Do Temporary Cash Transfers Stimulate the Macroeconomy?

Evidence from Four Case Studies

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Abstract

In this paper, I evaluate the effectiveness of temporary transfers in stimulating the macroeconomy using four case studies. I begin by discussing the rebirth of Keynesian stabilization policy and the lingering costs of stimulus packages in terms of higher debt paths. I then summarize a method I developed in my recent work --- historical plausibility analysis --- which can be used by researchers and policymakers to choose from a range of estimates from the literature. For the case studies, I begin by briefly summarizing the results of my recent work on the 2001 and 2008 U.S. tax rebates. I then analyze two new case studies of temporary transfers in Singapore and Australia. In all four instances, the evidence suggests that temporary cash transfers to households likely provided little or no stimulus to the macroeconomy.

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1. **Introduction**

It is a great honor and a privilege to give the Mundell Fleming lecture at the IMF. I was initially reluctant because so little of my research has been specifically focused on international macro issues. However, when I saw the theme of this year's conference --- "Rethinking the Policy Toolkit in a Global Turbulent Economy" --- I realized that my research fit well. In choosing my specific topic, I was inspired by what I learned the past year while conducting an external evaluation of the IMF's Fiscal Affairs Department. After reading through many of their publications, I was struck by the substantial challenges that IMF staff face when they must decide what policy advice to give. When they review empirical and model estimates from the literature, they are often faced with a very wide range of estimates. How should one decide which estimate to use? In this lecture, I suggest that *historical plausibility analysis* can help determine which estimates are most plausible and I illustrate the method with case studies of temporary cash transfers.

Temporary cash transfers were widely used by governments to stimulate their macroeconomies during the Global Financial Crisis and again during COVID. Most policymakers and economists believe that temporary cash transfers are effective macro stimulus tools. My 2019 review of the state of knowledge about the effects of fiscal policy noted, however, that estimates based on aggregate time series data tended to suggest lower multipliers for both government purchases and transfers than macro models calibrated with marginal propensities to consume estimated from household data or subnational data (Ramey (2019a). That paper analyzed the discrepancy between estimates from subnational data and aggregate data in one study, but did not address the estimates from household data.

This paper documents and analyzes the discrepancy between the implications of macro models calibrated with micro estimates of the marginal propensity to consume and the behavior of aggregate data using four case studies. The first two are from the U.S. and are based on the results of my recent work. The second two are for episodes in two small open economies, Singapore and Australia. Of the four cases, two are from the Global Financial Crisis, a third is from the 2001 recession, and the fourth is from a recurring natural experiment in Singapore.

COVID is a fascinating episode and the stimulus payments were very large. However, I do not apply the techniques to any of the COVID stimulus episodes in this paper because the many unprecedented circumstances surrounding COVID make it difficult to do historical plausibility analysis. The special circumstances include the following. First, lockdowns prevented certain

types of consumption, such as for restaurants and travel, so there were constraints on the MPCs for some types of consumption. Second, being locked down created sectoral shifts in demand to other types of consumer goods, such as furniture for home offices and home exercise equipment. Third, constraints on labor supply, both domestically and internationally, led to contractions in supply of certain goods and services and supply chain problems. Fourth, some of the fiscal stimulus may have further reduced supply because in numerous cases the government payments to unemployed workers were greater than the workers' employed wages. Fifth, economies were hit by multiple waves of new COVID variants and vaccinations, creating significant variations in the willingness of individuals to return to more normal routines. All of these factors make it difficult to extract general lessons about MPCs and stimulus. In addition, these same factors make it very difficult to make a case for or against the historical plausibility of a counterfactual since these events were unprecedented. For example, when aggregate consumption rose steeply in late winter and early spring 2021 to a new higher path, persisting through the present, it is difficult to determine how much was due to the March 2021 stimulus and how much was due to successful rollout of vaccinations that allowed individuals to venture out and make up for delayed categories of consumption, such as restaurant dining and travel. For these reasons, a case study of COVID transfers is beyond the scope of the current paper.

The paper proceeds as follows. In Section 2, I take a step back to discuss why Keynesian stimulus experienced a rebirth after falling into disuse for decades. I also note some of the costs of using Keynesian stimulus in terms of rises in debt-to-GDP ratios. Section 3 summarizes current thinking on the macroeconomic effects of temporary cash transfers to households. Section 4 summarizes a recent method I developed with Jacob Orchard and Johannes Wieland that subjects micro estimates and macro models to macroeconomic plausibility tests. Section 5 illustrates those methods using two case studies of the macroeconomic effects of temporary transfers for the U.S. These are for the U.S. rebates in 2001 and 2008, which were analyzed in in my recent work (Orchard, Ramey, and Wieland (2024a,b)). Section 6 and 7 conduct case studies of two small open economies, Singapore and Australia. My assessment of the evidence in all four cases suggests that temporary transfers likely provided little or no macroeconomic stimulus in advanced economies. Section 8 discusses implications and broader lessons.

2. **The Rebirth of Keynesian Stimulus Policy and Its Consequences**

Keynesian fiscal stimulus was a significant part of countercyclical policy from the 1930s through the 1970s, but subsequently fell out of favor as a stabilization tool. Both new academic ideas and practical considerations drove its decline. Research during the 1950s through the 1970s changed researchers and policymakers' views on the relative effectiveness of monetary versus fiscal policy. The Life-Cycle/Permanent Income Hypothesis of Friedman (1957) and Modigliani and Brumberg (1954), augmented with rational expectations by Hall (1978), convinced many economists that these forward-looking theories were better approximations to consumption behavior than the traditional Keynesian consumption function, which had produced so many consumption puzzles. The new theories implied that the marginal propensity to consume (MPC) out of temporary changes in disposable income were close to zero, so fiscal stimulus such as temporary cash payments or tax rebates were unlikely to have much effect on aggregate consumption and GDP. Friedman and Schwartz's (1963) monetary history provided strong evidence for the power of monetary policy. After many lively debates over the relative effectiveness of monetary versus fiscal policy, many economists and policymakers became convinced that monetary policy could have powerful effects on economic activity. Practical considerations further favored monetary policy over fiscal policy as a countercyclical policy tool. Because fiscal policy has competing medium and long-run goals, the political debates are often so lengthy that stimulus packages are not enacted until the economy has already begun to recover. For this reason, monetary policy became the preferred tool for stabilizing the economy because it was believed to be more nimble and powerful.

The situation changed dramatically with the onset of the Global Financial Crisis of 2007-2009. As financial markets melted down and economic activity fell, central banks lowered their policy rates to stimulate their economies, but many hit the effective lower bound. As a result, monetary policy turned to untested, and likely less effective measures, such as quantitative easing. Outside of central banks, policymakers dusted off the traditional Keynesian playbooks involving government purchases and transfers. Because little work had been done on fiscal multipliers since the 1970s, there was much uncertainty about how big they were and how they differed across the various fiscal instruments. The implementation of the Keynesian policies generated important new data, leading to a surge of research on the stimulus effects. The strand of this research that calibrated macro models with micro estimates of marginal propensities to consume suggested that

government purchases and transfers could have significant stimulus effects. Thus, when COVID-19 hit and interest rates fell back to the zero lower bound, policymakers turned again to fiscal policy, both to provide income insurance and as a countercyclical stimulus.

These Keynesian policies have lingering macroeconomic consequences, though. To maximize their stimulative effects, the packages were financed with deficits. Contrary to some theoretical arguments (Delong-Summers (2010), Angeletos et al. (2024)), the deficits do not seem to have financed themselves, except perhaps with some inflation. Despite this, many countries still have not enacted fiscal consolidations to bring debt to GDP ratios to more sustainable paths.

Figure 1 shows the ratchet effect of these two crises on the debt-to-GDP ratios of groups of countries. The top row of graphs shows the responses for advanced economies and emerging market and developing economies. In response to the GFC, the debt-to-GDP ratio in advanced economies rose from 70 percent in 2007 to almost 100 percent in 2010 and did not decline. With COVID, it shot up from just over 100 to over 120, before falling back down to 111 percent. The rise in debt during the GFC was small for emerging markets and developing economies, but the rise during COVID was 10 percentage points and it has not receded. The numbers beyond 2023 reflect the IMF's projections of debt-to-GDP ratios, which show a continuing steep upward march.

The bottom row of graphs in Figure 1 looks at debt-to-GDP ratios of the European Union and the U.S. Both show increases during the crises, but the European Union exhibits a significant reversal after the peak and then projected leveling off at around 85 percent. In contrast, the U.S. starts at a lower level of debt in 2000 but then outpaces the European Union by soaring to 130 percent. Unlike the European Union, there is no reversal in the U.S. after the GFC. There is a small reversal after COVID, largely due to inflation, but then debt continues on a steep upward trajectory.

{{Figure 1 here}}

Why have so many countries been reluctant to enact fiscal consolidations? I suggest that two main reasons apply to advanced economies. The first is current thinking on multipliers. As I summarized in Ramey (2019a), empirical estimates based on narrative evidence suggest that tax multipliers are between -2 and -3 (e.g. Romer and Romer (2010), Mertens and Ravn (2012, 2013)),

which are substantially greater in magnitude than estimated spending multipliers. Studies that compare the output effects of tax-based consolidations versus spending-based consolidations find that the output effects of tax-based consolidations are much more negative (e.g. Leigh et al. (2010), and Alesina et al. (2019)). Thus, the evidence suggests that consolidations through tax increases can be very painful. Spending-based consolidations would thus seem to be the optimal strategy. However, much spending in advanced economies is entitlement spending on older adults, which can be politically difficult to cut.

The second reason that debts have not been reduced is that politicians have seized on academic discussions about the possibility of rolling debt over forever without ever having to increase taxes (e.g. Blanchard (2019)). Some policymakers have taken the ideas to an extreme, particularly in the U.S. where politicians no longer react to deficits (Auerbach and Yagan (2024)).

Most economists have now concluded that debt paths are unsustainable and that fiscal consolidations are in order. These consolidations are likely to create significant GDP losses. Thus, it is useful to assess whether the benefits of the Keynesian stimulus packages were worth the cost.

3. **Current Thinking on the Macroeconomic Effects of Cash Transfers to Households**

The resurgence of fiscal research inspired by the events of the Global Financial Crisis built on an important empirical literature that started in the 1980s. Hall and Mishkin (1982) were the first to use the data from the Panel Study of Income Dynamics to test the permanent income hypothesis on individual households. Their estimates led them to conclude: "The observed covariation of income and consumption is compatible with pure life cycle/permanent income behavior for 80 per cent of consumption and simple proportionality of consumption and income for the remaining 20 per cent." Campbell and Mankiw (1989) later estimated the time series relationship between aggregate consumption and income and explained their results as stemming from half the households following a "rule of thumb" of consuming all their current income.

Building on Hall and Mishkin's (1982) micro analysis, numerous studies have provided estimates that have shifted beliefs of many academic economists and policymakers away from the permanent income hypothesis back toward consumption theories that looked more like the traditional Keynesian consumption function. These empirical studies use household-level data and applied-micro techniques to estimate the response of household consumption to temporary changes

in income, some anticipated and some unanticipated. Most of the published studies have found violations of the permanent income hypothesis. Examples of these studies include Zeldes (1989); Parker, Souleles, and co-authors (multiple studies from 1989 to the present); Jappelli and Pistaferri (multiple studies from 2000 - 2014); Shapiro and Slemrod (multiple studies from 2001 – 2014, some with Sahm); Agarwal, Liu, and Souleles (2007), Agarwal and Qian (2014); Kueng (2018); Fagereng et al. (2021); Chetty and Friedman (2021); and Baker et al. (2023).

The implementation of the Keynesian-style policies in the 21st century produced rich new data for economists and policymakers to analyze. The studies of the effects of the 2001 U.S. tax rebate by Shapiro and Slemrod (2003a,b) and Johnson, Parker, Souleles (2006); and of the 2008 U.S. tax rebate by Sahm, Shapiro, and Slemrod (2012), Broda and Parker (2014), and Parker, Johnson, Souleles, and McClelland (2013) are stellar examples of creatively adding questions about rebates to existing research and government surveys and using applied micro methods to shed light on macro questions. The estimates produced by Parker and co-authors suggested surprisingly high marginal propensities to spend out of temporary transfers.[1]

Recent additional studies that exploited natural experiments, such as Agarwal and Qian (2014), Kueng (2018), and Fagereng et al. (2021), also found very high marginal propensities to consume (MPC). For example, Agarwal and Qian (2014) find cumulative MPCs of 0.8 eight months after the payout. These results along with the ones based on tax rebates led to a burst of theoretical work on heterogeneous agent models, particularly in a New Keynesian context, to explain why MPCs out of temporary payments could be so high. Examples include the Two-Agent New Keynesian (TANK) models by Gali, Lopez-Salido, Valles (2007) and Bilbiie (2008) and the Heterogeneous Agent New Keynesian (HANK) models by Kaplan, Violante (2014), Kaplan, Moll, and Violante (2018), and Auclert, Rognlie, and Straub (2024).

