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RDP 2009-07

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Research Discussion Paper
2009-07

November 2009

Economic Research Department
Reserve Bank of Australia

We would like to thank Christopher Kent, Adrian Pagan, Robert Breunig, Henry Siu, Nir Jaimovich and Tim Robinson for their feedback and discussions. Responsibility for any remaining errors rests with the authors. The views expressed in this paper are those of the authors and are not necessarily those of the Reserve Bank of Australia.

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Abstract

This paper uses micro data from the Household, Income and Labour Dynamics in Australia Survey to estimate the marginal propensity to consume. Estimates are made by examining two types of policy changes – to income tax rates and lump-sum transfers – which help to identify the effect of shocks to income on consumption. A standard Euler equation for consumption is used to analyse the effect of income tax changes. The marginal propensity to consume out of lump-sum transfers is estimated using fixed effects and propensity score matching. In addition, the paper examines differences in the marginal propensity to consume across households according to measures of liquidity constraints and unemployment risks.

JEL Classification Numbers: C23, D12, H31
Keywords: marginal propensity to consume, panel data, HILDA

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1. Introduction

The response of household consumption to changes in income – the marginal propensity to consume (MPC) – influences how the macroeconomy responds to various shocks. Under the permanent income hypothesis (PIH), the change in consumption in response to a shock to income depends on the nature of the shock. For permanent changes in income, households that are not liquidity constrained can in theory be expected to adjust their consumption by close to the full amount of the shock, and do so once the change is apparent rather than waiting for the extra income to arrive. When income shocks are temporary, households are likely to alter consumption by some fraction of the change in income, at the time it becomes apparent, as they attempt to smooth their consumption over time. Of course, if the income tax changes and the lump-sum transfers have the same present discounted value and households are not liquidity constrained, their effect on aggregate consumption could be expected to be similar even though the MPC from each will differ. In practice, however, at least some households are likely to face borrowing constraints, which by itself would tend to increase the correlation between household consumption and current income.

In this paper, we exploit the rich dataset provided by the Household, Income and Labour Dynamics in Australia (HILDA) Survey to estimate the marginal propensities to consume in Australia between 2005 and 2007 from two types of policy changes: changes in income tax rates and changes in lump-sum transfers to households. Changes in income tax rates are often viewed as persistent (if not permanent) by households, so we would expect households to have a high MPC out of income tax cuts. In contrast, lump-sum transfers to households are often one-off or temporary measures, so we would expect a somewhat lower MPC out of these payments.

Fiscal policy changes provide a good means of estimating MPCs since they are easily identifiable, and from the perspective of households can be considered to be exogenous. Also, such policies are often directly targeted to subsets of the population or differentially affect population subgroups, generating natural control groups to aid estimation.

Despite widespread interest in the United States and elsewhere in these issues, there has been little work done on estimating MPCs in Australia using micro data. This largely reflects data constraints; panel data on income were not available until the HILDA Survey began in 2001, and data on broad expenditure categories were not collected until 2005. Other (purely cross-sectional) expenditure surveys conducted by the Australian Bureau of Statistics (ABS) are available only infrequently.

We utilise the variation in the effect of the different fiscal policies across households to help identify the MPC out of these policies. In particular, we estimate the MPC out of income tax cuts by using a panel Euler equation linking consumption to estimates of income tax paid and various control variables (adapting the methods used by Souleles 2002 and Johnson, Parker and Souleles 2006). In the case of targeted lump-sum transfers, the Baby Bonus and the Carer Bonus, we estimate the MPC by comparing expenditure for those households that received the payment with expenditure for households that did not receive the payment but were otherwise similar in some key respects. For the Baby Bonus, the source of identification is a change in the value of the payment in the sample of households that qualified for the Baby Bonus. For the Carer Bonus, we do not have variation in the lump-sum transfer over time so our comparison group (non-recipient households) is chosen using propensity score matching (as in Brzozowski 2007). This is intended to ensure that those in the comparison group who do not receive the payment had similar characteristics to payment recipients.

Our results, though tentative due to data limitations, show that there is a wide range of estimated MPCs in Australia. The estimated MPC for non-durable goods and services out of income tax cuts, over 2005 to 2007, is relatively high within about three months of receipt. The estimated MPC for non-durables out of the Baby Bonus is lower but suffers from some issues with identification. We also find that the MPC estimates vary across different households. Households with

low incomes have a higher MPC, consistent with the idea that these households are more likely to face liquidity constraints. We also examine how variations in financial stress, debt and perceptions of unemployment risk across households affect the estimates of the MPC.

The rest of the paper proceeds as follows. Section 2 explores how this study complements the existing literature. Section 3 describes the HILDA Survey and the fiscal policies examined. The empirical methodology is outlined in Section 4. Sections 5, 6 and 7 report the results for the basic specifications as well as those exploring heterogeneity in households' responses. Finally, Section 8 concludes.

2. Related Literature

Most estimates of the MPC are by-products of tests of the excess sensitivity of consumption to changes in predictable income, in other words of tests of the permanent income hypothesis (PIH). Historically, these tests were conducted using the time-series properties of the consumption Euler equation (as in Hall 1982, and in the Australian context in McKibbin and Richards 1988). However, there are difficulties in estimating the causal effect of a change in income on consumption, in part reflecting the difficulty distinguishing between income changes and other macroeconomic factors. This has led to a greater focus on the use of micro data to both test the PIH and estimate MPCs. Micro data has also allowed analysis of heterogeneity in households' responses, particularly in response to liquidity constraints.

A series of recent studies use the detailed data available in the US Consumer Expenditure Survey (CEX) in order to estimate the effect on consumption of predicted changes in income (see, for example, Souleles 2002 and Johnson *et al* 2006). The CEX is well suited to such a task; the survey contains comprehensive expenditure data, it is conducted monthly and, although households move in and out of the survey, they are interviewed every three months for up to a year. These studies focus on the effect of anticipated fiscal policies on expenditure at the time when households receive additional income. Souleles (2002) examines the effect of the Reagan tax cuts in the United States in

the early 1980s and finds that household expenditure increased in the quarter after taxes were actually cut even though these tax cuts had been pre-announced, with an MPC of between 0.6 and 0.9. Johnson *et al* (2006) exploit the random timing of the distribution of the 2001 tax rebates and find that households, on average, had an MPC of 0.2 to 0.4 out of the rebate income. The authors also find an important role for liquidity constraints; households with low income or low assets spent a significantly greater share of their rebates.

While each of the above studies formally reject the PIH, the results are consistent with consumers treating temporary and permanent shocks differently: consuming more out of permanent than out of transitory changes. These studies also shed light on how different income changes affect different types of expenditure. Souleles (1999) finds that lump-sum increases in income are largely spent on durable purchases, while Parker (1999) shows that a large share of (permanent) changes in net pay received continuously is spent on non-durables.

Using a noticeably different data source (which complicates a direct comparison of the results to those of the US studies), this paper applies some of these same techniques to the Australian context. We examine the effect of both tax cuts and specific lump-sum transfers – the Baby Bonus and the Carer Bonus – on consumption, and explore the role of liquidity and financial constraints.[1] In order to provide some insights into how MPCs might potentially change across the economic cycle, we also explore how differences in perceptions of risks to income across households affect MPCs.

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[1] With the exception of Johnson *et al* (2006), which relies on the random timing of the delivery of the tax rebates, all of the above studies assume that the income change is uncorrelated with unobserved determinants of consumption. While this may be a reasonable assumption when the fiscal policies affect a broad cross-section of the population, it is less likely to hold when fiscal policies are targeted towards specific groups in the population, since it will be difficult to distinguish between the effect of the unobserved characteristics of these groups on consumption and the effect of fiscal policies. Hence, for the Carer Bonus we use propensity score matching (see, for example, Brzozowski 2007) to identify a control group that did not receive the payment but otherwise had similar characteristics.

