Early pension withdrawal as stimulus*

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Abstract

During the COVID-19 pandemic, the Australian government allowed the withdrawal of up to A$20,000 (around half median annual wage income) from mandatory private retirement accounts, ordinarily inaccessible until retirement. One in six withdrew A$38 billion (2% of GDP). These transfers were much larger than those typically considered in the literature and represented a liquidity shock, holding lifetime income constant. Using administrative and high-frequency bank transactions data, we find a marginal propensity to spend of at least 0.43–0.48 (90% of this in four weeks), high given the transfer size. Gambling was the largest discernible spending category (larger than credit card repayments). Low rates and levels of saving and high levels of gambling before withdrawal strongly predict withdrawal and spending. We calibrate a heterogeneous-agent model, demonstrating that only under present bias ($\beta = 0.58$) can we reconcile the observed magnitude and frequency of the spending response and pre-withdrawal saving behavior.

JEL codes: E21, E63, E71, H31, H55, J32
Keywords: Stimulus, pension, liquidity, marginal propensity to consume, heterogeneous-agent model, present bias

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1 Introduction

There is a vast literature documenting much larger responses to cash transfers than would be predicted under the permanent-income hypothesis and Ricardian equivalence (Ricardo, 1820; Friedman, 1957; Barro, 1974).[1] Cash transfers are a tiny fraction of lifetime income, so a rational, forward-looking, and liquid person would increase consumption only by a tiny fraction in response—or not at all given higher future taxes. Scholars have proposed two explanations for these responses: 'behavioral', considering deviations from rational, forward-looking behavior such as present bias (Angeletos, Laibson, Repetto, Tobacman and Weinberg, 2001; Parker, 2017; Laibson, Maxted and Moll, 2021);[2] and 'rational', considering liquidity constraints, taking as given rational and forward-looking behavior (Zeldes, 1989; Deaton, 1991; Carroll, 1997; Kaplan and Violante, 2014). Which prevails has implications for policy: if a consumer is rational and forward-looking, easing their liquidity constraint raises their welfare; if they are present-biased, it could lower it.[3]

Under typical cash stimulus, in the order of US$1,000, it can be difficult empirically to distinguish between these explanations. In this paper, we leverage high-frequency bank-transactions and administrative data to study the effects of a very large and unexpected liquidity shock to a broad swath of the population. In this setting, the alternative consumption models generate clearly divergent predictions. During the COVID-19 pandemic, the Australian government for the first time allowed eligible people to withdraw up to A$20,000 (US$13,600)[4] across two tranches from their private retirement savings accounts (called 'Superannuation', or 'Super'), normally inaccessible until retirement.[5,6] This was a large, voluntary, and direct transfer from a withdrawer's future self: the modal withdrawal by the modal-aged withdrawer can

[1]Examples include Johnson, Parker and Souleles (2006), Shapiro and Slemrod (2009), Sahm, Shapiro and Slemrod (2010), Parker, Soulless, Johnson and McClelland (2013), Broda and Parker (2014), and Agarwal and Qian (2014). Several recent studies have documented large responses to transfers received during the COVID-19 pandemic, including Chetty, Friedman, Hendren and The Opportunity Insights Team (2022), Yannelis and Amato (2022), Karger and Rajan (2021), Kubota, Onishi and Toyama (2021), Baker, Farrokhnia, Meyer, Pagel and Yannelis (2020*b*), Baker, Farrokhnia, Meyer, Pagel and Yannelis (2020*a*), and Coibion, Gorodnichenko and Weber (2020).

[2]Pfäuti, Seyrich and Zinman (2023) offer the alternative behavioral explanation that a subset of people are persistently over-confident / over-optimistic, with similar behavioral implications to present bias.

[3]Maxted (2022) finds the welfare cost of present bias is equivalent to a consumption tax of up to 17.2%.

[4]All figures are in Australian dollars unless otherwise stated. As of 2021, in PPP terms, US$1 bought A$1.45.

[5]We estimate at least 70% of working-age people were eligible to withdraw under the program.

[6]We know of two papers on this program. Sainsbury, Breunig and Watson (2022) use the same admin data to study the effect of program participation on employment outcomes. Wang-Ly and Newell (2022) study the program using coarser and more limited data (e.g., monthly, no admin data) from an Australian bank, but find a similarly large spending response.

be expected to reduce their balance at retirement by more than $120,000 in today's dollars.[7] In practice, one in six working-age people (or one in four 34-year-olds) participated, withdrawing $38 billion (2% of GDP) in total.

Two features of this setting frame our analysis. First, because withdrawals were optional, we can examine separately the decision to withdraw and to spend conditional on withdrawal. The sign of this selection effect is ambiguous *ex ante*. Because withdrawals could effectively be re-contributed pre-tax (implying a subsidy of up to $7,528), there was a strong financial incentive to withdraw.[8] And because contributions are mandatory (at 10.5% of wage earnings) and otherwise inaccessible until retirement, the program offered a one-time opportunity to rebalance the portfolio towards higher liquidity. On the other hand, frictions may have prevented withdrawals, such as the mechanism proposed by Attanasio, Kovacs and Moran (2020), in which people deliberately choose low liquidity as a commitment device.

Second, the transfers were an order of magnitude larger than those typically considered in the literature but did not affect permanent income. As noted by Laibson et al. (2021, p.32), to the extent that models with standard preferences can generate large consumption responses, these responses decline rapidly with transfer size. For example, Kaplan and Violante (2014) predict that, with liquidity constraints alone, the marginal propensity to consume (MPC) falls from 0.20 to 0.03 as the transfer size rises from US$500 to US$5,000. Present bias boosts the consumption response substantially even under large transfers (Laibson et al., 2021). As we show, out of a single Super withdrawal the two models predict consumption responses that differ by an order of magnitude.

We begin by using administrative records on the full working-age population and a large panel of weekly bank transactions to study selection into the program. We find the vast majority remained constrained even after accessing roughly half median annual wage income in liquidity: five in six withdrew as much as possible, nearly half withdrew in the first 10 days, and three quarters who had funds remaining after the first round withdrew again. Those in 'blue-collar' occupations and those located farther from cities were far more likely to withdraw. Withdrawers had slightly lower wages, but this was persistent not transitory: wages were lower in the two months prior, the three years prior, and the life to date, and there was no discontinuity in

[7]Based on historical 30-year net returns of 8.3%, average inflation of 2.5%, and a retirement age of 65.

[8]People could contribute pre-income-tax an additional $25,000 per year at a concessional tax rate of 15%.

weekly wages upon withdrawal. But withdrawers had far lower rates of saving and levels of savings, both immediately prior to withdrawal and in the three years prior, and they had substantially lower stock and investment property holdings. These features were particularly pronounced among those who withdrew earlier or again.

Next we study the effect of the program on spending. Using modern difference-in-differences techniques (Callaway and Sant'Anna, 2021; Sun and Abraham, 2021) and exploiting the differential timing of withdrawal, we find a high marginal propensity to spend (MPX) out of the first withdrawal of at least 0.43 (0.01) over eight weeks,[9] spread broadly across categories and with at least 60% of discernible spending on non-durables. This spending response was very sharp, with 71% occurring within two weeks and 90% within four weeks. The MPX out of the second withdrawal, which two-thirds returned for and which occurred after activity had recovered, was 0.48 (0.01). Applying quantile difference-in-differences to spending changes, we find that the MPX was near-uniformly distributed up to 0.95 but with a long right tail (5% had an MPX of greater than one and 1% greater than 2.3). Among withdrawers, there were strong negative relationships between the MPX and several measures of pre-withdrawal financial health, and strong positive relationships with pre-withdrawal gambling and cash withdrawal. The least-liquid 40% spent more than double that of ($2,500 more than) the most-liquid 20%.

Finally, to interpret our results, we develop a heterogeneous-agent model with two assets, income risk, and borrowing constraints in the spirit of Kaplan, Moll and Violante (2018), but with naively present-biased households as in Laibson et al. (2021). With exponential-discounting but liquidity-constrained households and calibrated at the monthly frequency, the model fails to match the evidence; households smooth the transfers excessively even if at their borrowing limit. With impatient households (very high but still exponential discounting), the model can match the average MPC,[10] but not the heterogeneity in MPCs and liquidity, and with an unrealistically low subjective discount factor (0.15 annually). We obtain the best fit with naively present-biased households, with a present-bias parameter ($\beta = 0.58$) of similar magnitude to recent estimates (e.g., Ganong and Noel (2019); Gerard and Naritomi (2021)). Not only does the model match the average MPC and average liquid-assets-to-income ratio, but also the joint distribution of liquidity and MPC.

[9]Excluding debt repayments and external transfers, which will have accounted for additional spending.

[10]The model is calibrated using MPCs, which we assume are 60% of the observed MPXs reflecting our estimated lower bound on the share of non-durables in the total discernible spending impact.

The calibration frequency is critical. Liquidity constraints alone can match the data at an annual frequency, but this breaks down at higher frequencies as the liquidity shock is a larger multiple of wages and the spending response is condensed.

Our findings have direct implications for the design of private retirement saving systems, which have gained renewed interest as the US Social Security trust funds near depletion. The US 401(k) system is unique in its high degree of liquidity (Beshears, Choi, Hurwitz, Laibson and Madrian, 2015), loosened further during the pandemic. Australia has had a privatized social security system since 1993, with mandatory saving and withdrawals not permitted before retirement. The Super withdrawal program was an unexpected shock to this system. In practice, the vast majority of those who were eligible chose not to access their retirement savings when given a one-off chance to do so, while those who did appeared overwhelmingly to be driven by present bias. The clear implication is that, as a matter of retirement saving policy, illiquidity would seem to be welfare-improving in the aggregate.

At the same time, we find withdrawals generated at least 0.8% of GDP in direct spending, almost entirely within a four-month period. Based on the stated up-front fiscal cost of 0.06% of GDP, this implies a direct fiscal multiplier of 13.4, at least an order of magnitude larger than with cash stimulus (Ramey, 2019).[11] By generating a high MPX, the program raised the numerator; by being self-financed, it lowered the denominator. The government announced the fiscal cost as just $1.2 billion (reflecting foregone taxes over four years) and framed it as giving people access to "their own money". Under fiscal constraints, this self-financing can enable additional stimulus, but this comes at the cost of the sub-optimal future consumption of the present-biased. It is an open question whether macro-stability should be funded by the lower future retirement balances of the few or higher future taxes on the many, the consequences of which depend on the tax and transfer system.

To the best of our knowledge, the Super withdrawal program is unique in the literature in generating very large transfers to a broad swath of the population, holding lifetime income constant. Studies of stimulus checks consider transfers in the order of US$1,000, the responses to which may not differ markedly under different consumption models. Transfers from the Alaska Permanent Fund were larger (US$3,900 for an average family), but still far smaller than the Super withdrawals

[11]There are challenges in measuring comprehensively the fiscal multiplier (Nakamura and Steinsson, 2018, 2014). Our setting enables sharp identification of a direct increase in consumer spending in a short window of time driven by policy.

(up to US$27,000 for a family) and received regularly (Hsieh, 2003; Kueng, 2018). Lottery winnings are the largest transfers considered in the literature, but they increase lifetime income, often substantially (Imbens, Rubin and Sacerdote, 2001; Kuhn, Kooreman, Soetevent and Kapteyn, 2011; Fagerang, Holm and Natvik, 2021; Golosov, Greber, Mogstad and Novgorodsky, 2021).

The one direct precedent was in Denmark in 2009, studied by Kreiner, Lassen and Leth-Petersen (2019). There are some important differences between their setting and ours. The Danish system was relatively new and small: it was introduced 11 years earlier and had been closed to contributions for five years (vs 27 years earlier and ongoing for Super); contributions were 1% of earnings (vs 10.5% for Super); and all savings received the same, flat return subject to income taxes on withdrawal (vs no taxes on withdrawal and other tax concessions for Super). Consequently, after taxes, the average withdrawal was US$1,900, an order of magnitude lower than the modal Super withdrawal of US$13,600. The authors combine survey data on spending with administrative data on assets to establish a relationship between liquidity-constraint tightness, as measured by the marginal interest rate, and spending. In contrast, our focus—aided by rich, high-frequency administrative and bank transactions data as well as unique features of the Super withdrawal program—is on *why* there is an association between liquidity and spending.

In that respect, we contribute to an emerging literature across diverse settings in which high-frequency spending data reveals an excess sensitivity of spending to income that can only be rationalized by present bias. Ganong and Noel (2019) consider the exhaustion of unemployment benefits in the US, documenting a non-durables spending decline of 12%, consistent with a large share of recipients being present-biased ($\beta = 0.5$). Gerard and Naritomi (2021) consider São Paulo, Brazil, where workers laid off without cause are entitled to severance of 4.7 months' wages on average, estimating a spending jump of 35% despite a permanent-income decline of 14%, consistent with present bias ($\beta = 0.44$). Gelman (2022) finds that present bias ($\beta = 0.9$) best explains spending responses in the US to paychecks and tax returns. We also contribute to the literature on retirement saving, in particular to studies on early withdrawals, including Goda, Jones and Ramnath (2022), Coyne, Fadlon and Porzio (2022), Goodman, Mortensen, Mackie and Schramm (2021), Andersen (2020), Beshears, Choi, Harris, Laibson, Madrian and Sakong (2020), and Argento, Bryant and Sabelhaus (2014).

