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### Income and subjective well-being: Evidence from Singapore's first national non-contributory pension

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#### Citation

CHEN, Yanying and TAN, Yi Jin. Income and subjective well-being: Evidence from Singapore's first national non-contributory pension. (2018). 1-48.

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**Income and subjective well-being:**  
**Evidence from Singapore's first national non-contributory pension\***

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First Draft: 30 June 2017

This Draft: 10 May 2018

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\* We would like to thank our advisor, Jing Li, for her advice and guidance; Luca Facchinello, Tomoki Fujii, Christine Ho, Seonghoon Kim, Olivia Mitchell, as well as participants at the Singapore Economic Review Conference for helpful comments. In addition, we are grateful to the Centre for Research on the Economics of Aging at the Singapore Management University for financial support, and for providing access to data from the Singapore Life Panel. The Singapore Life Panel is funded by the Singapore Ministry of Education (MOE) Academic Research Fund Tier 3 grant MOE2013-T3-1-009.

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## Abstract

We use a new high-frequency (monthly) longitudinal survey and a difference-in-differences strategy to separately estimate the announcement and disbursement effects of an exogenous permanent income shock on subjective well-being. This permanent income shock is the introduction of Singapore's first national non-contributory pension, the Silver Support Scheme. The pension improved the life satisfaction of recipients; this effect appears to be driven by social, household income, and economic satisfaction. Consistent with the predictions of the permanent income hypothesis, well-being improved at announcement, but did not improve significantly further at disbursement (i.e. the difference between announcement and disbursement effects is not statistically different from zero). In addition, we find evidence that the marginal utility of income varies – recipients who reported being less financially prepared for retirement exhibited larger increases in well-being. Surprisingly, we find little evidence of such heterogeneity by individuals' net assets. Lastly, well-being did not improve if an individual's spouse received SSS payouts but he/she did not. Our results suggest that future policies could consider heterogeneity among individuals for greater welfare gains.

JEL classification: H3, I1, I3

Keywords: subjective well-being, life satisfaction, health, income, non-contributory pension

## 1 Introduction

Since Easterlin (1974)'s work on income and happiness, interest in the empirical link between income and subjective well-being has grown substantially amongst economists. In recent years, more convincing evidence on the causal impact of income on happiness (e.g. Frijters et al., 2004; Gardner & Oswald, 2007) has emerged. In addition, researchers are starting to go beyond average effects to look at heterogeneity in the effect of income on happiness arising from differences in individual characteristics (e.g. personality (Boyce & Wood, 2011) and health (Finkelstein et al., 2013)). Two recent papers also show that – consistent with the permanent income hypothesis – permanent income shocks lead to larger subjective well-being responses than transitory shocks (Bayer & Juessen, 2015; Cai & Park, 2016).

We add to this literature by using a new, high-frequency (monthly) panel and a natural experiment in Singapore to separately estimate the announcement and disbursement effects of an exogenous permanent income increase on life satisfaction and its sub-domains. In addition, we examine how these effects may vary by self-assessed financial preparation for retirement versus net assets, and investigate whether an individual's subjective well-being improves with increases in his or her spouse's permanent income.

The permanent income increase we study comes in the form of Singapore's first national non-contributory pension scheme (the Silver Support Scheme, or SSS). The SSS targets the neediest 20 – 30% of Singaporean citizens aged 65 and above. Details of the SSS (e.g. exact qualifying criteria and payout quantum) were announced in end-March 2016, followed by the disbursement of its first cash payout in end-July 2016. Eligibility for payouts in the period we study is pre-determined, as it is based on the government's administrative data from 2015. Eligibility is automatically assessed by the government, and payouts are made automatically to all who are eligible via well-established channels.

The design of the SSS allows us to identify its causal impact via a difference-in-differences (DiD) strategy which includes individual<sup>1</sup> and time fixed effects. Our sample consists of those who are age-eligible (i.e. aged 65 and above in 2016), and the treated group is defined as those who received SSS while controls are those who did not. To address concerns that baseline differences in the characteristics of the treatment and control groups could invalidate the underlying DiD assumption of parallel trends, we construct more similar treatment and control groups by trimming our sample based on their propensity to receive SSS. In addition, we carry out a battery of robustness checks (described more fully in Section 7), which include checks that address potential violations of the parallel trends assumption in different ways. These robustness checks include the addition of different group-specific time fixed effects and the use of different reweighting schemes to better match the treatment and control groups (e.g. using Abadie (2005)'s semi-parametric DiD on the full sample). Our results are robust to these checks.

Our data comes from a new population-representative, high-frequency longitudinal survey of elderly Singaporeans – the Singapore Life Panel, or SLP<sup>2</sup>. The SLP is run by the Centre for Research on the Economics of Ageing (CREA) at the Singapore Management University, and was designed in collaboration with the creators of the US Health and Retirement Study (HRS) and a number of its sister surveys, such as the American Life Panel. 15,000 Singapore citizens and permanent residents aged between 50 and 70, and their spouses, were initially recruited for a baseline survey in May – July 2015. Follow-up surveys were sent monthly, and attrition has been low. The monthly surveys allow us to track the changes in individuals' subjective well-being from the period before the announcement in end-March

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<sup>1</sup> The use of individual fixed effects when studying subjective well-being is important, to ensure that individual unobserved heterogeneity (e.g. personality differences) does not drive our results (Ferrer-i-Carbonell & Frijters, 2004).

<sup>2</sup> See Vaithianathan et al. (2017) and <https://crea.smu.edu.sg/singapore-monthly-panel> for additional information on the SLP.

2016, through the post-announcement / pre-disbursement period, to the post-disbursement period after July 2016.

We find that recipients of SSS payouts, who receive an average of around S\$500 per quarter in our sample, experienced a statistically significant improvement of 2.5% of the baseline mean (or 0.11 SD) in overall life satisfaction upon SSS announcement; there seems to be no additional improvement after the disbursement of SSS payouts (i.e. the disbursement effect is not statistically different from the announcement effect). These results seem to be driven in part by improvements in recipients' satisfaction with their social contacts and family life, household income and economic situation<sup>3</sup>. In addition, we find evidence that the marginal utility of income varies across different dimensions. First, SSS recipients who felt less financially prepared for retirement at the baseline survey wave experienced a higher increase in satisfaction. Surprisingly, we find little evidence of such heterogeneity when we examine heterogeneity in responses by differences in baseline asset levels. Second, the lack of statistically significant overall effects in health satisfaction and condition masks considerable heterogeneity. Those who report being less financially prepared for retirement experienced a statistically significant larger improvement in self-rated health condition. Third, an individual's satisfaction did not improve if only his/her spouse received SSS payouts while he/she did not.

Our findings contribute mainly to the literature on income and subjective well-being. While the permanent income hypothesis (PIH) implies that forward-looking individuals' utility should react only to unanticipated, but not anticipated income shocks (see e.g. Cai & Park, 2016), empirical evidence on how subjective well-being reacts to unanticipated versus anticipated income shocks is scarce. We separately estimate the announcement and

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<sup>3</sup> The result for recipients' satisfaction with the social contacts and family life is only marginally significant, while those for household income and economic situation are statistically significant.

disbursement effects of income increases on subjective well-being<sup>4</sup>, using a unique monthly longitudinal survey<sup>5</sup>. Our results are consistent with the predictions of the PIH: the release of detailed information about SSS eligibility led to immediate increases in subjective well-being even before payouts started, but there was no additional increase in subjective well-being upon disbursement of SSS payouts (i.e. the disbursement effect is not statistically different from the announcement effect). We also add to the evidence on how marginal utility of income may vary by individual characteristics, when we examine heterogeneous effects along the dimensions of perceived financial preparedness for retirement, net assets, and spousal receipt of income. Our finding that the marginal utility of income varies by perceived financial preparedness for retirement (i.e. subjective wealth) but not actual net assets suggests that perceived financial preparedness for retirement could be more important than net assets in understanding how the marginal utility of income varies, at least among the age and socioeconomic group we study.

Beyond the income and subjective well-being literature, our results add to the literature studying the effect of non-contributory pensions on subjective well-being (e.g. Bando et al., 2016; Galiani et al., 2016). When coupled with results from a companion paper (Chen & Tan, 2017), our findings suggest that the SSS improved recipients' welfare without substantial crowding out of private transfers or changes in labour market behavior of current and future SSS beneficiaries<sup>6</sup>. The heterogeneous responses we document suggests that researchers should move beyond studying the average effects of non-contributory pensions, to provide a

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<sup>4</sup> Even in other fields, few non-finance papers have succeeded in capturing announcement effects. Blundell et al. (2011) and Agarwal and Qian (2014) are two examples that manage to do so; both papers show that ignoring announcement effects could bias policy effects downwards.

<sup>5</sup> The high-frequency data also addresses a common source of estimation bias arising from individuals' adaptation to changes in their circumstances over time (see e.g. Clark et al. (2008) for evidence that people adapt to changes in conditions in the context of life satisfaction). Such high frequency data on subjective well-being, however, is uncommon. One example of a paper that tracks subjective well-being almost as frequently is by Frijters et al. (2011), who use quarterly data from Australia to study significant life events.

<sup>6</sup> We study the effects of SSS receipt on expenditure in the companion paper as well, but our results for expenditure are too imprecise for us to draw any definitive conclusions.

fuller picture of the effects of said pensions. Policymakers wishing to maximize welfare gains may also want to take such heterogeneity into consideration when designing new or refining existing non-contributory pensions. In addition, we add to the external validity of the existing non-contributory pension literature by presenting evidence from a country with different institutions and at a different stage of development, compared to those studied earlier. Lastly, our observation that recipients with a lower perceived level of financial preparedness experienced a larger improvement in self-rated health condition strengthens existing evidence on the causal impact of income on health (e.g. Frijters et al., 2005; Lindahl, 2005).

The rest of this paper proceeds as follows. Section 2 gives a brief review of the literature on income and subjective well-being. Section 3 provides background information on the Silver Support Scheme (SSS). Section 4 describes our data, and in Section 5 we elaborate on our identification strategy and empirical model. In Section 6, we present our results, while Section 7 covers our robustness checks. Section 8 concludes.

## **2 Literature review on the impact of income on subjective well-being**

An attractive feature of self-reported levels of happiness or subjective well-being is that they can serve as alternative measures of utility and complement traditional revealed preference approaches. The suitability of self-reported happiness or well-being for this purpose is supported by a substantial amount of evidence. For example, there is a strong positive nexus between self-reported well-being and actual well-being (see e.g. Frey & Stutzer, 2002; Kahneman & Krueger, 2006). More recently, Fleurbaey and Schwandt (2015) present survey results suggesting that 90% of their sample do attempt to maximise their subjective well-being.

Much research on subjective well-being within the economic literature focuses on the relationship between income and happiness. Early research using country-level data found that there was little, if any, increase in happiness even when real gross domestic product per capita had risen substantially over the years (e.g. Easterlin, 1974; Easterlin, 1995; Oswald, 1997). In



contrast, the positive relationship between income and happiness at a particular snapshot in time within a country is well established, i.e. richer people tend to report higher subjective well-being on average (e.g. Frey and Stutzer (2000), Blanchflower and Oswald (2004)). These seemingly conflicting conclusions can be reconciled if happiness were affected not only by absolute income, but also relative income. For example, Ferrer-i-Carbonell (2005) finds that an individual's happiness is equally dependent on one's own income and the income of one's reference group. In addition, people are happier the larger their income gap as compared to their reference group.

The main criticism levelled at research in this area is its lack of causal identification. For example, unobserved individual heterogeneity such as personality differences could affect both income and happiness. Increases in income can likewise be accompanied by a rise in working hours or occupational risk which may not be accounted for but can also affect subjective well-being.

To address this critical weakness, later papers utilise exogenous shocks such as historic institutional upheavals to establish a causal link between income and happiness. Frijters et al. (2004) find that increased household incomes in East Germany, after the German reunification in 1990, contributed to around 12% of the East Germans' improvement in life satisfaction over time. Similarly, by exploiting the income changes during Russia's post-transition years, Frijters et al. (2006) report that changes in real household incomes explain 10-30% of changes in life satisfaction.

Making use of a much more common wealth shock, Gardner and Oswald (2007) and Apouey and Clark (2015) find that lottery winners have a higher level of mental wellbeing (but not self-assessed overall health).

Other authors draw on income variations resulting from changes in public policies. Kronenberg et al. (2017) report that those who benefited from the 1999 introduction of the

United Kingdom National Minimum Wage experienced only limited short-run effects on mental health, while Reeves et al. (2016) find that the same policy reduced the probability of mental ill health for those who benefited. Lachowska (2016) shows that the 2008 economic stimulus tax rebates in the United States reduced feelings of stress and worry, with weaker evidence for an improvement in life and health satisfaction. Boyd-Swan et al. (2016) find positive effects of the 1990 earned income tax credit expansion in the United States, among potentially eligible women, on a range of subjective well-being measures covering mental well-being, overall happiness, and self-esteem.