3. Data

3.1 The HILDA Survey

The HILDA Survey, which is conducted annually, collects income, expenditure and other demographic information about a panel of Australian households. While the first wave was gathered in 2001/02, comprehensive expenditure data are currently only available for Waves 5–7. Thus our sample starts in the 2005/06 financial year (Wave 5) and ends in the 2007/08 financial year (Wave 7). The balanced panel for Waves 5–7 consists of 6 392 households and 10 023 individuals. The vast majority of the households (90 per cent) are surveyed between August and October, while the remaining 10 per cent are surveyed before March of the following year.

Following other studies (such as Johnson *et al* 2006) we focus on non-durable expenditure (for a precise definition see below). Provided consumption of non-durables and durables are additively separable in the utility function, this will provide an accurate measure of the MPC on non-durables. We examine two classifications of non-durable expenditure.[2] The first is a broad measure, which we call 'non-durables' (see Table A1). The second measure, 'strictly non-durable' expenditure, excludes the semi-durable categories of clothing and footwear, education, and motor vehicle repairs and maintenance following Lusardi (1996) (Column (I) of Table A1).

The HILDA expenditure data are collected using a self-completion recall survey. HILDA respondents are asked 'on average how much do you usually spend on' various expenditure categories, with the reference period varying from one week to a whole year depending on the expenditure category. In general, the expenditure categories that are more non-durable in nature have a reference period of one week, with the reference period increasing the more durable the good or service. These estimates are then converted into an annual estimate of expenditure for each category. The nature of the expenditure questions are thus substantially different to those in other expenditure surveys, most notably the CEX, where actual expenditures are recorded. This difference makes our results a little more

[2] There are 14 broad non-durable expenditure categories available in the HILDA Survey across all three waves (for details see Table A1). A similar set of non-durable categories represent around half of the value of total household consumption in the national accounts.

difficult to interpret, and makes it hard to compare our results directly to others in the literature. By asking about 'usual' rather than actual expenditure over a given period, households may be more likely to omit spending out of temporary transfers or more recent spending from their responses. By itself, this is likely to lead to an underestimate of the true effect of temporary transfers on consumption.

The frequency of the HILDA Survey also has important implications for the interpretation of our results. The annual frequency of the survey makes it almost impossible for our tax cut results to be interpreted as a test of the permanent income hypothesis. Other studies test the PIH by examining whether consumption responds to predicted changes in income. For example, Johnson *et al* (2006) and Souleles (2002) use data that capture the increase in consumption at the time of the actual increase in current income (and so can be used to test the PIH which predicts no change in consumption at the time, in the absence of liquidity constraints). However, the tax cuts we examine are announced and implemented between surveys, which implies that our estimates of the MPC are capturing the full effect of a policy change on consumption. In contrast, the Baby Bonus changes are announced prior to the beginning of our sample period. We thus only capture the response of consumption to an actual change in current income, enabling the Baby Bonus estimation to be used as a test of the PIH.

Each year, the HILDA Survey collects two different types of income data for each individual, in both gross and net terms. These are substantially different in terms of their scope and timing. The first type is a comprehensive measure of income received over the previous financial year. The second type is more limited in scope and is intended to measure current income as at the time of the survey (although the exact reference period is chosen by the respondents). We use previous financial year income (that is, for example, the 2006/07 financial year income collected in interviews conducted during 2007/08) to estimate the size of tax reductions. This more comprehensive measure is likely to be a better measure of annual income.[3] The financial year income is the sum of wages and salaries, government transfers, and investment and business income. Figure 1 shows the periods when the HILDA Surveys were conducted and the financial year income available from each wave.

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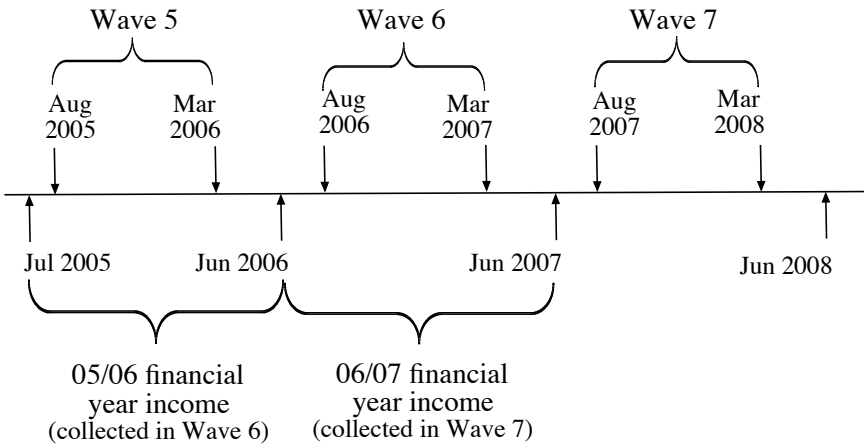
[3] Current income measures (for example, 2006/07 income collected in Wave 6) tend to be a poor proxy for the financial year income of that same year reported in the following year's survey (for 2006/07 financial year income collected in 2007/08).

Figure 1: HILDA Survey Timeline



3.2 Income Tax Changes

Between 2005/06 and 2007/08, the Federal Government introduced changes to the personal income tax scale, lifting selected income thresholds and lowering marginal tax rates. The majority of the tax cuts in our sample were announced in May of the year they were introduced.[4] However, given the recent history of tax cuts and the economic environment at the time, it is possible that some form of tax cuts were anticipated prior to their announcement. The changes were part of a run of consecutive tax cuts from 2003/04 to 2009/10 following fiscal surpluses from 2002/03 to 2007/08 and the retirement of Federal Government debt (net public debt was around zero in 2006). These economic factors are likely to have created an expectation that the tax changes were persistent, if not permanent.[5] The tax rates relevant to our sample period are shown in Table 1 with the changes marked in bold.

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4 The changes to the income thresholds in 2005/06, however, were announced in the year prior.

5 The fiscal surpluses partly reflected the high terms of trade during this period. If this was a temporary phenomenon, then fully rational households would have expected that the tax cuts were also temporary. However, we think that expectations of persistent or permanent tax cuts are probably realistic at the individual household level on average given the uncertainty around the shift in the terms of trade.

Table 1: Personal Income Tax Scale

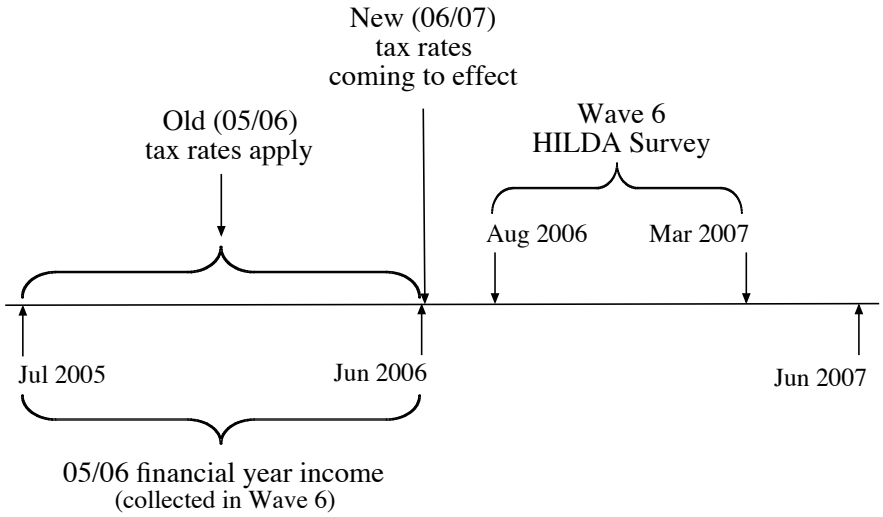
	Income range Dollars	Marginal tax rate
2004/05	0–6 000	0.00
	6 001–21 600	0.17
	21 601–58 000	0.30
	58 001–70 000	0.42
	70 001 and over	0.47
2005/06	0–6 000	0.00
	6 001–21 600	**0.15**
	21 601–**63 000**	0.30
	63 001–95 000	0.42
	95 001 and over	0.47
2006/07	0–6 000	0.00
	6 001–**25 000**	0.15
	25 001–75 000	0.30
	75 001–150 000	**0.40**
	150 001 and over	**0.45**
2007/08	0–6 000	0.00
	6 001–**30 000**	0.15
	30 001–75 000	0.30
	75 001–150 000	0.40
	150 001 and over	0.45

Source: Australian Taxation Office

We estimate the change in the amount of tax paid by applying the change in the tax rate to the amount of gross income the household was earning in the previous financial year. We use this measure for two reasons. First, we want to get a pure measure of the effect of the tax cuts, abstracting from any changes in household labour supply (which itself might respond to the tax changes). Second, the timing of the tax cuts relative to the survey (the tax year begins on the 1st of July while the survey is largely conducted between August and October) makes it difficult to determine which annual income measure is the most appropriate. Figure 2 illustrates a timeline of the data available in Wave 6 to assess the impact of the new tax rates coming into effect on 1 July 2006. Although information about income as reported at the time of the survey is available, as already mentioned

it tends to be a poor proxy for financial year income.[6] Instead, we assume that the annual income of the previous financial year is a good approximation of the income households perceive as usual at the time that the survey is conducted, which for most households is around three months after the end of the financial year.