We proceed as follows. In Section 2, we describe the policy context and data. In Section 3, we study selection along three dimensions: whether people withdrew; how early they withdrew; and whether they withdrew a second time. In Section 4, we estimate the spending responses to each withdrawal and how it was allocated across categories. In Section 5, we consider heterogeneity in the response. In Section 6, we develop and calibrate a heterogeneous-agent model to compare our results to predictions under different consumption models. And in Section 7, we conclude.

2 Preliminaries

2.1 Policy

Australia has a compulsory, defined-contribution private retirement saving system called Superannuation (or 'Super').[12] All employers are required to contribute an additional 10.5% of pre-tax wages to their employees' accounts, inaccessible outside exceptional circumstances until age 58 if retired or 65 if working. For most people, Super is tax-preferred over most other forms of saving: employer contributions are made pre-income-tax, then taxed at 15% on entering the fund (compared to a modal marginal income tax rate of 34.5% and a top rate of 47%); before retirement, cash returns are taxed at 15% and capital gains at 10%; and in retirement neither returns nor withdrawals are taxed. Additional voluntary contributions can be made, up to $27,500 per year before tax and $100,000 per year after tax, but less than 1% of people are above the pre-tax cap.[13] The median growth fund had an annual return net of fees and taxes of 9.5% over 10 years and 8.3% over 29 years.[14]

On March 22, 2020, during the initial COVID-19 outbreak, the Australian government announced eligible people could withdraw up to $10,000 from their Super accounts by June 30, followed by another $10,000 from July 1, the first time broad early access had been granted. Notably, the government eschewed the universal cash transfers employed in other countries during the pandemic and which an Australian government of the opposite major party had implemented during the Global Financial Crisis. Cash transfers were limited to lump-sum payments for

[12]A more detailed description with references can be found in Appendix A.

[13]See Chan, Morris, Polidano and Vu (2022) for a study of the income and saving responses to these concessions.

[14]People can choose between portfolios based on risk. Typically, people keep their savings in a 'growth' fund for most of their working life and switch to a more conservative fund near retirement.

those receiving government benefits, alongside wage subsidies and supplemental unemployment insurance. Announcing the program, the then-Prime-Minister said it would "help Australians access more of their own resources to get through this time of crisis".[15] The government stated the fiscal cost as $1.2 billion, which reflected only foregone taxes over four years (not the $38 billion eventually withdrawn).[16] The government would later add broad eligibility conditions.[17,18] Applications for the first round opened around a month later on April 20 via the 'myGov' online portal. Applicants were to nominate a withdrawal amount and a reason for withdrawing, self-assessing eligibility. Applications were processed in three days on average, at which time the withdrawal was deposited into the applicant's bank account.

Australia's experience of the pandemic was far milder than most other countries'. Australia closed its borders, with international travel virtually impossible for non-citizens and highly restricted for citizens for the first 18 months. From March 28, all international arrivals had to complete two weeks of hotel quarantine. Public health measures were imposed locally until cases were eliminated. Australia's per-capita death rate was less than a tenth that in the US. Meanwhile, Australia had a large federal fiscal response at 17% of GDP (excluding 2% of GDP in Super withdrawn). The Super withdrawal program was part of the second of three rounds of fiscal support announced during March 12–30.[19] While the initial outbreak generated a substantial reduction in mobility, this recovered rapidly as cases were eliminated (Figure 1a), and the decline in personal net income excluding Super withdrawals was negligible (Figure 1b). Through May and June, all domestic restrictions lifted nation-wide.[20]

[15] https://www.smh.com.au/politics/federal/a-support-package-built-on-debt-and-hope-but-approach\-must-change-20200322-p54cpa.html

[16] https://ministers.treasury.gov.au/ministers/josh-frydenberg-2018/media-releases/supporting-australian-workers-and-business

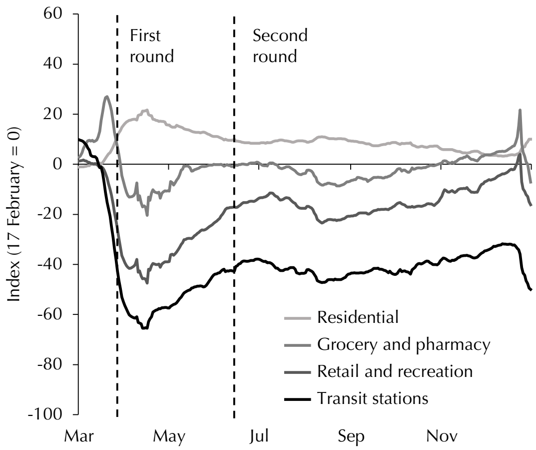
[17] This included those who: were unemployed; were in receipt of certain government benefits (e.g., unemployment insurance); had been made redundant or had their working hours reduced by at least 20% on or after 1 January 2020; were a sole trader whose business was suspended or had experienced a reduction in turnover of at least 20%; or were on a temporary visa and met certain conditions (e.g., not being able to meet immediate living expenses). In Appendix B, we construct an eligibility proxy suggesting more than 70% were eligible.

[18] https://treasury.gov.au/sites/default/files/2020-04/Fact_sheet-Early_Access_to_Super.pdf

[19] Hamilton (2020) compares the Australian and US fiscal responses. Breunig and Sainsbury (2023) consider the distribution of fiscal transfers. Bishop and Day (2020) and Watson, Tervala and Sainsbury (2022) study Australia's wage subsidy program.

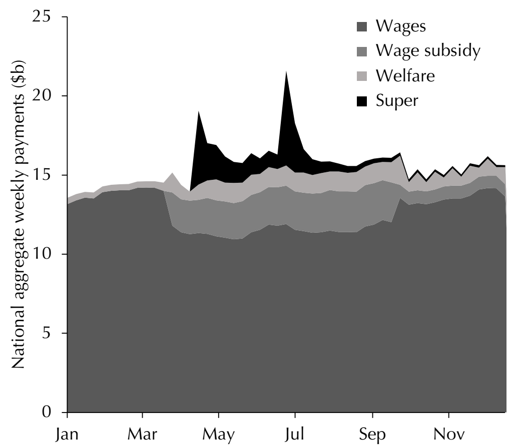
[20] https://www.aph.gov.au/About_Parliament/Parliamentary_Departments/Parliamentary_Library/pubs/rp/rp2021/Chronologies/COVID-19StateTerritoryGovernmentAnnouncements#_Toc52275795

Figure 1: Economic activity indicators in Australia during 2020



(a) Mobility indicators by establishment

Data: Google trends
Note: Physical locations visited.



(b) Working-age payments by type

Data: Australian Taxation Office and Department of Social Services

2.2 Data

We rely on two data sources: administrative data covering all working-age Australians, which allow us to study the withdrawals; and bank transactions data covering a subset of people, which allow us to study how the withdrawals were used. We are not permitted to link individuals across the two datasets. On the measures we observe in both datasets, including state, sex, welfare receipt, and the wage distribution, they appear quite similar (Appendix C). Elias (2022) presents evidence high-frequency spending records in our bank transactions data closely match those from other sources, including official statistics.

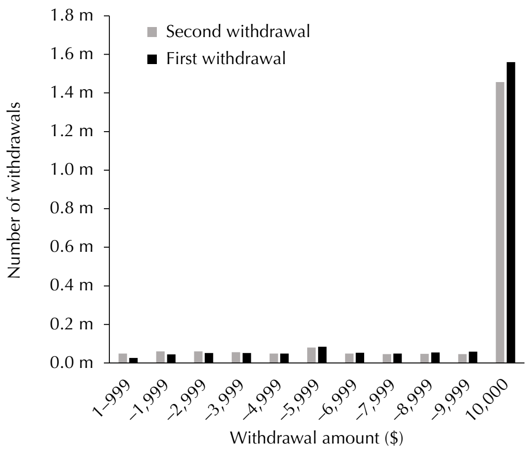
We access the administrative data via the Australian Bureau of Statistics' Multi Agency Data Integration Project (MADIP), which links de-identified, individual-level datasets across agencies. For our purposes, these cover all working-age (16–65) Australians who reported positive Super balances in the financial year July 1, 2018 to June 30, 2019 (2018-19), which yields a sample of 15.2 million (versus 25 million in the population). In MADIP, we observe age, sex, and location at the suburb level. Via personal tax records, we observe: occupation; a spouse indicator; the number of dependents; three years of tax returns, including the income derived from wages, interest, rent, and dividends; the Super balance at June 30, 2019; all Super contributions for 2018-19; and all information on the Super withdrawal program for all 4.5 million approved applications,[21] including the withdrawal amount, date, and reason. Via the Single Touch Payroll system, we observe all weekly pre-tax wages. Via the welfare payment system, we observe all weekly government benefits, including unemployment benefits and pandemic support payments. We also observe all fortnightly wage subsidies that each worker's employer received.

The bank transactions data are provided by Illion, one of Australia's three large credit bureaus. Data are collected during credit-check events initiated by Illion clients, including telcos, utilities, and financial institutions (more than 6,000 in total). When an individual triggers a credit-check event, Illion collects all bank transactions across their accounts, including transactions made with associated debit and credit cards over the prior 13 weeks. The original transactions data include the date and time the transaction was processed (usually within a few business days for card purchases and instantly for transfers), description, transaction type (e.g., card payment, direct debit, external transfer), transaction value, and account. The dataset we use is an aggregated version of this transaction-level data. Transactions are classified by Illion into spending categories (e.g., supermarkets, retail, department stores) and income categories (e.g., wage income, Super withdrawal, welfare income) using the transaction description and type. External transfers are not counted towards spending. Single transactions are aggregated into weekly transaction amounts across all accounts for each spending and income category. This results in an individual-level panel of weekly spending and income by category, with a 13-week observation window for each individual.

Observing a limited window for each individual introduces the possibility of assigning someone who withdrew before the start of their window as not having withdrawn. For the sample of withdrawers in the first round, we exclude those who: 1) did not have a recorded Super withdrawal, and 2) had an observation window that began after April 19 (the week preceding the first withdrawals). The second withdrawal sample was constructed identically, but relative to June 21. Among those we do observe having withdrawn super, we drop those for whom we do not observe three weeks of pre-withdrawal data. For the first round of withdrawals, we do not consider transactions beyond June 28 to avoid contamination by the second round. No other modifications were made to the data for the main analysis. This generates samples of around 340,000 and 410,000, respectively.

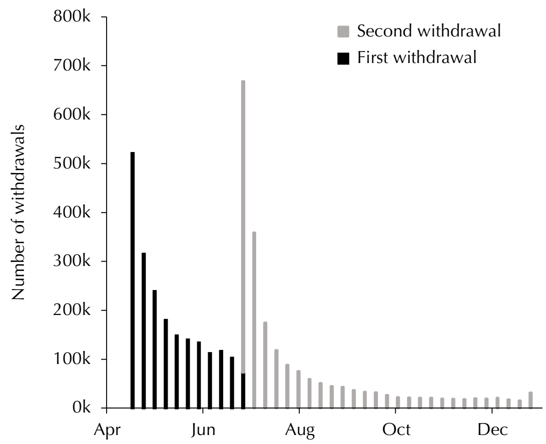
[21] 3.05 million people lodged 4.78 million applications, of which 232,000 were rejected.

Figure 2: Number of withdrawals



(a) Number of withdrawals by amount

Data: Australian Taxation Office



(b) Number of withdrawals by week

Data: Australian Taxation Office

3 Withdrawal

3.1 Withdrawers v non-withdrawers

We begin with some descriptive facts about withdrawal. Around one in six (2.6 million) working-age people withdrew $37.8 billion (around 1% of assets or 2% of GDP) in total. Including those who did not withdraw, the average withdrawal was 9% of the balance; among only those who did withdraw, it was 51%. Around three quarters in each round withdrew the maximum $10,000, with the modal withdrawal across the two rounds $20,000 and the average $13,584 (Figure 2a). Among those who withdrew less than $10,000 in the first round, one third drained their account, suggesting they were constrained by their available balance. This means only around one in six chose an 'interior' withdrawal amount. Around three quarters of those who still had a positive balance remaining after the first round withdrew again, with a similar pattern of withdrawals to the first round (Figure 2a). In the two rounds, 25% and 30% withdrew within the first three days and 40% and 48% within the first 10 days (Figure 2b).