Apart from natural experiments, Haushofer and Shapiro (2016) and Kilburn et al. (2016) also present positive evidence on happiness, life satisfaction, stress, and future outlook from experimental unconditional cash transfers in Kenya and Malawi respectively.

The introduction of non-contributory pensions is another source of income shocks that generally improved well-being. Case (2004) find that beneficiaries of South Africa's state old age pension experienced a lower level of depression and better self-reported health. Similarly, Galiani et al. (2016) and Bando et al. (2016) show that beneficiaries of a new pension experienced improved mental health and self-worth in Mexico and Peru respectively, but not life satisfaction. Cheng et al. (2016) find that the New Rural Pension Scheme in China led to reduced depression and better self-perceived relative economic situation, while Tseng and Petrie (2014) show that Taiwan's permanent cash injection to elderly farmers improved mental health but not self-assessed health or life satisfaction.

In addition to the focus on causal identification, recent research has moved beyond average effects of income on happiness. For example, Boyce and Wood (2011) investigate the heterogeneity of effects based on personality, while Finkelstein et al. (2013) look at how effects could vary by health status. Two recent papers show that the type of income shocks matters. Consistent with the permanent income hypothesis, permanent income shocks lead to larger

subjective well-being responses than transitory shocks (Bayer & Juessen, 2015; Cai & Park, 2016).

Our paper contributes to the literature by separately estimating the announcement and disbursement effects of an exogenous permanent income increase on life satisfaction and its sub-domains. We also examine how these effects may vary by self-assessed financial preparation for retirement versus net assets, and investigate whether an individual's subjective well-being improves with his or her spouse's receipt of income.

### **3 Background on the Silver Support Scheme**

The Silver Support Scheme (SSS) is Singapore's first national means-tested, non-contributory pension, which permanently supplements the income of the neediest 20 – 30% of Singaporean citizens aged 65 and above. The SSS is an important addition to Singapore's social security system, which has until now been mostly addressed by a defined contribution system known as the Central Provident Fund (CPF). Singaporeans (and their employers) contribute a proportion of income into their CPF accounts. These contributions are split across three accounts – funds from one account can be used for home purchase, funds from another can be used for healthcare expenses, while the last account sets aside money for retirement.

Details of the SSS (e.g. exact qualifying criteria and payout quantum) were announced in end-March 2016 during Singapore's annual Budget speech<sup>7</sup>. Eligible individuals receive quarterly payouts of S\$300 – S\$750, depending on the type of public housing (HDB) flat they live in<sup>8</sup>. Singaporeans who live in smaller flats will receive a larger payout, as flat-type is used

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<sup>7</sup> The Government first announced the introduction of the Silver Support Scheme (SSS) in August 2014, but details on qualifying criteria were not announced then. This implies that even if Singaporeans had some expectations about whether they would receive payouts from SSS, these expectations were probably weak.

<sup>8</sup> Most Singaporeans (80% as of 2016 – see Department of Statistics (2017)) live in high-rise public housing apartments (flats) purchased directly from the government, or in the resale market. These flats are often called HDB flats, after the statutory board (the Housing Development Board) that oversees public housing, and are categorised based on the number of rooms within each flat. The government often uses flat-type as a proxy for socio-economic status to target subsidies and transfers. In the case of the SSS, the payout quantum for individuals living in each type of flat is as follows. 1- and 2-room flats: S\$750; 3-room flats: S\$600; 4-room flats: S\$450; 5-room flats: S\$300.

as a proxy for socioeconomic status. These payouts constitute a significant increase in permanent income, corresponding to 7% – 18% of mean monthly household expenditure among those who received SSS payouts in our sample, or 5 – 13% of labour income at the 20<sup>th</sup> percentile of full-time resident employees (Department of Statistics, 2016). On average, recipients of SSS payouts in our sample receive around S\$500 per quarter. In the period we study, the first payout was made in end-July 2016, followed by another in end-September 2016<sup>9</sup>. We focus on the effects of the announcement and disbursement of these payouts in this study.

Eligibility is automatically determined annually based on a combination of lifetime wages, housing type, housing ownership, and per-capita household income. To qualify, individuals must (i) have contributed no more than S\$70,000 to their Central Provident Fund (CPF) accounts by age 55<sup>10</sup>; (ii) live in a 1- to 5-room HDB flat; (iii) not personally own or have a spouse who owns 5-room or larger HDB flats, private property, or multiple properties; and (iv) live in a household with a per-capita income of S\$1,100 or below. They must also be Singapore citizens. For our study, receipt of Silver Support Scheme (SSS) payouts made in end-July and end-September 2016 can be seen as exogenous. This is because eligibility for the 2016 payouts is based on government data available in 2015 (i.e. before the announcement of eligibility details in March 2016), and hence pre-determined.

Payments of the SSS payouts are credited to the bank accounts that Singaporeans have already registered with the government<sup>11</sup>. Those without a registered bank account will receive a cheque that is mailed to the residential address they registered with the government. If the cheque is not encashed or banked in within six months, the payouts will be credited into the

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<sup>9</sup> Payouts meant for the year 2017 and after will be made in end-December, end-March, end-June and end-September, ahead of the start of each quarter.

<sup>10</sup> Self-employed persons should also have an average annual net trade income of not more than \$22,800 when they were between the ages of 45 and 54.

<sup>11</sup> The Singapore government has a long history of giving out ad-hoc or regular cash transfers to Singaporeans, and hence has efficient systems in place that can be used to disburse any new types of cash transfers.

individuals' CPF account, and can be subsequently withdrawn within a year. This disbursement set-up suggests that Singaporeans who are eligible for the SSS payouts will almost certainly receive their payouts.

## 4 Data and variables

### 4.1 Data source

We use monthly data from waves 0 – 17<sup>12</sup> (covering the period May 2015 – Dec 2016) of a large new longitudinal survey of elderly respondents in Singapore, the Singapore Life Panel (SLP)<sup>13</sup>. The SLP is run by the Centre for Research on the Economics of Ageing (CREA) at the Singapore Management University, and was designed in collaboration with the creators of the US Health and Retirement Study (HRS) and its sister surveys, such as the American Life Panel. 15,000 Singapore citizens and permanent residents aged between 50 and 70, and their spouses, were initially recruited for a baseline survey in May – July 2015. Follow-up surveys were sent monthly. CREA has also put measures in place to limit attrition and ensure responses are population-representative<sup>14</sup>. These measures appear to have succeeded, as attrition has been low<sup>15</sup>, and comparison of SLP data to government statistics has shown that SLP data is population representative (Vaithianathan et al., 2017).

There are several advantages to using this dataset for our study. First, the high frequency at which the survey is carried out allows us to time the effects of this permanent income shock (i.e. the Silver Support Scheme (SSS)) more precisely, compared to most other surveys which

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<sup>12</sup> Wave 0 refers to the baseline survey which was carried out during recruitment (between May – July 2015), while wave 1 was conducted in August 2015. Wave 17 was conducted in December 2016.

<sup>13</sup> See Vaithianathan et al. (2017) and <https://crea.smu.edu.sg/singapore-monthly-panel> for additional information on the SLP.

<sup>14</sup> For example, the surveys are available in all four major languages spoken by Singaporeans. In addition, while the surveys are conducted over the internet, respondents who are unable to understand the survey questions or who do not have access to the internet can answer the survey over the phone, or at centres set up at convenient locations around Singapore, where the survey will be conducted by trained interviewers. CREA also conducts ongoing campaigns to encourage participation.

<sup>15</sup> While not everyone responds to every survey wave, the number of participants who respond in any particular wave has remained stable at around 8,000.

are carried out at yearly or quarterly intervals. Second, the survey had been running for a few months before announcement of the SSS details, allowing us to separately identify the announcement and disbursement effects, as well as examine the credibility of our DiD identifying assumption. Third, as we will see later in this section, the SLP questions on subjective well-being go beyond the more commonly asked life satisfaction question, and include questions on narrower domains of subjective well-being such as satisfaction with one's social life or household income, allowing us to study which of these narrower domains might drive changes in overall life satisfaction. Lastly, the richness of data in the SLP will also allow us to explore how responses to the SSS might vary among different segments of the population.

## 4.2 Variables

We focus our analysis on the effect of receiving a permanent income shock on subjective well-being, and restrict the sample for our main analysis to age-eligible respondents (aged 65 and above in 2016) who are Singapore citizens and who live in public housing flats (as of 2016, about 80% of resident households live in public housing flats (Department of Statistics, 2017)). These are three out of the list of eligibility criteria that individuals must meet to qualify for SSS payouts in 2016. This sample includes individuals who receive SSS as well as those who do not. SSS recipients are identified using a deliberately timed quarterly question in the SLP which asks whether the respondent received SSS in the previous month; individuals who report receiving at least one out of the two payouts in 2016<sup>16</sup> are coded as SSS recipients.

Apart from questions on treatment status, our dataset includes a rich set of baseline demographics (age, marital status, gender, ethnicity, education, housing type, number of household members, assets, and self-reported financial preparedness for retirement), as well as measures of different domains of evaluative subjective well-being, which will serve as our outcome variables.

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<sup>16</sup> Once in end-July 2016, and another in end-September 2016.

The subjective well-being variables include a broad measure of overall life satisfaction, as well as satisfaction within narrower domains such as household income; these variables are rated from 1 (worst) to 5 (best). Data on these are collected via the following questions: (i) “Taking all things together, how satisfied are you with your life as a whole these days?”; (ii) “How satisfied are you with your social contacts and family life?; (iii) “How satisfied are you with your daily activities, and if you are working, your job?”; (iv) “How satisfied are you with the total income of your household”; (v) “How satisfied are you with your overall economic situation?”; (vi) “How satisfied are you with your health”; and (vii) “Would you say your health is excellent, very good, good, fair, or poor?”<sup>17</sup>.

To end off this section, we report summary statistics for the baseline demographics and subjective well-being outcomes for those who are age-eligible for the SSS (i.e. aged 65 and above in 2016), disaggregated by treatment status, in **Table 1** and **Table 2** respectively. Focusing on the full age-eligible sample in **Table 1**, we can see that SSS recipients and non-recipients differ in terms of several baseline characteristics. E.g., women are more likely to receive SSS, and SSS recipients report being less financially prepared for retirement than non-SSS recipients.

While these differences are relatively small in magnitude (judging by the normalised differences), and causal identification in our study relies on a Difference-in-Differences (DiD) strategy which allows for differences in baseline characteristics, one might be concerned that the treated and control groups are different enough that the DiD identifying assumption – parallel trends in the absence of treatment – might not hold. In Section 5, we discuss our methods for addressing this concern and explain **Table 1** in greater detail.

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<sup>17</sup> Options for the first six questions are “very dissatisfied”, “dissatisfied”, “neither satisfied nor dissatisfied”, “satisfied”, and “very satisfied”; options for the last question are “poor”, “fair”, “good”, “very good” and “excellent”.

## 5 Identification strategy and empirical model

### 5.1 Identification strategy

We use a difference-in-differences (DiD) strategy to identify the average treatment-on-treated (ATT) effect of receiving Silver Support Scheme (SSS) payouts on a broad range of subjective well-being domains. Our main analysis focuses on age-eligible individuals (aged 65 and above in 2016) who are Singapore citizens and who live in public housing flats<sup>18</sup>. These are three out of the list of eligibility criteria that individuals must meet to qualify for SSS payouts in 2016. This sample includes both SSS recipients (the treated group) and non-recipients (the control group). Waves 0 – 8 (May 2015 – Mar 2016)<sup>19</sup> make up the pre-announcement period; waves 9 – 12 (Apr – Jul 2016) are the post-announcement and pre-disbursement period; and waves 13 – 17 (Aug – Dec 2016) are the post-disbursement period.

As we note in Section 4, the treated and control groups differ from each other in terms of several baseline characteristics. One might be concerned that differences between these groups could invalidate the DiD identifying assumption (i.e. outcomes in both groups follow the same trend in the absence of treatment).

Our main strategy for addressing this concern is to construct a sample where the treatment and control groups are more similar. We start by using logistic regression to estimate the propensity score for receiving SSS payouts. The covariates are selected from a rich pool of key baseline demographic variables that could affect one’s eligibility for SSS payouts<sup>20</sup>. Using the algorithm outlined in Imbens (2015), which proposes a data-driven way of selecting a subset of baseline covariates and their interactions, we select the following covariates and some of their interactions: age, marital status, gender, ethnicity, education, public housing flat type, whether respondent’s father is still living, number of household members, number of living

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<sup>18</sup> As of 2016, 80% of resident households live in public housing (Department of Statistics, 2017).

<sup>19</sup> Wave 0 covers May-Jul 2015.

<sup>20</sup> E.g. Total CPF contributions of not more than \$70,000 by age 55, living in a household with per capita income of not more than \$1,100



children, income of self and spouse, baseline self-assessment of financial preparedness for retirement and baseline satisfaction with one's economic situation.

