) sample that includes the outliers without winsorization; (6) total spending normalized by income; (7) the logarithmic transformation of total spending; (8) Poisson pseudo maximum likelihood (PPML) estimation technique; and (9) alternative benchmark period from 2016M1-2016M2, respectively. Observations from 2016M1 to 2016M7 are absorbed to identify the benchmark spending pattern. Consumer and year-month fixed effects are included. Standard errors clustered by consumers are in the parentheses. \*, \*\* and \*\*\* denote significant at 10%, 5% and 1% level respectively.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	Original Sample	Taxable Income	Expanded Control Group	Treatment Only	No Winsorization	Normalized Spending	Log transform	PPML	Alternative Benchmark
ExtraTax×D <sub>preImp</sub>	0.002 (0.002)	0.020 (0.014)	0.001 (0.002)	0.001 (0.002)	-0.004 (0.015)	-0.000 (0.000)	-0.000 (0.000)	0.000 (0.000)	-0.003 (0.002)
ExtraTax×D <sub>Imp</sub>	-0.000 (0.003)	-0.003 (0.016)	-0.001 (0.002)	-0.001 (0.002)	0.018 (0.024)	0.000 (0.000)	-0.000 (0.000)	-0.000 (0.000)	-0.004 (0.003)
Observations	541,822	105,313	294,108	237,442	500,684	481,710	473,153	481,713	481,713
R-squared	0.614	0.541	0.643	0.650	0.452	0.269	0.681		0.619

### Appendix Table 8. Alternative samples

This table reports the average spending response to every dollar of extra tax based on the bank-provided sample using alternative samples:

$$Y_{it} = a \times ExtraTax_i \times D_{preImp} + \beta \times ExtraTax_i \times D_{Imp} + c_i + m_t + \varepsilon_{it}.$$

The dependent variable  $Y_{it}$  is the dollar amount of total spending.  $ExtraTax_i$  is extra tax in Singapore dollar for the treated individuals, whose annual income exceeds S\$160K, after the implementation of the tax increase.  $D_{preImp}$  is a dummy that equals to 1 for the pre-treatment period from 2016M8 to 2017M2, which is after the announcement and before the implementation of the new tax policy.  $D_{Imp}$  is a dummy that equals to 1 for the treatment period from 2017M3 to 2017M12, which is after the implementation of the new tax policy. Observations from 2016M1 to 2016M7 are absorbed to identify the benchmark spending pattern. Columns 1-4 use the subsamples that, respectively, exclude those earning between (155K,165K), (150K,170K), (140K, 180K), and (130K, 190K). Consumer and year-month fixed effects are included. Standard errors clustered by consumers are in the parentheses. \*, \*\* and \*\*\* denote significant at 10%, 5% and 1% level respectively.

	(1) (155K,165K)	(2) (150K,170K)	(3) (140K, 180K)	(4) (130K, 190K)
ExtraTax×D <sub>preImp</sub>	0.000 (0.003)	0.000 (0.003)	-0.001 (0.003)	-0.000 (0.003)
ExtraTax×D <sub>Imp</sub>	0.002 (0.005)	0.002 (0.005)	0.002 (0.005)	0.003 (0.005)
Observations	468,207	440,979	387,639	332,139
R-squared	0.572	0.573	0.578	0.585

**Appendix Table 9. Heterogeneous spending response by spending categories**

This table reports the response of spending in various categories to every dollar of extra tax using the matched sample from the bank based on the following model:

$$Y_{it} = a \times ExtraTax_i \times D_{preImp} + \beta \times ExtraTax_i \times D_{Imp} + c_i + m_t + \varepsilon_{it}.$$