Next, we compare the pre-withdrawal characteristics of those who withdrew and those who did not.[22] In the first column of Table 1, we present means among

[22]Eligibility was self-assessed. As discussed in Appendix B, we estimate at least 70% were eligible, and conditioning on

Table 1: Estimated differences in means between withdrawers and non-withdrawers in the first round

	Non-withdrawer	Withdrawer (difference)				
Controls	None	None	Wages	Plus age	Plus all	*Data*
Demographics						
Age	41.09	-2.68	-2.09			A
	(0.00)	(0.01)	(0.01)			
Female	0.49	-0.05	-0.08	-0.07		A
	(0.00)	(0.00)	(0.00)	(0.00)		
Had spouse	0.57	-0.12	-0.09	-0.06		A
	(0.00)	(0.00)	(0.00)	(0.00)		
Had dependents	0.38	0.08	0.09	0.10		A
	(0.00)	(0.00)	(0.00)	(0.00)		
Long-term financials						
Annual wage income	47,340	-4,050				A
	(15)	(35)				
Super balance	121,398	-61,237	-48,383	-35,882	-34,520	A
	(66)	(157)	(143)	(133)	(134)	
Interest income	420	-314	-306	-258	-261	A
	(1)	(2)	(2)	(2)	(2)	
Rental income	958	-369	-296	-240	-229	A
	(1)	(3)	(3)	(3)	(3)	
Dividends	1,106	-857	-809	-657	-669	A
	(4)	(10)	(9)	(9)	(9)	
Voluntary Super	2,467	-2,199	-2,159	-1,692	-1,637	A
	(4)	(11)	(11)	(11)	(11)	
Short-term financials						
Weekly wage income	786	-21				B
	(2)	(7)				
Saving / spending	0.37	-0.20	-0.22			B
	(0.01)	(0.03)	(0.03)			
Savings / spending	5.38	-3.31	-3.34			B
	(0.07)	(0.25)	(0.25)			
Debt payment / spending	0.14	0.01	0.01			B
	(0.00)	(0.00)	(0.00)			
Had negative balance	0.09	0.02	0.02			B
	(0.00)	(0.00)	(0.00)			
Data	N					
A: Administrative data	15,249,488					
B: Bank transactions data	336,809					

Data: Australian Taxation Office and Illion.
Notes: Results are from simple linear regressions of outcomes on a binary first-withdrawal indicator, controlling cumulatively for wages and the 'Demographics' variables. Wage control for Demographics and Long-term Financials is average pre-tax wage income in the prior three years. Spouse and dependents are from the tax return in the financial year prior to withdrawal (July 1, 2018–June 30, 2019). Long-term financials except Super Balance and Voluntary Super are averages across the three prior tax returns (2016–17, 2017–18, and 2018–19). Super balance is as at June 30, 2019. Voluntary Super contributions are for the prior year (2018–19). Annual wage income is pre-tax and weekly wage income is post-tax. All short-term variables are averages for the month prior to program commencement. Standard errors in parentheses. All estimates statistically significant at the 95% level.

the non-withdrawers, and in the remaining columns we present differences in means between the withdrawers and non-withdrawers, with the third through fifth columns including controls for pre-treatment wages and then cumulatively for the variables listed under 'Demographics'. Those who withdrew were on average three years younger, five percentage points more likely to be male, 12 percentage points more likely to be single, and eight percentage points more likely to have dependents.

In probing the motivation for withdrawal, we are particularly interested in withdrawers' financial circumstances. Those who withdrew had lower wages, both during the month before withdrawals commenced (3% lower) and during the three years before (8% lower). We find no change in individual-level weekly wages coincident with withdrawal.[23] Withdrawers had around half the Super balances of non-withdrawers, which mechanically reflects lower wages over the working life to date.[24] Collectively, these results indicate that the difference in wages was persistent, being present in the month prior, the three years prior, and the working life to date. Because the withdrawers were not on average suffering a temporary wage shortfall, this does not seem to be a motivation for withdrawal.

There were much larger differences in the levels of financial assets. In the month before withdrawal, the withdrawers had a 54% lower saving rate, 62% lower balances relative to spending, a 23% higher probability of being overdrawn, and 10% higher debt repayments relative to spending, all robust to wage differences. There were similar differences in the longer term, with withdrawers having received 75% less interest income, 24% less rental income, and 60% less in dividends in the past three years. Withdrawers also made 89% lower voluntary Super contributions in the past year, reflecting that they were 10 percentage points (17.3% v 7.7%) less likely to have made any voluntary contribution, while those who did made 74% lower contributions on average ($3,327 v $12,766). Overall, withdrawal does not appear to have been motivated by a temporary liquidity shortfall.

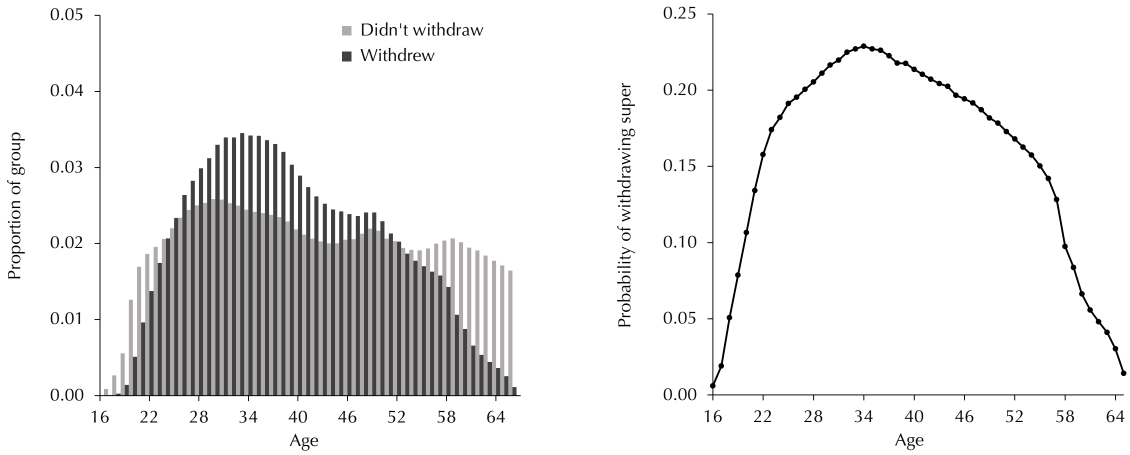
For further context, in Figure 3 we present the age distributions of the withdrawers and non-withdrawers along with the associated probability of withdrawal by age. As noted earlier, the withdrawers were three years younger on average, but this masks a compression of the age distribution among the withdrawers. The withdrawers were underrepresented up to age 23 and beyond age 51, with a modal age of 33. The

eligibility doesn't alter meaningfully any of the patterns observed in Table 1.

[23]Based on an event study on wages using the same method we apply to spending later (Appendix D).

[24]Additionally, the Super balance and wage densities among the withdrawers were compressed. (Appendix E).

Figure 3: The relationship between withdrawal and age



(a) Age histogram by withdrawal status

Data: Australian Taxation Office
Note: Sample includes those with a positive Super balance.

(b) Probability of withdrawal by age

Data: Australian Taxation Office
Note: Sample includes those with a positive Super balance.

maximum take-up rate was among those aged 34, at 23%.[25,26]

Finally, in Table 2, we divide the Australian working-age population by occupation and location and calculate withdrawal rates among these divisions.[27] There was strong variation in withdrawal along both dimensions. Withdrawal was highest in 'blue-collar' professions and lowest in 'white-collar' professions, with teachers the lowest-withdrawing occupation at 6.3% and construction and mining laborers the highest at 40.2%. Withdrawal was also strongly, monotonically, and negatively related to the proximity to cities, with those in very remote areas 40% more likely to withdraw than those in major cities. In the most remote locations, more than half of people withdrew; in Australia's capital just 3–5% withdrew.

3.2 Timing

People faced not only a choice of whether to withdraw but also how soon to withdraw. The differences between the withdrawers and non-withdrawers in Table 1 were greater the earlier the withdrawal (Figure 4). Setting aside the first and last weeks, which may be subject to idiosyncratic factors, the average Super balance of a withdrawer increased by 28% over the intervening seven weeks, average wages by

[25] This rises to 27% at an earlier age of 32 when we consider take-up only among the eligible (Appendix B).

[26] The drop in take-up at age 57 coincides with the age individuals could begin to access their Super if they were retired.

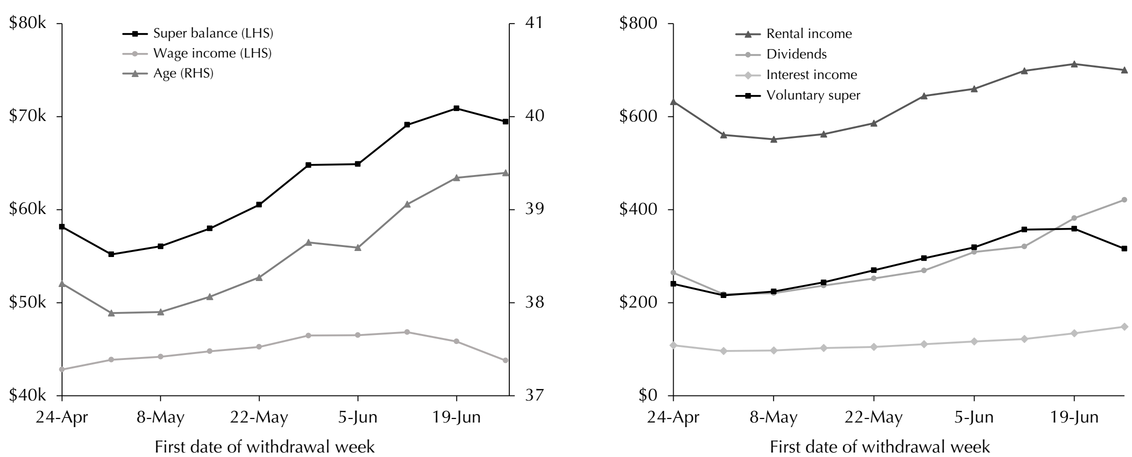
[27] Appendix F includes a full break-down of occupation into 46 categories.

Table 2: Withdrawal rates by occupation and location

	Withdrew (%)
Occupation	
Machinery operators and drivers	32.3
Laborers	30.5
Technicians and trades workers	24.4
Community and personal service workers	22.7
Sales workers	20.0
Managers	16.8
Clerical and administrative workers	15.4
Professionals	9.4
Location	
Very remote	24.2
Remote	21.1
Outer regional	19.5
Inner regional	18.1
Major cities	17.3

Data: Australian Taxation Office and Australian Bureau of Statistics
Note: Occupation based on tax return in prior financial year. Location based on suburb from combined administrative data.

Figure 4: Average characteristics by withdrawal week during the first round



Data: Australian Taxation Office
Notes: Outcomes as per 'Long-term financials' in Table 1.

Data: Australian Taxation Office
Notes: Outcomes as per 'Long-term financials' in Table 1.

4.5%, and average age by 1.5 years. Average pre-withdrawal interest, dividends, and rental income all increased with the withdrawal date, a relationship that remained statistically significant even after controlling for the characteristics conditioned on earlier (Appendix G). With the program having offered temporary access to ordinarily inaccessible wealth, the front-loading of withdrawals suggests urgency.

3.3 Second withdrawal

In addition to having a choice of whether and how soon to withdraw, people could also choose to withdraw a second time, 72 days after applications for first withdrawals opened. One advantage of observing the second withdrawal is that it came well after the initial pandemic shock. While both rounds were announced in late May, the first round opened at the peak while the second round opened after activity had recovered substantially (Figure 1a). Observing a second opportunity to withdraw also allows us to gauge the intensity of the desire for liquidity.

We observe similar characteristics among those who withdrew a second time as among those who withdrew at all or earlier (Table 3). Those who withdrew a second time did have higher wages, were slightly older, and had higher Super balances, but this is because we removed those having already drained their accounts, who were disproportionately lower-income, younger, and with lower Super balances. Even so, those who withdrew twice had 22% less interest income, 36% lower voluntary Super contributions, 14% less in dividends, and 6% lower rental incomes. And when we condition on wages and age, these differences increase substantially.

Table 3: Differences in means between those who did and did not withdraw a second time

	Withdrew first only	Withdrew first and second (difference)			
Controls	None	None	Wages	Plus age	Plus all
Wages	43,173	4,897			
	(54)	(64)			
Age	37.88	1.82	1.67		
	(0.01)	(0.02)	(0.02)		
Super balance	55,652	16,226	11,774	6,024	6,024
	(128)	(150)	(139)	(128)	(128)
Interest income	137	-30	-32	-40	-38
	(1)	(2)	(2)	(2)	(2)
Rental income	707	-43	-122	-160	-130
	(5)	(6)	(6)	(6)	(6)
Dividends	317	-43	-52	-87	-80
	(7)	(9)	(9)	(9)	(9)
Voluntary Super	394	-142	-161	-194	-190
	(4)	(5)	(5)	(5)	(5)
N = 1,862,516					

Data: Australian Taxation Office
Notes: Results are from simple linear regressions of outcomes on an indicator for withdrawing in the second round, controlling cumulatively for the wages and 'Demographics' variables listed in Table 1. Excluded from the regression are those who did not withdraw in the first round as well as those who did but drained their balance. Variable definitions as per 'Demographics' and 'Long-term financials' in Table 1.

4 Spending

4.1 Marginal propensity to spend

In the previous section, we saw that low pre-withdrawal liquidity is strongly predictive of withdrawal, the urgency of withdrawal, and repeated withdrawal. Participation was not driven by temporary wage or liquidity shortfalls as those differences were permanent. As noted by Parker (2017), this suggests a behavioral explanation. Our descriptive findings on age, occupation, and location point in a similar direction. Low liquidity presents a fundamental identification challenge because both rational and behavioral motives imply low liquidity—we simply can't tell whether that low liquidity is a consequence or cause of the motivation. In this section, we study the spending response to distinguish between the two.