The Auclert et al. (2024) work explores the effects of fiscal policy in a variety of HANK models. They calibrate the intertemporal MPCs in their model to the estimates from Fagereng et al.'s (2021) study of consumption responses of lottery winners in Norway. Fagereng et al.'s estimates imply an MPC of 0.5 in the first year and 0.18 in the second year. Auclert et al. (2024) show that with this calibration, deficit-financed multipliers on government purchases can be very

[1] In some cases, that spending was on durable goods purchases. This type of spending has the potential to stimulate the economy, though it is not technically a marginal propensity to consume since durable consumption is proportional to the stock of durable goods, not the purchases of durable goods. I nonetheless use "marginal propensity to consume," or MPC, even for spending on durable goods.

high. If real interest rates are held constant, the cumulative multipliers on government purchases are 3 or above, depending on the model; with active Taylor rules, the multipliers are around 1.3. As they point out, the transfer multiplier is approximately equal to the government purchases multiplier minus one. Thus, using their results, the cumulative multipliers on transfers are 2 or above when real interest rates are held constant. These results mean that transfers can be powerful in stimulating the macroeconomy, particularly when monetary policy is accommodative.

4. **Assessing Plausibility Using Historical Counterfactuals**

The large aggregate multipliers implied by some micro estimates and HANK models along with the large size of stimulus payments in many countries during the last few decades suggests that we should see big effects in the aggregate data. Do we see those effects in the macro data?

In Orchard, Ramey, and Wieland (2024a, 2024b), we develop a new methodology that seeks to answer that question. The methodology assesses whether the implications of macro models calibrated with micro estimates lead to accounts of historical periods that are *plausible*. This methodology, which has antecedents in Chetty, Guren, Manoli, and Weber (2013), Sahm, Shapiro, and Slemrod (2012), and Ramey (2019a, b), first uses a macro model calibrated with micro estimates to compute the deviation in the path of macro variables caused by an actual aggregate policy. Second, it creates a counterfactual path --- how the economy would have behaved absent the policy --- by subtracting the deviation implied by the model from the actual path of the macro variables during the historical period when the policy was in force. Third, using techniques such as narratives and forecasting, it assesses whether that counterfactual path is plausible. If the counterfactual path is judged implausible, researchers should then revisit the original micro estimates and/or the macro model to determine what elements could be causing the implausible counterfactual.

This type of analysis can be useful to economists and policymakers who often must choose among a wide range of estimates of the effects of a particular policy. My coauthors and I argue that researchers and policymakers should subject the evidence and models to what we call historical plausibility analysis. It creates a rigorous basis for choosing between models and estimates so that one is not tempted to choose the estimate and/or model that is the most convenient for the present purpose.

There are several key ingredients required for this type of analysis. First, one needs either micro or subregional estimates of key parameters that are relevant for macro. These could be MPCs or state, province, or city multipliers. Second, one needs a policy or event that is big enough to be visible in the aggregate data. This ingredient is essential. Note that the micro estimates do not necessarily need to be based on micro data from that episode, although whatever episode those micro estimates are based on should have similar conditions. For example, if the historical event occurs in an advanced economy, then it is best if the micro estimates are based on micro data from an advanced economy. Third, one needs a macro model that translates those micro or subregional estimates to general equilibrium effects in order to create the counterfactual. Fourth, one needs well-measured aggregate data to be able to compare to the counterfactual. Fifth, one must then conduct a narrative analysis and/or forecasts from the historical period surrounding the policy or event to assess whether that macro counterfactual is plausible.

Three of the case studies analyzed in this paper use the historical plausibility method. The fourth case study on Singapore does not, but only because the narrative research revealed that the natural experiment was repeated several times so that standard time series methods could be used.

5. Two U.S. Case Studies: The 2008 and 2001 Tax rebates.

This section summarizes the results of Orchard et al. (2024a,b) concerning the two "Bush" tax rebates in the U.S. I begin with the 2008 rebate both because it is the most striking and it reveals more economic insights. I then briefly review our findings regarding the 2001 rebate.

A. The 2008 U.S. Tax Rebates

The 2008 U.S. tax rebates were a leading counterexample to the belief that discretionary fiscal policy was not sufficiently nimble to be used for stabilization policy. The economy had just peaked in December 2007, but by January 2008 most contemporaneous observers could already see that the US economy was starting to go downhill. A tax rebate of $100 billion, equal to 11 percent of monthly income, was passed in February 2008. It was paid out mostly from May through July 2008. Among the 85 percent of the tax units receiving a rebate, the average check was $1,000.

{{Figure 2 here}}

Figure 2 shows real U.S. aggregate disposable income and consumption, in billions of January 2008 dollars at a monthly rate. The left and right axes have different starting points but the same vertical length so that changes are equivalent across the two series. Disposable income exhibits a large spike, with a peak in May 2008 when almost $50 billion was disbursed. In contrast, consumption expenditures display a small hump, with a peak in May, but the magnitude of the rise is tiny compared to the spike in disposable income.

A similar comparison of the behavior of aggregate disposable income to consumption and saving rates led Feldstein (2008) and Taylor (2009) to conclude that the marginal propensity to consume must have been low and therefore the rebate did little to stimulate the economy. A few years later, however, some influential new micro estimates appeared. In particular, Parker, Souleles, Johnson, and McClellan (2013) added questions about the rebate to the BLS's Consumer Expenditure Survey (CEX) and Broda and Parker (2014) added questions to the Nielsen survey. In both cases, they applied what were then standard applied micro methods, which were two-way fixed effects regressions that exploited the variation in timing. For the CEX data, they estimated very high MPCs, between 0.5 and 0.9 for total consumption. The 0.5 estimate was for a three-month period and the 0.9 estimate was for a six-month period. Moreover, an important part of that spending was on motor vehicles.

At that time, the profession was impressed with applied micro techniques, which according to Angrist and Pischke (2010) had undergone a "credibility revolution," so more weight was put on the estimates obtained with applied micro methods than with macro time series methods. To obtain aggregate effects, many macroeconomists constructed models that could generate high micro MPCs and used the models to show that the macroeconomic effects could be large. Policymakers and researchers saw these results and concluded that temporary transfers were very effective stimulus programs. They ignored the simple macro analysis conducted by Feldstein and Taylor. No one bothered to explain how such high MPCs could be reconciled with the behavior of the aggregate data shown in Figure 2.

In Orchard et al. (2024a), we searched for a reconciliation. First, we rigorously quantified the extent to which the aggregate data was at odds with the micro estimates and macro models. We used the Parker team's micro MPC estimates based on CEX data to construct counterfactuals to see what the estimates implied would have happened to aggregate consumption in 2008 if there had been no rebate. For the micro counterfactual, we multiplied the estimated micro MPC by the aggregate path of rebates to calculate what the induced aggregate spending was, holding all general equilibrium effects constant. Then, to allow for general equilibrium effects, we calibrated a medium-scale, two-good, two-agent new Keynesian (TANK) model to create the consumption counterfactuals.

Figure 3 shows counterfactuals for several values of the micro MPCs. The black line is the actual data. The lines associated with micro MPCs of 0.5 and 0.9 represent the lower bound and upper bound of the range of the Parker team's estimates. I will discuss the other two lines below.

{{Figure 3 here}}

According to both the micro and macro counterfactuals, if there had been no rebate, the US economy would have collapsed in the summer of 2008 and then mostly recovered even with the failure of Lehman Brothers. We conducted an exhaustive narrative and statistical assessment of the plausibility of these counterfactual paths and concluded that they were implausible. One part of that assessment was studying contemporary professional forecasts. We then developed a time-series forecasting model that incorporated foresight about the negative shocks, such as the failure of Lehman Brothers, and were able to create an even more pessimistic forecast. Our most pessimistic forecast is shown by the dotted line in Figure 3. Even this most pessimistic forecast suggests that the multiplier on the rebate could not have been more than 0.2.

How, then, did we reconcile these aggregate results with micro MPCs and the macro model? Our analysis found issues with both the micro estimates and the macro model.

In recent years, the econometrics literature discovered that the two-way fixed effects estimators could be biased if there are heterogeneous treatment effects. Borusyak and Javarvel (2017) introduced improved methods and applied them to the Broda and Parker (2014) Nielsen data analysis. They found much smaller MPCs than Broda and Parker did.

We applied new econometric techniques to the CEX and also found much lower MPCs. Our estimated MPC for total consumption was 0.3, with all of it spent on motor vehicles. However, as Figure 3 shows, even an MPC of 0.3 produces a counterfactual that looks implausible; it lies below even the most pessimistic forecast. When compared to motor vehicle sales, even the micro counterfactual was strikingly implausible.

We discovered that the relative price of motor vehicles temporarily rose by one percent in the summer of 2008, just as the rebates were being distributed. This pattern was counter to our baseline model which assumed that the relative supply of motor vehicles was infinitely elastic. Once we modified our model to allow for an upward sloping relative supply of motor vehicles, our model captured the relative price increases and featured significant dampening of the general equilibrium effects of the rebate. Because motor vehicles are durable goods, the intertemporal elasticity of substitution for motor vehicles purchases is much greater than for nondurables and services. Thus, even the one percent rise in prices leads to significant crowding out of motor vehicle purchases for the Ricardian consumers. Thus, the generalized model along with our revised estimates reconciled the micro estimates and macro model with the aggregate data. In both cases, the implied multiplier is less than 0.2, suggesting that the rebates contributed little macro stimulus.

B. The 2001 U.S. Tax Rebates

Seven years earlier, in June 2001, President Bush signed into law a 10-year tax cut. As part of that legislation, the government mailed initial tax rebate checks of $300 or $600 to almost 100 million households over the period from late July through late September 2001. The total rebates were $38 billion, 6 percent of initial monthly income. Thus, they should be evident in aggregate disposable income.

Shapiro and Slemrod (2003a,b) added questions to the Michigan Survey of Consumers asking households if they received a rebate and whether they planned to mostly spend, mostly save, or mostly pay off debt with the funds. They found that households mostly saved the rebates.

Johnson, Parker, and Souleles (JPS) (2006) added questions to the BLS's Consumer Expenditure Survey (CEX) asking households if and when they received a tax rebate. This allowed them to match household-level consumption to the receipt of the rebate. JPS constructed nondurable spending categories based on Lusardi's (1996) categories for her earlier work using the

CEX. The nondurable category contains a variety of nondurable goods and services. For their broadest nondurable spending category, they estimated an MPC of 0.375 for the first three months and 0.66 for a six-month period. They noted in passing that they found no effects on durable expenditures and that the MPC estimates on total consumption were much less precise and often negative.

Figure 4 shows the aggregate behavior of disposable income and the aggregate version of JPS's definition of nondurable consumption. Echoing our findings for the 2008 rebate, we see that the rebate led to a significant spike in disposable income in the summer of 2001 but no unusual movement in the consumption aggregate. Following the method described earlier, we constructed macro counterfactuals implied by the JPS micro estimates using a TANK model. Using narratives, forecasts, and comparisons to historical changes in consumption, we argued that the counterfactuals were implausible.

{{Figure 4 here}}

We then investigated how to reconcile the JPS micro estimates with the behavior of the macro data. We did not find evidence of econometric biases this time. However, our examination of the consumption expenditure categories revealed some categorization issues that affected the estimates. We discovered that the JPS nondurable category, which was based on Lusardi's (1996) work, included some durables, such as jewelry and medical equipment. Moreover, we found that it excluded some of the nondurables included in the Bureau of Economic Analysis category of non-durables and services.

To determine whether the MPCs were robust across methods for categorizing goods and services, we used the BLS's (2019) concordance between CEX categories and the BEA's categories to create BEA categories for nondurables. When we used JPS's nondurable consumption definition, we estimated an MPC of 0.32 with a standard error (s.e.) of 0.15, similar to JPS's findings. However, when we use the BEA categories, we estimated MPCs ranging from -0.16 to 0.06 for durables, services, and nondurable goods. For total consumption, we estimate an MPC of -0.07, but with a large standard error of 0.58. When we estimated MPCs for detailed categories, we noticed that some categories had positive MPCs and others had negative MPCs, and that the items included in Lusardi and JPS's nondurable category were mostly the positive

MPC items. What matters for the stimulus is the effect on total consumption. However, the estimated micro MPC for total consumption was estimated to be near zero. That estimates was very imprecise, but this is why the historical plausibility analysis is so valuable: the aggregate data and counterfactuals all point toward MPCs near zero.

Thus, in the case of both the 2001 rebates and the 2008 rebates, we found evidence that the rebates did not stimulate the economy. In the case of the 2001 rebate, our estimates for BEA categories and for total consumer expenditures suggested a micro MPC near zero. The story was more nuanced for the 2008 rebates. Our new estimates implied a micro MPC near 0 for non-motor vehicle spending, but an MPC of 0.3 for motor vehicles, so there was a potential for stimulus. However, aggregate demand for motor vehicles rose very little because relative prices rose. Through the lens of our general equilibrium model, the increased demand of the hand-to-mouth households led to a rise in the relative price of motor vehicles, which crowded out the Ricardian households, resulting in little macro stimulus.

6. **A Case Study of Singapore Cash Transfers**

I now turn to the analysis of two small open advanced economies, Singapore in this section and Australia in the next. I chose Singapore because one of the important papers that estimates very high micro MPCs uses a natural experiment in Singapore.

A. Micro MPC Estimates for Singapore

Agarwal and Qian's (2014) paper is another example of the clever use of natural experiments and rich household data to gain insight into consumer responses to temporary transfers. In February 2011, the Singapore government announced a surprise program of S$3.2 billion, with S$1.5 billion of the amount as a one-time cash transfer "Growth Dividend."[2] 2.5 million adult Singaporean citizens received Growth Dividend payouts between S$600 and S$800, with lower income individuals receiving the higher amounts. These payments were large, 11

[2] S$ denotes Singapore dollars. At the time, one Singapore dollar was worth 0.8 U.S. dollars.

percent of disposable income on average. Foreigners, who were almost 40 percent of the resident population, were ineligible. The payouts were dispersed during the last part of April 2011.

