Fiscal Stimulus and Households' Non-Durable Consumption Expenditures: Evidence from the 2009 Australian Nation Building and Jobs Plan

by

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Abstract: In 2009 the Australian government delivered approximately $8 billion in direct payments to households. These payments were pre-announced and randomly allocated to households based on postal codes over a 5-week period. We exploit this random allocation to estimate the causal response of households' non-durable consumption expenditures to a transitory, anticipated income increase. Our main findings are that: (i) non-durable consumption expenditures did not react significantly during or after the one-time, pre-announced transfer; (ii) there is a small, albeit statistically significant increase in non-durable consumption expenditures at the time of the announcement of the fiscal stimulus.

Key words: Excess Sensitivity, Rational Expectations Permanent Income Hypothesis, Fiscal Stimulus, Transitory Income, Anticipation, Randomization

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1. Introduction

In response to the global financial crisis of 2008-09, the Australian government disbursed approximately $8 billion in direct payments to 8.7 million taxpayers. The disbursement of these payments was announced in February 2009 and the actual payout was randomized across households, based on postal codes, over a 5-week period during April-May 2009. Using weekly data of household non-durable consumption expenditures, we exploit the random allocation of the fiscal transfer to estimate the causal effect that a transitory, anticipated rise in household income has on non-durable consumption expenditures. We also use time-series analysis to test separately for an impact of the fiscal stimulus announcement. The paper's results thus speak to two important literatures: one on the household consumption response to changes in household income; and the other on the economic effects of fiscal stimulus.

Our main finding is that households' non-durable consumption expenditures did not react significantly during or after the one-time, pre-announced transfer. The estimated effects are also quantitatively small. They imply that upon receipt of the transfer, the average household spent less than 0.2 percent of the payment on non-durable goods. The paper documents that the finding of no significant effect on non-durable consumption is robust to including several lags of the transfer indicator variable on the right-hand side of the regression; excluding large changes in household consumption expenditures; controlling for leads and lags of the dependent variable; and separating households into different groups based on income per household member. An examination of households' non-durable consumption responses to the one-time transfer thus does not point to significant excess sensitivity in the Euler equation.

The rational expectations permanent income hypothesis predicts that consumption should respond significantly to new information about (future) income. Because the fiscal transfer to households was announced about two months before the actual payout period, we are also able to study the consumption response to the news shock using conventional methods of time-series

analysis (such as employed, for example, in Ramey (2011)). The time-series analysis shows a statistically significant increase in non-durable consumption at the time of the announcement of the fiscal stimulus. Quantitatively, the estimates suggest that the announcement of the fiscal stimulus induced a change in households' non-durable consumption expenditures of around $22. It is, furthermore, noteworthy that the time-series analysis shows a statistically insignificant and quantitatively small response of non-durable consumption expenditures to the actual receipt of the pre-announced transfer. This result is similar to our randomization analysis and, thus, re-assuring that our time-series analysis is uncovering causal effects.

One important strand of literature that our results speak to is the fiscal policy literature on anticipation. When fiscal policy is pre-announced (i.e. there is a time-gap between the actual fiscal stimulus and the period in which the news of fiscal stimulus is made available to the public) the response of economic variables to the actual fiscal stimulus may not be the same as the response to the fiscal stimulus announcement. In particular, the response of economic variables to the actual fiscal stimulus may be insignificant since, with fiscal anticipation, economic variables already respond at the time of the announcement. For examples of papers that analyse the role of fiscal anticipation see Leeper et al. (2012), Mertens and Ravn (2010), or Ramey (2011).

A second strand of literature that our results speak to is the large and well-established literature on the consumption response to income changes.[1] A small subset of this literature uses a quasi-natural experiment design to examine household consumption responses to pre-announced income changes.[2] The current paper is one of only a few that exploit random cross-sectional variation in the payment timing to avoid endogeneity problems which limited earlier studies of the

1 Japelli and Pistaferri (2010) provide a comprehensive survey of this literature.

2 Souleles (2000) studies the effect of an anticipated increase in college expenditures in the US and finds an insignificant effect on US households' consumption expenditures. Hsieh (2003) studies the effect of dividend payments from the State of Alaska's Permanent Fund and finds an insignificant effect on Alaskan households' consumption expenditures. Browning and Collado (2001) examine the effect of anticipated seasonal income fluctuations in Spain and find an insignificant effect on Spanish households' consumption expenditures. On the other hand, Johnson et al. (2006) and Parker et al. (2013) detect a significant effect on durable consumption expenditures, but not on strictly non-durable consumption expenditures, of an anticipated increase in household income arising from the 2001 and 2008 tax rebates in the US.

impact of fiscal stimulus payments. All the previous studies of this type examined recent US fiscal interventions. The most closely related to our paper are Johnson et al. (2006) (who estimate the consumption effects of the 2001 tax rebates) and Parker et al. (2013) (who estimate the consumption effects of the 2008 tax rebates). Both papers find a quantitatively small and statistically insignificant effect on strictly non-durable consumption expenditures, as we do. Our findings on non-durable consumption expenditures are thus consistent with the results in Johnson et al. (2006) and Parker et al. (2013). In contrast to our paper, Johnson et al. (2006) and Parker et al. (2013) do not present estimates of the announcement effects associated with the fiscal stimuli; they only present estimates that the receipt of the tax rebates had on household consumption.

It should also be pointed out that Johnson et al. (2006) and Parker et al. (2013) find a significant effect of the stimulus payments when using a broad measure of consumption that includes durables goods. We do not have data on (weekly) durables goods expenditures, hence are unable to explore the causal response of durables goods. While our findings thus do not allow us to reach a conclusion on the effect that the fiscal stimulus had on overall household consumption, they do provide support for a modelling approach that treats non-durable goods expenditures as behaving in line with the rational expectations permanent income hypothesis.