Our spending measure includes all debits associated with an individual's bank accounts (such as with a debit or credit card), cash withdrawals, and 'buy now, pay later' repayments. It excludes all debt repayments and external transfers. In

Australia, cash transactions fell from 40% in 2007 to just 10% in 2019 (compared to 22% in the US).[28,29] In a 2019 official survey, 12% of those who reported holding cash outside their wallet did so to fund a large purchase, suggesting cash withdrawals overwhelmingly are used for immediate consumption.[30] Because we exclude all debt repayments and external transfers, which account for some spending, our measure can be viewed as a conservative lower bound on true spending.[31]

We begin by plotting average income and spending among all units in the bank transactions data by 2020 calendar week, with the timing of each round clearly visible (Figure 5). There is a sharp spike in income coincident with the first weeks of withdrawals in late April and early July, and concomitant but more diffuse increases in spending, mirroring the timing of withdrawals we saw in the administrative data (Figure 1b). There are two issues with this aggregate view. First, there were concomitant income shocks, including supplementary unemployment insurance, wage subsidies, cash transfers, and tax refunds distributed from July, which explain the concomitant spikes in non-withdrawer income. Second, withdrawals occurred over time in each round, with the calendar-based income and spending profiles aggregating potentially heterogeneous income and spending shocks across cohorts at different times relative to withdrawal.

Accordingly, we consider the effect of withdrawal on income and spending in an event study, with non-withdrawers serving as our comparison group (we consider identification later). We take the approach of Callaway and Sant'Anna (2021) and Sun and Abraham (2021), estimating average treatment effects on the treated (ATTs) *separately* for each cohort (those who withdrew in a given week) then averaging across cohorts weighted by size. This addresses several problems with the two-way fixed-effects estimator commonly used in event studies (de Chaisemartin and D'Haultfœuille, 2020; Callaway and Sant'Anna, 2021; Goodman-Bacon, 2021; Sun and Abraham, 2021; Borusyak, Jaravel and Spiess, 2022; Wooldridge, 2022).[32]

Specifically, in Figure 6, we display cohort-specific event-study plots reflecting

[28] https://www.rba.gov.au/publications/bulletin/2020/mar/pdf/consumer-payment-behaviour-in-australia.pdf

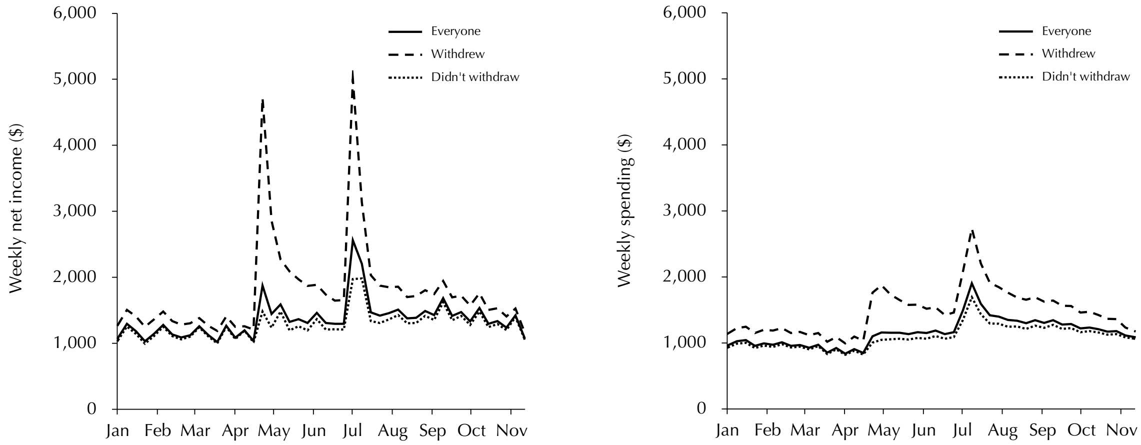
[29] https://www.atlantafed.org/banking-and-payments/consumer-payments/survey-of-consumer-payment-choice

[30] https://www.rba.gov.au/publications/bulletin/2020/jun/pdf/cash-use-in-australia-results-from-the-2019-consumer-payments-survey.pdf.

[31] We focus on the MPX rather than MPC because the bank transactions data cannot precisely be disaggregated into durables and non-durables. But we do observe spending categories likely to include only non-durables, which we consider later.

[32] Of concern are situations in which: different units are treated at different times; there is no 'never-treated' group; there are heterogeneous treatment effects across cohorts; or time-varying controls are used. For a review of these developments, see Roth, Sant'Anna, Bilinski and Poe (2022).

Figure 5: Average weekly income and spending in 2020 by group



(a) Income

Data: Illion

(b) Spending

Data: Illion

estimates for the following interacted TWFE model:

$$Z_{it} = \alpha_i + \lambda_t + \sum_{\ell \neq -1} \delta_{e\ell} \left(\mathbf{1}\{E_i = e\} \cdot D_{it}^{\ell} \right) + \varepsilon_{it},$$

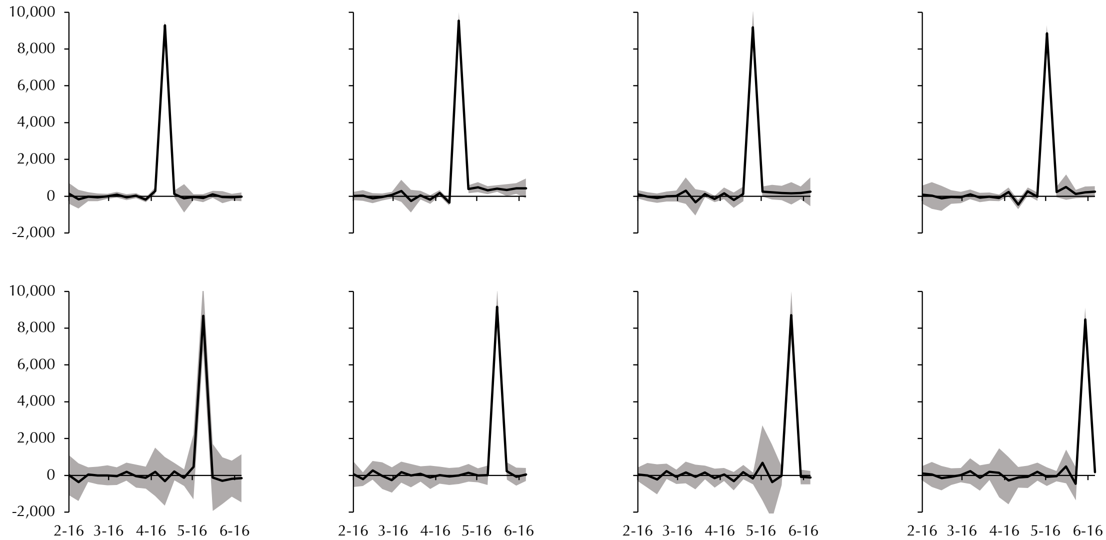
where Z_{it} is the outcome for unit i in time t (either income or spending), α_i is an individual fixed effect that controls for all time-invariant unit-level characteristics, λ_t is a time fixed effect for each week, ℓ refers to event time (periods relative to treatment), e refers to the cohort receiving the treatment at a given time, E_i refers to the time when unit i receives the treatment, and D_{it}^{ℓ} is a treatment dummy. Never-treated units are coded as $D_{it}^{\ell} = 0$ in all periods. The method computes, *for each cohort*, the average difference across treated and never-treated units between the outcome in the current period and that in the period immediately prior to treatment.

In Figure 7, we display event-study plots aggregated using cohort share weights, $\text{ATT}_\ell = \sum_e \delta_{e\ell} \cdot \Pr[E_i = e]$.[33] We then sum these aggregate ATTs across the post-treatment periods for which we observe a positive treatment effect on spending, which based on Figure 7b is eight weeks.[34] To construct the MPX, we need to divide this by the withdrawal amount. But because non-super-withdrawal income may vary post-treatment between the withdrawers and non-withdrawers, we divide by

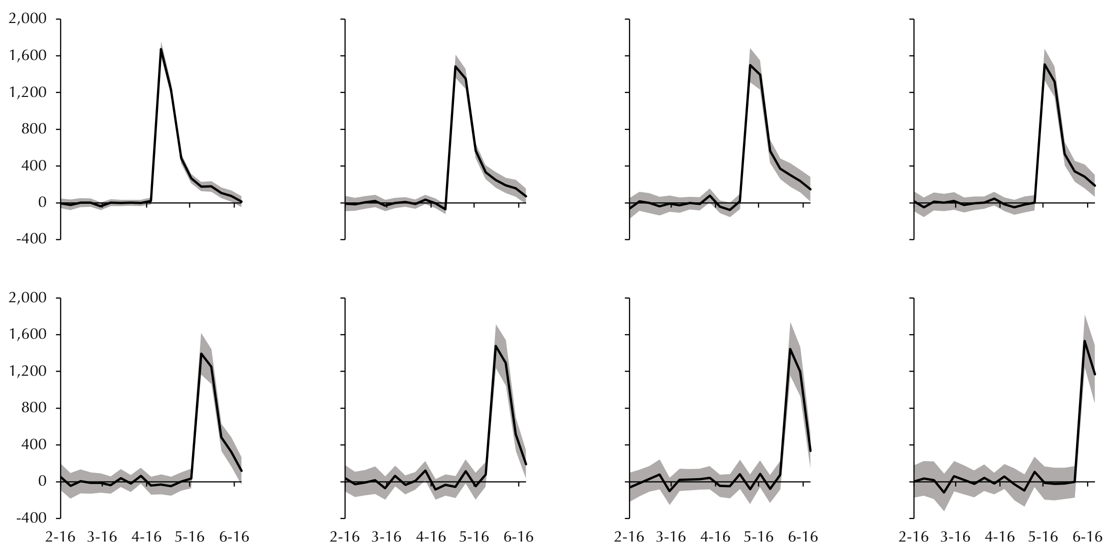
[33]The tight correlation between the TWFE and aggregate consumption paths following Super withdrawal (Figures 7 and 5), and our use of high-frequency consumption data, address concerns raised by Orchard, Ramey and Wieland (2023).

[34]Appendix H includes estimated weekly cohort ATTs for income and spending.

Figure 6: Estimated cohort ATTs ($) of the first withdrawal by calendar week



(a) Income

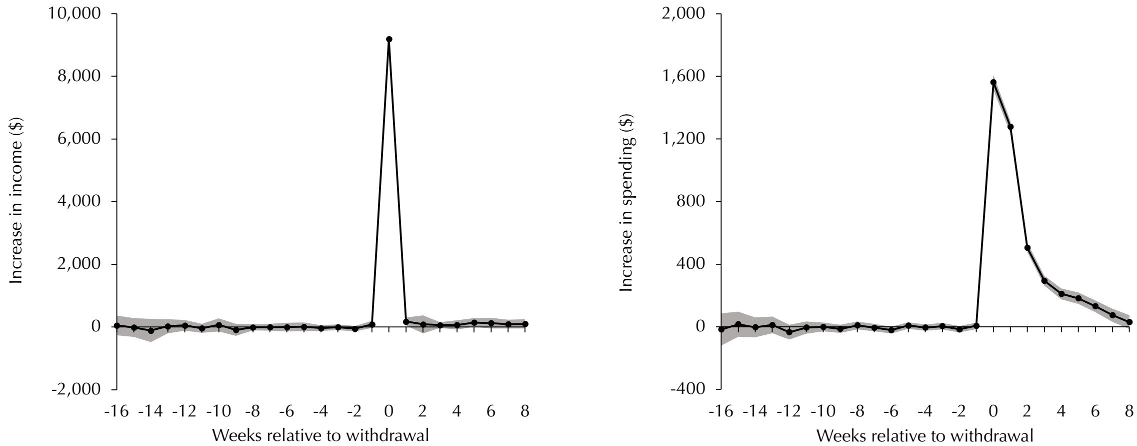


(b) Spending

Data: Illion

Notes: Results are cohort ATTs estimated via the R package, ‘did’, which implements Callaway and Sant’Anna (2021). Comparison group is the never-treated. Estimation is ‘doubly-robust’, with standard errors computed using the bootstrap procedure of Callaway and Sant’Anna (2021). Confidence intervals are at the 95% level. Cohorts 1 to 4 are displayed left-to-right in the top rows; cohorts 5 to 8 left-to-right in the bottom rows. Calendar time is truncated on the right to avoid contamination by the second withdrawals, beginning on July 1. The ‘did’ package, by default, uses a ‘varying’ base period when estimating cohort ATTs for the pre-treatment periods, where the base period is the period immediately prior. This is why estimates for the period immediately prior to withdrawal (and their confidence-interval estimates) are not zero. For all post-treatment estimates, the period immediately prior to treatment is used as the base period.