Agarwal and Qian (2014) used a proprietary dataset from the largest Singapore bank to create a monthly panel of 180,000 consumers from April 2010 to March 2013. The dataset contained information on credit card, debit card, and checking account spending. It did not include information on the amount of the dividend payment received by the individuals, so the authors used the government rules and individual characteristics to impute a dividend payment. The natural experiment provided the perfect control group --- the foreigners who were ineligible. Agarwal and Qian created two types of control groups, one with all foreigners and another with foreigners matched to citizens based on similar characteristics.

Agarwal and Qian (2014) analyzed their data using a distributed lag model of individual expenditures on the growth dividend interacted with a dummy variable for the two months between announcement ("announcement effect") and a dummy variable for the ten months after the disbursal ("disbursement effect"). They also included individual and month fixed effects. The baseline results shown in their Table 2 indicate that individuals used their credit and debit cards to spend an average of 7.4 cents (std. error 0.027) of every dollar each month between announcement and disbursal and an average of 8 cents (std. error 0.023) of every dollar each month for the eight months after the payout. These dummy variables are precisely estimated and imply a sizeable cumulative ten-month consumption response of 80 cents of every dollar of payout. Moreover, since credit cards and debit cards account for just 30 percent of overall spending in Singapore, this estimate is a lower bound on possible consumer responses.

Several figures in the paper show the cumulative spending by month and by type of card. Agarwal and Qian kindly sent me their unpublished estimates for combined card spending. Figure 5 shows my plot of their estimates. The left panel shows the cumulative MPC and the right panel shows the MPC month-by-month, i.e., the "intertemporal MPCs". While the monthly response estimates are less precise than the two dummy variable estimates featured in the published paper, they offer the same conclusion: the cumulative marginal propensity to consume was almost 80 percent 10 months after the announcement. The right panel shows that the intertemporal MPC varies quite a bit from month to month. It hits a peak during the month of the payout, but also a second peak five months later.

{{Figure 5 here}}

B. Aggregate Effects of Singapore's Growth Dividend Payments

What do these micro MPC estimates imply about aggregate multipliers? To answer this question, we can use the small open economy HANK model of Aggarwal, Auclert, Rognlie, and Straub (2023) (henceforth abbreviated "AARS"). They show that for a fully open economy such as Singapore's, if the central bank holds real interest rates constant, the impulse response of aggregate consumption will follow the identical pattern of the household intertemporal MPCs. The response of imports will also follow the identical pattern of the household intertemporal MPCs. The reason is that all consumption spending goes to imports so there is no further multiplier effect. For the same reason, there is no impact on GDP.

Figure 6 shows the behavior of real aggregate disposable income and consumption in the period surrounding the 2011 payout. The data are from the national accounts at Statistics Singapore and are at seasonally adjusted quarterly rates. Nominal series have been divided by the implicit price deflator for personal consumption. The effect of the Growth Dividend payouts is clearly evident in the disposable income series. While one might be tempted to imagine a response of consumption, the data are too noisy to make any definitive statement. The graph also shows the counterfactual if there had been no Growth Dividend. The counterfactual is constructed by subtracting induced consumption from the aggregate data. Induced consumption is calculated as the product of the aggregate dividend payments (S$1.5 billion) and Agarwal and Qian (2014) intertemporal MPC estimates, aggregated to a quarterly basis starting in 2011Q1. The counterfactual path lies below the actual expenditure line from 2011q1 through 2011q4. It would be difficult, however, to argue whether it was plausible or implausible because of the variation relative to the noise.

{{Figure 6 here}}

Fortunately, there are repeated examples of this natural experiment, so we can potentially use time series analysis to test whether aggregate consumption responds. Agarwal and Qian (2014) did not discuss why the government made the payout in 2011 since macro endogeneity was not a

concern for them because of their clever use of non-citizens as the control group. However, any macro analysis needs to address this endogeneity concern. My research into the background of this period revealed both the motivation for the 2011 payout and the existence of additional episodes. In particular, following up on a revealing discussion in a February 19, 2011 article in *The Strait Times* (Chew, p. A4), I discovered that 2011 was an election year in Singapore and that the Singapore government had twice previously distributed large payouts just before elections.

Singapore parliamentary elections must be held at least every 5 years. The first instance I found of an election year transfer was 2001. During an August 2001 political rally, the prime minister announced that the government would be distributing "New Singapore Shares" worth S$2.7 billion. The election was scheduled for November 3, and the payouts started Nov. 1 and continued through January. Households received between S$200 and S$1,700. Shares were like savings bonds, but households were allowed to cash up to half their shares in the first 12 months. Five years later, as part of the budget speech in February 2006, the prime minister announced a new "Progress Package" worth S$2.6 billion. Payouts were made in late April, before the election on May 6. The pattern was repeated five years later in 2001. The 2011 "Growth Dividend" program, worth S$1.5 billion, was part of the larger S$3.2 billion "Grow and Share" program. The program was announced in February and the growth dividends were disbursed by the end of April. The election was held on May 7.

From this narrative, I create two quarterly series as alternative external instruments, an announcement series and a payout series. The appendix gives details of the creation of the series. I estimate the effect of a shock to either the announcement or the actual payout on key macro variables using a four-variable structural vector autoregression (SVAR). I order the external instrument ordered first, followed by disposable income, consumer expenditures, and personal saving.[3] For all four variables, I use real values and normalize them by the trend in real disposable income. The trend is estimated from regressing log real disposable income on a quadratic in time and then converting it to a level. The reasons for dividing all variables by the trend in disposable income rather than using logarithms are two-fold: (i) one cannot take logs of the external instruments since they have zero values; and (ii) the normalization allows us to estimate MPCs instead of elasticities.

[3] This methodology that orders the narrative instrument first in the SVAR is the same used in my previous work, e.g. Ramey (2011). See Plagborg-Møller and Wolf (2021) for a discussion of this method.

The SVAR is estimated using quarterly data from 1999q1 through 2019q4; the start and end of the sample are chosen to omit the Asian Crisis and COVID. Four lags of all variables are included. I normalize the impulse response functions (IRFs) so that the peak response of the external instrument is unity.

Figure 7 shows the estimated IRFs when the announcement series is used as the external instrument. A S$1 announcement has no immediate effect on disposable income but temporarily raises it by about 50 cents a quarter later. This lag is expected since payouts always start a quarter after the announcement. The quarter 1 effect on disposable income is less than S$1 on average, likely because some of the payouts extend over more than one quarter. A statistically and quantitatively significant response of disposable income to the external instrument is key to the subsequent analysis: otherwise, a non-response of consumption would not be informative.

In contrast to disposable income, consumption does not rise in response to the news. It falls below zero for several quarters before returning to zero. On the other hand, personal saving rises significantly in quarter 1, at the same time that disposable income rises. Interestingly, the rise in personal savings is estimated to be approximately S$1, the same as the magnitude of the payout announcement. Thus, the results show that disposable income and saving respond robustly and significantly to the payout announcement with a lag, but consumption does not respond.

{{Figure 7 here}}

Since many models assume that hand-to-mouth households do not respond until the actual payout arrives, I also estimate IRFs from an SVAR in which I substitute the payout series for the announcement series. These estimates are shown in Figure 8. The jump of two of the three macro variables is synchronized with the payouts --- in response to a program payout of S$1, disposable income jumps by 50 cents and saving jumps by S$1. Again, consumption does not respond at all. Thus, the responses of consumption and saving are inconsistent with the predictions of a small open economy HANK model calibrated to Agarwal and Qian's (2014) micro MPCs.

{{Figure 8 here}}

I now summarize the results of some robustness checks, which are not shown in graphs. Recall that the HANK model also predicts the response of imports should follow a pattern identical to the household MPCs. To examine the response of imports, I estimated a three-variable SVAR with the external instrument, real disposable income and real imports (all divided by trend real disposable income). The response of imports was very noisy and never statistically different from zero. Thus, neither consumption nor imports appears to respond. As a robustness check on possible measurement error in the national account data, I also used data on monthly retail sales indexes to see if retail sales responded. I estimated a 3-variable SVAR with the external instrument ordered first, along with the logarithms of retails sales excluding motor vehicles and retail sales of motor vehicles. Neither series showed any response to the either the news or the payout. Finally, using a three-variable monthly SVAR, I study the effect of either the news or the payout series on the nominal interest rate, measured as the 1-year Treasury bill rate, and the real rate, which subtracts the centered three-month moving average inflation rate based on the CPI from the 1-year Treasury bill rate. Neither series shows responses to either shock. Thus, the election year payouts satisfy the assumption of AARS's (2023) small open economy HANK model, which assumes that real interest rates are held constant.

In sum, despite Agarwal and Qian's (2014) cumulative MPC estimates of 0.8 over ten months after a temporary payout, there is no evidence of an effect on aggregate consumption or imports. In contrast, saving rises dollar-for-dollar with the payout. Thus, I find no evidence that the Singapore election year payouts stimulate the macroeconomy. These results are consistent with the 2001 and 2008 tax rebates in the U.S., as summarized in the earlier section.

However, we are left with a puzzle: why are the high household MPCs estimated by Agarwal and Qian (2014) not showing up in aggregate consumption? Since I do not have current access to the micro data, I leave the reconciliation of the conflicting micro and macro results to future research.

7. **A Case Study of Australian Transfers During the Global Financial Crisis**

I chose Australia for my second small open economy case study for two reasons. First, Australia's fiscal stimulus during the Global Financial Crisis was the third highest as a percent of

GDP of any country; only the U.S. and South Korea spent more. Second, the MPC estimates from two notable household-level studies of the stimulus ranged from zero to 0.4.

A. The Australian Economy During the Global Financial Crisis

The October 2009 Australian Senate Report reviewing the government's economic stimulus initiatives offers a comprehensive summary of the economic situation and the range of views on the degree to which the stimulus helped prevent a deep recession. Australia entered the crisis period with budget surpluses. Its financial system remained sound throughout the crisis: the major Australian banks were some of the few in the world still rated double-A or better by the end of the crisis. The Australian economy slowed in 2008, but began to recover early in 2009. Mineral and other exports to East Asia rebounded quickly since the Global Financial Crisis did not slow the East Asian economies much. The policy rate of the Reserve Bank of Australia bottomed out at 3 percent, so there was still significant room for additional monetary policy stimulus. Nevertheless, Australia enacted a huge fiscal stimulus, equal to 4 percent of GDP. According to the Senate report, Australia felt it necessary to do so because of the dire IMF predictions about the trajectory of the global economy.

Fearful of the ramifications of the deteriorating global economy, the Australian government announced its first stimulus package in mid-October 2008. The package was AU$10.4 billion of transfers that were disbursed in the first few weeks of December 2008. The transfers were to pensioners, low- to middle-income households, and first-time homebuyers. A few months later in February 2009, they announced an additional stimulus package worth AU$42 billion. AU$12.7 billion of that was transfers, which were disbursed in April and the first week of May 2009. AU$26 billion was for infrastructure and AU$2.6 billion was for business investment tax breaks. Again in May, they enacted AU$22 billion more for infrastructure investment. The total fiscal stimulus was AU$90 billion over five years.

Figure 9 shows the paths of real government consumption purchases, real government investment purchases, and the stimulus transfers. The scale for government consumption is shown on the left axis and the scale for transfers and government investment is shown on the right axis. The lengths of both axes are the same, so vertical changes are comparable across all series. Government consumption mostly follows a gradual upward trend. Government investment is flat

between early 2008 and early 2009, but then climbs strongly. The stimulus transfer payments are very large, equal to or exceeding all government investment during the two quarters of transfer payments.

{{Figure 9 here}}

Figure 10 shows several monthly macroeconomic series for Australia in the several years surrounding the Global Financial Crisis. The unemployment rate fluctuated between 4 percent and 4.5 percent throughout 2007 and most of 2008. In the last couple months of 2008, it began to rise and hit a peak at 5.9 percent in May 2009, after which it gradually declined. In contrast, the U.S. unemployment rate rose from 4.5 percent in mid-2007 to a peak of 10 percent in October 2009. When the American Recovery and Reinvestment Act (ARRA) was enacted, the U.S. unemployment rate was above 8 percent.

The Australian inflation rate fell from around 5 percent in the first part of 2008, to minus 1 percent in the fourth quarter of 2008. Subsequently, it rose to between 2 and 4 percent. The Reserve Bank of Australia (RBA) lowered the cash rate from above 7 percent in the third quarter of 2008 to 3 percent by spring 2009. Australia's real effective exchange rate, based on a broad index, hit a peak in mid-2008, fell steeply in late 2008, but then began to recover in early 2009.

The real effective exchange rate collapses in the second half of 2008 but then begins to recover in early 2009. The terms of trade pattern (not shown) looks similar. Many Australian exports are commodities, whose prices boomed in early 2008 but then collapsed during the Global Financial Crisis.