The remainder of our paper is organized as follows. Section 2 describes the fiscal stimulus and household consumption data. Section 3 explains the estimation strategy. Section 4 discusses the empirical results. Section 5 concludes.

2. Data

2.1. The 2009 Fiscal Stimulus

Our analysis will focus on the household payments delivered through the Nation Building and Jobs Plan (NBJP). In particular, we focus on the Tax Bonus for Working Australians. This stimulus package was announced by the Prime Minister of Australia on the 3rd of February 2009 and was

subsequently passed by parliament with some amendments on the 18th of February. Shortly after, on the 26th of February, the act was challenged as unconstitutional at the High Court of Australia. The decision to uphold the tax bonus act was announced on the 3rd of April. The payment under the tax bonus act was $900 for individuals with taxable incomes[3] of $80,000 or less; $600 for individuals with taxable incomes of $80,001-$90,000; and $250 for taxpayers with incomes of $90,000-$100,000. Since tax is assessed on an individual basis, it was possible for both adults in a family to receive the payment. This payment was estimated to cover 8.7 million taxpayers (about 75% of all taxpayers). The enormous task to pay out money to over 8 million people could not be handled over night. Capacity constraints on both printing paper checks and initiating electronic transfers meant that the payments had to be scheduled over the course of five weeks. The Australian Taxation Office implemented a randomized payout by postcode.[4][5] In this way 98% of the tax bonus payments were delivered between 3 April and 15 May 2009.

The randomized payout scheme by postcode allows us to identify when a particular household received the tax bonus. We use confidential data from the Australian Taxation Office which tells us for each postcode the date on which check and electronic bonus payments were made. It is important to note that along with the payments, people were sent letters notifying them of the payment. Thus, households were made aware of the deposit even if they didn't look at their bank account.

Australian tax payers had the choice to register their account information with the Australian Tax Office and receive tax rebates via an electronic fund transfer (EFT) or via checks in the mail. Accordingly 52% of all individuals received their tax bonus payment via a check and 48% via a

3 Based on taxable income in the 2007-08 tax year.

4 The randomization was undertaken at the urging of Andrew Leigh who was at the time seconded to the Australian treasury from his Professorship of Economics at the Australian National University.

5 A random number was drawn for each of the 2965 country wide postcodes. These were paid out in the random order given daily fixed state size proportions. A set of 69 postcodes of recent disaster areas in Victoria and Queensland who needed financial assistance as a result of floods in Queensland and bushfires in Victoria were made an explicit priority and paid out first on the 3rd and 6th of April. As these payouts are not random but as a result of an exogenous shock to the local economy we don't include households from these postcodes in our baseline regressions.

direct deposit into their bank account. Although the postcode randomisation applied to both checks and electronic transfers, there is naturally a delay between when checks were posted and when they were received and banked. Although the Australian Tax Office campaigned to have taxpayers provide their home address or bank details, not all did so and consequently some checks were mailed to tax agents. This creates two complications: the tax agent's postcode may not be the same as the taxpayer's postcode; and the tax agent may have taken some time to forward on the check.[6] We address this issue by focusing in our baseline analysis on EFT payments. As a robustness check we will also present results that use check payments.

2.2. Household Non-Durable Consumption

Since the random variation in timing arises at the postcode-week level, we needed a dataset that measures expenditure at this level. We use the AC Nielsen Homescan panel, a proprietary dataset consisting of around 10,000 households.[7] The AC Nielsen Homescan panel comprises weekly data on all non-durable goods consumed at home. Homescan households are instructed to use a barcode scanner (plus manual entry for items like fresh fruit that do not have barcodes) to record all items that are purchased and subsequently brought into the home. Typically these items are purchased at supermarkets, supercenters, drug stores, and gas stations.[8] The panel is weighted to be representative of the total Australian population.

Table 1 shows summary statistics of our dataset. Our AC Nielsen data is comprised of 10615 Households which spend on average AU$ 120 a week on 41 non-durable items. The average size of a household in our sample is 2.9 with an annual household income of AU$ 66'000.[9] We have

6 Some checks did not reach individuals due to incorrect addresses. The tax office estimates that to be the case for 3% of all checks sent.

7 Potential alternative datasets are the Australian Bureau of Statistics' Household Expenditure Survey and the Household Income and Labour Dynamics in Australia Survey (HILDA), however, neither of these surveys were in the field at the time of the stimulus payments.

8 Panellists are asked to record all non-durable goods brought home, this includes fresh food (meat, vegetables, dairy, etc), packaged grocery (rice, flour, bread, etc), frozen foods (veggies, ice cream etc), beverages & confectionary, health & beauty products (shampoo, deodorants, headache pills etc) and household products (cleaning, toilet paper, laundry detergents, etc), as well as things like pet food.

9 We observe in our sample 21% single households, 12% are at retiring age and only 8% are young households without children.

additional demographic data on the primary shopper in the household. They are predominantly female (78%) with an average age of 48. Thirty seven percent of the primary shoppers are full time employed, 12% are retired and 20% have home duties as their occupation. Twenty percent of shoppers are foreign born, but half of these have an English language background.

In Table 2 we show variable means by the weeks in which households' postcodes were paid the stimulus money (according to the random pay schedule implemented by the taxation office). We find a slight difference between the average amount spent on non-durable goods of week 2 and week 4, which is significant at the 5% level. There are no statistically significant differences in any of the other household characteristics across the different weeks.

3. Estimation Strategy

Following the literature (see e.g. Johnson et al., 2006) our main estimating equation is:

(1) $$\Delta Expenditure_{pi,t} = \beta Payment_{pi,t} + [a_i] + b_t + \varepsilon_{pi,t}$$

In this equation, $\Delta Expenditure_{pi,t}$ is the change in non-durable consumption expenditures of household i living in postcode p between week t and t-1. This is modeled as a function of $Payment_{pi,t}$, which is a binary variable that is unity in week t when households living in postcode p received the bonus payment from the government and zero else. The week fixed effect, b_t, are important control variables because they capture any period specific effects on consumption that are common across households. Thus they capture the effects of Easter spending as well as the impact of new information about the national or international economic environment. Since the payment is randomized across households, there is no need to control for household fixed effects, a_i. Controlling for these fixed effects will however affect standard errors. We report these results as a robustness check to demonstrate that controlling for household fixed effects has inconsequential effects on the estimates.