The dependent variable  $Y_{it}$  is the dollar amount of card spending in the category specified in the second row.  $ExtraTax_i$  is the extra tax in dollar for the treated individuals, whose annual income exceeds S\$160K, after the implementation of the new tax policy.  $D_{preImp}$  is a dummy that equals 1 for the pre-treatment period from 2016M8 to 2017M2, which is after the announcement but before the implementation of the new tax policy.  $D_{Imp}$  is a dummy that equals 1 for the treatment period from 2017M3 to 2017M12, which is after the implementation of the new tax policy. Observations from 2016M1 to 2016M7 are absorbed to identify the benchmark spending pattern. Consumer and year-month fixed effects are included. Standard errors clustered by consumers are in parentheses; \*, \*\* and \*\*\* denote significant at 10%, 5% and 1% level respectively.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
	transportation	supermarket	entertainment	apparel	dining	travel	durable	service	others	online
ExtraTax×D <sub>preImp</sub>	0.000	0.000	0.001**	-0.001	-0.000	-0.000	0.001*	0.001	0.001	0.000
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.001)	(0.001)	(0.001)	(0.001)	(0.000)
ExtraTax×D <sub>Imp</sub>	0.000	0.000	0.001**	0.001	-0.000	-0.000	-0.001	0.003	0.001	-0.000
	(0.000)	(0.000)	(0.000)	(0.001)	(0.001)	(0.001)	(0.000)	(0.003)	(0.001)	(0.000)
Observations	458,226	458,226	458,226	458,226	458,226	458,226	458,226	458,226	458,226	458,226
R-squared	0.748	0.421	0.245	0.299	0.288	0.316	0.119	0.266	0.119	0.248

	(1)	(2)	(3)	(4)	(5)	(6)
	Charles, Hurst and Roussanov (2009)		Heffetz (2011)		Discretionary	
	visible	non-visible	visible	non-visible	Strict	Loose
ExtraTax×D <sub>preImp</sub>	0.000	0.003	0.001	0.003	0.000	0.001
	(0.001)	(0.003)	(0.001)	(0.003)	(0.000)	(0.001)
ExtraTax×D <sub>Imp</sub>	0.001	0.003	0.000	0.003	0.000	0.001
	(0.002)	(0.003)	(0.002)	(0.003)	(0.000)	(0.001)
Observations	458,226	458,226	458,226	458,226	458,226	458,226
R-squared	0.254	0.314	0.345	0.259	0.421	0.285

**Appendix Table 10. Average Consumption Response to Tax-Income Ratio**

This table reports the response of total spending to every basis point (bp) increase in income-normalized extra tax using the matched sample from DBS bank's data. The dependent variable is the dollar amount of total spending.  $ExtraTaxIncomeRatio_i$  is the extra tax normalized by annual income in basis point for the treated individuals, whose annual income exceeds S\$160K.  $ExtraTaxIncomeRatio_i$  is further decomposed into five quintiles denoted as  $ExtraTaxIncomeRatio_{x\ quintile}$ .  $D_{preImp}$  is a dummy that equals 1 for the pre-treatment period from 2016M8 to 2017M2, which is after the announcement but before the implementation of the new tax policy.  $D_{Imp}$  is a dummy that equals 1 for the treatment period from 2017M3 to 2017M12, which is after the implementation of the new tax policy. Observations from 2016M1 to 2016M7 are absorbed to identify the benchmark spending pattern. Consumer and year-month fixed effects are included, and standard errors reported in the parenthesis are clustered by consumers. \*, \*\*, and \*\*\* denote significant at 10%, 5% and 1% level respectively.

	(1) Total spending	(2) Total spending
$ExtraTaxIncomeRatio_i \times D_{preImp}$	5.213 (4.872)	5.210 (4.872)
$ExtraTaxIncomeRatio_i \times D_{Imp}$	1.790 (6.023)	
$ExtraTaxIncomeRatio_{First\ quintile} \times D_{Imp}$		-14.304 (70.722)
$ExtraTaxIncomeRatio_{Second\ quintile} \times D_{Imp}$		-3.430 (30.132)
$ExtraTaxIncomeRatio_{Third\ quintile} \times D_{Imp}$		-9.930 (18.084)
$ExtraTaxIncomeRatio_{Fourth\ quintile} \times D_{Imp}$		6.909 (11.340)
$ExtraTaxIncomeRatio_{Fifth\ quintile} \times D_{Imp}$		1.035 (6.433)
Observations	481,713	481,713
R-squared	0.619	0.619