Figure 2: Wave 6 Timeline



The amount of the tax change is calculated for each individual, then aggregated on a household basis. These changes across time can then be used to create a measure of the level of tax paid for the regression analysis described in Section 4.1 below.

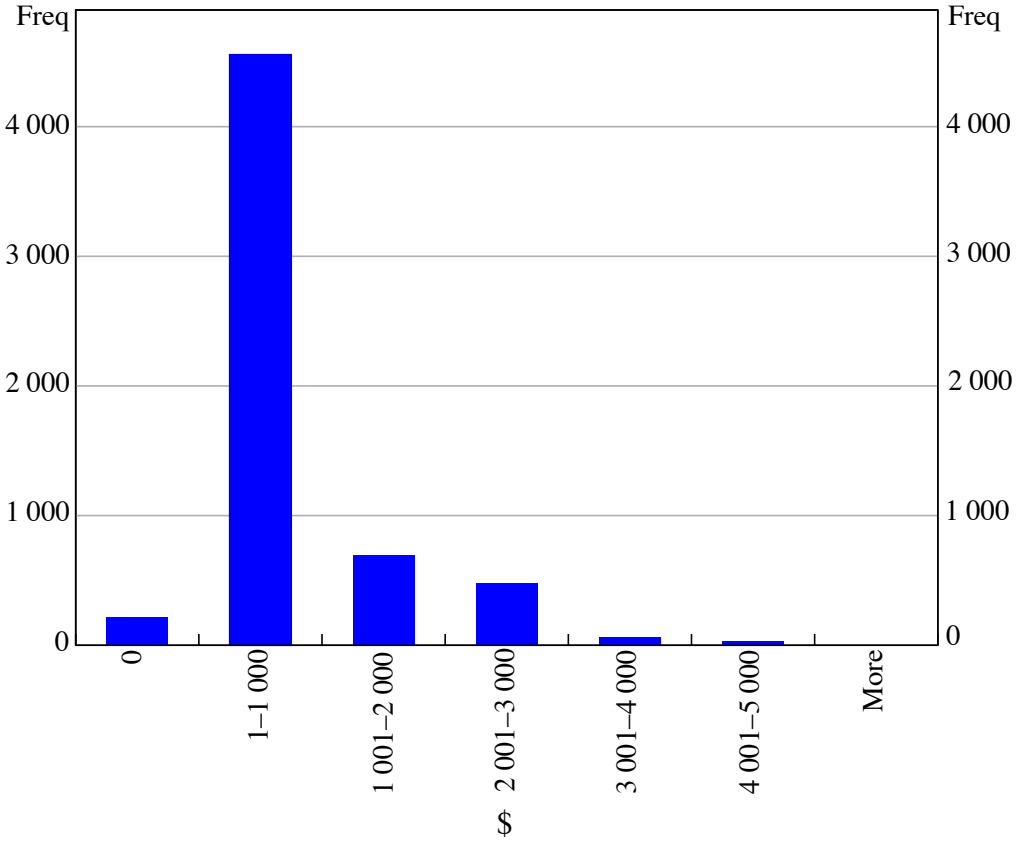
While most households are estimated to have received some tax cuts (according to our approach), the amount varies across the years and across different income groups. At the household level, the average estimated size of the tax cuts (again, according to our specific approach) across the three sample years were $731,

6 HILDA provides an estimate of the amount of tax paid over the new financial year using income earned as reported at the time of the survey. However, as discussed, this is not a reliable measure of income.

$1 543 and $956, respectively.[7] This represents around 1 to 3 per cent of average household disposable income. Figures 3, 4 and 5 show the distributions of the tax cuts in each financial year at the household level. The tax reduction was the largest and most dispersed in 2006/07.

Figure 3: Estimated Household Tax Reduction 2004/05–2005/06

Current values



[7] This approach overstates the share of the population that actually pays tax because we are unable to take into account various deductions and tax offsets. These estimates are broadly consistent with, although somewhat higher than, the averages implied by the estimated cost to government revenue, provided in the annual budget papers (Australian Government 2005, 2006 and 2007). HILDA attempts to take these various factors into consideration in its measure of tax. Our results are robust to limiting the sample to households for which HILDA imputes a positive amount of tax paid.