We then trim the sample progressively at both extreme ends of the propensity score till the treated and control groups are more similar in terms of the baseline characteristics as well as pre-announcement time trends for our outcome variables. **Figure 1** plots the unconditional mean of the life satisfaction variable across time, for four samples with different ranges of propensity score: (i) 0.00 to 1.00; (ii) 0.10 to 0.90; (iii) 0.15 to 0.85; and (iv) 0.20 to 0.80. As we restrict the sample to narrower ranges of propensity score, the pre-announcement time trends for the treated and control groups start to converge. For the smallest sample with propensity scores of 0.20 to 0.80, we see that the pre-announcement trends for the unconditional means are almost parallel. The same can be observed for other outcome variables in **Figure 2** and **Figure 3**.

We also see a noticeable improvement in the comparability of the treated and control groups in terms of baseline characteristics after trimming. The right panel in **Table 1**, which summarises statistics for the trimmed sample with propensity scores of 0.20 to 0.80, shows that differences in education, whether one lives in a 2-room flat, whether one owns a home, and perceived financial preparedness for retirement fall to statistical insignificance, and differences in all other variables shrink noticeably. All normalised differences<sup>21</sup> in the trimmed sample also drop to 0.16 or less in absolute magnitude, well below the value of 0.3 which Imbens (2015) deems “modest”<sup>22</sup>. In addition, **Figure 4** shows that the distributions of key covariates related to SSS eligibility become more similar after trimming. Based on our above comparisons of baseline characteristics and pre-treatment trends across different samples and between the

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<sup>21</sup> Normalised differences are computed as in Imbens (2015), as the difference in means standardised by the square root of the mean variances of both groups.

<sup>22</sup> **Table A1** in Appendix A compares the baseline characteristics for the treated and control groups for samples with propensity scores 0.10 to 0.90 and 0.15 to 0.85. The differences in means fall as we restrict the sample progressively.

treated and control groups, we deem it reasonable to use the sample with propensity scores 0.20 to 0.80 for our main DiD analyses.

While trimming the sample based on observables does not necessarily lead to comparability based on unobservables, it is unlikely that the remaining differences in unobservables will lead to a violation of the common trends assumption for DiD. Differences in unobservables could arguably be critical in the case where individuals self-select into the treatment group, but as we explained in Sections 1 and 3, the eligibility of individuals for the end-July and September payouts are predetermined before the announcement, and payouts are made automatically to all eligible individuals. In addition, the propensity score we use for trimming is also formulated based on variables that are related to individuals' eligibility for the SSS payouts.

In addition to the visual check of pre-announcement trends, we statistically test the DiD identifying assumption by adding pre-announcement leads to our regression specification. In the vast majority of the outcomes analysed, the coefficients of the pre-treatment leads are statistically insignificant, increasing the probability that our identifying assumption is valid. (The exceptions are pre-treatment leads for economic satisfaction and daily activities satisfaction which are significant in March -- the month of SSS announcement, though this could reflect early anticipation about the annual Budget announcements in general.)

Beyond this, we carry out a battery of robustness checks (described more fully in Section 7), which include checks that address potential violations of the parallel trends assumption in different ways. These checks include the addition of different group-specific time fixed effects and the use of different reweighting schemes to better match the treatment and control groups (e.g. using Abadie (2005)'s semi-parametric DiD on the full sample or matching-DiD using a 1:1 nearest neighbour match). Our results are robust to this battery of checks – across the numerous checks we perform, the estimated magnitudes remain similar, and the effect of

receiving SSS remains statistically significant in all but two checks (where we lose significance due to a smaller sample size).

Finally, we note that trimming our sample means that we identify the ATT for only a subset of those who are treated, if treatment effects are heterogeneous. However, we show in Section 7 that the ATT effects we estimate in our main specifications are close to those we estimate on the full sample (both with and without Abadie (2005)'s reweighting scheme<sup>23</sup>).

## 5.2 Empirical specifications

We start by estimating the following regression to study the overall effects of the Silver Support Scheme (SSS):

$$\begin{aligned}
 Y_{it} = & \sum_{k=-3}^{-1} \beta_{pre,k} (Treat_i \times Preann_{t,k}) \\
 & + \beta_{ann} (Treat_i \times Anntodisb_t) + \beta_{disb} (Treat_i \times Postdisb_t) \\
 & + \alpha_i + \gamma_t + \epsilon_{it}
 \end{aligned} \tag{1}$$

where  $Y_{it}$  is the outcome variable for respondent  $i$  at time  $t$ ;  $Treat$  is a dummy variable that takes on value one if the individual ever received SSS payouts;  $Preann_{t,-3}$ ,  $Preann_{t,-2}$ ,  $Preann_{t,-1}$  are the pre-announcement leads (dummy variables that take value 1 if time  $t$  corresponds to January, February, and March 2016 respectively)<sup>24</sup>;  $Anntodisb$  is a dummy for the period between announcement and disbursement (waves 9 – 12, Apr – Jul 2016);  $Postdisb$  is a dummy for the period after disbursement (waves 13 – 17, Aug – Dec 2016); while  $\alpha_i$  and  $\gamma_t$  are individual and time fixed-effects respectively. The suppressed period consists of waves 0 to 5 (May/Jun/Jul – Dec 2015). Standard errors are clustered at the household level.

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<sup>23</sup> Abadie (2005)'s semi-parametric DiD involves weighting each control observation by their propensity score, and estimating the effects of receiving SSS using the full age-eligible sample.

<sup>24</sup> March 2016 is considered in the pre-announcement period as the announcement is made only towards the end of March. The survey wave in March would have closed before the announcement is made.

$\beta_{pre,k}$  test the assumption of common time trends statistically. Our identifying assumption will be more credible if the coefficients in  $\beta_{pre}$  are statistically insignificant. For the age-eligible sample,  $\beta_{ann}$  captures the announcement effect, and  $\beta_{disb}$  captures the disbursement effect.

Next, we modify equation (1) to study how treatment effects may vary by baseline financial preparedness for retirement and wealth:

$$\begin{aligned}
Y_{it} = & \sum_{k=-3}^{-1} \beta_{pre,k} (Treat_i \times Preann_{t,k}) \\
& + \beta_{ann} (Treat_i \times Anntodisb_t) + \beta_{disb} (Treat_i \times Postdisb_t) \quad (2) \\
& + \beta_{ann,fin} (Treat_i \times Anntodisb_t \times Fin_i) \\
& + \beta_{disb,fin} (Treat_i \times Postdisb_t \times Fin_i) + \alpha_i + \gamma_t + \epsilon_{it}
\end{aligned}$$

where  $Fin$  can either be (i) a scale variable running from 1 (poor) to 5 (excellent) that measures subjective financial preparedness for retirement, or (ii) a scale variable running from 1 (poorest) to 5 (richest) in terms of pre-announcement assets quintile<sup>25</sup>. Financial preparedness for retirement can serve as a proxy for baseline assets levels, but it also contains additional information about an individual's financial burden, as well as consumption and risk preferences. E.g. compared to another individual with the same level of wealth, a person with lower risk tolerance (and who would want to build up more savings for low probability adverse events) or who must support more dependents would be likely to report being less financially prepared for retirement. Taken together, these two sets of regressions will provide a fuller picture.  $\beta_{ann,fin}$  and  $\beta_{disb,fin}$  capture heterogeneity in effects across either subjective financial preparedness for retirement or assets.

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<sup>25</sup> These asset quintiles are based on a sample that includes only those who are age-eligible (aged 65 and above in 2016), and who live in public housing.

To the extent that financial preparation for retirement / wealth is correlated with flat-type,  $\beta_{ann,fin}$  and  $\beta_{disb,fin}$  may also reflect the effects of differing payout levels, since payout levels are determined by the type of flat individuals live in. As such, we estimate another set of regressions which consider heterogeneity across  $Fin$  within each flat-type, to verify that our results from equation (2) indeed reflect heterogeneity in  $Fin$ , and not differing payout levels:

$$\begin{aligned}
Y_{it} = & \sum_{k=-3}^{-1} \beta_{pre,k} (Treat_i \times Preann_{t,k}) \\
& + \sum_{f \in F} \beta_{ann,f} (Treat_i \times Anntodisb_t \times Flat_{i,f}) \\
& + \sum_{f \in F} \beta_{ann,f,fin} (Treat_i \times Anntodisb_t \times Flat_{i,f} \times Fin_i) \\
& + \sum_{f \in F} \beta_{disb,f} (Treat_i \times Postdisb_t \times Flat_{i,f}) \\
& + \sum_{f \in F} \beta_{disb,f,fin} (Treat_i \times Postdisb_t \times Flat_{i,f} \times Fin_i) \\
& + \alpha_i + \gamma_t + \epsilon_{it}
\end{aligned} \tag{3}$$

$Flat_{i,f}$  is a dummy variable that takes value 1 if the individual lives in flat type  $f$ .  $F$  includes 1/2-, 3-, 4-, 5-room and other flats<sup>26</sup>.  $\beta_{ann,f,fin}$  and  $\beta_{disb,f,fin}$  are our coefficients of interest.

To investigate if responses may vary by whether an individual's spouse receives SSS payouts, we restrict our sample to respondents who are married and estimate:

$$\begin{aligned}
Y_{it} = & \sum_{k=-3}^{-1} \beta_{pre,k} (At\ least\ one\ treated_i \times Preann_{t,k}) \\
& + \sum_{g \in G} \beta_{ann,g} (Who\_treated_{i,g} \times Anntodisb_t)
\end{aligned} \tag{4}$$

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<sup>26</sup> Results for 1/2- and 5-room flats should be interpreted with caution, as relatively fewer people stay in these flat-types. We do not report results for the category "other flats" as it is made up of flats of unknown types. As we have already accounted for all flat-type categories in this specification, we exclude the interaction terms  $Treat_i \times Anntodisb_t$  and  $Treat_i \times Postdisb_t$  to avoid perfect collinearity.

$$\begin{aligned}
& + \sum_{g \in G} \beta_{disb,g} (Who\_treated_{i,g} \times Postdisb_t) \\
& + \alpha_i + \gamma_t + \epsilon_{it}
\end{aligned}$$

where *At least one treated<sub>i</sub>* is a dummy variable that takes value 1 if either respondent *i* or respondent *i*'s spouse received SSS; *Who\_treated<sub>i,g</sub>* can be a dummy variable indicating that (i) only the respondent received SSS; (ii) only the respondent's spouse received SSS<sup>27</sup>; or (iii) both the respondent and his/her spouse received SSS.  $\beta_{ann,g}$  and  $\beta_{disb,g}$  reflect the heterogeneity of effects based on the identity of SSS recipients within a couple. E.g.  $\beta_{disb,respondent\ only}$  will give the effect of only the respondent receiving SSS on his/her subjective well-being in the post-disbursement period<sup>28</sup>.

## 6 Results

### 6.1 Overall effects

**Table 3** reports the overall effect of receiving Silver Support Scheme (SSS) payouts on different domains of evaluative subjective well-being (specified at the top of each column). Coefficients for pre-announcement leads are generally statistically insignificant, providing evidence in favour of the DiD identifying assumption. The exceptions are leads for daily activities and economic satisfaction in March (the month of SSS announcement), which may reflect early anticipation about the annual Budget announcements in general, especially in the case of economic satisfaction.

We find that the SSS, which amounts to around S\$500 per quarter among recipients in our sample, leads to a statistically significant improvement in overall life satisfaction (column 1). This improvement starts immediately upon SSS announcement, and does not increase

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<sup>27</sup> This variable is captured through a question in the SLP on whether a respondent's spouse received SSS.

<sup>28</sup> The dynamics of how subjective well-being may change over time is interesting too. We estimated these regressions but do not observe any obvious patterns in the dynamics. This is not surprising as Frijters et al. (2011) find that the effects of an improvement in financial situation persists even up to 8 quarters after the event. In the interest of space, we will not report these results, though they are available on request.

further after disbursement starts (i.e. there is no significant difference between the announcement and disbursement effects). Life satisfaction rises by 0.09 (about 2.5% of the baseline mean / 0.11SD) upon announcement of the SSS, and stays elevated at this level upon commencement of the SSS payouts (see **Table 3**). As one would expect, this rise in life satisfaction is at least partly driven by statistically significant increases in household income satisfaction (0.11, or 3.5% of the baseline mean / 0.13SD) and overall economic satisfaction (0.08, or 2.5% of the baseline mean / 0.10SD). In addition, there is some evidence of a marginally significant rise in social and family life satisfaction (0.07, or 1.9% of the baseline mean / 0.09SD). The coefficients of the treatment variables for health satisfaction and self-rated health condition are positive but insignificant, suggesting that the SSS has little, if any, overall effect on health. Consistent with the predictions of the PIH (see e.g. Cai & Park, 2016), there is no *additional* increase in life satisfaction (and its sub-domains) upon disbursement of the payouts – the difference in magnitude between  $\beta_{ann}$  and  $\beta_{disb}$  is small in magnitude and not statistically significant.