In our baseline regressions the error term, ε, is clustered at the postcode level. This type of

clustering ensures that our computed standard errors are adjusted for arbitrary serial correlation within postcodes. As a robustness check we also report standard errors that are computed using the Cameron et al. (2011) multi-cluster estimator. When using this multi-cluster estimator we cluster at the postcode and weekly level. This type of clustering ensures that the computed standard errors are robust to arbitrary serial correlation within postcodes as well as arbitrary spatial correlation.

In our baseline regressions we estimate equation (1) for the largest possible sample. This sample comprises 479069 household-week observations in 1573 postcodes during the period 01 January 2008 to 31 December 2009. Using the largest possible sample has the advantage to maximize the variation in the independent variable.

One issue with using the largest possible sample is that the amount of rebate received depended on household characteristics. For this reason our baseline regression (as per equation 1) is estimated using a binary indicator that is unity in the week when a household received the payment. In order to check that our approach is not confounded by the variation in the amount of the payment, we will also present results that use only single-adult households which received $900 (i.e. those households with income below $80,000). These households comprise over 20 percent of the sample. Thus they provide a significant amount of variation. We will furthermore examine the randomness of assignment by reporting estimates that use a balanced sample. If the assignment is random, restricting the analysis to this sub-sample should yield similar results. In addition, in order to ensure that our results are not driven by outliers, we report as a robustness check estimates that exclude the top and bottom 1st and 5th percentile of (weekly) household consumption expenditure changes.

In equation (1), β captures the contemporaneous response of consumption to the bonus payment. By including additional lags of the bonus payment indicator variable on the right-hand side of equation (1) we can also explore whether household consumption reacted significantly after the receipt of the bonus payment. An alternative approach, for which we will also present estimates

in all tables, is to define an indicator variable *Paid* that is unity in the week of receipt of the bonus payment and all weeks thereafter (the indicator is zero in the weeks preceding the payment):

(1') $$\Delta Expenditure_{pi,t} = \phi Paid_{pi,t} + [a_i'] + b_t' + \varepsilon'_{pi,t}$$

In equation (1') ϕ captures the average effect that the bonus payments had on household consumption expenditures over the 5-week period during which the bonus payments were allocated to households.

4. Main Results

4.1 Baseline Estimates

Table 3 presents our baseline estimates of the nondurable goods consumption response to the pre-announced bonus payment. The dependent variable in columns (1)-(5) is the change in the level of household consumption expenditures. In order to ensure that the results are robust to alternative functional forms we report in columns (6)-(10) estimates where the dependent variable is the change in the log of household consumption expenditures. All estimates are computed from a regression that controls for weekly fixed effects. Hence, the slope estimate on the bonus payment indicator variable is identified from the random allocation of the payment across households in any given week.

The main message of the baseline estimates in Table 3 is that the household consumption response to the bonus payment is statistically insignificant. It is also quantitatively small. For example, the estimate in column (1) implies that the average household, which received a payment of $900, spent in the week upon receipt of the payment an additional 1.4 dollars on non-durables. The one-sided 95% confidence intervals (given in square brackets) show that we can reject an increase in spending of $5 per week or more. Given that the average household spent 120 dollar per week, the average 1.4 dollar increase implies that household expenditures on non-durables increased by around 1 percent. The estimates in column (6) of Table 3, where the dependent

variable is the change in the log of expenditures, convey a similar message. The coefficient (standard error) on the payment indicator is 0.006 (0.021). Hence, according to column (6) household consumption of non-durables increased by less than 0.006 log points (about 0.6 percentage points).

Columns (2)-(4) and (7)-(9) of Table 3 examine whether there are significant lagged effects of the bonus payment on household consumption. In columns (2) and (7) we report estimates from a regression that includes the bonus payment indicator in period *t* and *t*-1. The main result is that including also the *t*-1 indicator on the right-hand side of the regression barely changes the coefficient on the period *t* effect. Moreover, the coefficient on the period *t*-1 effect is statistically insignificant and negative in sign.

There are also no significant effects of the bonus payment on household consumption at further lags. In columns (3) and (8) we report the p-value on the joint hypothesis that the effects over periods *t* to *t*-4 of the bonus payment on consumption growth are zero. That test examines whether over one month there are significant effects of the bonus payment on household consumption. In similar vein, in order to test whether there are significant effects of the bonus payment on consumption when the time horizon is three months (one quarter), we report in columns (4) and (9) the p-value on the joint hypothesis of zero effects over weeks *t* to *t*-12. The main finding is that the p-values are always larger than 0.1. And this is true regardless of whether we use as dependent variable the change in the level of expenditures or the change in the log.

In columns (5) and (10) of Table 3 we report estimates of the average effect that the bonus payments had on consumption growth over the 5-week period during which the payouts were allocated to households. The coefficient (standard error) on the *Paid* indicator variable is 0.91 (1.80) in column (5) where the dependent variable is the change in consumption expenditures. The estimated effect over a 5-week period thus is an average 91 cent per week increase in non-durable consumption of a household that has received the stimulus payment compared to households that

have not yet received the payment. It is quantitatively small and not significantly different from zero. Similar results are obtained in column (10) where the dependent variable is the change in the log of consumption expenditures.

As an identification check we have estimated equation (1) including leads of the payment indicator variable on the right-hand side of the regression. Figure 1 plots the estimated coefficients and their 95% confidence bands. The figure clearly shows that the lead effects are quantitatively small and statistically insignificant. Also, there is no evidence of significant contemporaneous or lagged effects of the bonus payment. Hence the main conclusion from these baseline estimates is that household expenditures on non-durables did not react significantly to the bonus payment.