{{Figure 10 here}}

Figure 11 shows quarterly real GDP, consumption expenditures, private investment, and imports and exports from 2007 through 2010. Real GDP dips in 2008q4, but recovers completely by the following quarter. Consumption has a slight downward trend in 2008 but then recovers to its upward path in early 2009. Investment surges from early 2007 through mid-2008, but then falls back down again through mid-2009. Exports rise fairly steadily through the entire period. However, the smooth movement of real exports masks the significant reduction of nominal exports

in the second half of 2008, since commodity prices declined. Imports rise from early 2007 through early 2008, fall back to their 2007q1 level by 2009q1, but then recover strongly thereafter.

{{Figure 11 here}}

B. Micro MPC Estimates and Aggregate Effects of the Australian Transfers

How much did the fiscal payments stimulate household consumption? Two prominent household-level analyses find conflicting results. Andrew Leigh (2012) used data from a survey conducted by the Social Research Centre of Melbourne that asked how households had used their stimulus transfer payments. The questions were similar to those added by Shapiro and Slemrod (2003a,b; 2009) to the Michigan Consumer Survey. Leigh used their methods to convert the Australian survey responses, which were qualitative, to estimates of the marginal propensities to consume. He estimated that the aggregate MPC from the transfers was around 0.4. Recently, however, Aisbett, Brueckner, Steinhauser, and Wilcox (2024) used Australian Nielsen household data, augmented with information on the receipt of transfers, to estimate the responses for the nondurable goods included in the Nielsen survey. They estimated consumptions responses that were quantitatively and statistically indistinguishable from zero, during the week of the transfers, cumulatively for the four weeks after the transfer, and in the long run. Thus, they found an aggregate MPC that was indistinguishable from zero.

To sort through the conflicting household-level estimates, it is useful first to see whether there were any movements in aggregate consumption or saving. Figure 12 shows real disposable income vs. consumption and saving starting in 2008. Although the scale of the left and right y-axes are different, their lengths are the same so rises and falls are comparable across series. The arrival of transfer payments is clearly evident in the disposable income series --- the two vertical dashed lines indicate the quarter of payment. In contrast, the consumer expenditure series falls gradually throughout the end of 2008, before beginning to rise in 2009. The faster rate of increase between 2009q1 and 2009q2 coincides with the second round of transfer payments but is much less than the rise in disposable income.

In contrast, the surge in saving in 2008q4 is slightly greater than the rise in disposable income, suggesting no spending of transfers in 2008q4. Both disposable income and saving

decline in 2009q1 by similar amounts. When the second round of transfers arrives in 2009q2, saving rises only half as much as disposable income, which could indicate a non-zero consumption response.

{{Figure 12 here}}

To quantify these movements more rigorously, I estimated the same SVAR I used for Singapore. In this case, however, I have only two payouts during the sample so there is a question as to whether the aggregate effects will show up in an SVAR. The real values of the payout instrument (in 2008q3 AUD) are AU$10.1 billion in 2008q4 and AU$12.5 billion in 2009q2; all other values are zero.[4] I estimate the effect of a shock to the payout using the same four-variable structural vector autoregression (SVAR) with four lags over the sample 1999q1 through 2019q4 that I used in the Singapore analysis. I order the external instrument ordered first, followed by disposable income, consumer expenditures, and personal saving. For all four variables, I use real values and normalize them by the trend in real disposable income so that the coefficients on consumption are MPCs and not elasticities.[5] I normalize the impulse response functions (IRFs) so that the peak response of the external instrument is unity.

Figure 13 shows the estimated IRFs to a shock to payouts. The payout series spikes up to 1 on impact at quarter 0, and again to 0.56 at quarter 2, as we would expect. Interestingly, the peak is lower in quarter 2, despite the transfer payment being 20 percent greater that quarter. Disposable income spikes up statistically significantly by 73 cents in quarter 0 and by 48 cents in quarter 2. Thus, even with only two non-zero values, the external instrument has an effect on disposable income in the SVAR. The consumption response is a fairly precisely estimated zero at all horizons; the highest point estimate is 0.09 at quarter 4. Saving rises by 71 cents in quarter 0 and 36 cents in quarter 2. Thus, despite statistically and quantitatively significant responses of disposable income and saving to the payout in quarter 0, consumption shows no response.

[4] I do not use a separate announcement instrument because of the short lag between announcement and payout.
[5] I checked the robustness of the results by estimating an SVAR that used logarithms of disposable income, consumption, and saving instead of detrended values. The elasticity estimates were less precise, but they also showed responses of disposable income and saving, but not consumption.

{{Figure 13 here}}

C. Historical Counterfactuals

To further assess the macro implications of the high micro MPCs, I construct a HANK counterfactual as well as counterfactuals based on the Australian Treasury model estimates and Li and Spencer's (2016) model estimates.

I construct my counterfactual by calibrating Aggarwal, Auclert, Rognlie, and Straub's (AARS) (2023) small open economy HANK model, calibrated with Leigh's (2012) high MPC estimates. Because we cannot ignore the increases in government purchases occurring at the same time, I include both the transfer payment and government purchases stimulus in the change in the primary deficit. Note that the multiplier on GDP depends on the composition of spending, since the multiplier on government purchases will be equal to one plus the transfers multiplier.[6]

AARS's (2023) Proposition 3 proves results for the case of an economy with no investment, no long-run increase in government purchases, constant real interest rates, and real interest rates equal 0. I use their generalization of Proposition 3 that allows for non-zero real interest rates, shown in the appendix on page 380. The relevant equations for the effect of a change in the primary deficit (*dPD*) on GDP (*dY*) and consumption (*dC*) are:

$$(1) \qquad dY = dG + (1-\alpha)M[I-(1-\alpha)M]^{-1}\, dPD$$

$$(2) \qquad dC = M[I-(1-\alpha)M]^{-1} dPD$$

G is government purchases, *M* is the matrix of intertemporal MPCs (iPMCs), and α is the openness parameter, where $\alpha = 1$ indicates fully open.

These equations are valid only under certain conditions. I will comment on each of them. First, they are based on the assumption that there is no anticipation of the stimulus. This is reasonable for the transfers part of the stimulus, since in both cases only two months separated the announcement from the disbursement. On the other hand, the first part of the infrastructure

[6] See equation B20 on p. 380 of AARS (2023).

stimulus was announced in February and the second part in May, but the rise in government investment did not occur until 2009q3. This lag between the legislation and the actual outlays for infrastructure spending is well-known (e.g. Leeper, Walker, Yang (2010), Ramey (2021)). My assumption of no anticipation means that the infrastructure part of the stimulus is assumed to have no effect before 2009q3. Second, the equations are from a model that omits investment. Because the transfers were temporary, they would have a negligible effect on investment anyway, so adding investment would not change the results. Third, the equations assume that the central bank holds real interest rates constant. In fact, the ex post real interest rate varied significantly from 2008 through 2009, but was falling rapidly through the period of the disbursements of the transfers. As shown earlier in Figure 10, inflation began to rise in the second half 2007. In response, the RBA raised the cash rate from 6.25 to 7.25 percent. Inflation dropped rapidly after July 2008 and the RBA quickly lowered the cash rate from 7.25 percent in August 2008 to 3 percent in May 2009. Nevertheless, the real interest soared above 7 percent in early fall of 2008 before falling sharply thereafter, reaching negative values by spring 2009. Thus, when the transfers were being distributed, the real interest rate was falling quickly so monetary policy appeared to be very accommodative during the first half of 2009. It should be noted, however, that the RBA stopped lowering the interest rate when it hit 3 percent. It is very possible that they would have pushed the interest rate lower had there not been such a large fiscal stimulus.

I calibrate the model as follows. First, I set the openness parameter to 0.2 following Li and Spencer (2015). I then calibrate the *M* matrix to Leigh's (2012) MPC estimates. Leigh found higher MPCs for the first transfer (0.47 – 0.49) than the second (0.35-0.38) and speculated that since the survey was conducted less than two months after the disbursement of the second transfer, the full spending had not occurred. Thus, I set the iMPCs to 0.38 for the impact quarter, 0.11 for the following quarter.

I also align the values with the timing of the stimulus. The first transfer was disbursed in December 2008, which was the last month of the quarter. In order to allow for the MPC of 0.38 to be spread over three months, I assign one-third of the transfer to 2008q4 and two-thirds to 2009q1. For government investment stimulus, I estimated the path of the stimulus by calculating the difference between real government investment and its 2008q4 value and assigning the value to the stimulus if it is positive. Real government investment had a slight downward trend from

2008q1 to 2009q2, so my estimated stimulus value becomes positive starting only in 2009q3. See the appendix for the estimated values.

Figure 14 shows the actual data and the counterfactuals. The black lines are the actual path of consumption and GDP and the dotted lines are the counterfactual paths. The counterfactuals using Leigh's MPCs imply that there would have been a very sharp decline in consumption in early 2009 if there had been no fiscal stimulus. The counterfactual path of GDP would have also been noticeably different.

{{Figure 14 here}}

To put these counterfactuals in Australian historical perspective, I compare the implied decline in consumer spending from the HANK model to other episodes. Table 1 shows that the counterfactual consumer spending would have fallen 4.2 percent from its actual peak in 2008q1 to its counterfactual trough in 2009q2. Actual consumer spending rose 1 percent over this same period. The two other significant declines in consumption since 1959 were a 13.9 percent drop during COVID and a 2.5 percent drop during the severe 1975 recession. Thus, the counterfactual implies that the drop in consumption would have been the biggest other than during COVID. The U.S. drop in consumption during the Global Financial Crisis was 2.4 percent from 2008q2 to 2009q2. It is hard to imagine that Australia would have had a consumption drop that was 75 percent bigger than the U.S. despite Australia's sound banking system, recovering exports, and lack of zero lower bound constraints. On balance, the consumption counterfactual implied by Leigh's MPC estimates seems unlikely.

{{Table 1 here}}

The counterfactual implied by Aisbett et al.'s (2024) household MPC estimates is identical to the actual consumption path. They estimate MPCs of zero and a zero MPC implies that transfers have no impact. Thus, assessing plausibility of their estimates is equivalent to assessing whether the 1 percent rise in actual consumer spending was plausible even without any stimulus from transfers.

For comparison purposes, I also construct counterfactuals for GDP from two models in the literature. These are the Australian Treasury's model projections in 2009 and the estimates from Li and Spencer's (2016) estimated model.

The Australian Treasury calibrated its models with various multiplier estimates from the OECD and the IMF to produce its estimates of the effects of the fiscal stimulus (Australian Senate Report 2009). Gruen (2009) shows graphs of the projected path of real GDP with and without the fiscal stimulus (Chart 8). I extracted approximate values from the chart and divided the no stimulus path by the with stimulus path and multiplied it by the actual path of real GDP to derive the counterfactual.

I also construct an implied counterfactual from Li and Spencer's (2016) analysis. Li and Spencer analyzed the quantitative effects of both the fiscal and monetary stimulus in an estimated two-agent New Keynesian model assuming that 25 percent of the households were non-Ricardian. They estimated that the fiscal and monetary responses were equally important in stimulating the economy, and estimated fiscal multipliers of 0.9 on impact and 1.26 after one year. They modeled the transfers as the government purchasing private sector goods and transferring them to the non-Ricardian households.[7] However, in a New Keynesian model, government purchases multipliers are typically significantly larger than transfers multipliers, so this aspect of their model may have over-estimated the contribution of the fiscal stimulus. On the other hand, they ignored the infrastructure part of the fiscal stimulus, so they were undercounting the fiscal stimulus starting in the second half of 2009. I use estimates I extracted from Experiment 2 shown in Graph B9.

Figure 15 shows the counterfactuals implied by both the Treasury model and Li and Spencer's model, along with my HANK counterfactual for comparison. All three counterfactuals agree that the decline in real GDP in 2008q4 would have been greater without the fiscal stimulus. For the next four quarters, the Li-Spencer counterfactual and the HANK counterfactual are similar. In contrast, the Treasury counterfactual is substantially more pessimistic. By the second half of 2009, the Li and Spencer counterfactual is attributing less effect to the stimulus than the HANK model. The most likely source of the difference is that the HANK model takes into account the stimulus from government purchases and the Li and Spencer model omits that part of the stimulus.

[7] See equation (7) and surrounding text on page 98 of Li and Spencer (2016) and the discussion of G on the subsequent pages.

On balance, I think all three counterfactuals are too pessimistic for two reasons. First, my SVAR results suggest no effect of the transfers on consumption in the aggregate data. Second, my narrative analysis of the Australian economy and comparison of consumption declines in my HANK counterfactual to historical declines and U.S. declines suggest that the counterfactual is implausible.

{{Figure 15 here}}

D. Costs of the Fiscal Stimulus

There are lingering consequences of the large fiscal stimulus enacted by Australia. As discussed earlier, Australia entered the Global Financial Crisis with a budget surplus. Figure 16 shows the ratio of gross government debt to GDP for Australia. The ratio had been falling during the 2000s and reached a mere 10 percent before the Global Financial Crisis. However, the stimulus spending followed by subsequent deficits propelled the debt-GDP ratio to almost 47 percent by 2019. COVID then pushed it even higher, to 57 percent in 2020. Since then, the debt-to-GDP ratio has fallen, so that now it is 43 percent.

{{Figure 16 here}}

If the Australian government had adopted a more modest fiscal stimulus, would the economy have done much worse? The earlier discussion suggests that the Australian economy would not have experienced more than a mild recession because of the good state of their financial system and the quick recovery of their exports. It is plausible that the strong banking sector and quick export recovery were the key drivers of growth.