The direction of our main life satisfaction result is consistent with other published papers that look at the causal effect of income on life satisfaction (e.g. Frijters et al., 2004; Frijters et al., 2006; Boyd-Swan et al., 2016; Haushofer & Shapiro, 2016; Lachowska, 2016). In terms of magnitude, our main life satisfaction result is slightly smaller than the 0.17SD improvement in life satisfaction due to a large cash transfer<sup>29</sup> in Kenya reported in Haushofer and Shapiro (2016). Compared to East Germans who experienced an approximate 25% rise in real household incomes between 1991 and 1995, our life satisfaction result appears slightly larger as East Germans experienced a corresponding increase of 1.4 – 2.2% of their 1990 mean life satisfaction (Frijters et al., 2004).

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<sup>29</sup> Between US\$404 PPP to US\$1,525 PPP was transferred to households in this experiment. The mean transfer was US\$709 PPP, corresponding to almost two years of per-capita expenditure.

## 6.2 Heterogeneity by financial preparedness for retirement and assets

The overall effects mask heterogeneity by subjective financial preparedness for retirement. **Table 4** shows statistically significant evidence of variation by financial preparedness for retirement in life satisfaction, household income satisfaction, economic satisfaction, as well as self-rated health condition. Consistent with intuition, individuals who felt less financially prepared for retirement in the baseline experienced larger increases in subjective well-being from receiving SSS payouts; on average, a one-point decrease (on a 5-point scale) in retirement preparedness increases the improvement in satisfaction from SSS payouts by about 0.06 to 0.08.

In addition, once we allow for heterogeneous effects, the effect of SSS on health for those who are least prepared for retirement surfaces. E.g., self-rated health condition for those who are least prepared for retirement improved by 0.13 points<sup>30</sup> during the post-disbursement period (5% of the mean, or 0.15SD). Our results are consistent with that of Jones and Schurer (2011), who also document heterogeneity in the effect of income on health, as well as past papers looking at the effect of income on health (Frijters et al., 2005; Behrman et al., 2011; Gunasekara et al., 2011).

Financial preparedness for retirement, however, may be correlated with flat-type and thus payout quantum (payout levels are determined entirely by flat-type). This would imply that the variation we find above could be driven purely by the fact that less financially prepared individuals received a higher payout quantum. We verify that this is not the case in **Table A2** of Appendix A, by investigating the existence of variation by financial preparedness within each flat-type. Generally, we still find that individuals who felt less financially prepared for retirement experienced larger increases in subjective well-being.

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<sup>30</sup>  $0.19 - 0.06 = 0.13$



When we switch to heterogeneity by baseline asset quintiles in **Table 5**, we see that coefficients for interactions between the policy and asset variables are mostly insignificant and small in magnitude when compared to those for financial preparedness<sup>31</sup>. In all, the results in this sub-section suggest the marginal utility of income is decreasing in subjective financial preparedness for retirement, but the same extent of heterogeneity is not observed for net assets.

There are a few potential explanations for this somewhat surprising result. First, subjective financial well-being may incorporate more information than objective measures of wealth, such as net assets. For example, subjective financial well-being is likely to capture information related to financial burden or differences in risk aversion, while net assets may not. For any given asset level, individuals with higher financial burden or risk aversion may be likely to benefit more from an increase in permanent income, due to an alleviation in financial burden and a decrease in the probability that retirement income will be insufficient in future respectively, and may be a reason why we observe more heterogeneity in responses by subjective financial preparedness for retirement.

Second, survey respondents may be more likely to give accurate answers to subjective financial preparedness for retirement than net assets. This potentially greater mismeasurement in net assets may then contribute to the lower heterogeneity in responses we observe for net assets.

Regardless of the potential explanations, our results in this section suggest that perceived financial well-being could be more important than net assets, as a summary measure, in understanding how individuals' utility change in response to income, at least among the age and socioeconomic group we study. As this result may have implications for the welfare effects

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<sup>31</sup> We obtain similar results when we use baseline asset deciles instead of quintiles, and when we use non-housing assets instead of total assets (results available upon request).

of income transfers and how policymakers should target income transfers, it would be interesting to see if this result holds up in different contexts in future studies.

### **6.3 Effect of spousal receipt of SSS**

Finally, we report results on how spousal receipt of SSS payouts may affect subjective well-being. Results in **Table 6** are based only on married respondents. Our first result is intuitive: subjective well-being rises slightly more if both the respondent and his/her spouse received SSS payouts, compared to the case where only the respondent received SSS. E.g., life satisfaction increases by 0.12 if only the respondent received SSS, and it increases by an additional 0.03 if both the respondent and his/her spouse receive SSS. When only the respondent's spouse received SSS payouts, the respondent's well-being does not appear to increase by much: the results are statistically insignificant across all domains of subjective well-being.

Our results suggest that having only a spouse receive SSS payouts does not improve a respondent's subjective well-being by much (if at all), and are consistent with a scenario of limited income pooling within the household. These results are also consistent with findings from past papers that focus on testing the unitary household model and the income pooling property using different outcome variables (see Donni and Chiappori (2011) for a review)<sup>32</sup>.

## **7 Robustness Checks**

The overall impact of the SSS on each outcome we analyse is subjected to the following battery of robustness checks based on modifications of equation (1):

1. Removal of pre-announcement leads to verify that results are robust to changes in baseline period definition.

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<sup>32</sup> While we note that our results are consistent with a scenario of limited income pooling, we acknowledge that more work will need to be done if we wish to make a more definitive statement regarding the unitary household model. Such work, however, is beyond the scope of our current paper. Separately, we look at whether the effect of receiving SSS varies by gender as well, and find no evidence that it does (results available on request).

2. Restriction of sample to a “balanced” panel, where every individual has at least one observation in each of the pre-announcement, announcement-to-disbursement, and post disbursement periods, to verify that compositional changes in respondents across waves are not driving our results.
3. Addition of ethnicity- and flat-type-specific time fixed effects, to allow for differential time trends in different groups.
4. Addition of controls for additional welfare payments: whether the respondent received the Workfare Income Supplement (a wage subsidy) or GST vouchers (a modest one-off cash transfer)<sup>33</sup>.
5. Estimating the effects of SSS using Abadie (2005)’s semiparametric DiD<sup>34</sup> and DiD matching using a 1-1 nearest neighbour match based on the propensity of receiving SSS<sup>35</sup>, to verify that our results are robust to different methods of addressing imbalances in baseline characteristics that may affect the parallel trends assumption.

We find that our results are robust to the battery of checks we carry out. **Table A3** to **Table A6** in Appendix A report the results of these robustness checks for outcomes that show an overall response to receipt of SSS: life, social, household income, and economic satisfaction<sup>36</sup>. The estimated effect of receiving SSS payouts remains significant across almost all our robustness checks, except for two checks in social satisfaction, where we lose

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<sup>33</sup> We do not include controls for additional welfare payments in our main specification as these questions are only asked quarterly, so not all respondents reply to these questions. Including these controls in our main specification will reduce our sample size and reduce statistical power considerably.

<sup>34</sup> This method addresses the imbalance of baseline characteristics between the treated and control groups by reweighting control observations based on their propensity score; control observations with a higher propensity score are given a higher weight. We use the Stata package *absdid* described in Hounghbedji (2015) to implement this estimator.

<sup>35</sup> This method addresses the same issue – imbalance in baseline covariates that might be correlated with trends – that Abadie (2005) and our main strategy tries to address. We match each treated individual to a control individual based on a nearest neighbor match using the propensity score, compute the DiD for each pair, then aggregate these results to obtain the impact of SSS receipt. We compute p-values using a permutation test, using methods similar to those described in Abadie et al. (2010), Robbins et al. (2016), and Chang and Lee (2011).

<sup>36</sup> Results for other outcomes are available upon request.

significance due to the smaller sample used in those checks. The magnitude of the estimated coefficients remains quite stable across different checks for each outcome variable.

In addition, the estimated effect of receiving SSS from using Abadie (2005)'s semi-parametric DiD on the full/untrimmed sample (in column (6) of **Table A3** to **Table A6**) are similar in magnitude to those from our trimmed sample, providing evidence that estimated ATTs from our trimmed and full sample are likely to be similar. We also present results from our full sample estimated using Equation (1) in **Table A7**. The estimated SSS effects are generally similar.

## 8 Conclusion

In this paper, we use a DiD strategy to study the causal effect of a permanent income increase on a broad range of satisfaction domains. The source of exogenous variation comes from a new national means-tested non-contributory pension in Singapore, the Silver Support Scheme (SSS), for which eligibility is pre-determined and payouts to all eligible individuals are disbursed automatically. Using a new monthly longitudinal dataset of elderly Singaporeans, we precisely time and estimate the SSS announcement and disbursement effects. We find that announcement effects are important: life satisfaction rises among recipients by about 0.11SD upon SSS announcement, and this rise is sustained (but does not increase further) after disbursement of the payouts. This improvement appears to be driven by social, household income, and economic satisfaction. Consistent with the predictions of the PIH (see e.g. Cai & Park, 2016), there is no additional effect upon disbursement – the difference between the announcement and disbursement effects is small and statistically insignificant.

We also explore heterogeneity in the marginal utility of income. Consistent with intuition, recipients who reported being less financially prepared for retirement exhibited larger increases in well-being. Surprisingly, an analysis of how marginal utility of income varies by net assets shows little evidence of such heterogeneity, suggesting that subjective financial well-

being may be more important than net assets in understanding how the marginal utility of income varies, at least among the age and socioeconomic group we study. In addition, once we account for heterogeneity, we discover that the self-rated health condition of those who are least financially prepared for retirement improved. Lastly, we find that well-being improved negligibly when an individual's spouse received SSS payouts while he/she did not, a result that is consistent with limited income pooling within the household.

Our findings add to the literature on the effect of income on subjective well-being, by separately estimating the announcement and disbursement effects of income on subjective well-being. We also document the presence of heterogeneity in responses to the SSS, which suggests that future policies designed with greater consideration of heterogeneity among individuals could reap higher welfare gains. One surprising result is that subjective financial well-being may be more important than net assets in understanding how marginal utility of income varies. It would be interesting to see if this result holds up in different contexts in future studies, as such a result may have implications for the welfare effects and targeting of income transfers. Lastly, our results also strengthen the evidence on the effects of non-contributory pensions on subjective well-being, and of income on health.

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**Table 1: Summary statistics for baseline demographics**

	Full age-eligible sample						Trimmed sample (propensity score 0.2 – 0.8)					
	Received SS (N <sup>1</sup> =429)		No SS (N <sup>1</sup> =1,242)		Norm diff <sup>2</sup>	Diff in means <sup>3</sup>	Received SS (N <sup>1</sup> =324)		No SS (N <sup>1</sup> =580)		Norm diff <sup>2</sup>	Diff in means <sup>3</sup>
	Mean	SD	Mean	SD			Mean	SD	Mean	SD		
Age at 2016	68.86	3.32	68.04	2.66	0.27	0.83***	69.05	3.36	68.67	2.86	0.12	0.39*
Married	0.69	0.46	0.77	0.42	-0.18	-0.08***	0.66	0.47	0.73	0.45	-0.15	-0.07**
Male	0.40	0.49	0.54	0.50	-0.29	-0.15***	0.35	0.48	0.40	0.49	-0.11	-0.05
Chinese	0.87	0.33	0.87	0.34	0.00	0.00	0.87	0.34	0.90	0.30	-0.10	-0.03
Malay	0.07	0.25	0.07	0.25	0.00	0.00	0.07	0.25	0.06	0.24	0.03	0.01
Indian	0.05	0.22	0.05	0.22	0.01	0.00	0.05	0.22	0.03	0.18	0.09	0.02
No formal schooling	0.17	0.38	0.12	0.33	0.13	0.05**	0.17	0.38	0.16	0.36	0.05	0.02
Primary schooling	0.35	0.48	0.26	0.44	0.20	0.09***	0.41	0.49	0.37	0.48	0.07	0.04
Secondary schooling	0.36	0.48	0.41	0.49	-0.10	-0.05*	0.35	0.48	0.36	0.48	-0.01	-0.01
Lives in 1-room flat	0.05	0.22	0.01	0.11	0.22	0.04***	0.05	0.22	0.02	0.15	0.15	0.03**
Lives in 2-room flat	0.05	0.22	0.02	0.15	0.15	0.03**	0.05	0.22	0.03	0.17	0.11	0.02
Lives in 3-room flat	0.33	0.47	0.21	0.40	0.27	0.12***	0.39	0.49	0.32	0.47	0.16	0.07**
Lives in 4-room flat	0.36	0.48	0.37	0.48	-0.03	-0.01	0.36	0.48	0.42	0.49	-0.11	-0.05
Lives in 5-room flat	0.16	0.37	0.29	0.45	-0.32	-0.13***	0.10	0.30	0.15	0.36	-0.15	-0.05**
Owns home	0.83	0.38	0.88	0.33	-0.15	-0.05**	0.82	0.38	0.84	0.37	-0.05	-0.02
No. of hh members	2.95	1.49	3.27	1.51	-0.21	-0.32***	2.82	1.42	3.05	1.43	-0.16	-0.23**
No. of total children	2.18	1.43	2.15	1.25	0.02	0.03	2.16	1.47	2.21	1.31	-0.03	-0.05
Retirement preparedness <sup>4</sup>	2.17	0.90	2.33	0.88	-0.19	-0.17***	2.13	0.90	2.14	0.83	-0.02	-0.01

Notes:

<sup>1</sup> N refers to number of respondents.