4.2 Robustness

4.2.1 Single-Adult Households

We report in Panel A of Table 4 estimates for single-adult households with taxable annual income between $18000 and $80000.[10] Single-adult households with annual income between $18000 and $80000 cover about 20 percent of the total number of observations in our sample; they are spread out over nearly 1000 postcodes. Hence, these households comprise a significant part of our sample. By restricting the analysis to single-adult households in the relevant income range, we ensure that the only variation in treatment is the random variation in the timing of the bonus payment. The size of the payment is homogenous across households by construction and it amounted to exactly $900.

The estimates in Panel A of Table 4 show that for this group of single-adult households there was also no significant effect on consumption upon receipt of the payment or thereafter. The estimated coefficients are also quantitatively small. Their 95 percent confidence intervals span the estimated coefficients in Table 3. This is true regardless of whether we use the change in the level of household consumption expenditures or the change in the log. Hence for the sub-set of

10 This particular subgroup was chosen for our robustness checks as it is the only group in our dataset for which we can ensure a uniform payment given a reasonable sample size.

households that received a homogenous $900 bonus payment there is no evidence of excess sensitivity.

4.2.2 Non-Compliance

Our baseline regressions use the largest possible sample, but many of these households missed recording their expenditures for one week or more over the 2-year period. To demonstrate that this non-compliance does not significantly affect our results, we report in Panel B of Table 4 estimates from regressions that use only those households which recorded their expenditures for every week over the 5-week bonus payment period. Using only these households reduces the sample size to about 40 percent. For this subset also, we find that the effects of the bonus payment on consumption are not significantly different from zero and the 95 percent confidence intervals span the estimated coefficients obtained in our baseline regressions (see Table 3).

4.2.3 Excluding Large Positive and Negative Consumption Changes

Table 5 shows that our baseline estimates are robust to excluding consumption observations that could be deemed as outliers. In Panel A we report estimates from a regression that excludes the top and bottom 1st percentile of household consumption expenditure changes; in Panel B we repeat the exercise for excluding the top and bottom 5th percentile. The main finding is that the effects of the bonus payment on consumption are not significantly different from zero when excluding large positive and negative changes in consumption. Quantitatively the estimated effects continue to be small. We therefore conclude that large consumption expenditure changes are not driving our baseline results.

4.2.4 Heterogenous Effects by Household Income

In Table 6 we examine whether the response of non-durable consumption to the bonus payment

differed across households as a function of their income. We implement two alternative strategies to examine heterogenous effects by household income. First, we run a regression that adds to the right-hand side of the estimating equation an interaction variable between the bonus payment and households' income per member (controlling also for the linear effect of households' income per member). The results from this regression are reported in columns (1), (4), (5) and (8) of Table 6. The second strategy that we pursue is splitting the sample into above and below median per capita household income. The estimates from that sample split are reported in columns (2), (3), (6) and (7) of Table 6.

Consistent with our baseline results, the estimated coefficient on the bonus payment indicator shows that non-durable consumption did not respond significantly at the time of the payment. The negative interaction effect and the estimates for the below median household income sample suggest that the consumption response was somewhat larger for low-income households. However, none of these differences are statistically significant.

4.2.5 Multi-Clustering

Columns (1), (3), (5), and (7) of Table 7 show that the baseline results are robust to using the Cameron et al. (2011) multi-cluster estimator. In the baseline regressions we clustered standard errors at the postcode level, hence standard errors were robust to arbitrary within-postcode serial correlation. In order to also account for arbitrary spatial correlation, we now report estimates where the error term is multi-clustered at the postcode and weekly level. The results in Table 7 show that this type of clustering barely changes the standard errors, and thus, the estimated coefficients on the consumption response continue to be insignificant at the conventional significance levels.

4.2.6 Household Fixed Effects and Consumption Levels

Columns (2), (4), (6) and (8) of Table 7 show that the estimated effects and their significance barely

change when we control for household fixed effects. Given that the bonus payments were randomized across postcodes, and that identification of the estimated effects is obtained from both the time-series and cross-postcode variation, this is not surprising. The similarity in results justifies our use of the more parsimonious specification for our baseline regressions. We furthermore show in Table 8 that there is an insignificant effect of the bonus payments if we use as dependent variable the level of consumption and control for household fixed effects.

4.2.7 Controlling for Leads and Lags of Consumption

In Table 9 we document that our estimates are robust to controlling for leads and lags of household consumption. Habit formation in household consumption, for example, would imply that both past and future household expenditures enter the Euler equation. Indeed, we find that these leads and lags of household consumption expenditures are significant.[11] However, including leads and lags of household expenditures on the right-hand side of the estimating equation does not overturn our main finding: the response of non-durable consumption to the actual payment is quantitatively small and statistically insignificant.

4.2.8 Check Payments

In Table 10 we report estimates from a regression that includes -- in addition to the EFT payment indicator variable – an indicator variable for check payments. Consistent with our other results, we find that the current and lagged impact of both EFT and check payments on household consumption expenditures are small and insignificant. Furthermore, the p-values on the joint hypothesis of significance of EFT and check payments over a period of one month and one quarter are always larger than 0.1. This, in turn, indicates that there are no significant effects on consumption growth

11 The reason why in our baseline model we do not include leads and lags of consumption on the right-hand side of the regression is that the payment indicator is an exogenous variable, i.e. the variable is unaffected by future and past consumption. Hence, there is no bias associated with leaving lags and leads of consumption out of the regression; of course, the interpretation of the estimated coefficient changes as the model changes from a static model to a dynamic model.

of either the EFT or the check payments when longer time periods are considered.