A further question is whether the large fiscal package "crowded out" part of the monetary policy reaction. The RBA significantly slowed the rate at which they lowered the interest rate when the second round of transfers and the first infrastructure package were announced in February 2009, and they stopped lowering it after reaching 3 percent in May 2009. It is very possible that they would have pushed the interest rate lower had there not been such a large fiscal stimulus.

8. **Implications and Broader Lessons**

In this paper, I analyzed the macro effects of temporary transfers using four case studies across three countries. In each case, I assessed whether aggregate historical movements of consumption were consistent with the transfers providing a strong stimulus. In all four cases, the notion of a strong stimulus was either inconsistent with the time series analysis or the implied counterfactuals were not entirely plausible. The results of these four case studies suggest little macro stimulus from temporary transfers. In some cases, there was an issue with the econometric techniques or the classification of goods. In other cases, the macro models did not incorporate dampening effects that were evident in the data.

These results contrast with findings in the literature finding large multipliers on tax changes. These results are not contradictory. The temporary transfers in my case studies were lump sum whereas the tax changes studied by Romer and Romer (2010) and others were changes in distortionary taxation. Recent work by Ferrière and Navarro (2024) study several types of fiscal policy in a HANK model and find that changes in distortionary labor taxes have significant effects, even with labor supply elasticities calibrated to be consistent with micro estimates. These stimulus packages resulted in significant rises in debt-to-GDP ratios in many cases. Any fiscal consolidations that involve rises in distortionary taxes are likely to be costly in terms of GDP.

To what extent do these results generalize? All the cases I consider are for advanced economies, so my results do not directly inform us about the effects of cash transfers in middle- and low-income countriesNevertheless, similar issues may affect the estimates for other economies and other contexts. For example, similar to the Orchard et al. (2024a) finding that the 2008 tax rebate raised motor vehicle prices, Filmer, Friedman, Kandpal, and Onishi (2023) found that cash transfers in the Philippines raised food prices, resulting in the stunting of non-recipient children.

I did not conduct any case studies for the COVID episode for the reasons provided in the introduction. Because the COVID episode was so unusual, the effects may have been different. If the results generalize to the COVID episode, then they would suggest that the inflation pressures came from overly accommodative monetary policy and supply shocks.

A broader lesson emerges from these exercises. It is a mistake for researchers and policymakers to rely too heavily on one type of econometric technique or one class of macro models if those methods are giving answers at odds with other methods or aggregate data. Rather

than ignoring contradictory results, researchers should strive to understand why the results are different. As knowledge progresses, the profession sometimes learns that the econometric methods used in the past lead to biased estimates or that assumptions underlying standard macro models are not good approximations. Researchers owe it to policymakers and the people they serve to continually scrutinize results that are important for economic policy.

Data and Model Appendix

Sovereign debt data.

All data are for gross general government debt as a percent of GDP and are from the IMF's *World Economic Outlook* database, April 2024.

USA case study data.

All data are from the BEA, via FRED. Model counterfactuals and forecasts are from Orchard, Ramey, Wieland (2024a, 2024b).

Singapore data.

Most data are from https://www.singstat.gov.sg/.

The following describes the narrative external instruments I constructed based on election year payouts. I create two quarterly series as alternative external instruments, an announcement series and a payout series. Since only half of the shares distributed in 2001 could be cashed during the first 12 months, I used half of the total payout value of S$2.7 billion, i.e. S$1.35 billion. For 2011, I use only the growth dividend component of S$1.5 billion.

There were several other large payouts in the sample. In 2002 and 2007, payments were distributed to citizens to help them deal with recent increases in the goods and services tax. I excluded it because of these confounding policy changes. There was an additional payout in 2008; the results shown in the text are very similar if I include that payout. The following table shows the non-zero values of the two series.

Announcement Series

Date	Announcement value
2001q3	S$1.35 billion
2006q1	S$2.6 billion
2011q1	S$1.5 billion

Payout Series

Date	Payout value
2001q4	S$0.9
2002q1	S$0.45
2006q2	S$2.6 billion
2011q2	S$1.5 billion

Australian data.

Most data are from https://www.abs.gov.au/. The national income series are from 5206.0 - Australian National Accounts: National Income, Expenditure and Product. All series I used are seasonally adjusted. The chained volume measures of GDP expenditures are from Table 2, the current dollar measures are from Table 3, and the disposable income, consumption, and saving measures are from Table 20 of the Household Income Account, current dollars. (Note that the current dollar consumption series from Table 20 is identical to the one from Table 3, despite their having different variable codes.) The monthly CPI is from Table 1 of 6484.0 Monthly Consumer Price Index indicator. The unemployment rate is from Table 1 of 6202.0 Labour Force, Australia

The daily cash rate is from the Reserve Bank of Australia website.

The broad real effective exchange rate is from the Bank for International Settlements, via FRED, variable RBAUBIS.

The composite index of consumer confidence is from the OECD, via FRED, variable CSCICP02AUM460S.

Model

The time series of Australian real stimulus transfers and government purchases used to create the HANK counterfactuals are as follows:

Quarter	Real transfers	Real government purchases
2008q4	(1/3)*10.4/1.006	0
2009q1	(2/3)*10.4/1.012	0
2009q2	12.7/1.017	0
2009q3	0	1.3
2009q4	0	3.1
2010q1	0	5.1
2010q2	0	4.3
2010q3	0	3.7
2010q4	0	3.6

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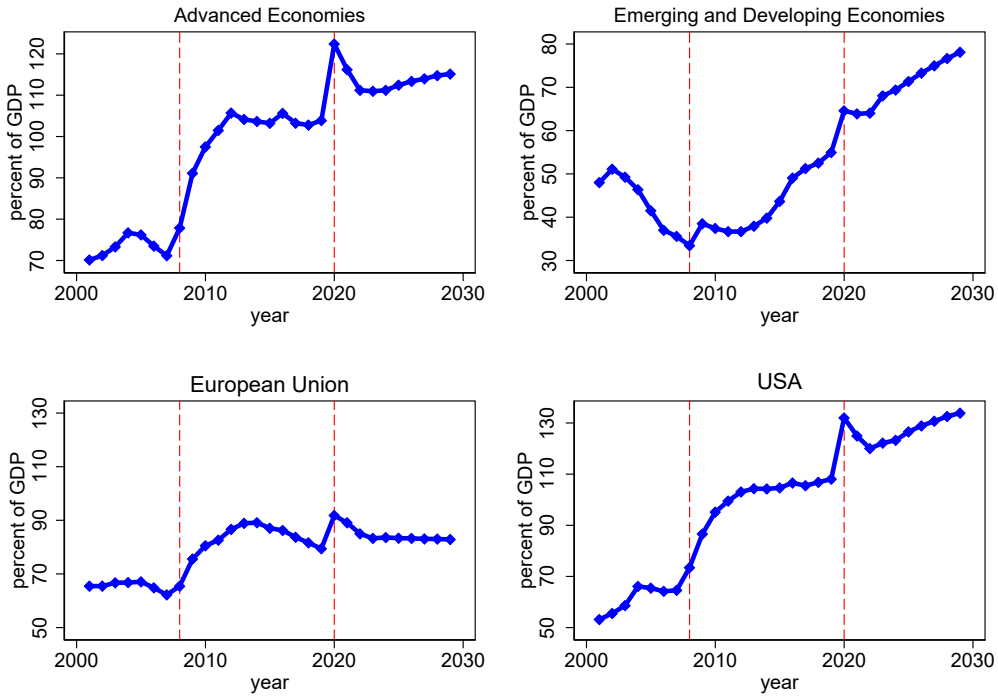
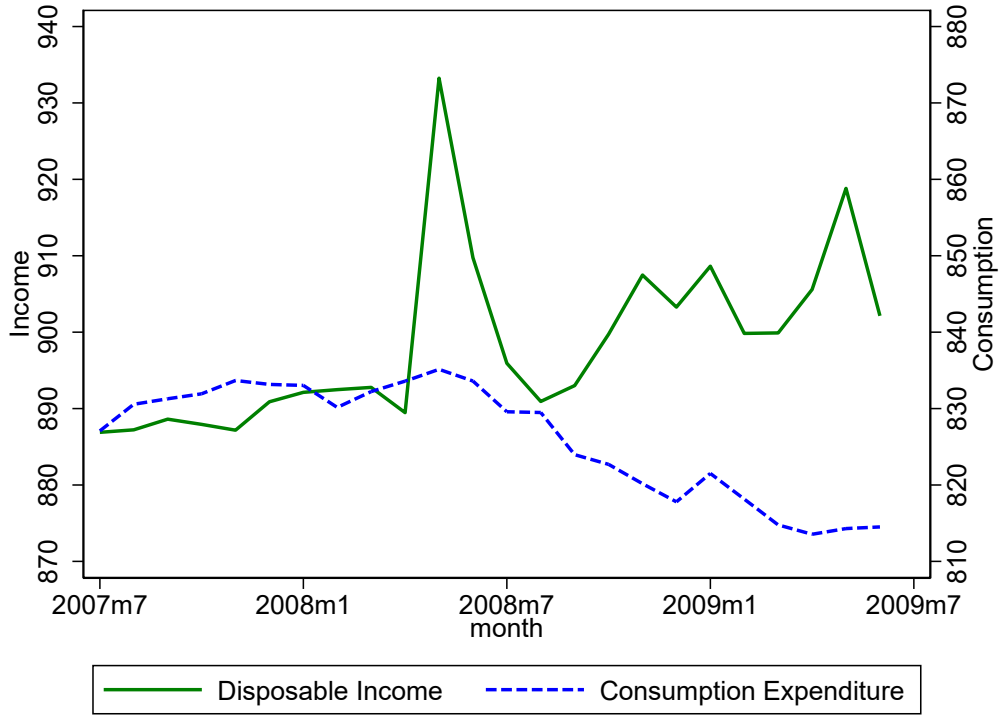
Figure 1. Ratios of Gross Government Debt to GDP



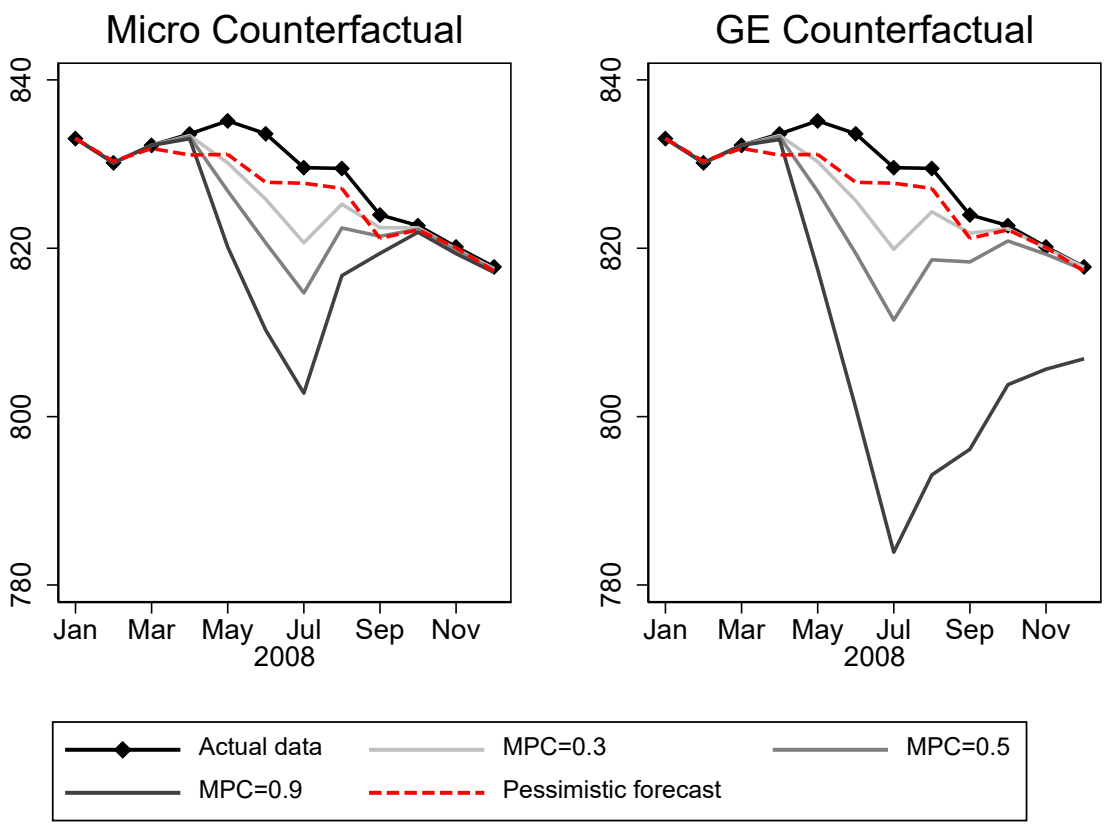
Notes: Data are from the IMF's *World Economic Outlook* database, April 2024. The vertical dotted lines mark 2008 and 2020.