<sup>2</sup> Normalised differences are computed as in Imbens (2015) (as the difference in means standardised by the square root of the mean variance of both groups)

<sup>3</sup> \*\*\*, \*\*, \* represent statistical significance at the 10%, 5% and 1% levels respectively

<sup>4</sup> This is a self-assessment on preparedness for retirement, captured on a scale of 1 to 5, with a higher value representing greater preparedness. This was captured during the baseline survey, which was conducted before the announcement of details on the Silver Support Scheme.

**Table 2: Summary statistics for pre-announcement dependent variables<sup>1</sup>  
(Trimmed sample with 0.20-0.80 propensity score)**

	Age-eligible for Silver Support (age 65 and above)					
	Received SS			Didn't receive SS		
	N <sup>2</sup>	Mean	SD	N <sup>2</sup>	Mean	SD
Life satisfaction	1,947	3.51	0.80	3,593	3.54	0.78
Social / family satisfaction	1,944	3.65	0.74	3,594	3.66	0.71
Daily activities satisfaction	1,622	3.52	0.73	3,009	3.47	0.79
Household income satisfaction	1,944	3.18	0.86	3,591	3.19	0.84
Economic satisfaction	1,948	3.19	0.85	3,591	3.21	0.83
Health satisfaction	1,947	3.26	0.90	3,594	3.29	0.91
Self-rated health condition	1,947	2.52	0.89	3,592	2.60	0.88

Notes:

<sup>1</sup> These variables are rated from 1(worst) to 5(best). Options for the first 6 variables are “very dissatisfied”, “dissatisfied”, “neither satisfied nor dissatisfied”, “satisfied”, and “very satisfied”, while the options for the last question are “poor”, “fair”, “good”, “very good” and “excellent”.

<sup>2</sup> N refers to the number of observations at the respondent-wave level.

**Table 3: Overall impact of the Silver Support Scheme on subjective well-being**

VARIABLES	(1) Life satis <sup>fn</sup>	(2) Social satis <sup>fn</sup>	(3) Daily activities satis <sup>fn</sup>	(4) HH income satis <sup>fn</sup>	(5) Economic satis <sup>fn</sup>	(6) Health satis <sup>fn</sup>	(7) Self-rated health cond
Received SS × Jan	0.0428 (0.0427)	0.0378 (0.0403)	0.00955 (0.0421)	0.0121 (0.0418)	-0.00109 (0.0416)	-0.00428 (0.0436)	-0.00601 (0.0446)
Received SS × Feb	0.0165 (0.0435)	-0.0116 (0.0427)	0.0345 (0.0474)	0.0719 (0.0491)	0.0631 (0.0476)	0.0425 (0.0463)	0.0266 (0.0462)
Received SS × Mar	0.0705 (0.0437)	0.0174 (0.0410)	0.0949** (0.0450)	0.0718 (0.0454)	0.109** (0.0467)	0.0635 (0.0508)	0.0721 (0.0448)
Received SS × announce-to-disb	0.0858** (0.0359)	0.0643* (0.0328)	0.0226 (0.0352)	0.0851** (0.0368)	0.0661* (0.0349)	-0.00177 (0.0366)	0.0194 (0.0352)
Received SS × post- disb	0.0892** (0.0409)	0.0695* (0.0366)	0.0549 (0.0400)	0.113*** (0.0391)	0.0825** (0.0381)	0.0383 (0.0380)	0.0556 (0.0380)
Mean	3.51	3.65	3.52	3.18	3.19	3.26	2.52
S.D.	0.80	0.74	0.73	0.86	0.85	0.90	0.89
Observations	12,652	12,651	11,742	12,646	12,650	12,651	12,649
R-squared	0.675	0.659	0.691	0.688	0.685	0.727	0.711
<u>P-values for <math>\beta_{disb} - \beta_{ann}</math> (i.e. test of whether <math>\beta_{disb} - \beta_{ann}</math> is statistically different from zero)</u>							
p-values	0.887	0.819	0.177	0.295	0.522	0.138	0.198

Notes:

<sup>1</sup> Standard errors clustered at the household level in parentheses. \*\*\*, \*\* and \* represent statistical significance at the 1%, 5% and 10% level of significance respectively.

<sup>2</sup> Dependent variables shown at the top of each column are measured on a scale of 1 – 5. 1 represents the worst (“very dissatisfied” or “poor”) and 5 the best (“very satisfied” or “excellent”).

<sup>3</sup> Results are estimates of coefficients in eq(1). The sample is restricted to respondents who are age-eligible for SSS (i.e. aged 65 and above in 2016), live in public housing flats, and with a propensity score of 0.2 – 0.8.

<sup>4</sup> Mean and standard deviation statistics are based on pre-announcement levels of the dependent variable for respondents who received SSS payouts.

**Table 4: Heterogeneous effects of SSS payouts by financial preparation for retirement**

VARIABLES	(1) Life satis <sup>fn</sup>	(2) Social satis <sup>fn</sup>	(3) Daily activities satis <sup>fn</sup>	(4) HH income satis <sup>fn</sup>	(5) Economic satis <sup>fn</sup>	(6) Health satis <sup>fn</sup>	(7) Self-rated health cond
Received SS × announce-to-disb	0.164** (0.0687)	0.0459 (0.0599)	0.0171 (0.0599)	0.176** (0.0700)	0.123* (0.0629)	0.0653 (0.0676)	0.160** (0.0674)
Received SS × post-disb	0.257*** (0.0846)	0.0543 (0.0732)	0.0793 (0.0799)	0.246*** (0.0785)	0.226*** (0.0770)	0.130* (0.0721)	0.194*** (0.0733)
Received SS × announce-to-disb× retirement prep	-0.0372 (0.0234)	0.00876 (0.0221)	0.00254 (0.0207)	-0.0434* (0.0244)	-0.0271 (0.0224)	-0.0319 (0.0260)	-0.0671*** (0.0257)
Received SS × post-disb× retirement prep	-0.0793** (0.0310)	0.00722 (0.0274)	-0.0115 (0.0295)	-0.0631** (0.0299)	-0.0677** (0.0299)	-0.0434 (0.0273)	-0.0655** (0.0302)
Leads significant?	NO	NO	YES (MAR**)	NO	YES (MAR**)	NO	NO
Mean	3.51	3.65	3.52	3.18	3.19	3.26	2.52
S.D.	0.80	0.74	0.73	0.86	0.85	0.90	0.89
Observations	12,652	12,651	11,742	12,646	12,650	12,651	12,649
R-squared	0.675	0.659	0.691	0.689	0.686	0.728	0.712

Notes:

<sup>1</sup> Standard errors clustered at the household level in parentheses. \*\*\*, \*\* and \* represent statistical significance at the 1%, 5% and 10% level of significance respectively.

<sup>2</sup> Dependent variables shown at the top of each column are measured on a scale of 1 – 5. 1 represents the worst (“very dissatisfied” or “poor”) and 5 the best (“very satisfied” or “excellent”).

<sup>3</sup> Results are estimates of coefficients in eq(2), where announcement and disbursement variables are also interacted with one’s self-assessed financial preparedness for retirement, which is rated from 1(Poor) to 5(Excellent). The sample is restricted to respondents who are age-eligible for SSS (i.e. aged 65 and above in 2016), live in public housing flats, and with a propensity score of 0.2 – 0.8.

<sup>4</sup>Mean and standard deviation statistics are based on pre-announcement levels of the dependent variable for respondents who received SSS payouts.

**Table 5: Heterogeneous effects of SSS payouts by baseline assets quintile**

VARIABLES	(1) Life satis <sup>fn</sup>	(2) Social satis <sup>fn</sup>	(3) Daily activities satis <sup>fn</sup>	(4) HH income satis <sup>fn</sup>	(5) Economic satis <sup>fn</sup>	(6) Health satis <sup>fn</sup>	(7) Self-rated health cond
Received SS × announce-to-disb	0.0840 (0.0561)	0.0484 (0.0495)	0.0644 (0.0511)	0.0966* (0.0572)	0.0566 (0.0548)	0.0363 (0.0597)	0.0794 (0.0543)
Received SS × post-disb	0.0723 (0.0660)	0.0224 (0.0603)	0.0621 (0.0602)	0.0714 (0.0631)	0.0711 (0.0580)	0.0742 (0.0595)	0.130** (0.0651)
Received SS × announce-to-disb × assets quintile	0.00130 (0.0149)	0.00905 (0.0145)	-0.0131 (0.0148)	-0.00727 (0.0173)	0.00275 (0.0160)	-0.0147 (0.0189)	-0.0243 (0.0172)
Received SS × post-disb × assets quintile	0.00455 (0.0188)	0.0157 (0.0175)	-0.00118 (0.0187)	0.0117 (0.0185)	0.00362 (0.0164)	-0.0185 (0.0195)	-0.0326* (0.0193)
Leads significant?	NO	NO	YES (MAR**)	NO	YES (MAR**)	NO	NO
Mean	3.51	3.65	3.52	3.18	3.19	3.26	2.52
S.D.	0.80	0.74	0.73	0.86	0.85	0.90	0.89
Observations	11,893	11,892	11,092	11,887	11,892	11,892	11,892
R-squared	0.685	0.665	0.694	0.695	0.692	0.736	0.722

Notes:

<sup>1</sup> Standard errors clustered at the household level in parentheses. \*\*\*, \*\* and \* represent statistical significance at the 1%, 5% and 10% level of significance respectively.

<sup>2</sup> Dependent variables shown at the top of each column are measured on a scale of 1 – 5. 1 represents the worst (“very dissatisfied” or “poor”) and 5 the best (“very satisfied” or “excellent”).

<sup>3</sup> Results are estimates of coefficients in eq(2), where announcement and disbursement variables are also interacted with one’s net assets (captured before announcement and expressed in quintiles which are computed off respondents who are aged 65 in 2016 and who live in public housing flats). The sample is restricted to respondents who are age-eligible for SSS (i.e. aged 65 and above in 2016), live in public housing flats, and with a propensity score of 0.2 – 0.8. This sample is smaller because data on assets was collected only once before announcement, and not everyone responded in that wave.

<sup>4</sup>Mean and standard deviation statistics are based on pre-announcement levels of the dependent variable for respondents who received SSS payouts.

**Table 6: Effects of spousal receipt of SSS**

VARIABLES	(1) Life satis <sup>fn</sup>	(2) Social satis <sup>fn</sup>	(3) Daily activities satis <sup>fn</sup>	(4) HH income satis <sup>fn</sup>	(5) Economic satis <sup>fn</sup>	(6) Health satis <sup>fn</sup>	(7) Self-rated health cond
Only respondent rcv SS × announce-to-disb	0.139*** (0.0493)	0.105** (0.0484)	0.0695 (0.0564)	0.144** (0.0608)	0.112** (0.0554)	0.0933 (0.0631)	0.0118 (0.0628)
Only respondent rcv SS × post-disb	0.116* (0.0622)	0.127** (0.0569)	0.0571 (0.0674)	0.167*** (0.0625)	0.0853 (0.0644)	0.0832 (0.0737)	0.0682 (0.0708)
Only spouse rcv SS × announce-to-disb	0.0216 (0.0705)	0.0115 (0.0616)	0.0168 (0.0681)	0.0991 (0.0718)	0.118 (0.0766)	-0.0274 (0.0682)	-0.0940 (0.0758)
Only spouse rcv SS × post-disb	0.0226 (0.0729)	0.0490 (0.0636)	0.0380 (0.0765)	0.0101 (0.0915)	-0.00816 (0.0774)	-0.00773 (0.0799)	0.0348 (0.0754)
Both rcv SS × announce-to-disb	0.0827 (0.0583)	0.0877* (0.0484)	-0.00178 (0.0556)	0.0615 (0.0584)	0.0801 (0.0542)	-0.00784 (0.0542)	0.0191 (0.0497)
Both rcv SS × post-disb	0.146** (0.0661)	0.136** (0.0537)	0.0928 (0.0631)	0.134** (0.0597)	0.146*** (0.0556)	0.0407 (0.0562)	0.0775 (0.0555)
Leads significant?	NO	YES (MAR*)	NO	YES (FEB*)	NO	NO	NO
Mean	3.51	3.65	3.52	3.18	3.19	3.26	2.52
S.D.	0.80	0.74	0.73	0.86	0.85	0.90	0.89
Observations	8,701	8,700	8,080	8,695	8,699	8,702	8,698
R-squared	0.679	0.670	0.697	0.697	0.704	0.723	0.706