4.2.9 Non-Durable Consumption Quantities

Up until this point we have looked at the effect of the payments on nondurable consumption expenditures. As a final robustness check we reproduce our baseline regressions replacing the value of nondurable goods purchased (i.e. nondurable consumption expenditures) with the number of nondurable goods purchased (i.e. nondurable consumption quantities). The results, presented in Table 11, show that our finding of no statistically significant impact from receipt of the bonus payment on nondurable consumption is robust to using quantity instead of total value as the measure of consumption.

4.3 Anticipation Effects

We now turn to discussing our estimates of anticipation effects. As discussed in detail in Section 2, the Nation Building and Jobs Plan stimulus package was announced on 3 February 2009. The package was passed by parliament on 18 February but subsequently challenged in the High Court of Australia on 26th of February before being declared constitutionally conforming on 3 April. The tax bonus payouts were then immediately commenced.

The AC Nielson consumption data, which are weekly, enable us to examine how household consumption expenditures responded to the announcement of the fiscal stimulus. The response of economic variables to the announcement of fiscal stimulus -- and how this response to the announcement differs from the response to the actual fiscal stimulus -- is an important topic in the fiscal policy literature (see e.g. Leeper et al., 2012; Mertens and Ravn, 2010; Ramey, 2011). The announcement effect is common across households, thus in order to estimate the dynamic response of household consumption expenditures to the announcement of the Nation Building and Jobs Plan, we have to rely on conventional time-series methods used in the literature. As a consequence we

can no longer control for common shocks and our findings in this section need to be interpreted with this caveat in mind. In order to test for the "news" effect the weekly data is key: monthly data would hide the news effect because the announcement and challenge was in the same month (see above).

We begin the analysis by collapsing the AC Nielson panel dataset by week. This generates an aggregate consumption time-series of 103 weekly observations. We detrend the obtained consumption data using a linear time trend and month dummies. We then estimate a five-dimensional VAR. This VAR contains the following variables (in that order): dummies for the week of the announcement, the court challenge and parliament date, a dummy for the weeks of the payment, and the log of the change in the collapsed AC Nielson consumption expenditures. We use the Choleski approach to obtain the orthogonalized impulse response functions. The identifying assumption in the Choleski approach is that contemporaneously, on a week-to-week basis, fiscal policy is exogenous to macroeconomic conditions.

Figure 2A shows the orthogonalized impulse response of consumption growth to the fiscal shocks; Figure 2B shows the cumulative orthogonalized impulse response. The top left-hand panel contains the impulse response of consumption growth to the announcement of the fiscal stimulus package. The impulse response function in Figure 2A shows that in the week of the announcement consumption growth increased. The contemporaneous response of consumption growth to the announcement effect is significant at the 10 percent level. The after-effects on consumption growth are statistically insignificant and quantitatively close to zero after about one month. From the cumulative impulse responses shown in Figure 2B we see that the cumulative effects on nondurable consumption are positive and significant. The impulse response analysis also shows that there are no significant effects of the actual bonus payment, which is consistent with the findings that we obtained from the randomization analysis.

In Table 12 we present estimates from a single-equation model where the dependent variable

is the weekly change in (log) non-durable consumption expenditures. Columns (1) and (3) show estimates if we only include in the econometric model the announcement dummy, controlling for month fixed effects. In columns (2) and (4) we add dummy variables for the week of the challenge in court, and parliament date, as well as a dummy for the weeks of the payment. In columns (1) and (2) the dependent variable is the change in non-durable consumption expenditures; in columns (3) and (4) the dependent variable is the change in the log of non-durable consumption expenditures.

Resonating the findings from VAR analysis, the single-equation estimates show that the effect of the announcement of the stimulus on non-durable consumption is positive and significant. On the other hand the effect of the actual payout is insignificant. Quantitatively, the estimated coefficient on the announcement dummy suggests that the announcement of the tax bonus payments induced a change in non-durable consumption expenditures of $22.48.

5. Conclusion

In response to the global financial crisis of 2008-09, Australia put in place one of the largest fiscal policy packages in the developed world. Approximately $8 billion was randomly disbursed to Australian households as part of the Australian Nation Building and Jobs Plan. In this paper we exploited the random allocation of the disbursements to households to estimate the response of household non-durable consumption expenditures to the transitory fiscal transfer, which was pre-announced. The prediction of the rational expectations permanent income hypothesis is that the pre-announced transfer will not affect the path of non-durable consumption during or after the time of receipt by the households. Consistent with this prediction, our empirical analysis showed that the effects of the fiscal transfer on the change in household non-durable consumption expenditures is statistically insignificant and quantitatively small. We also documented that this finding is robust across specifications and sub-samples.

In light of recent literature that has highlighted the importance of accounting for fiscal foresight when studying the effects of fiscal policy on the business-cycle we also explored the response of household non-durable consumption to the announcement of the stimulus. Using standard time-series techniques, we found a significant reaction of household consumption to the announcement of the stimulus. In line with our results from the panel analysis that exploited the randomization of transfers across households, the time-series analysis showed no significant response of non-durable consumption growth during or after the time when households received the transfer. Our findings from the time-series analysis underscore recent literature on fiscal foresight that has emphasized the importance of distinguishing between the effects of the actual fiscal stimulus and its announcement.