Figure 2. U.S. Real Disposable Income and Consumption Expenditures During GFC



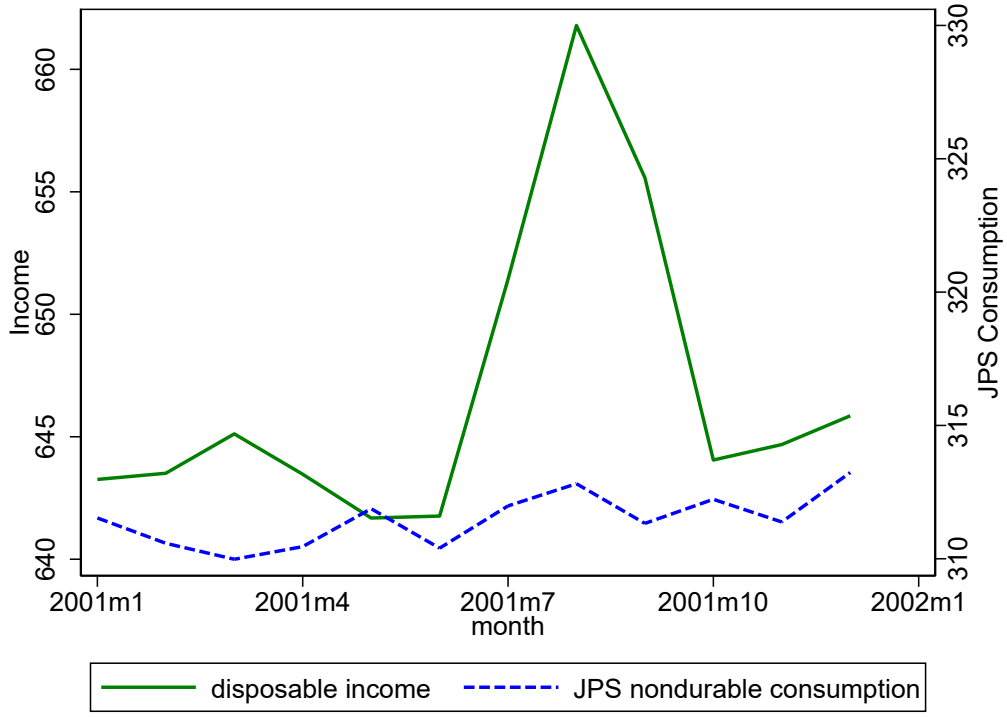
Notes. Data series are from the Bureau of Economic Analysis via FRED. Values are billions of January 2008 dollars, monthly rate.

Figure 3. Micro and General Equilibrium Counterfactuals for Total Consumption in 2008



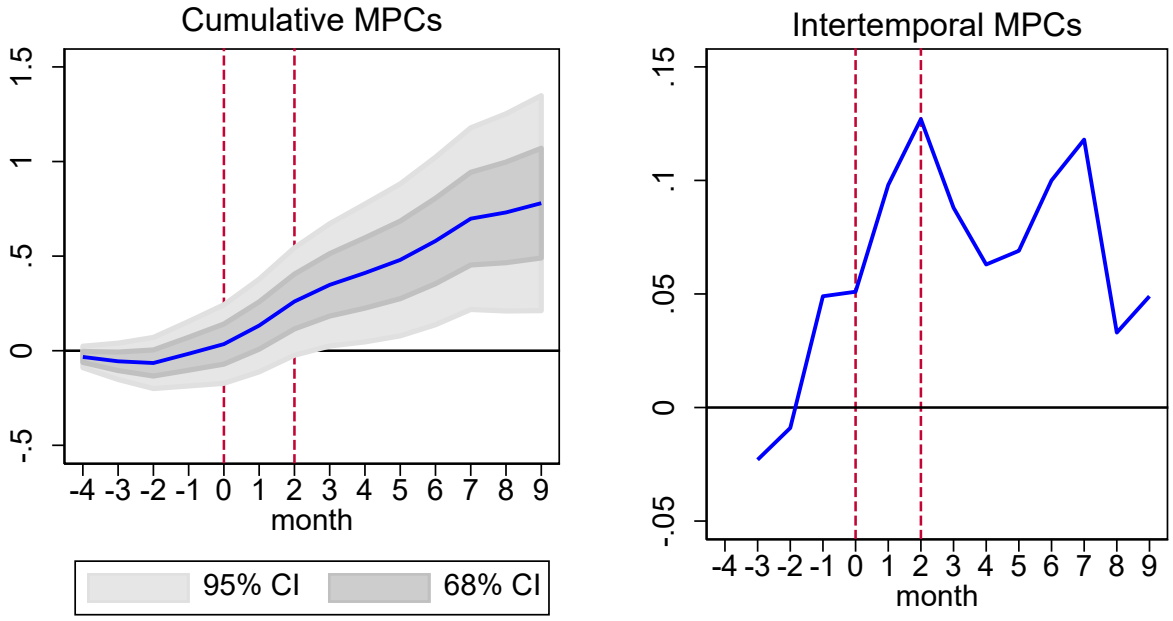
Notes. All variables are in billions of Jan. 2008 dollars, monthly rate. The actual data is BEA data on total personal consumption expenditures. The three lines with MPC labels are the model-implied counterfactuals if there had been no 2008 tax rebate from Orchard et al. (2024a). The pessimistic forecast is from Orchard et al. (20204a).

Figure 4. U.S. Real Disposable Income and JPS Consumption Expenditures in 2001



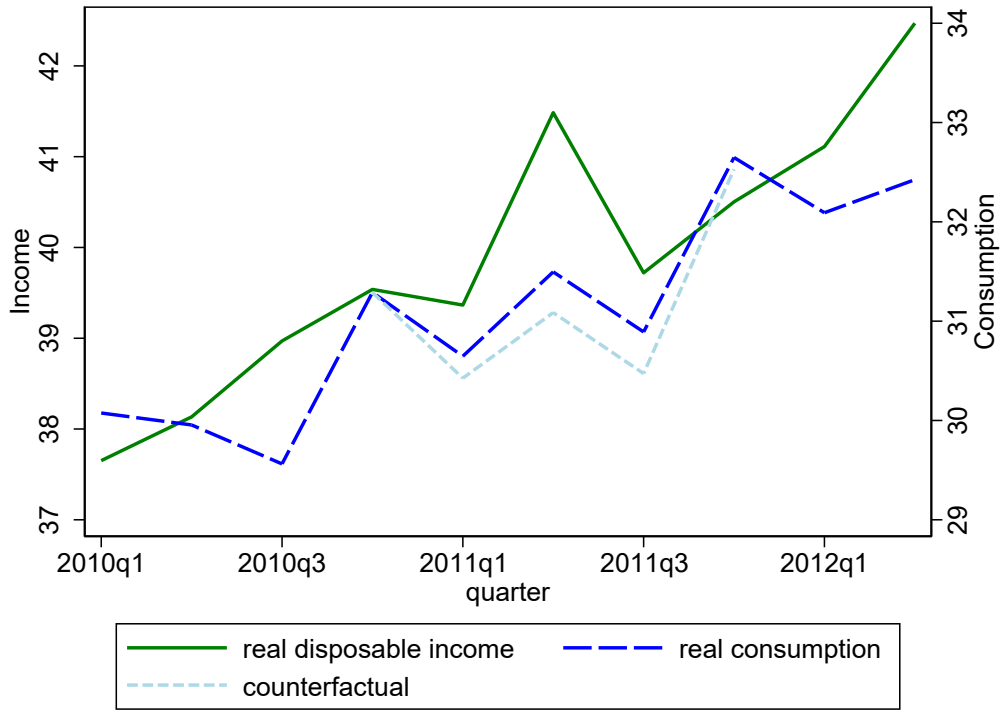
Notes. Data series are from the Bureau of Economic Analysis via FRED. Values are billions of May 2001 dollars, monthly rate. JPS nondurable consumption is the aggregate version of the Johnson, Parker, Souleles (2006) nondurable consumption category.

Figure 5. Agarwal-Qian (2014) Household MPCs for a Singapore Natural Experiment



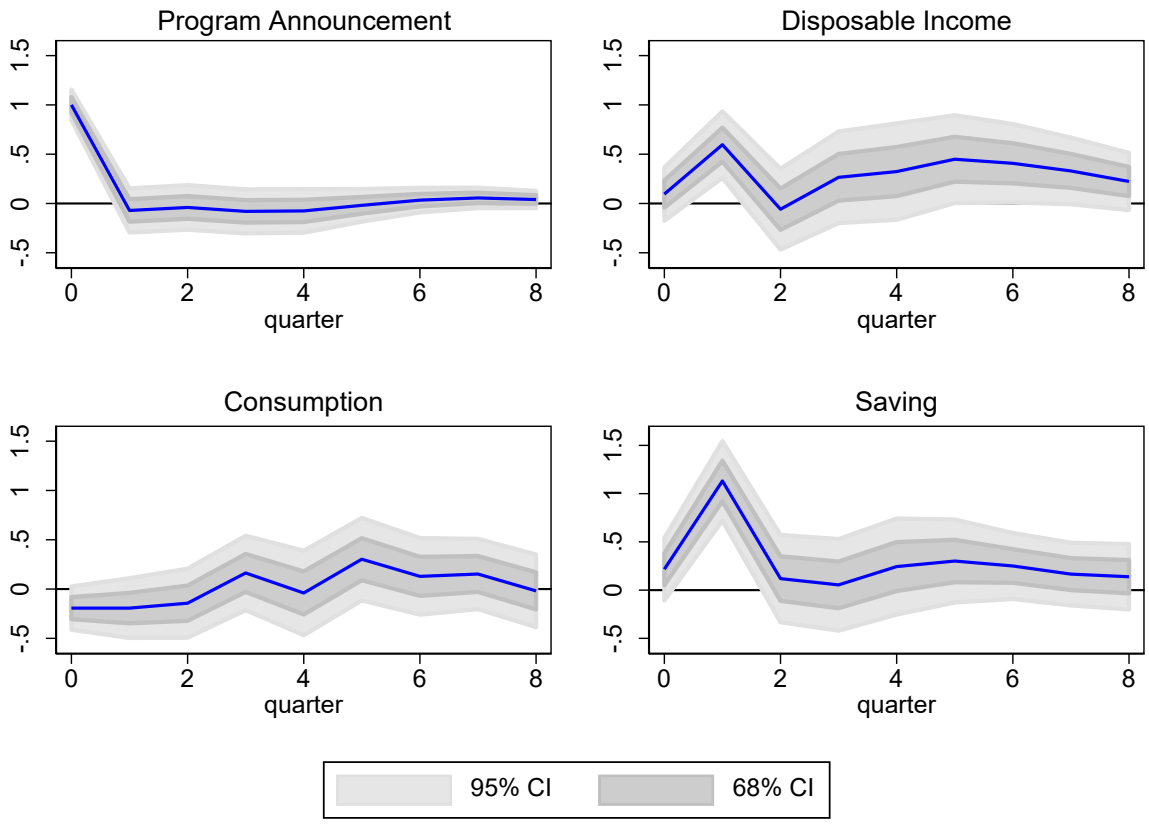
Notes. Month 0 is the date of the announcement (Feb. 2011) and Month 2 is the date of the payout (April 2011). Based on unpublished estimates shared by Agarwal-Qian (2014).

Figure 6. Singapore Aggregate Real Disposable Income and Consumption



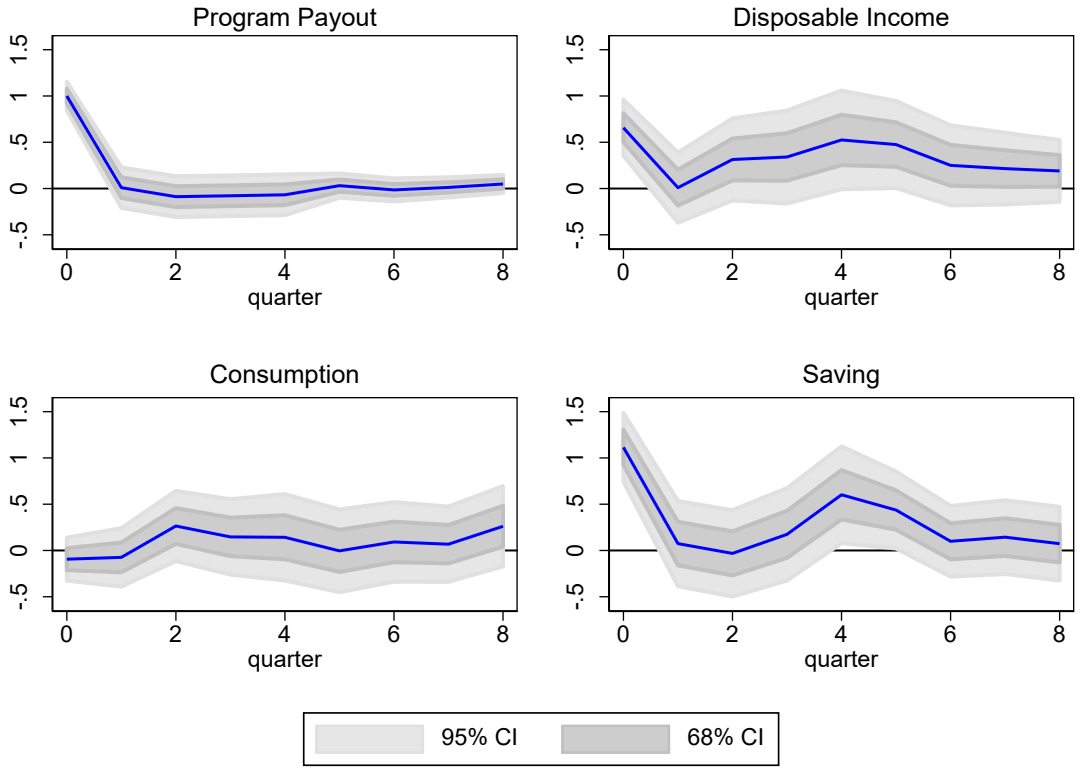
Notes. Aggregate data are from the Singapore National Accounts and are in billions of Singapore dollars. The counterfactual is based on subtracting induced consumption from aggregate data. Induced consumption is calculated as the product of the aggregate dividend payments (S$1.5 billion) and Agarwal and Qian (2014) monthly intertemporal MPC estimates, aggregated to a quarterly basis starting in 2011Q1.