Figure 4: Estimated Household Tax Reduction 2005/06–2006/07

Current values

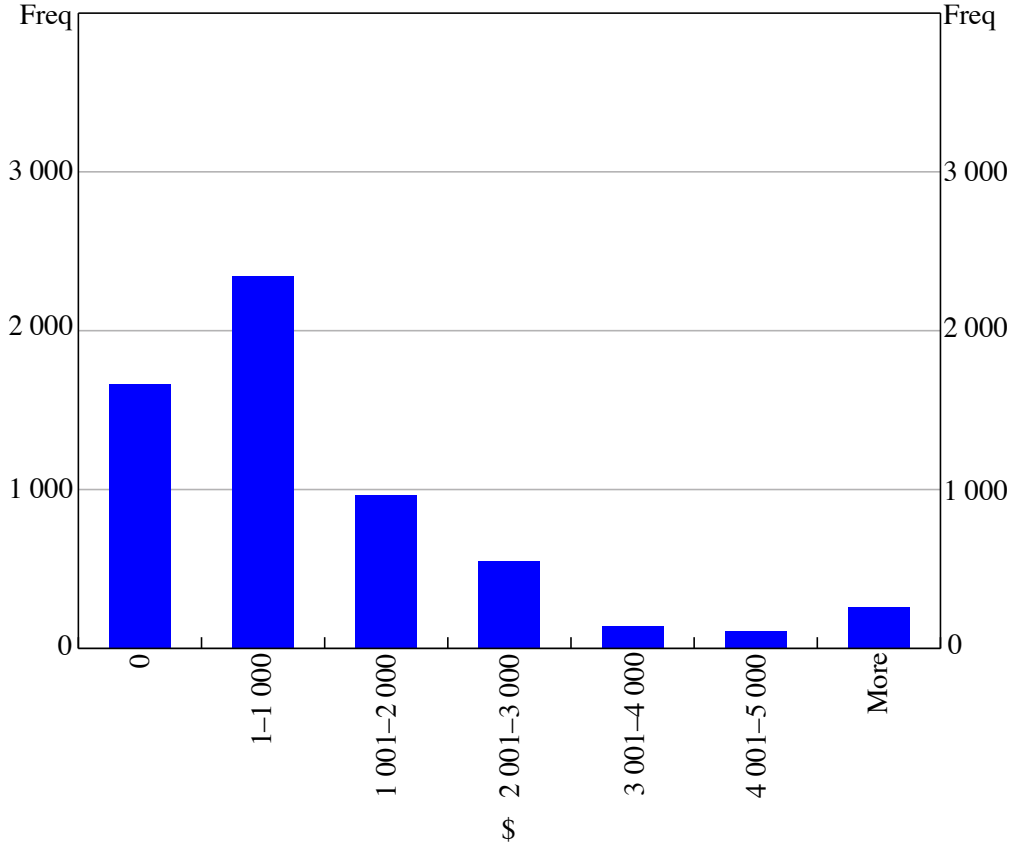
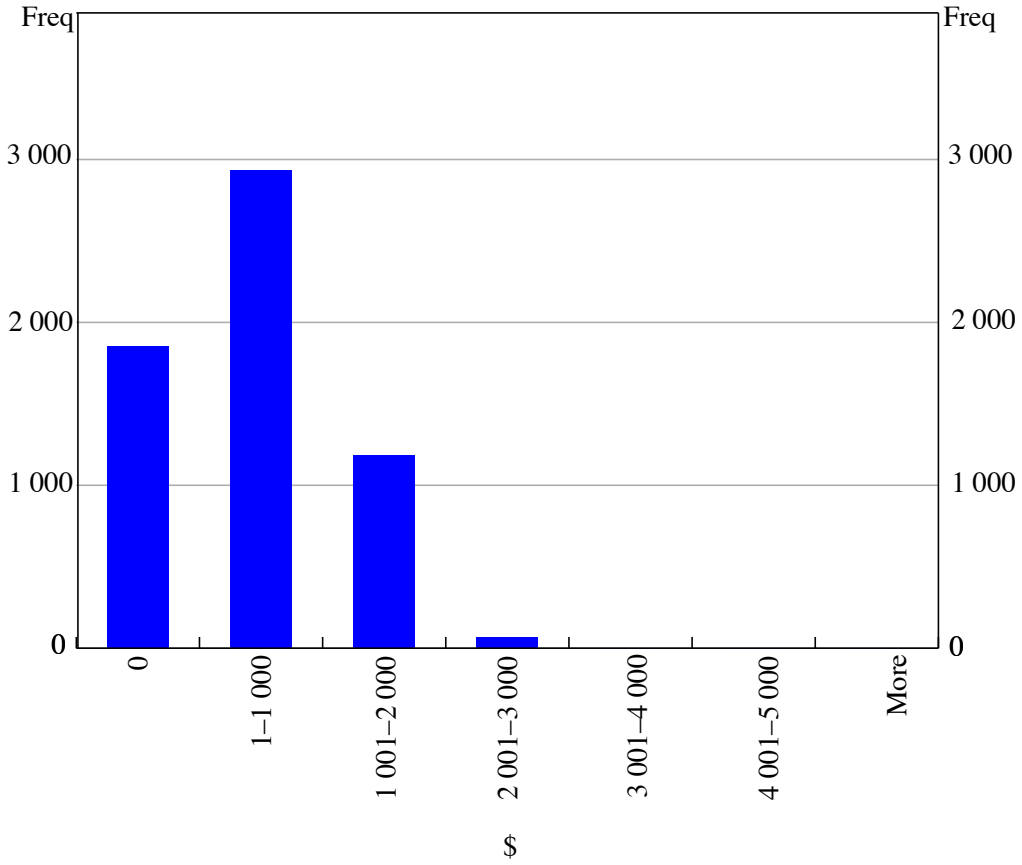


Figure 5: Estimated Household Tax Reduction 2006/07–2007/08

Current values



3.3 Lump-sum Transfers

We examine the MPC out of two lump-sum transfers: the Baby Bonus and the Carer Bonus. These transfers were introduced in 2004 and covered small subsets of the population based on strict qualifying criteria.

The Baby Bonus was introduced in the 2004 Australian Federal Budget. It was a non-means-tested lump-sum payment, initially of $3 000 designed to assist with the costs of having a child. The primary carer of a newborn baby, adopted child under the age of two, or mother of a stillborn baby could make a claim for the bonus within 26 weeks of birth or adoption.[8]

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[8] The payment could be split between multiple people if they could all make a substantiated claim to be the child's primary carer for the first 13 weeks.

In 2004, the basic rates of payment were announced for the next four years. There were two increases of $1 000 that were announced for 1 July 2006 and 1 July 2008. In addition, it was announced that the payment would be indexed to CPI inflation with increases being made biannually in March and September. Table 2 shows the rates of payments from 2005/06 to 2007/08 and the number of households in the survey that had a baby when each rate applied. While the pre-announcement of the policy enables the Baby Bonus to be used as a test of the PIH (abstracting from uncertainty associated with the CPI adjustment), this also creates a potential source of bias. With the policy changes announced so far in advance, some families may have delayed conception in response to the increase in the bonus.[9] Alternatively, when the birth date can be chosen (for example, through elective caesarean), some families might delay the birth of their child to qualify for the higher payment. The unobserved characteristics of the households that delayed conception or birth would thus be correlated with the transfer received, leading to inconsistent estimates of the MPC. In their study on the effect of the introduction of the Baby Bonus in 2004 and the increase in 2006, Gans and Leigh (2009) do not find evidence of households delaying conception as a result of the transfer, but find strong evidence of delays in elective birth procedures. While this is an important caveat to our results, we estimate that it would affect at most 1 per cent of our sample and thus we abstract from the issue.[10]

Unfortunately, the HILDA Survey does not ask whether a household received the Baby Bonus and, if it did, when the payment was received. Given that a household can receive the bonus up to 26 weeks after the birth or adoption of a child, those households in our sample that are interviewed less than 26 weeks after the birth of the baby may not have received their payment at the time of interview. However, given the size of bonus and the substantial cost of having a baby, we assume that all households that qualified for the bonus received it and that they either claimed the bonus immediately or were liquid enough to be able to adjust their expenditure

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9 They may also have increased consumption prior to having the baby in anticipation of the Baby Bonus. In addition, if some households adjusted their expenditure according to expectations of having a baby and these were unrealised, then our estimated coefficient would underestimate the aggregate effect of the policy on consumption.

10 Gans and Leigh estimate that around 4 per cent of births were delayed from June to July in 2006, because of the increase in the Baby Bonus. We use this estimate to infer the number of households with a baby born in July 2006 who may have delayed birth.

Table 2: Baby Bonus Rates and Recipients in HILDA

Date rate applies	Amount	No of households
20/03/2005	$3 079	63
20/09/2005	$3 119	89
20/03/2006	$3 166	71
01/07/2006	$4 000	46
20/09/2006	$4 100	85
20/03/2007	$4 133	124
20/09/2007	$4 187	–
20/03/2008	$4 258	–

Notes: Wave 7 was collected towards the end of 2007, thus data on births for the last two transfer rates will be available in Wave 8, which has not been released yet.
Sources: FaHCSIA (2008); HILDA Release 7.0 (in-confidence version)

with the knowledge that they would receive the bonus. When examining the role of liquidity constraints on the MPC out of the Baby Bonus, this will be an important caveat to the results.

We use the July 2006 change in the Baby Bonus to estimate the MPC. The 106 households who received the bonus nine months prior to this change are compared to the 55 households who received it in the subsequent nine months. Due to the incremental adjustments to account for inflation, the average difference in the size of the bonus the two groups received was $922.

To complement the estimates of the MPC out of the Baby Bonus we also analyse the MPC out of the Carer Bonus. This was a one-off lump-sum payment made to persons eligible for the Carer Payment or the Carer Allowance.[11] These transfers are made to people who provide unpaid care for a person in a private residence who has a disability, severe medical condition or is frail due to age.[12] The Carer Allowance is a universal payment, while the Carer Payment is an additional means-tested pension for people whose workforce participation is limited due to caring responsibilities. Someone can receive a larger Carer Allowance if they are caring for more than one person; a claim can be made for the care of up to two adults and for an unlimited number of children. The bonuses were announced

[11] Where there are multiple people who provide care to the same person, the payment is apportioned between them.