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Table 1 Summary Statistics

Variables	Mean	Standard Deviation
Weekly Household Expenditure (in $AU)	119.0	55.3
Weekly Quantity of Good	40.4	18.2
Household Size	2.9	1.4
Annual Household Income (in 1000 $AU)	66.1	41.3
Average age of Primary Shopper	48.0	13.6
Sex of Primary Shopper (Men=1)	0.22	
Shopper Full Time Employment	0.37	
Shopper Retired	0.12	
Shopper Home Duties	0.20	
Shopper Foreign Born	0.20	

Table 2 Summary Statistics by Payweek

	Variable Means by Week			
	Week 1	Week 2	Week 3	Week 4
Weekly Expenditure (in $AU)	119.8	117.2	119.8	121.3
Weekly Quantity of Good	40.4	40.0	40.7	40.4
Household Size	2.9	2.9	2.9	2.9
Household Income (in 1000 $AU)	66.0	65.7	66.9	66.3
Average age of Primary Shopper	48.2	47.7	48.2	48.1

Table 3 Response of Non-Durable Consumption to Fiscal Transfer (Baseline Estimate)

	1st Difference Non-Durable Consumption					1st Difference log(Non-Durable Consumption)				
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Payment, t	1.44	1.41				0.006	0.011			
	(2.11)	(2.13)				(0.02)	(0.02)			
upper 95% CI	[4.9]	[4.9]				[0.04]	[0.05]			
Payment, t-1		-1.70					-0.002			
		(2.02)					(0.02)			
upper 95% CI		[1.6]					[0.03]			
Paid					0.91					0.011
					(1.80)					(0.018)
upper 95% CI					[3.9]					[0.04]
Joint Significance 4 Lags, P-value			0.84					0.65		
Joint Significance 12 Lags, P-value				0.41					0.84	
Weekly FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	479069	407390	286750	151352	479069	479048	407373	286744	151348	479048
Postcodes	1571	1561	1520	1326	1571	1571	1561	1520	1326	1571
Weeks	103	102	99	93	103	103	102	99	91	103

Notes: Standard errors in parentheses; * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$;

Table 4 Response of Non-Durable Consumption to Fiscal Transfer (Robustness: One-Adult Households with $900 Payment and Balanced Panel)

	1st Difference Non-Durable Consumption					1st Difference log(Non-Durable Consumption)				
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
	Panel A: One-Adult Households with $900 Payment									
Payment, t	-2.42 (3.12)	-2.79 (3.30)				-0.01 (0.05)	-0.008 (0.05)			
Payment, t-1		0.39 (3.39)					0.02 (0.05)			
Paid					0.62 (2.83)					0.022 (0.04)
Joint Significance 4 Lags, P-value			0. 89					0.95		
Joint Significance 12 Lags, P-value				0.71					0.85	
Weekly FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	89189	75607	52583	27468	89189	89183	75602	52579	27466	89183
Postcodes	911	886	816	622	911	911	886	816	622	911
Weeks	103	102	99	91	103	103	102	99	91	103
	Panel B: Balanced Panel									
Payment, t	2.54 (2.61)	2.38 (2.38)				0.02 (0.026)	0.02 (0.02)			
Payment, t-1		-1.67 (2.34)					0.007 (0.02)			
Paid					1.10 (2.05)					0.023 (0.02)
Joint Significance 4 Lags, P-value			0.61					0.90		
Joint Significance 12 Lags, P-value				0.31					0.72	
Weekly FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	234103	216749	176851	112989	234103	234103	216749	176851	112989	234103
Postcodes	1125	1125	1124	1082	1125	1125	1125	1124	1082	1125
Weeks	103	102	99	91	103	103	102	99	91	103

Notes: Standard errors in parentheses; * p < 0.1, ** p < 0.05, *** p < 0.01;

Table 5 Response of Non-Durable Consumption to Fiscal Transfer (Robustness: Excluding Extreme Consumption Changes)

	1st Difference Non-Durable Consumption					1st Difference log(Non-Durable Consumption)				
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
	Panel A: Excluding Top and Bottom 1st Percentile of Dependent Variable									
Payment, t	0.20	-0.89				-0.001	-0.005			
	(1.78)	(1.83)				(0.02)	(0.02)			
Payment, t-1		-2.42					-0.013			
		(1.83)					(0.02)			
Paid					-0.40					0.003
					(1.58)					(0.018)
Joint Significance 4 Lags, P-value			0.75					0.57		
Joint Significance 12 Lags, P-value				0.40					0.68	
Weekly FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	469353	399573	281479	148710	469353	469334	399558	281473	148706	469334
Postcodes	1571	1560	1518	1326	1571	1571	1560	1518	1326	1571
Weeks	103	102	99	91	103	103	102	99	91	103
	Panel B: Excluding Top and Bottom 5th Percentile of Dependent Variable									
Payment, t	0.57	0.43				0.007	0.011			
	(1.40)	(1.48)				(0.02)	(0.02)			
Payment, t-1		-0.39					0.004			
		(1.42)					(0.02)			
Paid					-0.066					0.011
					(1.28)					(0.016)
Joint Significance 4 Lags, P-value			0.90					0.51		
Joint Significance 12 Lags, P-value				0.52					0.67	
Weekly FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	431010	367876	259811	137686	431010	430996	367865	259806	137683	430996
Postcodes	1570	1558	1511	1324	1570	1570	1558	1511	1324	1570
Weeks	103	102	99	91	103	103	102	99	91	103

Notes: Standard errors in parentheses; * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$;

Table 6 Response of Non-Durable Consumption to Fiscal Transfer (Heterogeneity by Household Income)

	1st Difference Non-Durable Consumption				1st Difference log(Non-Durable Consumption)			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Heterogeneity by Household Income							
		Bottom 50th Pctl.	Top 50th Pctl.			Bottom 50th Pctl.	Top 50th Pctl.	
Payment	1.49	2.37	0.49		0.006	0.003	0.01	
	(2.16)	(2.95)	(2.87)		(0.02)	(0.03)	(0.03)	
Payment * HH Income per capita	-0.01				0.0002			
	(0.08)				(0.0009)			
Paid				0.91				0.011
				(1.81)				(0.018)
Paid* HH Income per capita				0.0015				0.00
				(0.0075)				(0.00)
HH Income per capita	-0.005			-0.006	-0.00007*			-0.0001**
	(0.004)			(0.005)	(0.00004)			(0.00005)
Weekly FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	479069	238994	240075	479069	479048	238988	240060	479048
Postcodes	1571	1353	1286	1571	1571	1353	1286	1571
Weeks	103	103	103	103	103	103	103	103

Notes: Standard errors in parentheses; * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$; Per capita Household Income is centered around its mean.