Figure 7: Estimated ATTs of the first withdrawal by event week



(a) Income

(b) Spending

Data: Illion
Notes: Results are averages of cohort ATTs in Figure 6 weighted by cohort size, estimated via the R package, 'did', which implements Callaway and Sant'Anna (2021). Comparison group is the never-treated. Estimation is 'doubly-robust', with standard errors computed using the bootstrap procedure of Callaway and Sant'Anna (2021). Confidence intervals are at the 95% level. The 'did' package, by default, uses a 'varying' base period when estimating cohort ATTs for the pre-treatment periods, where the base period is the period immediately prior. This is why estimates for the period immediately prior to withdrawal (and their confidence-interval estimates) are not zero. For all post-treatment estimates, the period immediately prior to treatment is used as the base period.

the income analogue of our spending estimate:

$$\text{MPX} = \frac{\sum_{\ell=0}^{7} \text{ATT}_{\ell}^{X}}{\sum_{\ell=0}^{7} \text{ATT}_{\ell}^{Y}} = \frac{\sum_{\ell=0}^{7} \sum_{e=1}^{8} \delta_{e\ell}^{X} \cdot \Pr\left[E_i = e\right]}{\sum_{\ell=0}^{7} \sum_{e=1}^{8} \delta_{e\ell}^{Y} \cdot \Pr\left[E_i = e\right]},$$

where X is spending and Y is income.[35] We estimate standard errors via bootstrap.

In Table 4, we display the resultant estimates. Our estimated MPX of 0.43 over eight weeks is high in the context of estimates of the effects of far smaller cash transfers—Leigh (2012), for example, estimates an almost-identical MPX of 0.41–0.42 out of \$950 stimulus checks distributed in Australia during the Global Financial Crisis. Because we exclude debt repayments and external transfers, our estimate should be seen as a conservative lower bound on the true spending impact. The income and spending shocks are also large relative to their pre-withdrawal levels. The first round raised income by 93% over eight weeks. In the month before

[35]While the 95% confidence interval for the estimated income ATT shown in Figure 7 contains zero, the weekly point estimates are modestly but consistently positive—evidently driven by a small but statistically significant and persistent increase in income for the second cohort, as can be seen in Figure 6a. Attributing all additional post-treatment spending to Super would upwardly bias our MPX estimate. But note that 94% of the total income increase over eight weeks is Super.

Table 4: Estimated cumulative aggregate ATTs

Outcome	First withdrawal	Second withdrawal
Income	9,343	10,314
	(294)	(274)
Spending	4,033	4,982
	(59)	(169)
MPX	0.43	0.48
	(0.01)	(0.01)
N	337,223	410,761

Data: Illion
Notes: Results are based on cohort ATTs estimated via the R package 'fixest' (estimates in Appendix H), which implements Sun and Abraham (2021). We compute weekly aggregate ATTs by averaging across cohorts weighted by cohort share and accumulate over the first eight post-treatment weeks. MPX is spending divided by income. All standard errors (in parentheses) are estimated via a standard bootstrap procedure.

withdrawal, the withdrawers spent $1,107 per week on average. Our estimates indicate spending was 129% higher over two weeks and 46% over eight weeks.

The sharpness of the spending impact, observable via our high-frequency bank transactions data, is notable. Leading studies of the spending impact of transfers have tended to focus on an annual or quarterly frequency due to data availability (e.g., Parker et al. (2013)). Broda and Parker (2014) show that two-thirds of non-durable expenditures driven by rebates in the US in 2009 occur within a month and Aladangady, Aron-Dine, Cashin, Dunn, Feiveson, Lengermann, Richard and Sahm (2023) show that Earned-Income Tax Credit recipients spend 30% of their tax refunds within two weeks. In our setting, given a transfer much larger than in these prior studies, we see an even sharper spending impulse: 39% within a week, 71% within two weeks, 83% within three weeks, and 90% within four weeks. By the eighth week, spending had returned to its pre-withdrawal level.

Observing spending out of the second withdrawals allows us to probe further the desire to spend, and to gauge the spending impact in a more plausibly externally valid context. By July, when second withdrawals began, public health restrictions had lifted and case numbers had been reduced to zero. This may address suspicions that the large observed spending impact of the first withdrawals was driven by panic or early pandemic-related spending. The estimated MPX for the second withdrawal was even higher at 0.48 with a similarly sharp spending profile (Appendix I).

4.2 Identification

It is necessary to make a parallel trends assumption in order for our estimated ATTs to identify causal effects: that, following withdrawal, average income and spending among the withdrawers would have evolved similarly to those among the non-withdrawers had the withdrawers not withdrawn. This assumption does not require randomization of withdrawal nor even balance of observed variables. There are two potential threats to identification under this design: that withdrawers' and non-withdrawers' income or spending were on different trends leading up to withdrawal, or that the income or spending of only the withdrawers or non-withdrawers was subject to a confounding effect at the time of withdrawal.

Under the TWFE specification, we are making a comparison between withdrawers and non-withdrawers conditional on all observed and unobserved time-invariant but group-specific and time-varying but common characteristics. Thus the withdrawers and non-withdrawers can have very different incomes or spending so long as these differences are stable around withdrawal, and their incomes or spending can be subject to pandemic-related shocks so long as they are common to the two groups. And recall that we have addressed bias due to differential concomitant income shocks by dividing by our estimated cumulative aggregate income ATTs.

In Figures 6 and 7, one can observe clearly the evolution of tightly parallel pre-trends in every cohort and all withdrawers overall, even without conditioning on additional time-varying covariates. There is also no evidence of anticipation by any cohort or overall, which supports identification but is also interesting. The ability to withdraw was announced a month before the first withdrawals and almost half of withdrawals occurred in the first 10 days. The absence of an anticipatory effect on spending is consistent with liquidity being a constraint on spending.

Next, in Figure 6, there does not appear to have been any post-withdrawal confounder that, in calendar time, applied only to the withdrawers or non-withdrawers. Moving between cohorts, the withdrawal date advances by a week, each income spike advances by a week, and each spending spike advances by a week. The dynamic paths of income and spending were similar across cohorts, with spending consistently elevated over the first two weeks then tapering down over the subsequent six weeks. This suggests against a confounding event on a given date.

There remains a potential for a confounding effect on either group at the time of withdrawal within each cohort (in event rather than calendar time). Reverse causality is one possibility; this is a form of selection bias, with never-treated units switching to the treated group at the time of treatment. For this to be a major concern, given the large share of the withdrawers in the population and the large treatment effect among that group, it would have resulted in a noticeable decline in spending among the non-withdrawers as those whose spending would have been elevated even in the absence of withdrawal selected out of non-withdrawal.

The aggregate spending in Figure 5 suggests against this. Not only did spending among non-withdrawers not decline upon withdrawal, but spending in aggregate across all units (uncontaminated by selection bias as all units are present at all times) rose at a rate greater than would be explained by withdrawals alone. This was also the case with the second withdrawals, addressing concerns this may have been due to early pandemic-related spending (e.g., panic buying or home office purchases). For additional robustness, in Appendix D we present the result of an event study on weekly wages, which one might not expect to be affected by withdrawal but may have been associated with selection, showing no concomitant effect.

Lastly, there is the question of external validity. It is worth reiterating that our estimates are average treatment effects on the *treated*. As shown earlier, the withdrawers differed markedly from the non-withdrawers; in particular, having had persistently poorer financial health. While this in and of itself does not threaten identification, to the extent it explains selection it may still have mediated the spending induced by the program, and thus have implications for the external validity of the estimates. This effect is *ex ante* theoretically ambiguous because liquidity-constrained withdrawers may have withdrawn to finance spending or to rebuild liquidity. *Ex post*, the high spending we observe is consistent with the former. But there is no suggestion our MPX estimates apply to the broader population.

4.3 Spending categories

In Figure 8, we present estimated MPXs for each observed spending category, defined based on the merchant name.[36] The largest share of spending was 'uncategorized'—in reality, this will have been spread across the other categories. ATM withdrawals constituted the other large category. Recall that Australia is mostly a cashless society,

[36] A corresponding table of results can be found in Appendix J.

with cash accounting for just 10% of consumer spending. In a 2019 survey, just 12% of those holding cash outside their wallet did so to save for a large purchase, which suggests cash withdrawals were not only likely predominantly used for spending but spending predominantly on non-durables.[37] Also, we include debt repayments for scale, though these are not included in our aggregate spending estimates.

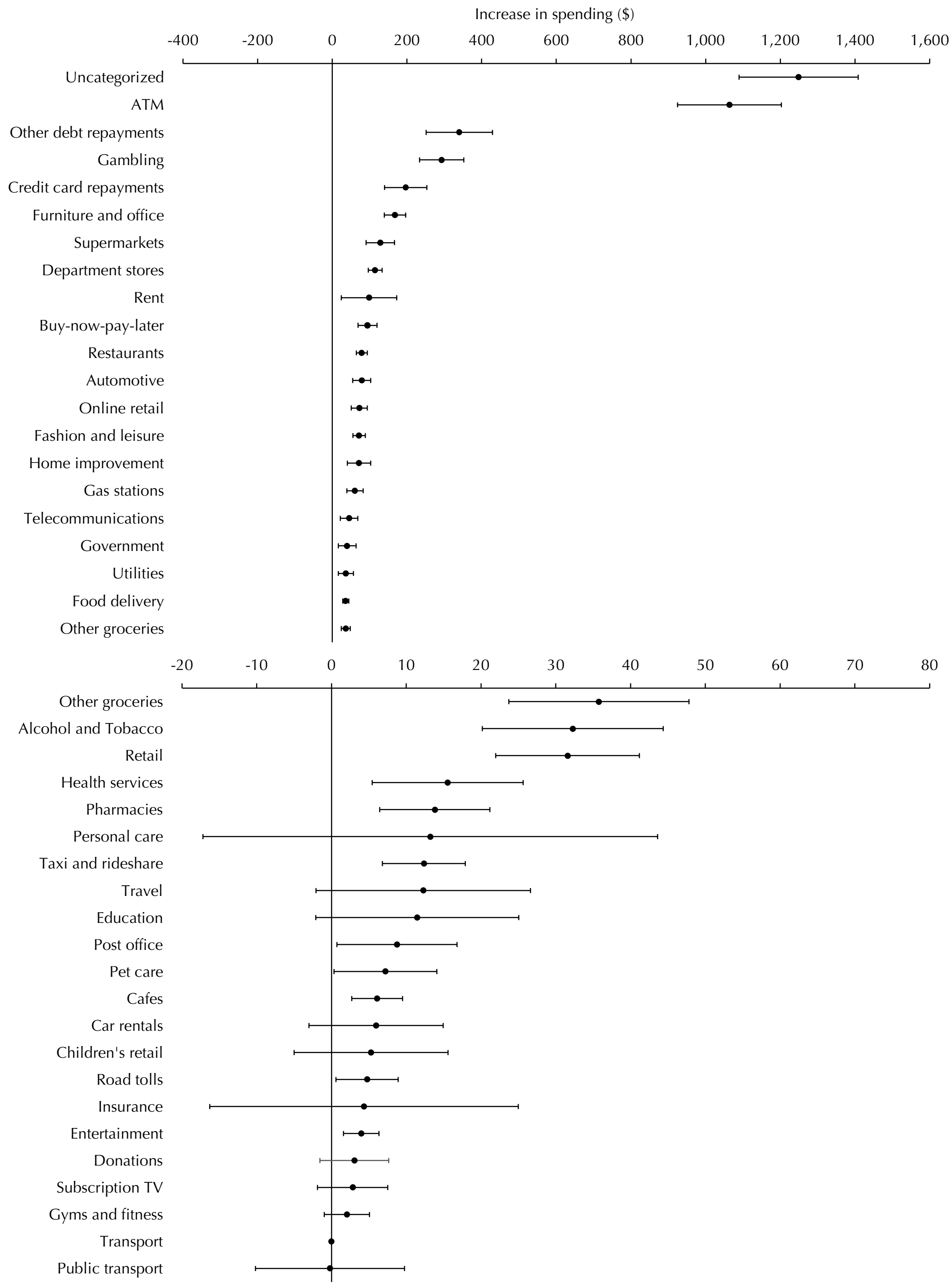
All other categories had spending impacts below $400 (or less than 5%). Spending was highly dispersed: of the 40 other discernible categories, 26 had spending impacts statistically significant at the 99% level, three at the 95% level, two at the 90% level, and only nine (personal care, car rentals, children's retail, insurance, donations, subscription TV, gyms and fitness, transport, and public transport) not statistically significant at the 90% level. Gambling was the third-largest discernible category.

We cannot distinguish perfectly between durables and non-durables, but many of the spending categories that had highly statistically significant spending impacts are clearly non-durables (e.g., food delivery and supermarkets); overall, categories that are clearly non-durables sum to 60% of the spending impact across all discernible categories.[38] This is conservative as the other categories (e.g., retail) will also have included spending on non-durables. If we assume the same share of non-durables spending within uncategorized and ATM withdrawals, then our estimates imply a lower bound on the marginal propensity to consume (MPC) out of the first withdrawal of 0.26 (0.29 for the second withdrawal).

[37] https://www.rba.gov.au/publications/bulletin/2020/jun/pdf/cash-use-in-australia-results-from-the-2019-consumer-payments-survey.pdf

[38] Clearly non-durable categories include gambling, supermarkets, rent, restaurants, gas stations, government, utilities, food delivery, other groceries, alcohol and tobacco, health services, pharmacies, personal care, taxi and rideshare, travel, education, post office, pet care, cafes, car rentals, road tolls, insurance, entertainment, donations, subscription TV, gyms and fitness, transport, public transport, totalling $1,039.26 out of $1,720.82 in total discernible spending.