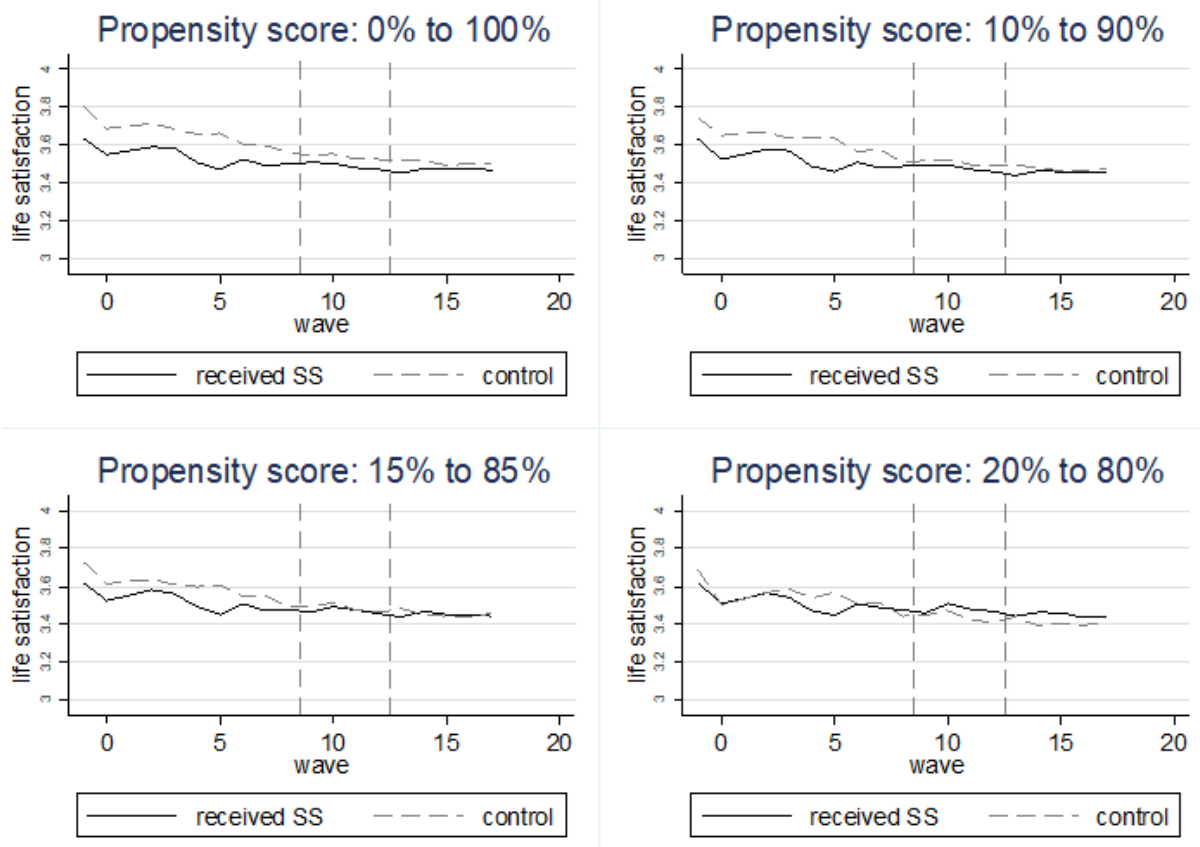
Notes:

<sup>1</sup> Standard errors clustered at the household level in parentheses. \*\*\*, \*\* and \* represent statistical significance at the 1%, 5% and 10% level of significance respectively.

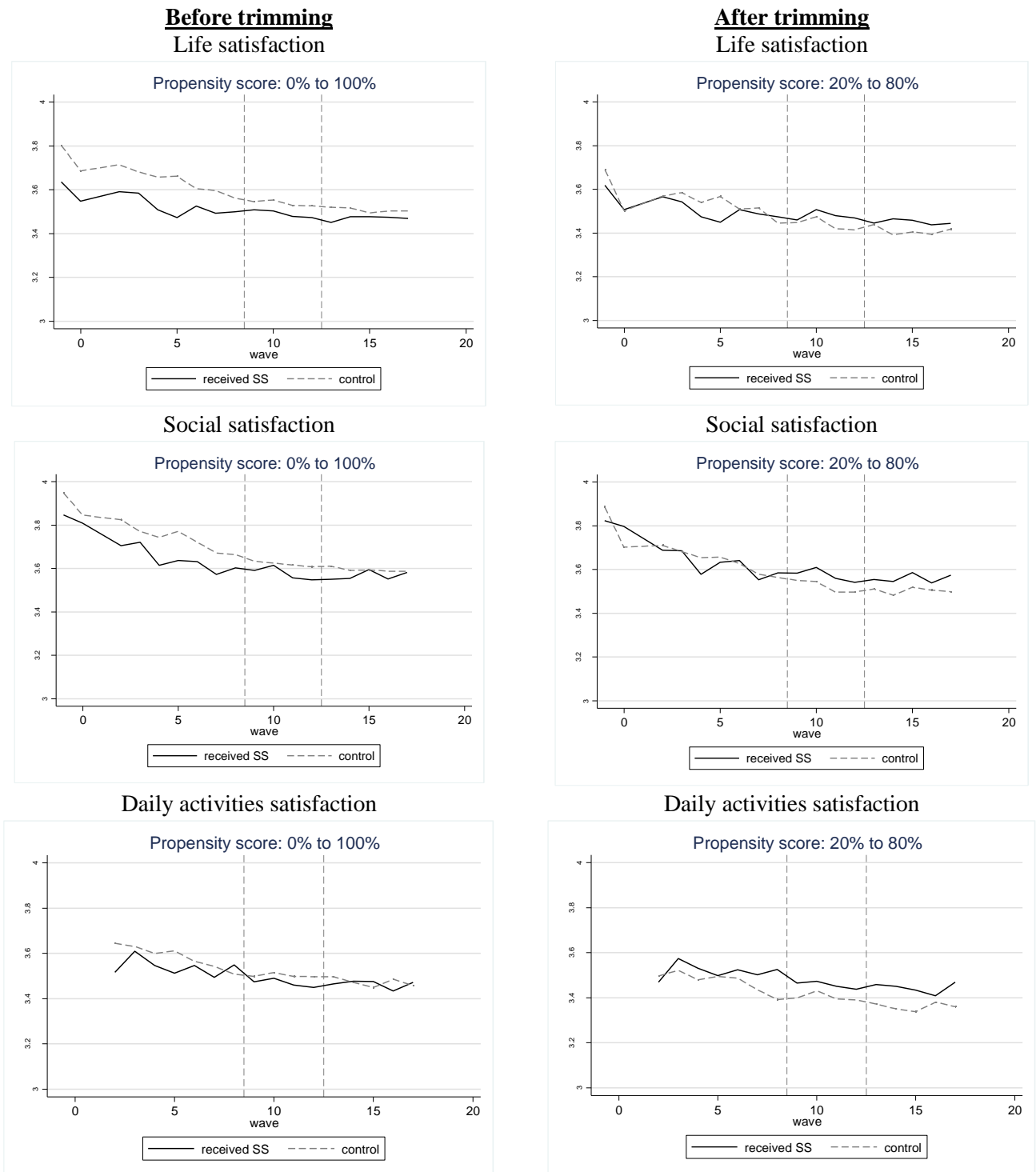
<sup>2</sup> Dependent variables shown at the top of each column are measured on a scale of 1 – 5. 1 represents the worst (“very dissatisfied” or “poor”) and 5 the best (“very satisfied” or “excellent”).

<sup>3</sup> Results are estimates of coefficients in eq(4), where announcement and disbursement variables are also interacted with the identity of recipient. The sample is restricted to respondents who are age-eligible for SSS (i.e. aged 65 and above in 2016), live in public housing flats, and with a propensity score of 0.2 – 0.8. This sample is smaller because only respondents who are married are included.

<sup>4</sup> Mean and standard deviation statistics are based on pre-announcement levels of the dependent variable for respondents who received SSS payouts.



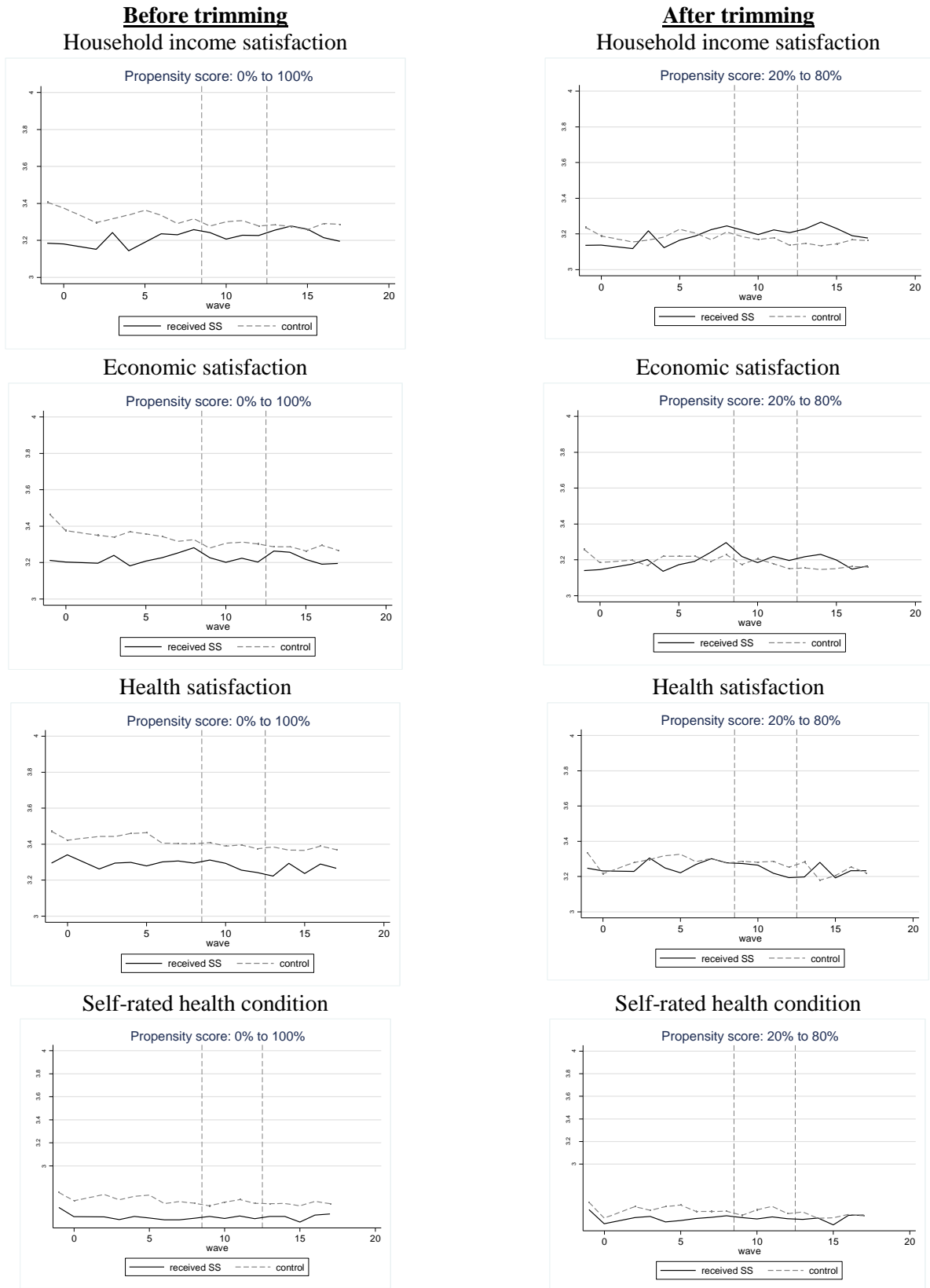
**Figure 1: Trends for life satisfaction, for samples trimmed based on increasingly narrower ranges of propensity score**