Figure 7. Macro Responses to Announcement of Temporary Payouts in Singapore



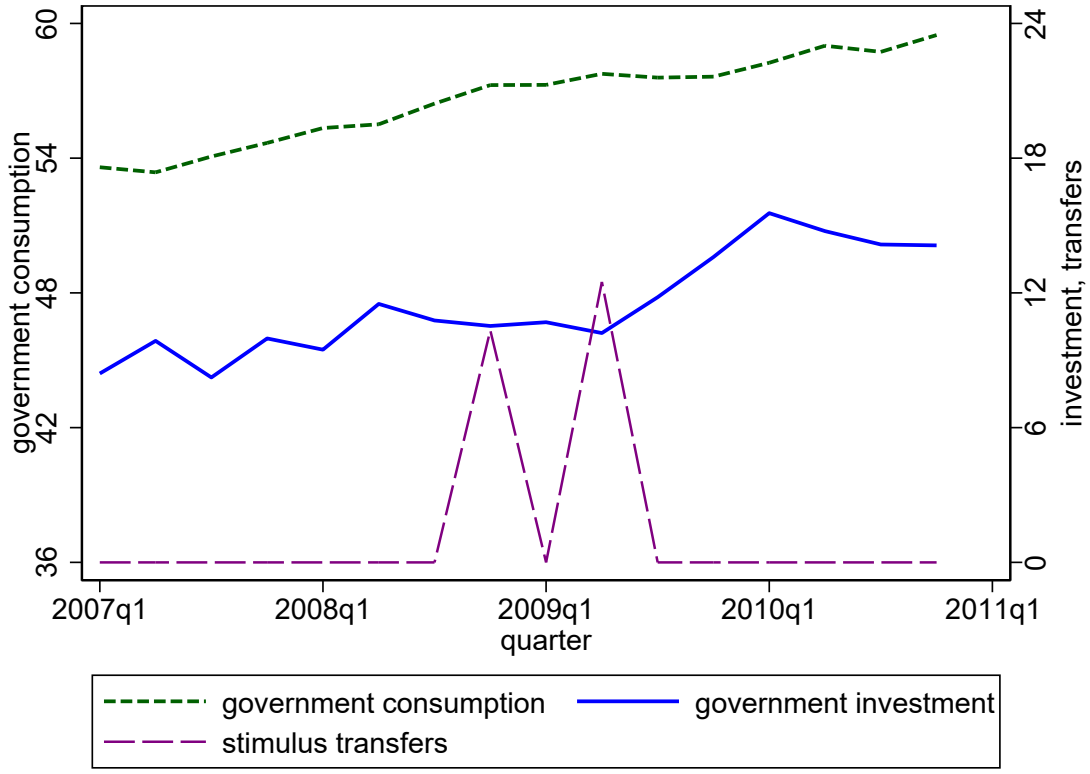
Notes. The program announcement series is based on the author's narrative. The aggregate data are from the Singapore National Accounts. The solid line is the point estimate and the two shaded areas are 68% and 95% confidence intervals. The estimates are from a 4-variable SVAR with 4 lags, estimated from 1999q1 through 2019q4.

Figure 8. Macro Responses to Temporary Payouts in Singapore



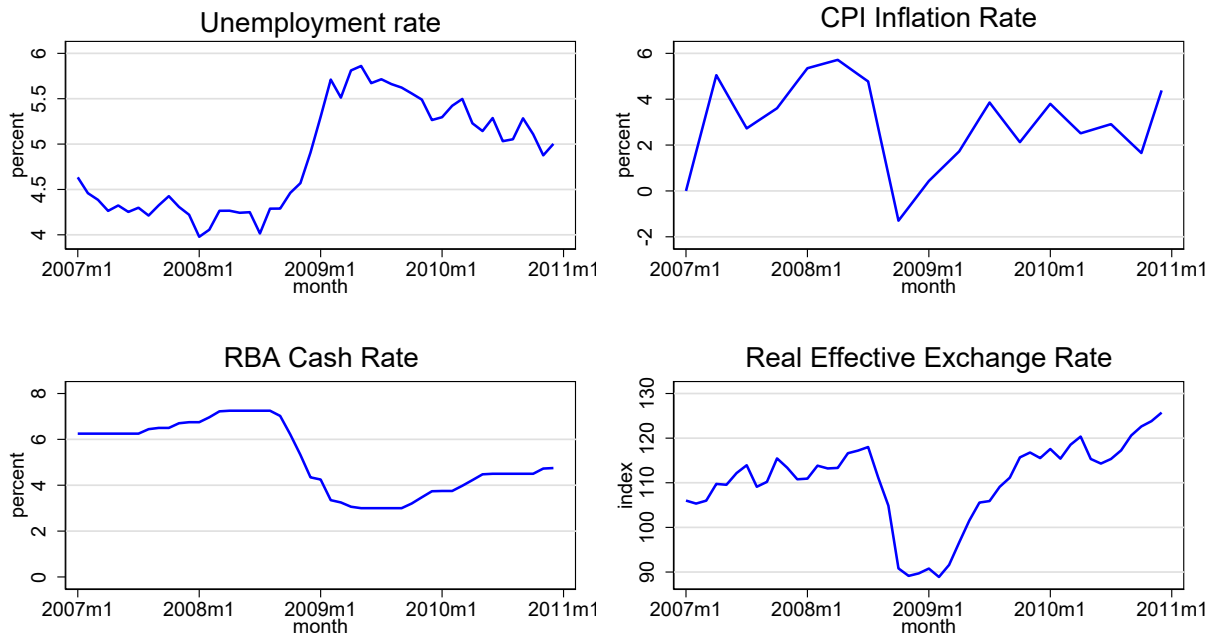
Notes. The program payout series is based on the author's narrative. The aggregate data are from the Singapore National Accounts. The solid line is the point estimate and the two shaded areas are 68% and 95% confidence intervals. The estimates are from a 4-variable SVAR with 4 lags, estimated from 1999q1 through 2019q4.

Figure 9. Australia Key Government Outlays



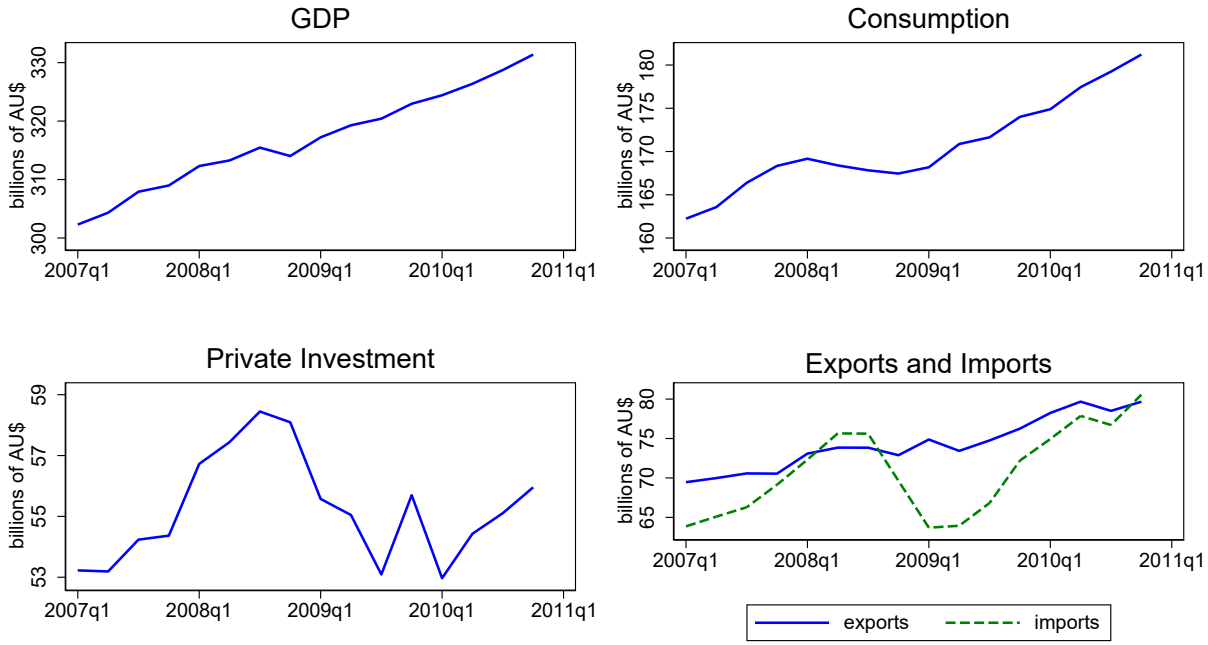
Notes. The government purchases data are from the Australia Bureau of Statistics and are seasonally adjusted quarterly rates of billions of chained AUD. The transfers data are based on descriptions from the Australia Senate Report, and are deflated by the consumption deflator, which was normalized to 1 in 2008q3. Government investment and stimulus transfers are on the same scale, shown on the right axis. Government consumption is on the left axis. The lengths of both axes are the same, so vertical movements of all series are comparable.

Figure 10. Australia Monthly Macro Indicators Surrounding the Global Financial Crisis



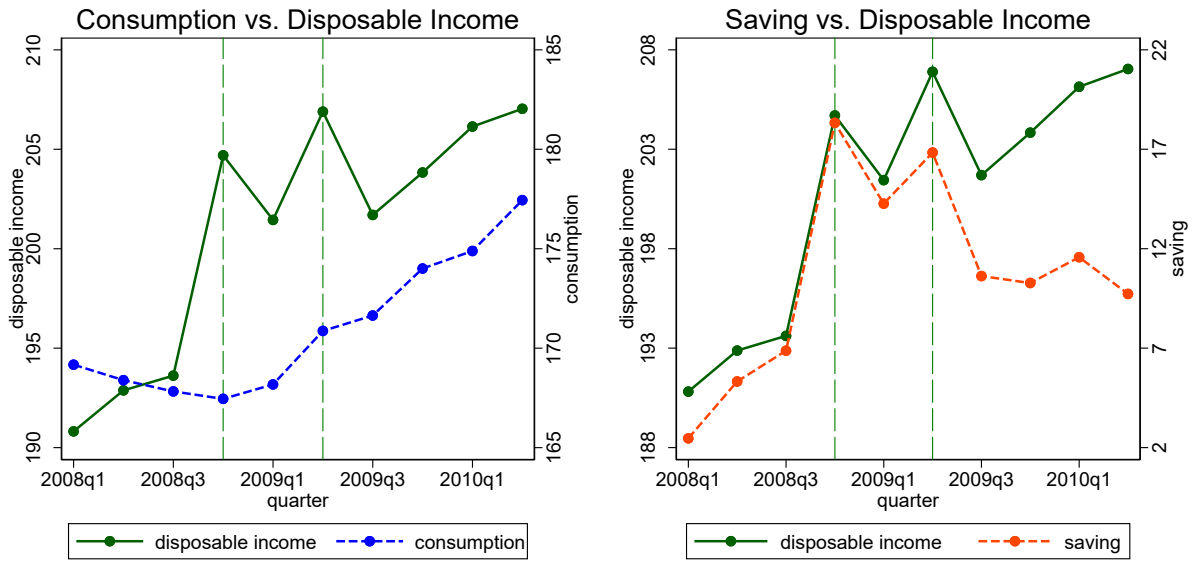
Notes. The unemployment and CPI index data are from the Australia Bureau of Statistics. The RBA Cash Rate is from the Reserve Bank of Australia daily rates, aggregated to monthly. The broad real effective exchange rate is from the Bank for International Settlements, via FRED. All rates are annual rates. The inflation rate is based on a centered 3-month moving average.