Figure 8: Estimated cumulative ATTs of the first withdrawal by category



Data: Illion
Notes: Results are based on cohort ATTs estimated via the R package 'fixest', which implements Sun and Abraham (2021). We compute weekly aggregate ATTs by averaging across cohorts weighted by cohort share. We then accumulate over the first eight post-treatment weeks. The 'did' and 'fixest' statistical packages do not generate standard error estimates for cumulative ATTs. Moreover, when summing coefficients across time, one requires the variance-covariance matrix from the estimation procedure, and this is only generated for the cohort regressions rather than the weighted average aggregate regressions. To derive analytically standard errors for our spending category cumulative aggregate ATT estimates, we take the variance-covariance matrix from the cohort regression and exploit the fact that: 1) $\mathrm{Cov}\left(\sum_{i=1}^{m} a_i X_i, \sum_{j=1}^{n} b_j Y_j\right) = \sum_{i=1}^{m} \sum_{j=1}^{n} a_i b_j \mathrm{Cov}\left(X_i, Y_j\right)$ and 2) $\mathrm{SE}\left(X_i + X_j\right) = \sqrt{\mathrm{Var}\left(X_i\right) + \mathrm{Var}\left(X_j\right) + 2 \cdot \mathrm{Cov}\left(X_i, X_j\right)}$. Confidence intervals are at the 95% level.

5 Heterogeneity

5.1 MPX distribution

In our bank transactions data, we observe individual income and spending before and after withdrawal, allowing us to estimate individual-level treatment effects. Specifically, for each treated unit we compute the difference in average spending between the three weeks before and the three weeks after withdrawal, then divide by the withdrawal amount (Figure 9b).[39,40] As is evident in Figure 9b, these estimates are subject to error. But because we observe a pre-period for both the withdrawers and non-withdrawers, we can remove its time-invariant component specific to the withdrawers and its time-varying component common to both groups.

To isolate the time-invariant component, among the same individuals we repeat the exercise just described on a timeframe three weeks earlier, computing average spending in the three weeks before withdrawal, subtracting average spending in the three weeks before that, and then dividing by the withdrawal amount (Figure 9a). As expected, this is random noise centered near zero. To isolate the time-varying component, we compute the same two estimates among the non-withdrawers, but scaled by the average withdrawal amount among the withdrawers.

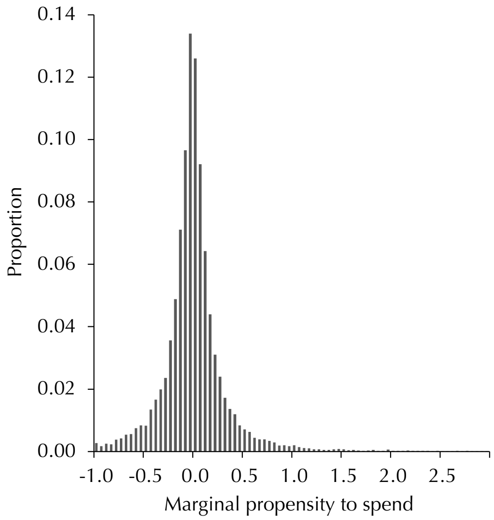
These spending differences then serve as the four quadrants in a two-by-two difference-in-differences setup, but applied to quantiles rather than the mean; i.e., quantile difference-in-differences (Athey and Imbens, 2006). This differs from the standard quantile regression approach taken in previous studies (Misra and Surico, 2014). In Figure 9c, we display our estimates by centile with 95% confidence intervals estimated via bootstrap. Each point is the upper bound of MPXs for a given share of withdrawers (e.g., half of the observations have an MPX of 0.51 or less).

This distribution has two notable features. First, for the lowest 95%, the distribution is near-uniform (the dashed 45-degree line). This part of the distribution is neither bimodal (with excess mass at zero and one) nor strongly right-skewed (with excess mass at zero). This near-uniformity is also present in the density estimated by Karger and Rajan (2021) for the US pandemic stimulus, though that density had

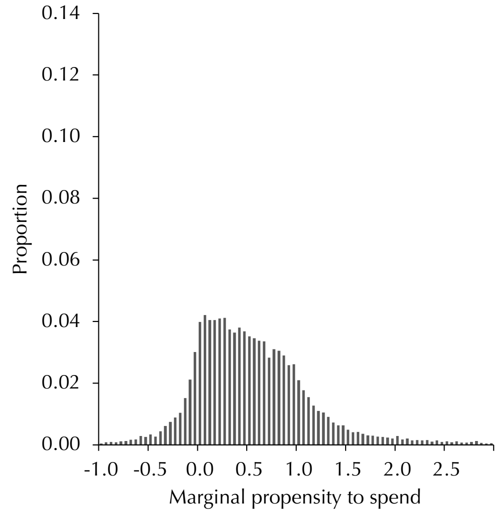
[39] This is similar to the approach taken by Karger and Rajan (2021) in studying the 2020 US stimulus program.

[40] Because the spending impact lasted eight weeks on average, this will underestimate the true treatment effect—though 83% of additional spending occurred in the first three weeks. Extending our individual-level estimates to eight weeks would require us to extend the pre-period, cutting the sample size and making it impossible to estimate a placebo-period distribution, given we observe each unit only for a 90-day window.

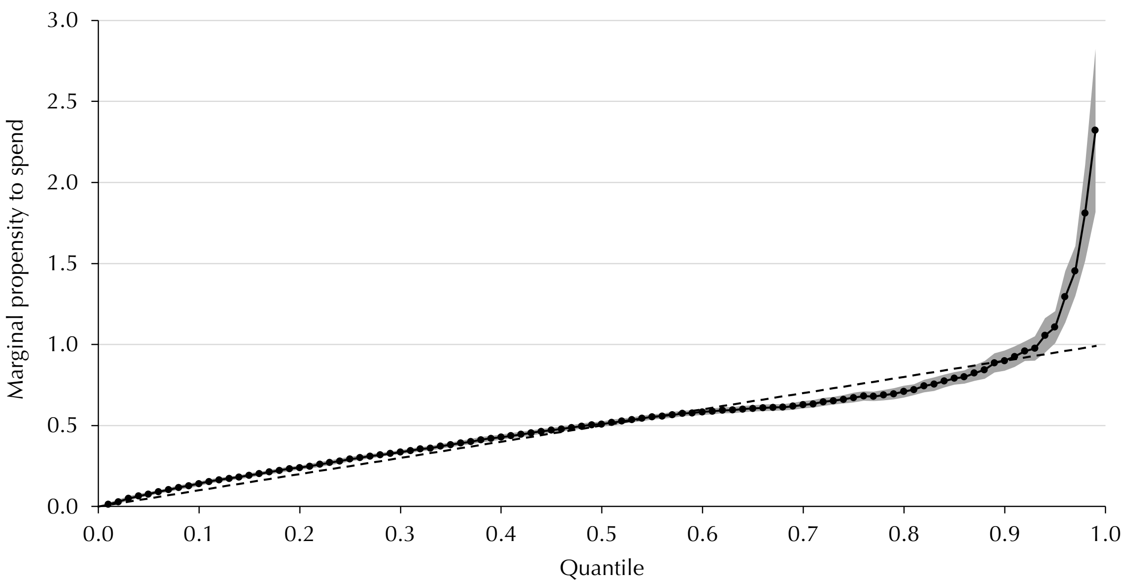
Figure 9: Estimated quantile treatment effects of withdrawal on spending among the treated



(a) Pre-treatment MPX distribution among treated



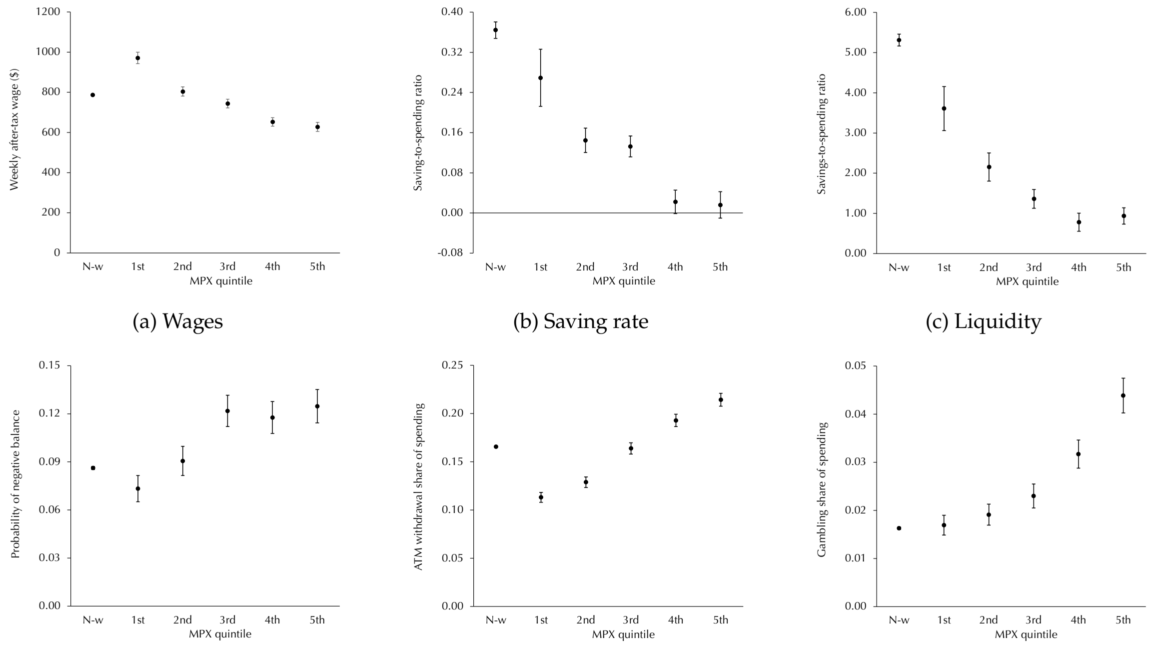
(b) Post-treatment MPX distribution among treated



(c) Quantile difference-in-differences of MPX

Data: Illion
Notes: Subfigures (a) and (b) display histograms (bin size 0.05) of individual-level MPX estimates via simple differences in spending across consecutive three-week periods among the withdrawers. Each unit's spending difference is divided by its eventual withdrawal amount to generate an individual-level MPX estimate. The treatment period (subfigure (b)) compares three weeks either side of withdrawal; the placebo period (subfigure (a)) compares three weeks prior to treatment and the three weeks prior to that. Subfigure (c) displays quantile difference-in-differences (qdid) estimates (by centile) with 95% confidence intervals estimated via bootstrap. The qdid estimates are constructed by computing the difference in the individual-level MPX at each quantile between the post-treatment period (subfigure (b)) and pre-treatment period (subfigure (a)) among those who withdrew, and subtracting the corresponding differences among those who did not withdraw. An MPX of 0.5 at a quantile of 0.5 indicates that the median of the individual-level MPX distribution rose 0.5 units more among those who withdrew than among those who did not. The dashed line indicates estimates under a uniform distribution.

Figure 10: Estimated pre-treatment means by estimated MPX quintile vs non-withdrawers



(a) Wages (b) Saving rate (c) Liquidity

(d) Overdrawn account (e) Cash withdrawals (f) Gambling

Data: Illion
Notes: Non-withdrawers on the left; recall they are roughly five times the withdrawers in number. Withdrawers are divided into quintiles based on the following MPXs: (-0.56, 0.09], (0.09, 0.34], (0.34, 0.63], (0.63, 0.97], (0.97, 2.7]. The 4% of withdrawers with MPXs outside this range are omitted. Outcomes are averages during the month prior to withdrawal. 95% confidence intervals shown.

a spike at zero. Second, the distribution has a long right tail, with 5% having an MPX above one (and the top 1% above 2.3). One possibility is that they faced lumpy expenditures with insufficient liquidity, and the withdrawal bridged that gap.

5.2 MPX predictors

In Section 3, we found that withdrawers had far worse financial health. Now we investigate how that covaries with their propensity to spend. In Figure 10, having split the withdrawer sample into MPX quintiles, we display estimated means of pre-treatment variables by quintile. For reference, we present on the left of each panel the mean among the non-withdrawers (a group five times larger).

Poorer pre-treatment financial health strongly predicts greater spending, consistent with the literature on substantially smaller transfers (Johnson et al., 2006; Broda and Parker, 2014; Kreiner et al., 2019). Wages vary modestly across the

MPX distribution, falling 35% between the first and fifth quintiles, with half this decline between the first two quintiles. The variation in financial health is more dramatic. Between the first and second MPX quintiles, the saving rate halves and liquidity almost halves. The 40% with MPXs exceeding 0.63 (around $6,000 of additional spending over eight weeks) were saving nothing and had savings at or below monthly spending. Even the 40% with MPXs between 0.09 and 0.63 had savings to cover no more than an additional month of spending, saving less than 15% of outlays. The 60% with MPXs of 0.34 or more were nearly twice as likely to have an overdrawn account than the 20% with MPXs of 0.09 or less.