**Figure 2: Trends for life, social, and daily activities satisfaction<sup>39</sup>**

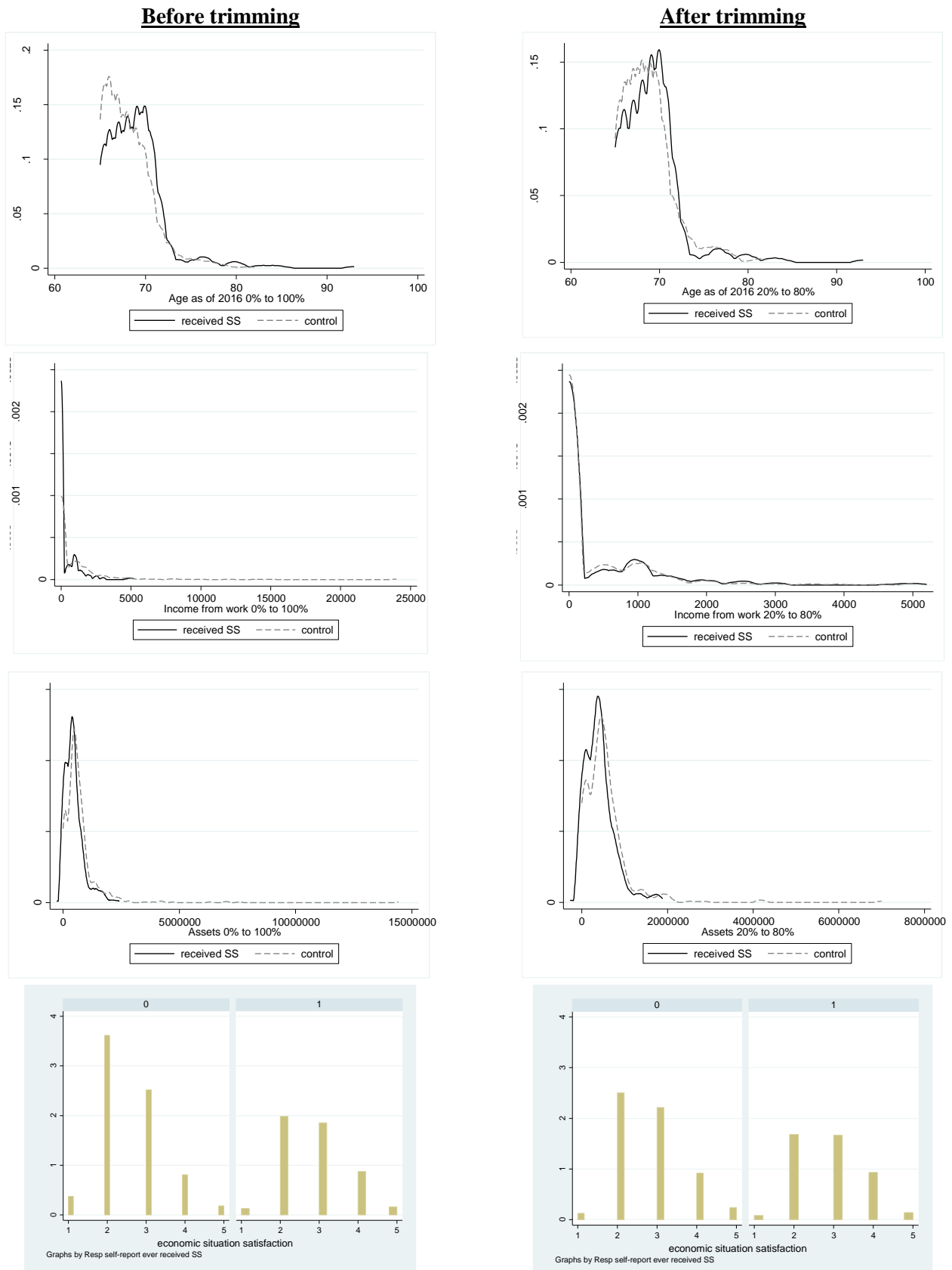
<sup>39</sup> The periods to the left of the first and second dotted vertical lines refer to the pre-announcement and pre-disbursement periods respectively.





**Figure 3: Trends for household income, economic, health satisfaction as well as health condition<sup>40</sup>**

<sup>40</sup> The periods to the left of the first and second dotted vertical lines refer to the pre-announcement and pre-disbursement periods respectively.



**Figure 4: Density plots / histograms of key demographics before (left panel) and after (right panel) trimming<sup>41</sup>**

<sup>41</sup> Note the change in scale of the horizontal axis after trimming.

## Appendix A

**Table A1: Summary statistics for baseline demographics**

	Trimmed sample (propensity score 0.10 – 0.90)						Trimmed sample (propensity score 0.15 – 0.85)					
	Received SS (N <sup>1</sup> =406)		No SS (N <sup>1</sup> =929)		Norm diff <sup>2</sup>	Diff in means <sup>3</sup>	Received SS (N <sup>1</sup> =369)		No SS (N <sup>1</sup> =739)		Norm diff <sup>2</sup>	Diff in means <sup>3</sup>
	Mean	SD	Mean	SD			Mean	SD	Mean	SD		
Age at 2016	68.93	3.37	68.40	2.72	0.17	0.52***	68.95	3.32	68.55	2.74	0.13	0.40**
Married	0.68	0.47	0.75	0.43	-0.16	-0.07***	0.67	0.47	0.74	0.44	-0.16	-0.07**
Male	0.38	0.49	0.48	0.50	-0.21	-0.10***	0.36	0.48	0.44	0.50	-0.15	-0.08**
Chinese	0.87	0.33	0.89	0.31	-0.06	-0.02	0.87	0.34	0.89	0.31	-0.08	-0.03
Malay	0.07	0.25	0.06	0.23	0.04	0.01	0.07	0.25	0.06	0.24	0.03	0.01
Indian	0.05	0.22	0.05	0.21	0.02	0.00	0.05	0.23	0.04	0.20	0.06	0.01
No formal schooling	0.17	0.38	0.14	0.35	0.10	0.04	0.18	0.38	0.15	0.36	0.06	0.02
Primary schooling	0.36	0.48	0.32	0.47	0.08	0.04	0.38	0.49	0.35	0.48	0.06	0.03
Secondary schooling	0.37	0.48	0.38	0.48	-0.02	-0.01	0.35	0.48	0.36	0.48	-0.02	-0.01
Lives in 1-room flat	0.05	0.22	0.02	0.13	0.20	0.04***	0.05	0.22	0.02	0.14	0.17	0.03**
Lives in 2-room flat	0.05	0.21	0.02	0.16	0.12	0.02*	0.05	0.22	0.03	0.16	0.11	0.02*
Lives in 3-room flat	0.34	0.47	0.25	0.43	0.20	0.09***	0.36	0.48	0.29	0.45	0.16	0.07**
Lives in 4-room flat	0.36	0.48	0.40	0.49	-0.09	-0.04	0.37	0.48	0.41	0.49	-0.08	-0.04
Lives in 5-room flat	0.15	0.36	0.23	0.42	-0.21	-0.08***	0.13	0.34	0.19	0.39	-0.17	-0.06***
Owns home	0.83	0.38	0.87	0.34	-0.11	-0.04*	0.82	0.38	0.86	0.35	-0.10	-0.04
No. of hh members	2.91	1.47	3.16	1.43	-0.17	-0.24***	2.85	1.42	3.12	1.42	-0.19	-0.27***
No. of total children	2.18	1.42	2.18	1.26	0.00	0.00	2.19	1.45	2.22	1.29	-0.03	-0.03
Retirement preparedness <sup>4</sup>	2.15	0.91	2.26	0.85	-0.13	-0.11**	2.12	0.89	2.23	0.86	-0.12	-0.10*

Notes:

<sup>1</sup> N refers to number of respondents.

<sup>2</sup> Normalised differences are computed as in Imbens (2015) (as the difference in means standardised by the square root of the mean variance of both groups)

<sup>3</sup> \*\*\*, \*\*, \* represent statistical significance at the 10%, 5% and 1% levels respectively

<sup>4</sup> This is a self-assessment on preparedness for retirement, captured on a scale of 1 to 5, with a higher value representing greater preparedness. This was captured during the baseline survey, which was conducted before the announcement of details on the Silver Support Scheme.

**Table A2: Silver Support payouts and subjective well-being – heterogeneous effects by flat-type and financial preparedness for retirement**

VARIABLES	(1) Life satis <sup>fn</sup>	(2) Social satis <sup>fn</sup>	(3) Daily activities satis <sup>fn</sup>	(4) HH income satis <sup>fn</sup>	(5) Economic satis <sup>fn</sup>	(6) Health satis <sup>fn</sup>	(7) Self-rated health cond
Received SS × ann-to- disb × 1/2 rm × retirement prep	-0.103* (0.0556)	-0.0445 (0.120)	0.0342 (0.0519)	0.0312 (0.128)	0.0600 (0.0980)	-0.160 (0.125)	-0.0596 (0.0735)
Received SS × ann-to- disb × 3 rm × retirement prep	-0.0301 (0.0363)	-0.00160 (0.0353)	0.0371 (0.0418)	-0.0676* (0.0364)	-0.0445 (0.0386)	-0.0596 (0.0534)	-0.143*** (0.0472)
Received SS × ann-to- disb × 4 rm × retirement prep	-0.0868** (0.0405)	0.00709 (0.0397)	-0.0593* (0.0349)	-0.0917** (0.0447)	-0.0573 (0.0455)	0.0361 (0.0338)	-0.00271 (0.0506)
Received SS × ann-to- disb × 5 rm × retirement prep	0.0663 (0.0666)	0.0496 (0.0477)	0.0639 (0.0392)	0.0330 (0.0469)	0.000767 (0.0414)	0.0202 (0.0411)	-0.0171 (0.0284)
Received SS × post- disb × 1/2 rm × retirement prep	-0.173* (0.0991)	-0.0415 (0.117)	0.0505 (0.0873)	-0.0343 (0.102)	-0.0397 (0.0778)	-0.219** (0.101)	-0.133 (0.106)
Received SS × post- disb × 3 rm × retirement prep	-0.0796* (0.0467)	-0.0646 (0.0472)	0.0262 (0.0458)	-0.116** (0.0452)	-0.126** (0.0493)	-0.0529 (0.0435)	-0.125** (0.0489)
Received SS × post- disb × 4 rm × retirement prep	-0.116** (0.0499)	0.0363 (0.0470)	-0.0966** (0.0468)	-0.117** (0.0467)	-0.107** (0.0499)	-0.00333 (0.0468)	-0.0288 (0.0553)
Received SS × post- disb × 5 rm × retirement prep	0.0645 (0.0681)	0.0640 (0.0534)	0.137*** (0.0528)	0.111 (0.0752)	0.0982 (0.0676)	0.0549 (0.0591)	-0.00343 (0.0689)
Leads significant?	NO	NO	YES (MAR**)	NO	YES (MAR**)	NO	NO
Mean	3.51	3.65	3.52	3.18	3.19	3.26	2.52
S.D.	0.80	0.74	0.73	0.86	0.85	0.90	0.89
Observations	12,652	12,651	11,742	12,646	12,650	12,651	12,649
R-squared	0.677	0.660	0.692	0.689	0.687	0.728	0.713

Notes:

<sup>1</sup> Standard errors clustered at the household level in parentheses. \*\*\*, \*\* and \* represent statistical significance at the 1%, 5% and 10% level of significance respectively.

<sup>2</sup> Dependent variables shown at the top of each column are measured on a scale of 1 – 5. 1 represents the worst (“very dissatisfied” or “poor”) and 5 the best (“very satisfied” or “excellent”).

<sup>3</sup> Results are estimates of coefficients in eq(3), where announcement and disbursement variables are interacted not only with one’s self-assessed financial preparedness for retirement (which is rated from 1(Poor) to 5(Excellent)) but also flat-type . The sample is restricted to respondents who are age-eligible for SSS (i.e. aged 65 and above in 2016), live in public housing flats, and with a propensity score of 0.2 – 0.8.

<sup>4</sup>Mean and standard deviation statistics are based on pre-announcement levels of the dependent variable for respondents who received SSS payouts.

**Table A3: Robustness checks for overall life satisfaction**

VARIABLES	(1) No leads	(2) Balanced	(3) Ethnic trends	(4) Flattype trends	(5) WIS/ GST	(6) Abadie	(7) 1-1 matching
Received SS × Jan	-	0.0360 (0.0422)	0.0376 (0.0422)	0.0294 (0.0417)	0.0274 (0.0467)	0.0283 (0.0345)	-
Received SS × Feb	-	0.0111 (0.0435)	0.0147 (0.0438)	0.00407 (0.0436)	0.0158 (0.0468)	-0.0281 (0.0399)	-
Received SS × Mar	-	0.0645 (0.0437)	0.0691 (0.0443)	0.0735* (0.0430)	0.0499 (0.0471)	0.0375 (0.0368)	-
Received SS × Jan - Mar	-	-	-	-	-	-	0.0201 P=0.58
Received SS × announce-to-disb	0.0685** (0.0289)	0.0819** (0.0359)	0.0841** (0.0363)	0.0729** (0.0352)	0.0911** (0.0412)	0.0460 (0.0301)	0.0530 P=0.11
Received SS × post-disb	0.0719** (0.0345)	0.0824** (0.0409)	0.0858** (0.0413)	0.0762* (0.0401)	0.0938** (0.0475)	0.0831** (0.0343)	0.0681* P=0.07
Observations	12,652	12,519	12,652	12,652	9,959	1,592	648
R-squared	0.675	0.679	0.677	0.678	0.693	-	-

Notes:

<sup>1</sup> Standard errors (SE) clustered at the household level in parentheses for columns (1) – (6); SE adjusted to account for uncertainty from propensity score estimation as in Abadie (2005) for column (7); p-values from a permutation test in parentheses in column (8). \*\*\*, \*\* and \* represent statistical significance at the 1%, 5% and 10% level of significance respectively.

<sup>2</sup> The dependent variable is measured on a scale of 1 – 5. 1 represents the worst (“very dissatisfied”) and 5 the best (“very satisfied”).