Table 7 Response of Non-Durable Consumption to Fiscal Transfer (Robustness: Multi-Clustering, Household Fixed Effects)

	1st Difference Non-Durable Consumption				1st Difference log(Non-Durable Consumption)			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Multi Cluster	Household FE	Multi Cluster	Household FE	Multi Cluster	Household FE	Multi Cluster	Household FE
Payment, t	1.44	1.44			0.007	0.007		
	(2.08)	(2.13)			(0.02)	(0.02)		
Paid			0.91	1.09			0.011	0.012
			(1.71)	(1.87)			(0.019)	(0.019)
Weekly FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	479069	479069	479069	479069	479048	479048	479048	479048
Postcodes	1571	1571	1571	1571	1571	1571	1571	1571
Weeks	103	103	103	103	103	103	103	103

Notes: The error term is multi-clustered at the postcode and weekly level. Standard errors in parentheses; * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$;

Table 8 Non-Durable Consumption Response to Fiscal Transfer (Robustness: Level of Consumption and Household Fixed Effects)

	Level of Non-Durable Consumption					Log(Non-Durable Consumption)				
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Payment, t	1.44	1.29				0.007	0.011			
	(2.11)	(2.13)				(0.02)	(0.02)			
Payment, t-1		-1.98					-0.004			
		(2.02)					(0.02)			
Paid					1.09					0.012
					(1.85)					(0.019)
Joint Significance 4 Lags, P-value			0.85					0.61		
Joint Significance 12 Lags, P-value				0.42					0.79	
Weekly FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Household FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	479069	407390	284787	150462	479096	479048	407373	284781	150498	479048
Postcodes	1570	1560	1519	1325	1570	1570	1560	1519	1325	1570
Weeks	103	102	99	93	103	103	102	99	91	103
Households	10196	9857	8368	5214	10196	10196	9856	8368	5214	10196

Table 9 Response of Non-Durable Consumption to Fiscal Transfer (Robustness: Consumption Dynamics)

	1st Difference Non-Durable Consumption				1st Difference log(Non-Durable Consumption)			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Payment, t	1.05	0.98			-0.000	-0.002		
	(1.46)	(1.60)			(0.01)	(0.02)		
Paid			-0.62	-0.39			-0.0079	-0.0079
			(1.64)	(1.64)			(0.018)	(0.017)
Consumption, t-1	-0.70***	-0.78***	-0.70***	-0.78***	-0.72***	-0.80***	-0.72***	-0.80***
	(0.01)	(0.00)	(0.01)	(0.00)	(0.01)	(0.004)	(0.01)	(0.004)
Consumption, t+1		0.22***		0.22***		0.21***		0.21***
		(0.005)		(0.005)		(0.004)		(0.004)
Weekly FE	Yes	Yes			Yes	Yes		
Observations	479069	407390	479069	407390	479048	407369	479048	407369
Postcodes	1571	1561	1571	1561	1571	1561	1571	1561
Weeks	103	102	103	102	103	102	103	102

Notes: Standard errors in parentheses; * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$;

Table 10 Response of Non-Durable Consumption to Fiscal Transfer (Robustness: Check Payments)

	1st Difference Non-Durable Consumption					1st Difference log(Non-Durable Consumption)				
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Payment, t	1.62	1.37				0.006	0.008			
	(2.13)	(2.20)				(0.02)	(0.02)			
Payment, t-1		-1.85					-0.01			
		(2.66)					(0.03)			
Check Payment, t	-1.04	0.24				0.003	0.01			
	(1.88)	(2.45)				(0.02)	(0.03)			
Check Payment, t-1		0.25					0.005			
		(1.85)					(0.02)			
Paid					1.71					0.013
					(2.38)					(0.025)
Check Paid					-1.28					-0.002
					(1.81)					(0.018)
Joint Significance Check Payment 4 Lags, P-value			0.56					0.38		
Joint Significance Check Payment 12 Lags, P-value				0. 40					0.76	
Weekly FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	479069	407390	284787	150462	479069	479048	407373	284781	150458	479048
Postcodes	1571	1561	1520	1326	1571	1571	1561	1520	1326	1571
Weeks	103	102	99	91	103	103	102	99	91	103

Notes: Standard errors in parentheses; * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$;The joint significant tests for the EFT payment lags are not reported, nor are the joint test of both payment type lags. Both of these have very low F-statistics and the null hypothesis can't be rejected for any of joint tests.

Table 11 Response of Non-Durable Consumption to Fiscal Transfer (Robustness: Quantities as Dependent Variable)

	1st Difference Non-Durable Consumption					1st Difference log(Non-Durable Consumption)				
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Payment, t	0.19	0.22				0.004	0.005			
	(0.61)	(0.63)				(0.02)	(0.02)			
Payment, t-1		-0.48					-0.008			
		(0.62)					(0.02)			
Paid					-0.002					0.001
					(0.52)					(0.018)
Joint Significance 4 Lags, P-value			0.87					0.63		
Joint Significance 12 Lags, P-value				0.55					0.88	
Weekly FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	479069	407390	284787	150462	479069	479067	407388	284786	150462	479067
Postcodes	1571	1561	1520	1326	1571	1571	1561	1520	1326	1571
Weeks	103	102	99	91	103	103	102	99	91	103