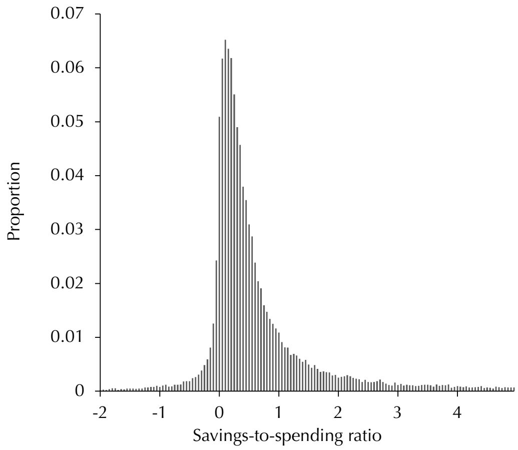
We also include two pre-treatment spending categories that were strongly related to the MPX: ATM withdrawals and gambling. As noted earlier, Australia is predominantly a cashless society, at least in formal markets. And gambling is more lightly regulated than in the US (online betting is legal, for example), with higher average losses.[41] Recall these two categories were the largest and third-largest discernible uses of withdrawn funds. Both ATM withdrawals and gambling prior to withdrawal were strongly predictive of MPX. Indeed, between the lowest and highest MPX quintiles, the rate of pre-treatment gambling more than doubled.

Comparing to the non-withdrawers is informative. There was a modest difference in wages between the withdrawers and non-withdrawers, but low spenders had higher wages than the non-withdrawers and high spenders had lower wages. This same pattern applied to cash withdrawals. Contrast this with the saving rate, liquidity, and gambling: for all three, the average for every MPX quintile was lower (higher in the case of gambling) than the average among the non-withdrawers. It was a similar story with the probability of being overdrawn, with all but the lowest MPX quintile having a higher probability than the non-withdrawers.

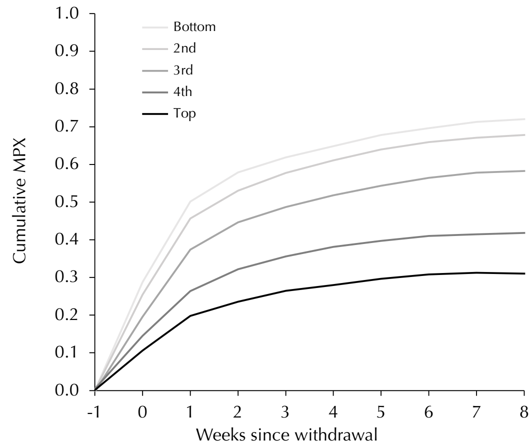
While there was variation among the withdrawers, they appear divisible: around 20% had MPXs below 0.09 and financial health that was similar to the non-withdrawers; the remaining 80% had MPXs above 0.09 and far worse financial health than the non-withdrawers. Various measures decline rapidly with the MPX and then plateau, with the top 60% homoegeneous along multiple dimensions.

[41] https://www.nytimes.com/2018/04/04/world/australia/australians-gambling-betting-machines.html

Figure 11: The relationship between liquidity and spending



(a) Liquidity histogram



(b) Spending by liquidity quintile

Data: Illion
Notes: Liquidity is savings divided by spending on average during the month prior to withdrawal. For each liquidity quintile, we estimate cohort ATTs via the R package, 'did', which implements Callaway and Sant'Anna (2021). We compute weekly aggregate ATTs by averaging across cohorts weighted by cohort share. We then accumulate these by week and divide by the average withdrawal amount in the relevant liquidity quintile.

5.3 Liquidity

To assess the relationship between financial health and the MPX with greater precision, we focus on the ratio of savings to spending (a measure of liquidity) just prior to withdrawal. In Figure 11a, we present the estimated density of liquidity among the withdrawers, the average of which (2.07) was substantially lower than that among the non-withdrawers (5.38). We then divide the liquidity distribution into quintiles and estimate our main difference-in-differences regression separately for each quintile. We then accumulate the resulting weekly spending impacts following withdrawal and divide by the average withdrawal amount in each quintile.

The results are displayed in Figure 11b, and indicate a substantial and monotonic increase in MPX, for every week post-treatment, as liquidity declines. The MPX among the bottom two liquidity quintiles is similar, with the least-liquid 40% having an MPX (0.68–0.72) more than double that among the most liquid 20% (0.31). This translates to additional spending of around $2,500 over eight weeks—equivalent to almost an entire month's post-tax wages for the former group.

6 Interpretation

6.1 Two-asset heterogeneous-agent model

In this section, we develop a theoretical framework to interpret the evidence quantitatively. The framework includes the following key ingredients: i) incomplete markets (partial equilibrium), income risk, and borrowing constraints (Huggett, 1993); ii) two assets, a liquid asset and an illiquid asset that is costly to adjust (Kaplan and Violante, 2014); and iii) naively present-biased households (Laibson et al., 2021).[42] We focus on non-durables as durables are difficult to discern in our data, and the non-durables MPX is closer to the theoretical MPC concept (Laibson, Maxted and Moll, 2022). We calibrate the model for the sub-population of withdrawers.

As in Kaplan et al. (2018), time is continuous and households maximize the present discounted value of utility:

$$\max_{\{c_t, d_t\}} \mathbb{E}_0 \int_0^\infty e^{\rho_t} u(c_t) dt, \tag{1}$$

subject to:

$$\begin{aligned}
\dot{b}_t &= (1-\xi)\, w_t z_t + \left(r^b + \phi \cdot \mathbf{1}(b<0)\right) b_t - d_t - \chi(d_t, a_t) - c_t \\
\dot{a}_t &= r^a a_t + \xi w_t z_t + d_t \\
a_t &\geqslant 0 \\
b_t &\geqslant \underline{b}
\end{aligned}$$

$$\chi(d,a) = -\chi_0 \cdot \min\{d,0\} + \frac{\chi_1}{2}\left(\frac{\min\{d,0\}}{a}\right)^2 a + \chi_2 \cdot \max\{d,0\} + \frac{\chi_3}{2}\left(\frac{\max\{d,0\}}{a}\right)^2 a,$$

where z_t is productivity following a Poisson process, c_t is consumption, b_t is liquid asset holdings, a_t is illiquid asset holdings, w_t is the wage, d_t is deposits into the liquid asset, r^b is the risk-free return on the liquid asset, ϕ is the wedge on liquid

[42] Alternative approaches include: Beraja and Zorzi (2023), in which durables consumption is lumpy and the MPC does not decrease substantially with transfer size; and Miranda-Pinto, Murphy, Walsh and Young (2023), in which non-durables consumption jumps following the liquidity shock for households that were unable to cover expenditures before withdrawal.

asset borrowing ($b < 0$), r^a is the risk-free return on the illiquid asset, ξ is the fraction of labor income deposited into the illiquid account at no cost (pension saving), $\underline{b}$ is the borrowing limit, $\chi(d, a)$ is the asymmetric cost of (voluntarily) adjusting illiquid asset holdings, and ρ is the discount rate adjusted by ζ, the death rate, given an average lifespan of 45 years (540 months).

Following Laibson et al. (2021), we accommodate present-biased households featuring quasi-hyperbolic discounting with a naive perception that their future selves will behave as rational households. In particular, for these households, we assume the following instantaneous gratification discounting function:

$$D(t) = \begin{cases} 1 & \text{if } t = 0 \\ \beta e^{-\rho t} & \text{if } t > 0 \end{cases}. \tag{2}$$

When $\beta = 1$, we recover the exponential discounting in Equation (1). As demonstrated by Laibson et al. (2021), the consumption policy rules for constant relative risk-aversion (CRRA) preferences are:

- for all $b > \underline{b}$, $\quad c(x) = \beta^{-\frac{1}{\gamma}} \hat{c}(x)$
- for $b = \underline{b}$, $\quad c(x) = \min \left\{ \beta^{-\frac{1}{\gamma}} \hat{c}(x), (1 - \xi)y + r\underline{b} \right\}$,

in which $\hat{c}(x)$ is the consumption policy function the naive household considers it will adopt in the future.

Present bias differs from impatience (high but exponential discounting) in several important ways. In particular, the portfolio choice of a present-biased household is more akin to that of a patient household. Intuitively, for a present-biased household, consumption decisions are *small* decisions on a flow variable, while illiquid assets' adjustments are lumpy and *large* decisions on a stock variable.

6.2 Calibration

In Table 5, we report the externally calibrated parameters in our model. We set the monthly interest rate on illiquid assets using the observed returns on Australian pension savings. The median return (net of fees and taxes) on Australian pension savings for the last 29 years is 8.3%, which implies an annual real return, net of average inflation (2.58%), of 5.72% (0.47% per month). The real return on liquid

Table 5: Externally calibrated parameters

Parameter	Description	Value	Source / Target
Preferences			
γ	Risk aversion	2	Standard
ζ	Death rate	$1/(45 \times 12)$	Avg. lifespan of 45 years
Assets			
$\underline{b}$	Borrowing limit	$-w/3$	Basic credit card limit Australia
ϖ	Interest rate wedge	0.75%	Reserve Bank of Australia
r^b	Liquid asset return	0.17%	Kaplan et al. (2018)
r^a	Illiquid asset return	0.47%	Australian Super
ξ	Share of income automatically deposited	10.5%	Australian regulation
χ_0	Adj. cost linear component withdrawals	1.1	Arbitrarily large (policy)
χ_1	Adj. cost convex component withdrawals	12	Arbitrarily large (policy)
χ_2	Adj. cost linear component deposits	0.002	Arbitrarily small (policy)
χ_3	Adj. cost convex component deposits	0.01	Arbitrarily small (policy)
Income process			
z_1, z_2	Income states	0.94, 1.06	Guvenen et al. (2023)
λ_1, λ_2	Income jumps	0.887	Guvenen et al. (2023)

wealth (2% per year) is from Kaplan et al. (2018). The monthly wedge on borrowing ϕ is 0.75%, which we obtain from the 14.18% interest on unsecured personal loans from the Reserve Bank of Australia, net of average inflation. The income process follows Achdou, Han, Lasry, Lions and Moll (2022), adapted to a monthly frequency. We discretize an AR(1) process, with annual persistence $\rho_z = 0.9$ and standard deviation $\sigma_z = 0.2$ (Guvenen, Kambourov, Kuruscu, Ocampo and Chen, 2023), into a monthly two-state Poisson process with income states $z_1 = 0.94$ and $z_2 = 1.06$, and a jump probability of 0.88. The borrowing limit $\underline{b}$ is one-third of monthly wages ($-w/3$), which corresponds to around a $2,000 credit card limit.

Our internal calibration entails finding the discount rate ρ and, where applicable, the present-bias parameter β that match the average MPC in the data. We obtain the average MPC out of pension withdrawals by multiplying the average MPX by our lower bound on the share of non-durables in the spending response, which is 60%. Because we are interested in the heterogeneity of spending responses, the empirical MPX we use is that from Figure 9c. In this case, the weighted average MPX equals 0.52, which implies a target average MPC of 0.31. We calculate the model's implied average MPC out of a two-wage windfall, as the first or second withdrawal.

Table 6: Calibration results

Moment	(1) Data	(2) Exponential benchmark	(3) Exponential impatient	(4) Present-biased
Preferences				
ρ	-	0.4%	17%	0.4%
β	-	1	1	0.58
Calibration target				
Average MPC	31%	2.9%	31.2%	31.3%
Additional moments				
Liquid assets to income	-	319%	1.9%	51.9%
Share at borrowing constraint	-	10.2%	94.4%	92.3%

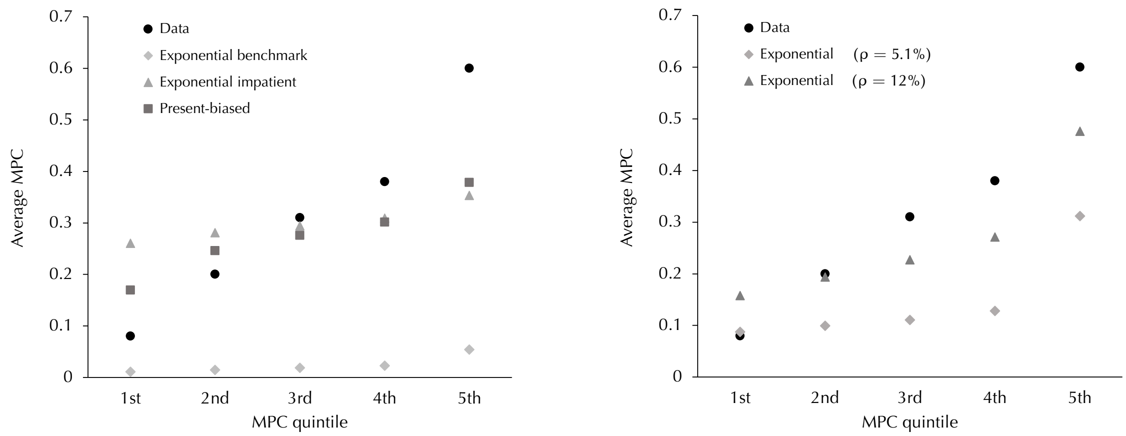
Note: In this table, we report the calibrated parameter values along with the model-implied moments for three versions of the model. In column (1), we report the average MPC out of the first withdrawal. In column (2), we show the parameter values and the moments from a model with exponential discounting and a standard calibrated value for the monthly subjective discount rate, ρ. In column (3), we provide the subjective discount rate that can match the average MPC in the data, assuming exponential discounting ($\beta = 1$). In column (4), we report the calibrated present-biased parameter, β, for a standard discount rate ρ, as in column (2).