[12] See Centrelink <www.centrelink.gov.au> for further details on welfare eligibility.

each year in the Federal Budget in May and were distributed within one month of announcement. This was done each year from 2004 to 2007. Like the Baby Bonus, the HILDA Survey does not directly ask people whether or not they received the Carer Bonus. We infer households' transfers based on the broader category of the amount of the Carer Payment and Carer Allowance they received each year. Because recipients of the Carer Allowance can receive many bonuses, we exclude households from the sample where the amount of allowance reported is inconsistent with the amount they would have received if they were caring for either one or two people.

Table 3 shows the schedule of transfers and the number of recipients in our sample. In all three years, people eligible for the Carer Payment received a bonus of $1 000. Those eligible for the Carer Allowance were given $600 for each person that qualified them for the transfer. Those who were eligible for both received a minimum of $1 600. In 2006 and 2007, persons who received the Carer Allowance and the Wife Pension or the Department of Veterans' Affairs Partner Service Pension were eligible for an additional $1 000, making their total transfer at least $1 600. As discussed below, we estimate the MPC out of the Carer Bonus by comparing expenditure of those that received this bonus with expenditure of households which are otherwise similar, but did not receive the Carer Bonus.

Table 3: Carer Bonus Rates and Recipients in HILDA

Receives	**Claims**	**Bonus rates**	**Bonus recipients[(b)]**		
	No of people cared for[(a)]		No of households		
			2005	2006	2007
Carer Allowance	1	$600	28	51	46
Carer Allowance	2	$1 200	2	3	6
Carer Payment	1	$1 000	72	46	54
Carer Allowance & Carer Payment/Wife Pension	1	$1 600	15	28	27
Carer Allowance & Carer Payment/Wife Pension	2	$2 200	0	2	7
Total			117	130	140

Notes: (a) The bonus can be claimed more than once if the carer provides care to multiple people. Due to data limitations we only consider households who we can determine would have made two claims or less.
(b) This corresponds to observations actually used in the estimation (see Table 7).
Sources: FaHCSIA (2008); HILDA Release 7.0 (in-confidence version)

4. Methodology

In this section, we outline the models used to estimate the MPC out of tax cuts, the Baby Bonus and the Carer Bonus. Different methodologies are employed due to differences in the nature of the policy changes and data availability.

4.1 Tax Cuts

Following specifications used in previous studies (for example, Lusardi 1996 and Parker 1999), we estimate a standard version of a consumption Euler equation using a fixed-effects regression model. Given our data, the model is based on a balanced panel of households across three years; we also estimate a model in first-differences as a robustness check. These specifications have the advantage of allowing for unobserved household characteristics that are time-invariant. The model is:

$$Expenditure_{it} = \alpha_i - \beta Tax_{it} + \delta x_{it} + \gamma_t + \varepsilon_{it} \quad (1)$$

where $Expenditure_{it}$ is the annual expenditure (on 'non-durables' or 'strictly non-durables') for household i at time t. The key variable Tax_{it} is the measure of the value of tax paid by the household (as described in Section 3). β is the MPC out of the extra income associated with the tax cuts. The household fixed effect α_i represents time-invariant unobserved heterogeneous characteristics such as household preferences. Variations in household characteristics over time are captured by the vector x_{it}, which includes the number of adults and the number of children in the household,[13] the self-reported value of a home owner's dwelling (excluding home contents),[14] and a dummy for whether the household head

[13] Children are defined as being under 15 years of age. The time variation in these variables is generated from respondents moving into, or out of, households and household members ageing and thus changing groups.

[14] Ideally we would like to have included a more comprehensive measure of wealth. However, other wealth measures are only available in Wave 6. The property value variable thus proxies for changes in household wealth over time. Given that 42 per cent of household wealth is held in housing assets, this is probably a reasonable assumption (Australian Bureau of Statistics 2004). There is variation in this variable for 60 per cent of households. However, it should be interpreted with care as this is a self-reported measure and variation could be due to arbitrary reporting differences across time.

possesses a credit card.[15] We also include time-specific effects, γ_t, which capture features of the macroeconomic environment that affect all households, such as interest rates, inflation and a general improvement in living standards. ε_{it} is the individual time-varying unobserved effect. Estimating the model using real variables (derived by using the private consumption deflator) does not materially change the results.

Consistent with the literature, current income is not included in the regression. One reason for this is that current income and consumption are likely to be endogenous, resulting in biased estimates (see Johnson *et al* 2006 for a discussion). This endogeniety arises because a household may consume more due to a higher income or might strive for higher income in order to be able to consume more. While we could instrument for current income, the better strategy is to utilise exogenous policy changes to identify the MPC. Another reason for excluding income is that it is potentially difficult to distinguish between temporary and persistent shocks to income – which theory suggests might have quite different effects on consumption. Again, this is a good reason to make use of the policy changes to identify shocks to income.[16]

In the sample, we exclude 401 multi-family households. We also exclude three extremely high-income households, although this makes little difference to our results. Due to missing expenditure data, we have around 5 600 households in our balanced panel, depending on the expenditure classification used (see Table 4 for precise details).

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[15] This accounts for the possibility that households who obtain a credit card may do so in order to fund greater consumption. Since this is a fixed-effects model, this variable is identified by comparing households that change status with those that do not. We do not account for the effect that servicing any credit card debt will have on consumption.

[16] We tried including lagged income (which by construction is highly correlated with our estimate for taxes) as a control in the regression. As discussed, any interpretation of the coefficient on income is problematic. However, it is important to note that the key results including the MPC estimates are robust to the inclusion of income. Results are available from the authors upon request.

4.2 Lump-sum Transfers

Both lump-sum transfers – the Baby Bonus and the Carer Bonus – apply only to a small subset of the population. To accurately estimate the effects of receiving such a transfer on expenditure we ideally would compare the expenditure of those households that received a bonus with the amount those households would have spent had they not received a bonus. Obviously, the latter amount is not observed, so the next best option is to use the expenditure of households that are similar in almost every way to the households that received the bonus, except for the fact that they did not receive the bonus.

To get a suitable control group to estimate the MPC out of the Baby Bonus, we restrict our sample to those households that had a baby between October 2005 and March 2007, nine months either side of the approximately $1 000 increase in the bonus in July 2006. Although all of these households would have received a bonus, the variation in the bonus payments across time, particularly the substantial change in July 2006, provides natural treatment and control groups. Since all households in this sub-sample had, or adopted, a baby around the same time, there is no reason to expect that there is some fundamental difference in the distribution of their characteristics; indeed, we find that the means of various characteristics for the two groups of households are not substantially different from each other. In contrast, the characteristics of households receiving the Baby Bonus are likely to be quite different to those that did not receive it, for example they are likely to be in a particular age group and have particular preferences. Combined with the additional costs of having a child, these differences imply that their expenditure patterns may be different from the rest of the population, and hence comparing the expenditure of households that received the bonus to that of the wider population would be inappropriate.

For the households that have had children in the 18-month period from October 2005 to March 2007, we use fixed effects to estimate a model of household expenditure across three waves of the HILDA Survey, as follows:

$$Expenditure_{it} = \alpha_i + \beta Bonus_{it} + \delta x_{it} + \gamma_t + \varepsilon_{it} \tag{2}$$

where: $Expenditure_{it}$ is annual household expenditure (on 'non-durables' or 'strictly non-durables'); $Bonus_{it}$ is the size of the Baby Bonus in the time period that the household receives it; x_{it} is a vector of household characteristics that vary across time; α_i is a constant unobserved household fixed effect; and ε_{it} is the individual time-varying unobserved effect. The household characteristics we control for are a dummy variable for whether or not the baby is the household's first, the number of adults (people aged 15 years and over) in the household, and the self-reported value of a home owner's dwelling (excluding home contents). We also include time-specific effects, γ_t, which capture features of the macroeconomic environment that affect all households. These time effects are highly correlated with the policy variable, $Bonus_{it}$. Consequently we also include a variant of the model that excludes time effects, although this makes the results more difficult to interpret. As with the model for tax cuts, estimating the model using real variables did not greatly affect the results.