<sup>3</sup> Columns (1) – (7) show results from additional robustness checks carried out. These checks are: (1) removal of the lead terms; (2) restricting the sample to a “balanced” panel, where each individual has at least one observation in the pre-announcement, announcement-to-disbursement, and post-disbursement periods; (3) allowing for ethnicity-specific time fixed effects; (4) allowing for flat-type-specific time fixed effects; (5) adding receipt of additional welfare payments (Workfare Income Supplement; GST Vouchers) as a control—sample is smaller because this data is not collected every wave and not everyone responds every wave; (6) reweighting each observation by their propensity of receiving SSS as in Abadie (2005); (7) DiD matching with a 1-1 nearest neighbour match. Eq(1) describes the baseline model used in these checks. The sample is restricted to those age-eligible for the Silver Support Scheme (i.e. aged 65 and above) as of end-Sep 2016 (the month of the most recent Silver Support payout in our data), and with a propensity score of 0.2 – 0.8 for checks in columns (1) – (5). The full/untrimmed sample is used in column (6). The number of observations in columns (1) – (5) refer to the number of individual-month observations; those in columns (6) and (7) refer to number of respondents.

**Table A4: Robustness checks for social and family life satisfaction**

VARIABLES	(1) No leads	(2) Balanced	(3) Ethnic trends	(4) Flattype trends	(5) WIS/ GST	(6) Abadie	(7) 1-1 matching
Received SS × Jan	-	0.0322 (0.0398)	0.0296 (0.0403)	0.0345 (0.0406)	0.0263 (0.0426)	0.00710 (0.0337)	-
Received SS × Feb	-	-0.0125 (0.0428)	-0.0170 (0.0423)	-0.00839 (0.0434)	0.00159 (0.0455)	-0.0488 (0.0404)	-
Received SS × Mar	-	0.0153 (0.0410)	0.0113 (0.0407)	0.0223 (0.0401)	-0.00389 (0.0438)	-0.0129 (0.0353)	-
Received SS × Jan - Mar	-	-	-	-	-	-	-0.0254 P=0.44
Received SS × announce-to-disb	0.0581** (0.0264)	0.0632* (0.0329)	0.0594* (0.0328)	0.0570* (0.0328)	0.0667* (0.0367)	0.00651 (0.0299)	0.0141 P=0.62
Received SS × post-disb	0.0633** (0.0312)	0.0675* (0.0366)	0.0633* (0.0368)	0.0627* (0.0362)	0.0498 (0.0420)	0.0523* (0.0313)	0.0458 P=0.17
Observations	12,651	12,518	12,651	12,651	9,959	1,592	648
R-squared	0.659	0.661	0.661	0.662	0.677	-	-

Notes:

<sup>1</sup> Standard errors (SE) clustered at the household level in parentheses for columns (1) – (6); SE adjusted to account for uncertainty from propensity score estimation as in Abadie (2005) for column (7); p-values from a permutation test in parentheses in column (8). \*\*\*, \*\* and \* represent statistical significance at the 1%, 5% and 10% level of significance respectively.

<sup>2</sup> The dependent variable is measured on a scale of 1 – 5. 1 represents the worst (“very dissatisfied”) and 5 the best (“very satisfied”).

<sup>3</sup> Columns (1) – (7) show results from additional robustness checks carried out. These checks are: (1) removal of the lead terms; (2) restricting the sample to a “balanced” panel, where each individual has at least one observation in the pre-announcement, announcement-to-disbursement, and post-disbursement periods; (3) allowing for ethnicity-specific time fixed effects; (4) allowing for flat-type-specific time fixed effects; (5) adding receipt of additional welfare payments (Workfare Income Supplement; GST Vouchers) as a control—sample is smaller because this data is not collected every wave and not everyone responds every wave; (6) reweighting each observation by their propensity of receiving SSS as in Abadie (2005); (7) DiD matching with a 1-1 nearest neighbour match. Eq(1) describes the baseline model used in these checks. The sample is restricted to those age-eligible for the Silver Support Scheme (i.e. aged 65 and above) as of end-Sep 2016 (the month of the most recent Silver Support payout in our data), and with a propensity score of 0.2 – 0.8 for checks in columns (1) – (5). The full/untrimmed sample is used in column (6). The number of observations in columns (1) – (5) refer to the number of individual-month observations; those in columns (6) and (7) refer to number of respondents.

**Table A5: Robustness checks for household income satisfaction**

VARIABLES	(1) No leads	(2) Balanced	(3) Ethnic trends	(4) Flattype trends	(5) WIS/ GST	(6) Abadie	(7) 1-1 matching
Received SS × Jan	-	0.00623 (0.0413)	0.00205 (0.0419)	0.0122 (0.0412)	-0.0286 (0.0454)	0.0394 (0.0350)	-
Received SS × Feb	-	0.0714 (0.0491)	0.0621 (0.0490)	0.0789* (0.0473)	0.0672 (0.0521)	0.0484 (0.0393)	-
Received SS × Mar	-	0.0681 (0.0455)	0.0579 (0.0454)	0.0718 (0.0449)	0.0580 (0.0499)	0.0675* (0.0388)	-
Received SS × Jan - Mar	-	-	-	-	-	-	0.0324 P=0.36
Received SS × announce-to-disb	0.0654** (0.0304)	0.0865** (0.0369)	0.0753** (0.0371)	0.0823** (0.0367)	0.0931** (0.0405)	0.0623* (0.0321)	0.0712** P=0.05
Received SS × post-disb	0.0929*** (0.0333)	0.114*** (0.0393)	0.106*** (0.0392)	0.105*** (0.0389)	0.122*** (0.0437)	0.109*** (0.0332)	0.102*** P=0.01
Observations	12,646	12,506	12,646	12,646	9,953	1,590	648
R-squared	0.688	0.691	0.691	0.691	0.710	-	-

Notes:

<sup>1</sup> Standard errors (SE) clustered at the household level in parentheses for columns (1) – (6); SE adjusted to account for uncertainty from propensity score estimation as in Abadie (2005) for column (7); p-values from a permutation test in parentheses in column (8). \*\*\*, \*\* and \* represent statistical significance at the 1%, 5% and 10% level of significance respectively.

<sup>2</sup> The dependent variable is measured on a scale of 1 – 5. 1 represents the worst (“very dissatisfied”) and 5 the best (“very satisfied”).

<sup>3</sup> Columns (1) – (7) show results from additional robustness checks carried out. These checks are: (1) removal of the lead terms; (2) restricting the sample to a “balanced” panel, where each individual has at least one observation in the pre-announcement, announcement-to-disbursement, and post-disbursement periods; (3) allowing for ethnicity-specific time fixed effects; (4) allowing for flat-type-specific time fixed effects; (5) adding receipt of additional welfare payments (Workfare Income Supplement; GST Vouchers) as a control—sample is smaller because this data is not collected every wave and not everyone responds every wave; (6) reweighting each observation by their propensity of receiving SSS as in Abadie (2005); (7) DiD matching with a 1-1 nearest neighbour match. Eq(1) describes the baseline model used in these checks. The sample is restricted to those age-eligible for the Silver Support Scheme (i.e. aged 65 and above) as of end-Sep 2016 (the month of the most recent Silver Support payout in our data), and with a propensity score of 0.2 – 0.8 for checks in columns (1) – (5). The full/untrimmed sample is used in column (6). The number of observations in columns (1) – (5) refer to the number of individual-month observations; those in columns (6) and (7) refer to number of respondents.

**Table A6: Robustness checks for economic satisfaction**

VARIABLES	(1) No leads	(2) Balanced	(3) Ethnic trends	(4) Flattype trends	(5) WIS/ GST	(6) Abadie	(7) 1-1 matching
Received SS × Jan	-	-0.00844 (0.0412)	-0.00825 (0.0416)	-0.00151 (0.0421)	-0.0306 (0.0454)	0.0241 (0.0396)	-
Received SS × Feb	-	0.0610 (0.0475)	0.0517 (0.0480)	0.0588 (0.0470)	0.0455 (0.0515)	0.0382 (0.0412)	-
Received SS × Mar	-	0.106** (0.0468)	0.102** (0.0477)	0.108** (0.0465)	0.119** (0.0523)	0.0691* (0.0394)	-
Received SS × Jan - Mar	-	-	-	-	-	-	0.0306 P=0.40
Received SS × announce-to-disb	0.0443 (0.0281)	0.0665* (0.0350)	0.0601* (0.0352)	0.0588* (0.0348)	0.0754* (0.0399)	0.0267 (0.0307)	0.0454 P=0.21
Received SS × post-disb	0.0608* (0.0320)	0.0854** (0.0383)	0.0787** (0.0383)	0.0706* (0.0380)	0.105** (0.0425)	0.0650* (0.0332)	0.0768** P=0.05
Observations	12,650	12,517	12,650	12,650	9,959	1,592	648
R-squared	0.685	0.688	0.687	0.689	0.706	-	-

Notes:

<sup>1</sup> Standard errors (SE) clustered at the household level in parentheses for columns (1) – (6); SE adjusted to account for uncertainty from propensity score estimation as in Abadie (2005) for column (7); p-values from a permutation test in parentheses in column (8). \*\*\*, \*\* and \* represent statistical significance at the 1%, 5% and 10% level of significance respectively.

<sup>2</sup> The dependent variable is measured on a scale of 1 – 5. 1 represents the worst (“very dissatisfied”) and 5 the best (“very satisfied”).

<sup>3</sup> Columns (1) – (7) show results from additional robustness checks carried out. These checks are: (1) removal of the lead terms; (2) restricting the sample to a “balanced” panel, where each individual has at least one observation in the pre-announcement, announcement-to-disbursement, and post-disbursement periods; (3) allowing for ethnicity-specific time fixed effects; (4) allowing for flat-type-specific time fixed effects; (5) adding receipt of additional welfare payments (Workfare Income Supplement; GST Vouchers) as a control—sample is smaller because this data is not collected every wave and not everyone responds every wave; (6) reweighting each observation by their propensity of receiving SSS as in Abadie (2005); (7) DiD matching with a 1-1 nearest neighbour match. Eq(1) describes the baseline model used in these checks. The sample is restricted to those age-eligible for the Silver Support Scheme (i.e. aged 65 and above) as of end-Sep 2016 (the month of the most recent Silver Support payout in our data), and with a propensity score of 0.2 – 0.8 for checks in columns (1) – (5). The full/untrimmed sample is used in column (6). The number of observations in columns (1) – (5) refer to the number of individual-month observations; those in columns (6) and (7) refer to number of respondents.



**Table A7: Overall impact of the Silver Support Scheme on subjective well-being for full sample**

VARIABLES	(1) Life satis <sup>fn</sup>	(2) Social satis <sup>fn</sup>	(3) Daily activities satis <sup>fn</sup>	(4) HH income satis <sup>fn</sup>	(5) Economic satis <sup>fn</sup>	(6) Health satis <sup>fn</sup>	(7) Self-rated health cond
Received SS × Jan	0.0470 (0.0329)	0.00342 (0.0325)	0.0390 (0.0323)	0.0457 (0.0324)	0.0288 (0.0343)	0.0120 (0.0348)	-0.00775 (0.0356)
Received SS × Feb	0.0149 (0.0348)	-0.0181 (0.0333)	-0.00302 (0.0366)	0.0682* (0.0377)	0.0645* (0.0379)	0.0430 (0.0372)	0.00273 (0.0362)
Received SS × Mar	0.0582* (0.0350)	0.00581 (0.0340)	0.0821** (0.0342)	0.0886** (0.0363)	0.105*** (0.0389)	0.0347 (0.0407)	0.0401 (0.0364)
Received SS × announce-to-disb	0.0875*** (0.0291)	0.0462* (0.0275)	0.0247 (0.0276)	0.0957*** (0.0289)	0.0712** (0.0295)	0.0185 (0.0298)	0.0252 (0.0283)
Received SS × post-disb	0.102*** (0.0332)	0.0662** (0.0300)	0.0559* (0.0310)	0.133*** (0.0315)	0.113*** (0.0315)	0.0288 (0.0314)	0.0456 (0.0313)
Mean	3.51	3.65	3.52	3.18	3.19	3.26	2.52
S.D.	0.80	0.74	0.73	0.86	0.85	0.90	0.89
Observations	23,665	23,663	21,983	23,655	23,657	23,662	23,658
R-squared	0.684	0.658	0.693	0.707	0.698	0.720	0.724

Notes:

<sup>1</sup> Standard errors clustered at the household level in parentheses. \*\*\*, \*\* and \* represent statistical significance at the 1%, 5% and 10% level of significance respectively.

<sup>2</sup> Dependent variables shown at the top of each column are measured on a scale of 1 – 5. 1 represents the worst (“very dissatisfied” or “poor”) and 5 the best (“very satisfied” or “excellent”).

<sup>3</sup> Results are estimates of coefficients in eq(1). The sample is restricted to respondents who are age-eligible for SSS (i.e. aged 65 and above in 2016), live in public housing flats. This sample is not trimmed based on propensity score.

<sup>4</sup> Mean and standard deviation statistics are based on pre-announcement levels of the dependent variable for respondents who received SSS payouts.