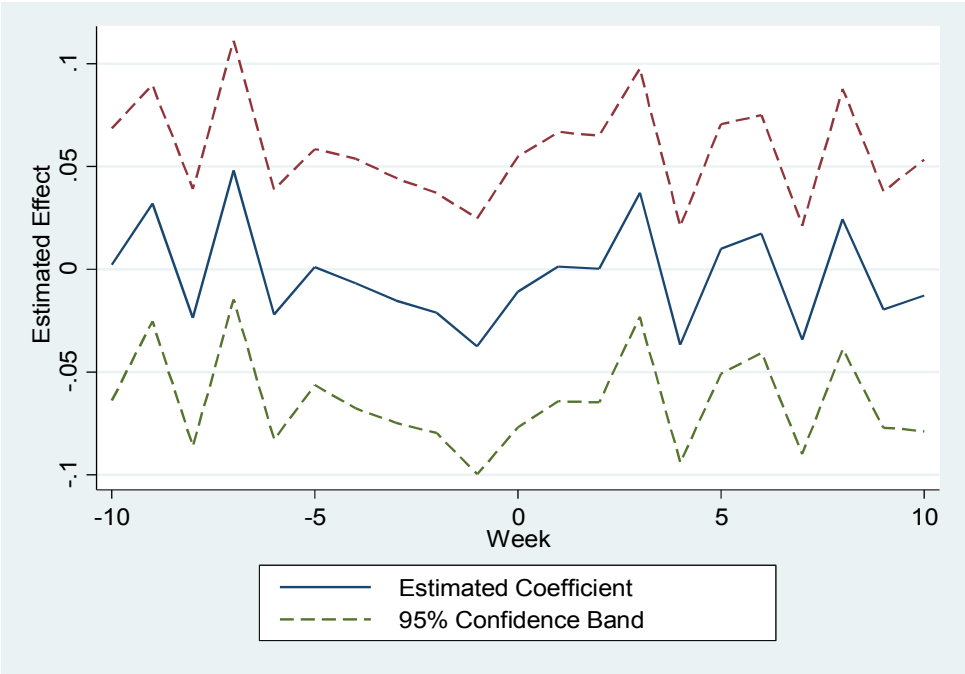
Notes: Standard errors in parentheses; * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$;

Table 12. Effects of Stimulus Announcement on Non-Durable Consumption

Dependent Variable	1st Difference Non-Durable Consumption Expenditures		1st Difference log(Non-Durable Consumption Expenditures)	
	(1)	(2)	(3)	(4)
Announcement Dummy	22.46* (12.98)	23.00* (13.33)	0.22** (0.11)	0.23** (0.11)
Parliament Dummy		3.77 (13.31)		0.08 (0.11)
Court Dummy		4.92 (13.58)		0.07 (0.11)
Payment Dummy		-1.12 (7.62)		-0.00 (0.06)
Month Fixed Effects	Yes	Yes	Yes	Yes
Observations	103	103	103	103

*Notes: Standard errors in parentheses; * p < 0.1, ** p < 0.05, *** p < 0.01;*

Figure 1 Plot of Impact, Lag, and Lead Effects



Note: The solid line shows the estimated coefficients on the payment indicator in week *t*-10 to t+10. The dashed line shows the 95% confidence bands. The estimates are obtained from estimating equation (1) with the t-10 to t+10 payment indicator; the dependent variable in that estimation is the change in the log of supermarket expenditures.

Figure 2 Impulse Response of Household Consumption to the Fiscal Stimulus

Figure 2A: Orthogonalized Impulse Response

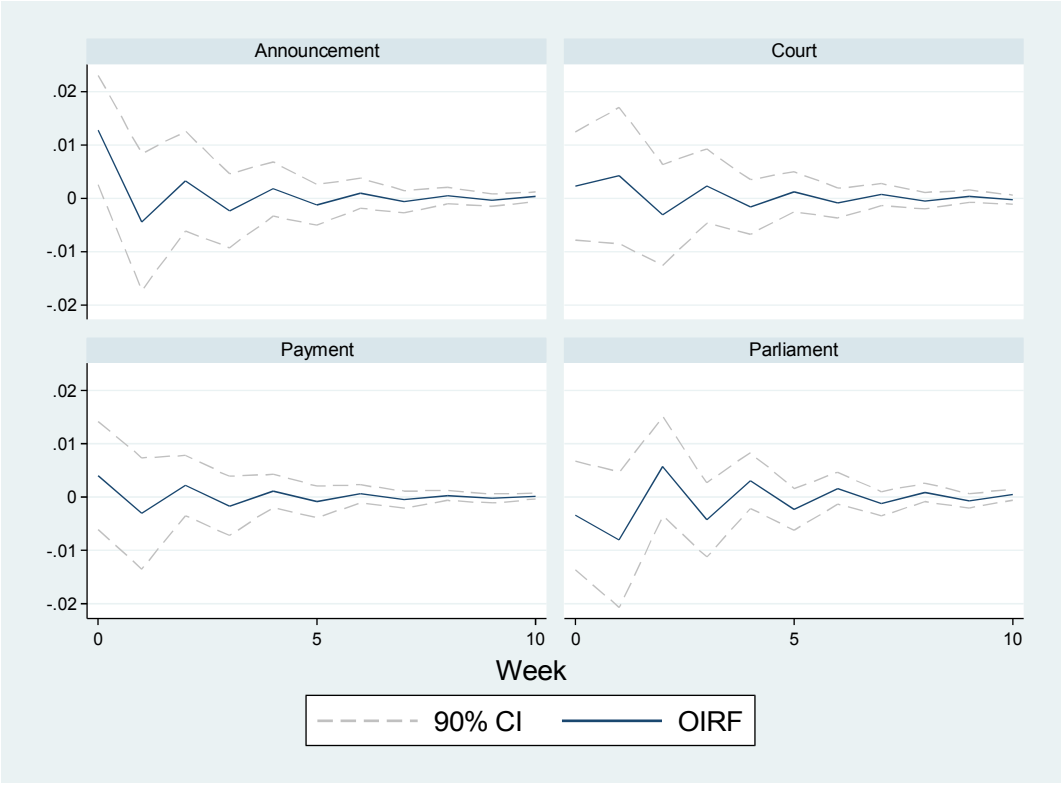


Figure 2B: Cumulative Orthogonalized Impulse Response