In the HILDA Survey, 247 households had, or adopted, a baby between October 2005 and March 2007.[17] Of these, 80 households have some missing or inconsistent expenditure data and so are dropped from the sample.[18] We further restrict the sample by dropping those very few households that had two babies during the sample period. The final sub-sample for the Baby Bonus regressions consists of 161 households, 106 households that received the pre-July 2006 rate and 55 that received approximately $1 000 more.

Like the Baby Bonus, households that receive the Carer Bonus are likely to be noticeably different from other households. At least one person in these households provides care to another person in a private dwelling. These households are also likely to have other inherent differences to non-carer households, which makes it difficult to find suitable households for comparison in order to predict how carer households were likely to have behaved had they not received the bonus.

Given that there was no change in the Carer Bonus over our sample period, we use one-to-one matching techniques to find a suitable control group. If we can

[17] We restrict our sample to two-parent or single-parent households. Group households and multi-family households were excluded.

[18] Households were only dropped from the sample if data were missing in the year immediately before and after the baby was born or adopted.

find suitable 'matches', then these techniques assume that there are no important differences between the two households, other than the fact that one received the bonus and one did not. Our preferred matching method is nearest-neighbour propensity score matching. The propensity score is the estimated probability that a household received the transfer, and this method matches each recipient household to the non-recipient household with the closest propensity score. The propensity score is estimated using household characteristics that simultaneously affect the participation decision (to receive the transfer) and household expenditure to ensure that recipient households are matched to an appropriate control. The propensity score is estimated for each household using a logit model:

$$P(D_i = 1|x_i) = \Lambda(x_i'\beta) \tag{3}$$

where: D_i is one if the household is in the treatment group and zero otherwise, and x_i is a vector of household characteristics. Household characteristics must satisfy the condition that they simultaneously influence household expenditure and eligibility for the bonus. We include a number of variables to control for: the number of children under 15 years of age; the number of adults; the share of adults working full-time; the highest level of education in the household; income; the value of assets in Wave 6;[19] whether the household has a mortgage; the number of household members reported to have a long-term health condition, disability or impairment; and a variable that indicates whether someone in the household is a carer of a person who is not a resident in that household. Once a suitable control sample has been formed we compare the difference in the (weighted) conditional means of expenditure of the treatment and control groups to get an estimate of the MPC.

5. The Response of Expenditure to Tax Cuts

The results from estimating the effect of tax cuts on both non-durable and strictly non-durable expenditure are presented in Table 4. All standard errors are corrected for heteroskedasticity using the White's transformation of the error matrix. In all fixed-effects specifications, a Hausman test confirmed that the fixed-effects estimation was more appropriate than random effects. The estimated MPC of 1.0 for non-durables is broadly consistent with the PIH (the point estimate for

[19] The value of assets is available in Wave 6 and is assumed to be constant over the three waves.

Model (I) implies that for every $100 reduction in tax paid consumption is higher by $103).[20] This falls to 0.8 when we restrict expenditure to strictly non-durables.[21] The estimates are also lower when estimated using first-differences as compared with using fixed effects.

Table 4: Expenditure (in $100) – Tax Cut Regressions

	Non-durable		Strictly non-durable[(a)]	
	Fixed effects	First differences	Fixed effects	First differences
	I	II	III	IV
Tax ($100)	1.03^{***}	0.87^{***}	0.78^{***}	0.68^{***}
Wave 5 dummy	-35.70^{***}	-38.74^{***}	-31.47^{***}	-33.68^{***}
Wave 6 dummy	-4.56^{**}	-5.70^{***}	-3.92^{**}	-4.79^{***}
No of children	8.65^{**}	7.17^{**}	8.47^{***}	7.65^{**}
No of adults	38.55^{***}	38.78^{***}	30.76^{***}	31.82^{***}
House value ($10 000)	0.16^{*}	0.12^{*}	0.11	0.08
Credit card	5.63^{**}	4.71	4.13^{*}	3.60
R^2-within	0.17	–	0.16	–
R^2-between	0.13	–	0.12	–
R^2-adjusted	–	0.12	–	0.11

Notes: ***, ** and * denote statistical significance at the 1, 5 and 10 per cent levels respectively. There are 13 494 observations in the fixed-effect specification and 7 429 observations in the first-differences equation (5 576 households).
(a) Expenditure excludes education, clothing and footwear, and motor vehicle repairs and maintenance.

The estimated MPCs are slightly larger than estimates in the literature for the United States. In particular, the fixed-effects model suggests that all of the extra income is allocated to non-durable expenditure. A high estimate is consistent with Parker (1999), who finds that reductions in withholding taxes in the United States are disproportionately spent on non-durables. This may reflect the fact that small, persistent increases in disposable income are treated differently to large increases

[20] However, this evidence is not sufficient to be able to accept the PIH outright, since the PIH also implies that households do not respond to anticipated changes in income.

[21] If the excluded expenditure items are in fact durables then the meaning of this fall is difficult to interpret. This is because the relationship between current income and current expenditure on durables is different to that of non-durables. Durable goods provide a stream of consumption benefits, which households will take into consideration when making a purchase. If they receive a permanent increase in income and are able to borrow, they could purchase a durable good immediately, even if the value of that good is greater than the size of the extra income received in each period.

in income, and are more likely to be spent on non-durables. The fact that our estimate is higher than many US studies can be partly explained by the different nature of the income changes examined. Since the tax changes we examine were announced and implemented between two interview dates, our MPC results are more likely to reflect the fact that the increase in income was unexpected (at least relative to the timing of the previous observation for expenditure). This will imply a larger effect on consumption than in the case where the increase was anticipated.[22] It may also reflect the fact that there is a longer time period between the receipt of the extra money and the interview in these data than there is in the US studies, allowing more time for expenditures to adjust.

The other variables in the regressions are generally correctly signed. As expected, consumption rises with family size, particularly with respect to the number of adults. Higher wealth (as captured by the house value variable) is associated with higher consumption. The coefficient estimate on the dummy variable for credit cards implies that acquiring a credit card (at some time during the sample) is associated with higher consumption thereafter, and vice versa for those that no longer hold a credit card; although this does not imply a causal relationship.[23] The time dummies imply that non-durable consumption was around $456 lower (1.8 per cent) in Wave 6 compared with Wave 7. However, the coefficient on the Wave 5 dummy variable is large, implying a sizeable rise in non-durable expenditure from Wave 5 to Wave 6. This is likely to reflect the effect of some changes to the expenditure questions across these waves.[24]

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[22] Many other studies have focused on estimating the MPC out of changes in actual income that were anticipated; this is likely to be smaller since at least some (non-liquidity-constrained) households will have responded to the expected effect of the fiscal policy on income when it was announced rather than waiting to receive the extra income.

[23] As mentioned above, we do not control for any effect on consumption of having credit card debt that otherwise needs to be serviced.

[24] Wave 5 was the first year that detailed expenditure data were collected. In Wave 6, additional expenditure categories, mostly durable expenditure items, were added to the survey. The level of aggregation of several non-durable expenditure categories was also reduced in the survey. It is possible that more detailed and comprehensive coverage, as well as greater familiarity with the questions, contributed to the sizeable jump in expenditure observed between Waves 5 and 6.

6. The Response of Expenditure to Lump-sum Payments

The MPC for the Baby Bonus is estimated using Equation (2) and the results are shown in Tables 5 and 6.[25] As before, we report the results for both fixed effects and first differences. However, unlike with the tax cuts, we estimate a variant of the model without time effects because they are highly correlated with the policy variable. The coefficient on the amount of the Baby Bonus provides an estimate of the MPC.

Table 5: Non-durable Expenditure (in $100) – Baby Bonus Regressions

	Fixed effects		First differences	
	I	II	III	IV
Bonus size ($100)	0.30	0.70^{***}	0.29^{*}	0.53^{***}
Wave 5 dummy	-59.50^{***}	–	-59.33^{***}	–
Wave 6 dummy	-22.50^{***}	–	-23.88^{***}	–
First child	3.68	11.25	8.29	13.66
No of adults	30.76^{**}	45.53^{***}	30.50^{*}	42.72^{**}
House value ($10 000)	0.30	0.72^{***}	0.27	0.54^{**}
R^2-within	0.31	0.13	–	–
R^2-between	0.27	0.28	–	–
R^2-adjusted	–	–	0.21	0.10

Notes: ***, ** and * denote statistical significance at the 1, 5 and 10 per cent levels respectively. There are 470 observations (161 households) in the fixed-effect specification and 310 observations in the first-differences equation.