Figure 11. Australia Quarterly Macro Indicators Surrounding the Global Financial Crisis



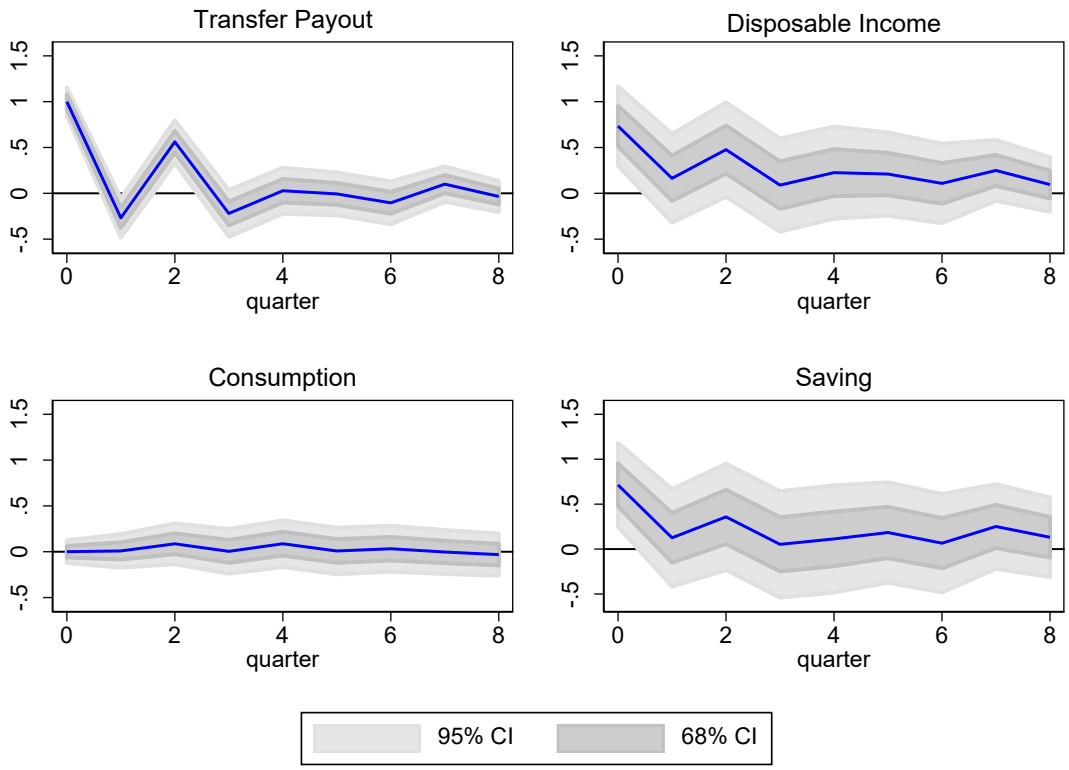
Notes. The data are from the Australia Bureau of Statistics and are seasonally adjusted quarterly rates of billions of AUD. All series are in chained 2008q3 dollar values.

Figure 12. Australia Disposable Income, Consumption, and Saving



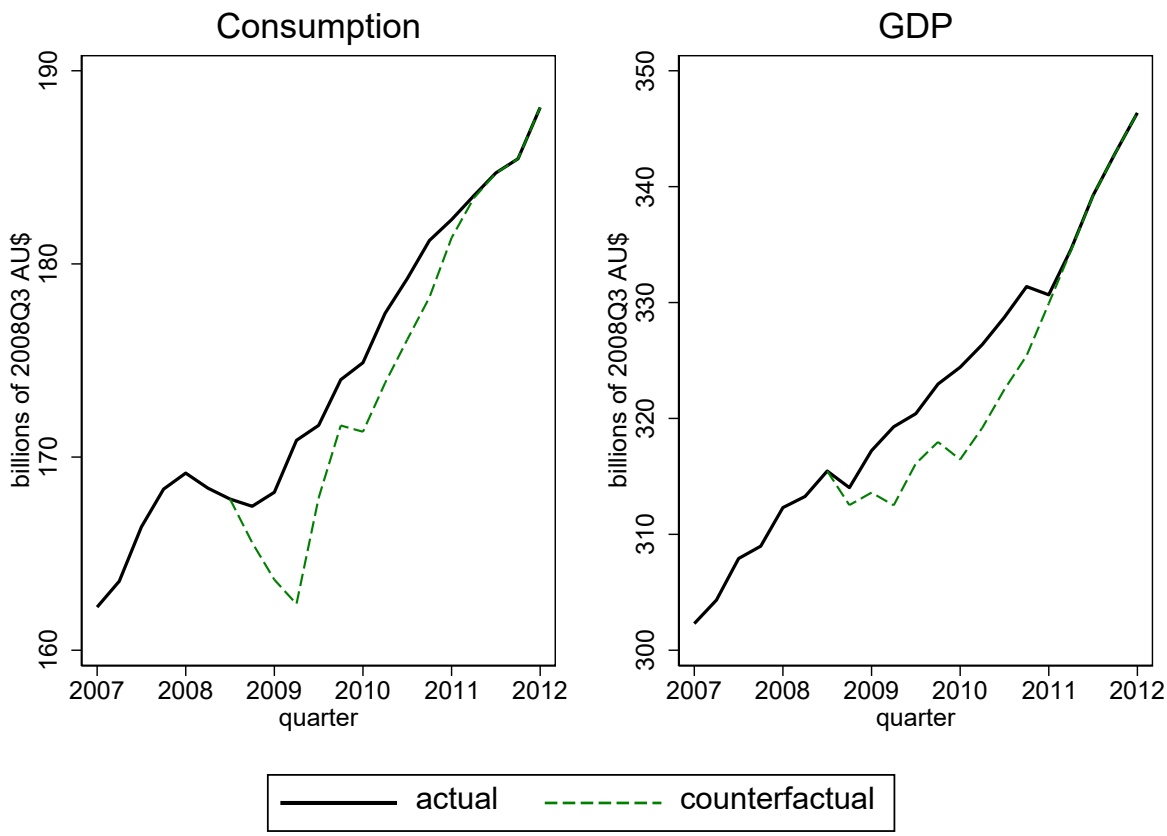
Notes. The data are from the Australia Bureau of Statistics and are seasonally adjusted quarterly rates of billions of 2008q3 AUD. The two vertical dashed lines indicate the quarters of the transfer payouts. The length of the axes on each graph are equal so vertical changes are equivalent across series.

Figure 13. Macro Responses to Temporary Payouts in Australia



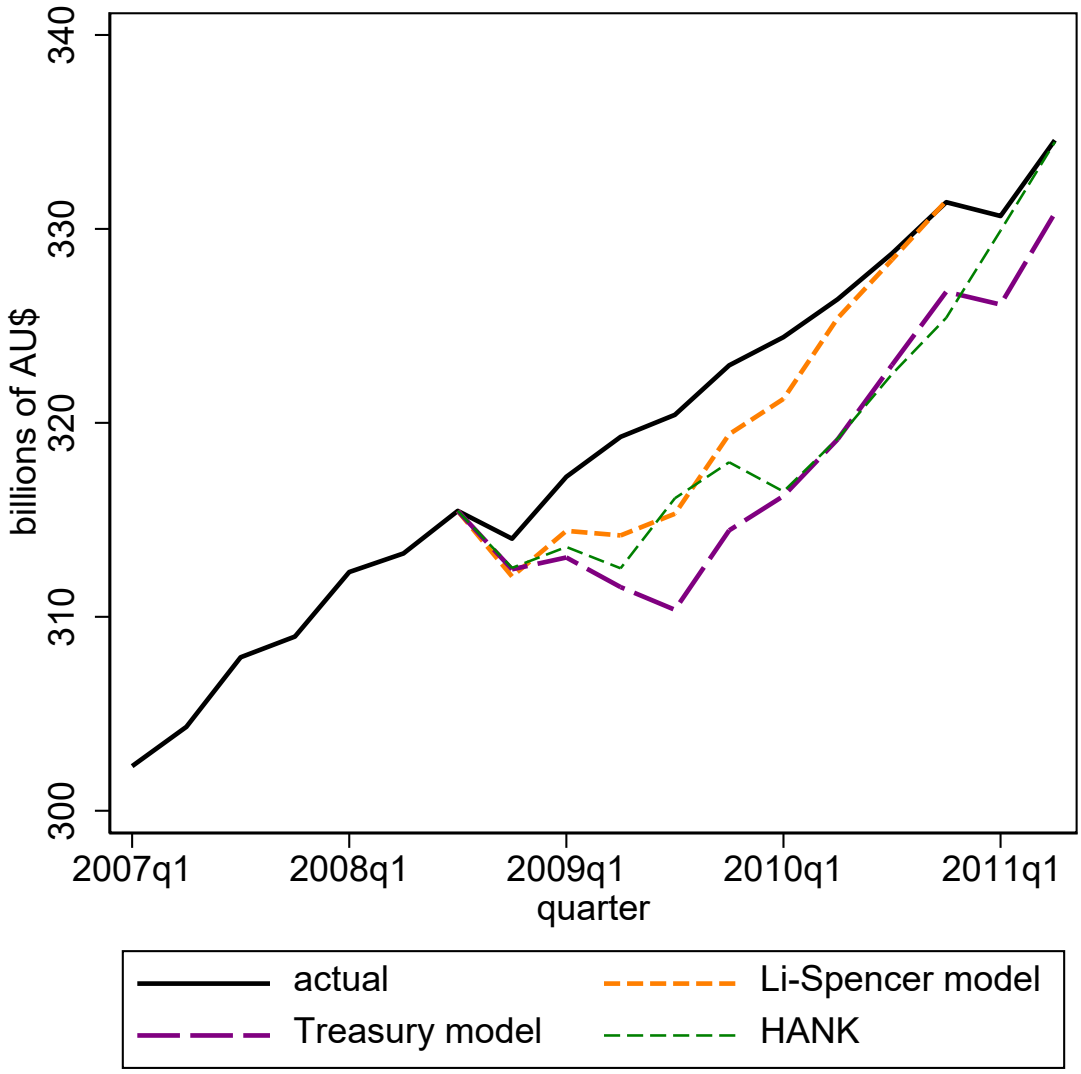
Notes. The program payout series is based on the author's narrative. The aggregate data are from the Australia Bureau of Statistics. The solid line is the point estimate and the two shaded areas are 68% and 95% confidence intervals. The estimates are from a 4-variable SVAR with 4 lags, estimated from 1999q1 through 2019q4.

Figure 14. Australia HANK Counterfactuals Calibrated to Leigh (2012) MPCs



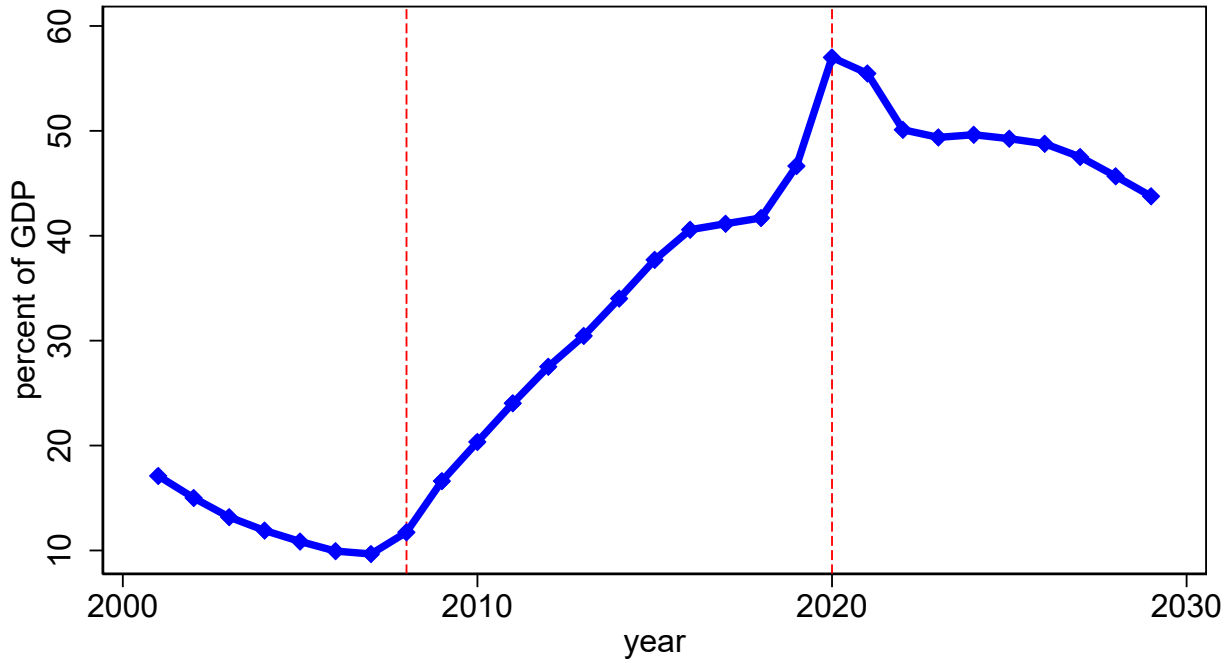
Notes. Actual data are from the Australia Bureau of Statistics and are seasonally adjusted quarterly rates of billions of 2008q3 AUD. The counterfactual paths show the path the economy would have followed had there been no fiscal stimulus. The counterfactual path subtracts induced consumption and GDP from the actual data. The induced values are based on the AARS (2023) HANK model calibrated to the Leigh (2012) MPCs.

Figure 15. Australia Counterfactuals from Treasury (2009) and Li-Spencer (2016)



Notes. Actual data are from the Australia Bureau of Statistics and are seasonally adjusted quarterly rates of billions of 2008q3 AUD. The counterfactual paths show the path the economy would have followed had there been no fiscal stimulus. The HANK counterfactual is the same shown in Figure 13. The two other counterfactuals are based on Australian Treasury projections and the Li and Spencer (2016) results.

Figure 16. Australia Ratio of Gross Government Debt to GDP



Notes: Data are from the IMF's *World Economic Outlook* database, April 2024. The vertical dotted lines mark 2008 and 2020.

Table 1. Comparison of Consumption Declines

Series and Time Period	**% Change in Consumer Spending**
Australia actual, 2008q1 – 2009q2	1 %
Australia counterfactual, 2008q1 – 2009q2	- 4.2 %
Australia actual, 1975q2 – 1975q4	- 2.5 %
Australia actual, 2019q4 – 2020q2	-13.9 %
U.S. actual, 2008q2 – 2009q2	-2.4 %

Notes: Australian actual data are from the Australian Bureau of Statistics. The counterfactual is from a HANK model, described in the text. The U.S. actual data are from the U.S. Census, Bureau of Economic Analysis.