We find that the estimates of the MPC out of the Baby Bonus for non-durable expenditure range from around 0.3 to 0.7, while the MPC for strictly non-durable expenditure is slightly lower, ranging from 0.2 to 0.5. The estimated MPCs on both non-durable and strictly non-durable expenditure rise noticeably when the time variables are excluded from the regression (Models (II) and (IV)). While ideally we would want to control for the time variation, the coefficients on both of the time dummies are implausibly large, implying that expenditure grows by up to 12.5 per cent per year. The difficulty is that with little variation in bonus payments, the time variables are highly correlated with the size of the bonus received and whether or not the household had a baby that year. It is likely that these large coefficients

[25] Heteroskedacity does not appear to be a problem in this specification. We do not correct for it because the sample size is small.

are picking up some of the Baby Bonus effect and the effect of having a baby. However, not accounting for time variables is likely to overstate the effect of the bonus payment on consumption. This suggests that the true effect lies somewhere between the range of estimates shown.

Table 6: **Strictly Non-durable Expenditure (in $100) – Baby Bonus Regressions**

	Fixed effects		First differences	
	I	II	III	IV
Bonus size ($100)	0.25	0.54^{***}	0.23	0.42^{***}
Wave 5 dummy	-40.50^{***}	–	-41.55^{***}	–
Wave 6 dummy	-13.94^{**}	–	-14.94^{**}	–
First child	3.20	8.83	5.21	9.83
No of adults	25.68^{*}	35.75^{**}	27.11^{*}	35.83^{**}
House value ($10 000)	0.27	0.54^{***}	0.18	0.36^{*}
R^2-within	0.24	0.11	–	–
R^2-between	0.29	0.29	–	–
R^2-adjusted	–	–	0.15	0.07

Notes: ***, ** and * denote statistical significance at the 1, 5 and 10 per cent levels respectively. There are 472 observations (161 households) in the fixed-effect specification and 311 observations in the first-differences equation. Expenditure excludes education, clothing and footwear, and motor vehicle repairs and maintenance.

The range of estimated MPCs for the lump-sum Baby Bonus payment is lower than that for the tax cuts. There are two possible explanations for this. First, since the Baby Bonus amounts were announced prior to the beginning of our sample, there may have been more of an expectation that households would receive the bonus than there was for the tax cuts, even accounting for the uncertainties surrounding conception or adoption. The expectation of the bonus would have allowed some households to adjust expenditure prior to actually receiving it, lessening the increase in expenditure on receipt. Second, compared with the tax cuts, the bonus payments were more likely to have been perceived by the households as temporary. According to the PIH, non-liquidity-constrained households would thus have smoothed consumption over time, lowering the estimated MPC for more temporary payments. However, direct comparisons between tax cuts and lump-sum transfers are difficult since a lump-sum transfer is unlikely to be spent evenly across a year, so the reported expenditure of the household at the time it is surveyed may not be a good estimate of the annual

average expenditure of the household (on average, seven months pass between when households are assumed to receive the bonus and the survey date). Tax cuts, in contrast, are received continuously, so their effect should be more evenly distributed.

Caution should be exercised in interpreting the Baby Bonus MPCs as an indication of the MPC out of all lump-sum transfers. There are two main reasons for this. First, and probably more importantly, the Baby Bonus is received during a time of large personal and financial adjustment for a household, including expenditure on many new consumption goods and services (particularly if it is a household's first child). In such an environment, a household might spend more out of a lump-sum transfer than usual. Second, we found it very difficult to control for the separate effects of having a baby and receiving the bonus, given that we had to assume that these events coincided. While variation in the magnitude of the Baby Bonus itself separates these effects, the Baby Bonus and new baby variable are highly correlated. Hence no baby born variable was included in the regression. In short, combined with the multicollinearity problem of the time variables, there is considerable uncertainty surrounding the estimates of the MPC out of the Baby Bonus, with estimates ranging between 0.25 and 0.7 for non-durable expenditure.

Unfortunately the Carer Bonus results are not reliable. This is because it is very difficult to find appropriate matches for carer households – the selection equations tend not to meet the balancing property across all three years.[26] In addition, the estimates are very sensitive to the variables included in the selection equation, implying that small changes in specification lead to quite different matches. As summarised in Table 7, these problems lead to varied and statistically insignificant estimates of the MPCs across the three years and across the different expenditure classifications.

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[26] The balancing property is satisfied when there is no statistically significant difference between the mean of the estimated propensity score in both groups, Carer Bonus recipients and non-recipients.

Table 7: MPC Out of the Carer Bonus

Bonus	Non-durable	Strictly non-durable	No of carers/Non-carers[(a)]
Carer Bonus 2005	−0.01	−0.29	117/4 492
Carer Bonus 2006	0.49	0.52	130/4 675
Carer Bonus 2007	0.10	−0.29	140/4 680

Notes: ***, ** and * denote statistical significance at the 1, 5 and 10 per cent levels respectively.
(a) These are the pre-matched sample sizes. After the matching, the number of non-carers in the control group is the same as the number of carers.

7. Differences in Responses Across Households

As noted in the literature for the United States, there is likely to be heterogeneity in households' responses to an income shock. One explanation for this heterogeneity is the presence of liquidity constraints, usually proxied for by low income or low wealth. Liquidity-constrained households are likely to consume out of current, rather than permanent income, leading to a higher estimated MPC. In this paper we consider the level of income and other financial constraints as indicators of liquidity constraints as well as exploring another source of heterogeneity – household's perceptions of risk. If households are concerned about prospects for income, they may be more likely to save extra income rather than spend it.

Our first measure of liquidity constraints is based on income, which is standard in the literature. We classify a household as liquidity constrained if equivalised household income is less than the cut-off point for the bottom 30 per cent of equivalised income for the HILDA sample in each year,[27] and it meets the low-income definition in all three periods.[28] A dummy variable is created to identify low income households. This dummy variable is interacted with all of

[27] Income is equivalised using the ABS method of dividing income by a weighted sum of the number of household members. Household members are assigned varying weights according to age and numbers of household members: 1 for the first person aged over 15 years and 0.5 thereafter, and 0.3 for each person aged under 15 years. The 30 per cent cut-off points are $21 831, $23 730 and $25 000 for Waves 5, 6 and 7 respectively.

[28] Only 14 per cent of households change their classification (in terms of this income threshold) over time, which creates a potential difficulty when using fixed effects or first differences. The issue is that the small number of observations that do change will have an unduly large influence on the coefficient estimates. To avoid this we apply a strict definition of low income, which removes all time variation in this variable.

the explanatory variables allowing us to test for differences in the responses of the variables.[29]

Table 8 shows the effect of liquidity constraints on the estimates of the MPC for both the tax cuts and the Baby Bonus. On non-durable expenditure, we find that households with low incomes spend more of their tax cuts and lump-sum payments than higher income households, but these are imprecisely estimated. The results are mixed for strictly non-durable expenditure. However, none of these differences are statistically significant.

Table 8: Estimates of the MPC – Liquidity Constraints (Income Level)

Low income	No of observations	Non-durable	Strictly non-durable[(a)]
		Tax cuts[(b)]	
Yes	2 789	1.61*	0.61
No	11 374	0.97***	0.71***
		Baby bonus[(c)]	
Yes	63	0.90*	0.93**
No	420	0.67***	0.48***

Notes: ***, ** and * denote statistical significance at the 1, 5 and 10 per cent levels respectively. White's heteroskedastic robust standard errors are used for the tax cuts. No correction was needed in the case of the Baby Bonus.
(a) Excludes education, clothing and footwear, and motor vehicle repairs and maintenance.
(b) Estimated using fixed effects, however the model includes an interaction term between the liquidity constraint dummy and the Wave 5 dummy as described in the text. As a result, the MPC in Table 4 may not lie between the estimates shown here.
(c) Estimated without time-fixed effects.

Our second measure of constraints come directly from relevant questions in the HILDA Survey. Following La Cava and Simon (2003), households are classified as financially constrained or stressed if they answer yes to any of the options on the question: 'Since January [year] did any of the following happen to you because of a shortage of money?'. The options are: could not pay electricity, gas or telephone

[29] We find that the only variable (other than the *Tax* and *Bonus* variables) that has a statistically significant different effect between groups is the Wave 5 time dummy. As previously mentioned, there is an unusually high difference between the Wave 5 expenditure data and the other two waves. This suggests that whatever is causing this difference varies across income groups. However, further investigation shows that this relationship does not hold consistently across income deciles. Since there is a significant difference between the effect of the Wave 5 dummy for low-income earners and everyone else, we include separate Wave 5 dummy variables for these groups of households as well as allowing for separate MPCs.

bills on time; asked for financial help from friends or family; could not pay the mortgage or rent on time; pawned or sold something; was unable to heat home; went without meals; and/or asked for help from welfare/community organisations.

A dummy variable is created for households that are financially constrained in all three periods.[30] Once again we interact the dummy variable with all (non-policy) variables and test for differences, however, we find none that are statistically significant. We thus only interact the financial constraint dummy with the policy variables to test for differences in the estimated MPCs for constrained and unconstrained households. The results are reported in Table 9. Surprisingly perhaps, we find that households that report constraints tend to spend less of their extra income than households that do not report constraints, although again these differences are not statistically significant.

Table 9: Estimates of the MPC – Self-reported Financial Constraints

Constrained	No of observations	Non-durable	Strictly non-durable[(a)]
		Tax cuts	
Yes	2 577	0.90**	0.54*
No	11 453	1.03***	0.79***
		Baby bonus	
Yes	105	0.58	0.55**
No	354	0.71**	0.53**

Notes: All estimated using fixed effects. Time effects are excluded in the Baby Bonus estimation. ***, ** and * denote statistical significance at the 1, 5 and 10 per cent levels respectively. White's heteroskedastic robust standard errors are used for the tax cuts. No correction was needed in the case of the Baby Bonus.
(a) Excludes education, clothing and footwear, and motor vehicle repairs and maintenance.

There are two potential explanations for this result. First, only about 40 per cent of financially constrained households have low incomes (across the whole HILDA sample), suggesting that this variable is an imprecise measure of the existence of liquidity constraints. The inclusion of wealthier people, who are more likely to be under financial stress because they have large amounts of debt to repay, may explain why those households who report themselves to be financially constrained seem to spend less of the extra income than those that are not constrained. It seems likely that such households might use extra income to pay down debt, rather than increase expenditure. We test this hypothesis by splitting the sample into those

[30] Time-series variation is again a potential issue in this specification, with only 26 per cent of households varying their response over time.

with and without housing debt (since this is the only measure available in all three waves). However, we find that financially constrained households tend to spend less than unconstrained households regardless of whether or not they have housing debt. Second, since the survey question asks households if they have been in difficulty at any time since the beginning of the year, we cannot separate temporary from more persistent financial difficulty. While we have attempted to exclude temporary difficulty by only considering households that report difficulty in all three years, the measure is still quite imprecise.

Household perceptions of risk may also be an important driver of heterogeneity in household MPCs. If households are especially concerned about maintaining their income in the future, they may be more likely to save any extra income they receive, lowering their MPCs. In order to examine this precautionary savings motive, we use the self-reported unemployment risk variable for the household head. Respondents are asked to rank their feeling about the statement 'I worry about the future of my job' on a scale of one to seven (strongly disagree to strong agree). We classify households as worried if they answer 'five' or above in all three years.[31] In contrast, for the Baby Bonus sample, we classify households as worried in any given year if they answer 'five' or above to the same question. We allow for this time variation since too few households were worried in all three years for reliable estimates.[32]

To estimate the effect of perceived unemployment risk on the MPC, we interact the unemployment risk dummy variable with the tax variable in Equation (1) and the Baby Bonus variable in Equation (2). Our results, shown in Table 10, are consistent with our expectation that households facing more employment uncertainty will spend less out of additional income. The differences in the estimates of the MPC for 'worried' and 'not worried' households are not statistically significant for either the tax cuts or the Baby Bonus. However, they are noticeably different in

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[31] Once again there is not very much time variation in this variable. Since the question is answered on a scale of 1–7, a substantial amount of the variation that does occur could be generated by arbitrary changes in a respondent's interpretation of the ranking from year-to-year. If time variation is included, we find that risk aversion has no effect on the estimate of the MPC. However, this is not surprising since there could be any number of unrelated reasons why households change their reported job security across time.

[32] There is also much more time variation in perceived job security in the Baby Bonus sample as compared with the tax cut sample.

terms of magnitude. Although the Baby Bonus should be interpreted with caution given the difficulties with sample size, our finding is consistent with Leigh (2009). Our results are not affected by restricting the sample to households where the reference person is employed.

Table 10: Estimates of the MPC – Unemployment Risk

Job security	No of observations	Non-durable	Strictly non-durable[(a)]
		Tax cuts	
Worried	348	0.54*	0.55***
Not worried	10 452	0.87***	0.67***
		Baby bonus	
Worried	73	0.41	0.41
Not worried	293	0.67***	0.46**

Notes: All estimated using fixed effects. Time effects are excluded in the Baby Bonus estimation. ***, ** and * denote statistical significance at the 1, 5, and 10 per cent levels respectively. White's heteroskedastic robust standard errors are used for the tax cuts. No correction was needed in the case of the Baby Bonus.
(a) Excludes education, clothing and footwear, and motor vehicle repairs and maintenance.

8. Conclusion

Using household level data this paper finds a range of estimates for the marginal propensity to consume out of lump-sum transfers and income tax cuts on non-durables in Australia. Although the results need to be interpreted with a degree of caution, consistent with the permanent income hypothesis the estimates of the marginal propensity to consume out of the Baby Bonus, which is temporary, are generally lower than the estimates of the marginal propensity to consume out of income tax cuts, which are likely to have been perceived as more persistent. Also, the paper finds some tentative evidence that there is heterogeneity in households' responses, for both temporary and persistent income shocks. Lower income households tend to consume a greater share of extra income, consistent with the idea that these households are more likely to be liquidity constrained, while households that perceive that they are at greater risk of losing their jobs tend to consume less out of lump-sum transfers and income tax cuts.

Appendix A: Data Sources

Column (I) in Table A1 shows the components we include in strictly non-durable expenditure. The categories in Column (II) are the additional components included in non-durable expenditure.

Table A1: Expenditure Items in the HILDA Survey

Non-durable	
Strictly non-durable	
(I)	(II)
Grocery	Clothing and footwear
Alcohol	Motor vehicle repairs and maintenance
Cigarettes and tobacco	Education fees
Public transport and taxis	
Meals eaten out	
Motor vehicle fuel	
Telephone rent, calls and internet charges	
Holidays and holiday travel	
Private health insurance	
Health care[(c)]	
Utility, gas and other heating bills	

Notes: (a) In Waves 6 and 7, clothing and footwear was disaggregated into mens clothing and footwear, female clothing and footwear, and children's clothing and footwear.
(b) Telephone rents and calls are reported excluding internet charges in Wave 5. Including internet charges in Wave 6 is about 80 dollars higher than Wave 5. Between Waves 6 and 7, the difference in the amount spent in telephone rent and calls (both including internet charges) is also around 80 dollars, suggesting that the internet component may be small. We ignore this difference in definition between Wave 5 and the other two waves.
(c) In Waves 6 and 7, we add two components: fees paid to health practitioner; and medicines, prescriptions, pharmaceutical and alternative medicine, to form a measure of health care to resemble that reported in Wave 5.

Source: HILDA Release 7.0 (in-confidence version)

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