In Table 6, we report moments for three versions of the model. In Column 2, we show the model-implied moments of a standard exponential calibration in which we assume $\rho = 0.4\%$, which is equivalent to a monthly subjective discount factor of $1/(1 + 0.4\% + \zeta) = 0.99$ and an annual discount factor of 0.93, as in Kaplan et al. (2018). This calibration delivers an average MPC, out of a two-wage windfall, of 2.9%, which is an order of magnitude smaller than that observed in the data. In Column 3, we allow the discount rate ρ to vary so as to match the observed average MPC. The implied calibrated discount rate is 17%, which is equivalent to an annual discount factor of 0.15, substantially below calibrated values in the literature.[43] In the final column, we report our naively present-biased calibration. Given a standard value of $\rho = 0.4\%$, the calibrated value of the present-bias parameter β is 0.58, which lies between the values obtained by Ganong and Noel (2019) ($\beta \in [0.5, 0.9]$) and Gerard and Naritomi (2021) ($\beta = 0.44$).

In the final two rows of Table 6, we report additional moments that shed light on the key differences between the models. First, the three models have very different implications for liquid asset holdings. While the standard exponential model delivers an excessive liquid-savings-to-income ratio (319%), the model with

[43] For instance, Aguiar, Bils and Boar (2020) calibrate a model with heterogeneous discount factors, and their lowest calibrated (annual) value for the discount factor is 0.72.

Figure 12: Calibrated average MPC by MPC quintile



(a) MPC heterogeneity by model (monthly freq.)

(b) Exponential discounting at annual frequency

Notes: On the vertical axis, we report the average MPC of the corresponding MPC quintiles in the horizontal axis. To obtain the average MPC, we use the weights from the stationary distribution of assets.

high impatience delivers too little savings (1.9%). The present-biased model has an average ratio of liquid assets to income of 51.9% which is indeed close to the one observed in the data.[44] In the final row, we report the share of households at the borrowing limit. The model displays shares of 10.2%, 94.4%, and 92.3% in the exponential benchmark, impatient, and the present-biased cases, respectively.

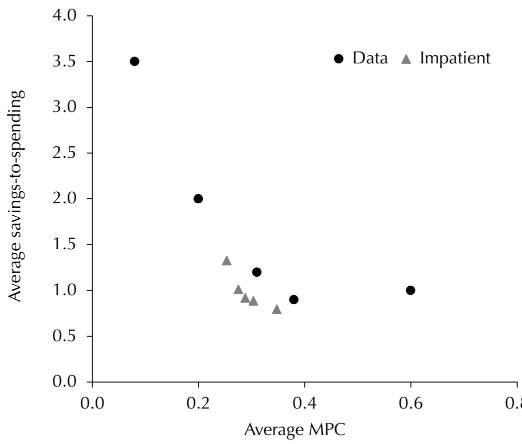
6.3 MPC heterogeneity

Next we study the fit of the models in terms of MPC heterogeneity and the empirical relationship between liquidity and spending responses. In Figure 12a, we display the average MPC by MPC quintile for each of the three models. One can see that the exponential benchmark delivers very limited heterogeneity in consumption responses (noted also by Laibson et al. (2021)). Even the top quintile, mainly composed of liquidity-constrained households, displays a small average MPC of 5.3%. On the other hand, while the model with extremely impatient households is able to match the average MPC, it fails to deliver sufficient MPC heterogeneity. The present-biased model provides the best fit to the data.

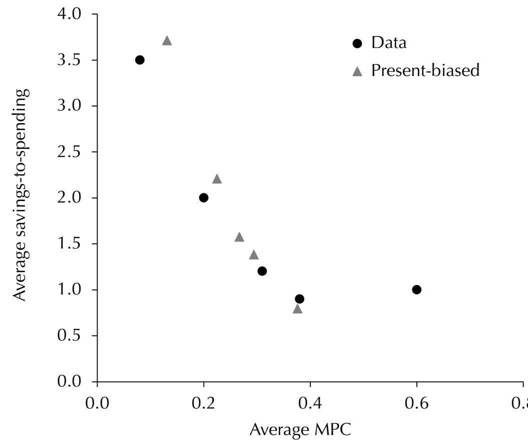
We now investigate the model-implied relationship between liquidity and

[44]We do not explicitly target the liquid asset holdings as in our data we do not have information on the stock of debt (only flow debt payments) or the stock of additional liquid savings (we only have information on checking and savings accounts).

Figure 13: Calibrated liquidity and average MPC by MPC quintile



(a) Exponential impatient



(b) Present-biased

Notes: The savings-to-spending ratio in the model is $(b+w)/c$, which compares to its empirical counterpart. On the vertical axis, we report the average liquidity for each MPC quintile. The average MPC is weighted using the stationary distribution of assets of the respective quintile.

average MPC by MPC quintile. In Figures 13a and 13b, we display results for the exponential-impatient and present-biased models, respectively. It is clear that the model with impatient households not only lacks MPC heterogeneity but also fails to match the heterogeneity in liquid asset holdings. On the other hand, the model with present-biased households is very close to replicating the observed relationship between liquidity and MPC. The one exception is the very highest MPC quintile, in which the observed MPC is 0.6 but the model can only deliver an MPC of 0.38. In evaluating the strongly right-skewed MPX distribution in Section 5, we posited that the top 5% having MPXs significantly exceeding one may be due to lumpy expenditures in the presence of liquidity constraints. Since our model does not include this feature, this lack of fit in the right tail is unsurprising.

6.4 Importance of high-frequency data

A key insight of the calibration exercise is the importance of high-frequency (at least monthly) data. To draw this out, we ignore the high-frequency spending patterns and calibrate our model with aggregated annualized data. We thus consider both withdrawals, which for the modal withdrawer represented $20,000 (in the order of half annual wages). In Figure 12b, we report the average MPC across the MPC distribution. We plot the results for a standard discount factor value (0.93 annual) and for a value that matches the average MPC (0.31) in the data (0.87 annual). At the annual frequency, a two-asset heterogeneous-agent model with borrowing constraints is able to explain the observed consumption patterns.

However, such a calibration is misleading, for a few reasons. First, the majority of the spending increase occurred within just the first two weeks after withdrawal. Thus, a calibration that targets the annual MPC implicitly imposes a counterfactually "smoother" spending impulse. Second, at the annual frequency, the relative size of the liquidity injection is four times smaller than at the monthly frequency. While the two pension withdrawals represent a liquidity injection of half average annual wage, the first (or second) withdrawal represents more than two months of wages. This is an important consideration as the spending responses in the models considered here decrease considerably with the size of the transfer. Third, the discount factor at the annual frequency is significantly smaller than the discount factor at the monthly frequency, implying a higher degree of impatience.

7 Conclusion

The Super withdrawal program gave people a seemingly one-off chance to withdraw $20,000 from their retirement accounts before retirement at an expected cost to their balance at retirement of perhaps $120,000 in today's dollars. Those who chose to withdraw were strongly selected—younger, with very little assets, very low rates of saving, more likely to be in blue-collar occupations and to live in rural or remote areas, with much greater spending in cash and on gambling. And, overwhelmingly, they did not use their withdrawals to repair their financial circumstances—indeed, the worse their circumstances, the less likely they were to do so. The magnitude and frequency of their spending is inconsistent with rational, forward-looking behavior.

A natural question is *why* their behavior was so strongly time-inconsistent. The clinical psychology literature suggests a link between gambling and impulsivity, implying a link between impulsivity and present bias in our setting (Maclaren, Fugelsang, Harrigan and Dixon, 2011). The survey evidence of Parker (2017), that the consumption response is associated with sophistication and planning, and impatience, is consistent with this, and implies a role for better financial literacy (Lusardi and Mitchell, 2014; van Rooji, Lusardi and Alessie, 2012). Given the broad swath of the population—roughly one in four 34-year-olds—that withdrew, our evidence suggests a high lower bound on the number of present-biased people in the population. By withdrawing, participants have revealed their type (a 'tag' as per Akerlof (1978)), which facilitates linking to administrative data so that we may learn more about the present-biased in future research.

To the extent the Super withdrawal program overcame fiscal constraints, it may have improved macro-stability, improving welfare. The present-biased will pay in sub-optimal future consumption—but this is also true of cash stimulus, which can be saved rather than spent. By connecting directly the individual-level costs and benefits of using cash transfers to stabilize the economy (as opposed to an indirect connection via higher future taxes), the program highlights a quirk of cash stimulus generally. The greater the present bias, the more effective it is (Laibson et al., 2021). But those induced to spend make themselves worse off (and others better off (Auclert, Rognlie and Straub, 2023)), at least directly, by not saving to offset future taxes. The net impact depends on the properties of the tax and transfer system. The application of a comprehensive welfare frame to stimulus under present bias is critical to designing optimal macro-stabilization policy (Maxted, 2022). Channelling Tobin (1977): just how many Harberger triangles does it take to fill an Okun's gap?

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Appendices

A Detailed description of the Super system

Australia has a compulsory, defined-contribution private retirement saving system, called 'superannuation'.[45] Under the system, all employers are required to contribute an additional 10.5% (rising to 12% by 2026) of the pre-tax wages (including bonuses but excluding overtime) of their employees to a private pension account.[46] The median balance by age in 2019 is shown in Table A1.[47]

Compulsory contributions are made by employers and subject neither to personal nor to corporate income tax. They are taxed at a flat rate of 15% when they enter the fund (compared to a modal marginal income tax rate of 34.5% and a top rate of 47%). People can contribute voluntarily up to an additional $27,500 per year pre-tax and $100,000 post-tax (both also attracting the standard 15% tax on entry). Before the retirement phase, cash returns on Super are taxed at 15% and capital gains at 10%, levied on the fund. Withdrawal is prohibited until age 58 if retired (rising to 60 by 2024) or 65 if still working, and is tax-free.[48] All returns generated during the retirement phase are untaxed.

Super is not paid on welfare payments, including paid parental leave. Sole traders do not have to make Super contributions for their own earnings. Historically, Super payments were compulsory for all workers with gross earnings of at least $450 per month, with this minimum recently removed. Super is paid on "ordinary time earnings", which is the gross amount the employee earns for their ordinary hours of work (before tax). It includes commissions, allowances, and bonuses, but excludes overtime.

For almost everyone, Super is highly tax advantaged relative to other forms of saving,[49] at the cost of being perfectly illiquid until retirement. Super is the only form of pre-tax saving, and cash returns to all other forms of savings are taxed at

[45] All details in this subsection can be found on the website of Australia's tax authority: `https://www.ato.gov.au`.

[46] On all wage earnings up to $240,880 per year.

[47] Because Super was introduced in 1983 and the mandatory contribution amount has increased over time, the median balance in the cross section increases less with age than the median currently young person can expect their own balance to increase over their working life.

[48] There are limited circumstances in which working-age people can access their Super early. See: `https://www.ato.gov.au/individuals/super/withdrawing-and-using-your-super/early-access-to-your-super/`

[49] The one exception is the primary residence, which is untaxed (but mortgage interest on the primary residence is not tax-deductible) and does not affect eligibility for the public pension (described later).

Table A1: Median Super balance by age

Age	Median balance ($)
20	3,264
30	34,908
40	82,208
50	124,146
65	207,071

Data: Australian Taxation Office
Note: Balances as at June 30, 2019.

full marginal personal rates (more than two times the Super tax rate for the median taxpayer and more than three times for those in the top 3.6%). This includes interest, dividends, and rental income from investment properties. All capital gains (held for more than a year and excluding the primary residence) are taxed at half the marginal income tax rate (17.25% for the median individual and 23.5% for those in the top 3.6%) upon realization. Australia does not have a step-up in basis for inherited assets.[50]

Employers allocate employees to a default fund, but employees can instead nominate an alternative fund, and recent reforms have encouraged competition between and consolidation across funds. Within their nominated Super fund, account holders can typically choose an investment strategy with a particular risk–return trade-off. Super funds invest in listed assets but also make direct investments in unlisted assets like infrastructure. People can also manage their own Superannuation savings via a 'self-managed Super fund', with around 3% of all Super account holders doing so.

There are $3.5 trillion (160% of GDP) in total Super assets, constituting one of the largest private pension pools in the world.[51] Over the past decade, the median 'growth' fund has achieved an average annual return of 9.5% after taxes and fees. Over the 29 years the Super system has existed, the average annual return has been

[50]Other than the primary residence of the deceased, which remains exempt from capital-gains tax provided it is sold within two years.

[51]https://www.oecd.org/daf/fin/private-pensions/Pension-Funds-in-Figures-2021.pdf

8.3%, with four years posting losses (the largest of which was 21.5% in 2008).[52] There are 128 Super funds, many of which were initiated by the labor movement ('industry' funds), which agitated for the establishment of the Super system.[53] In Australia, the Superannuation system is strongly affiliated with the centre-left Australian Labor Party, which introduced the system. The centre-right Liberal Party government, which introduced the early Super release program, would later propose that first-home buyers be able to access Super for a home deposit. This was strongly opposed by the then-Labor-Opposition on the basis that early access undermines the Super system.

Alongside Super, Australia also has a public pension system paying up to around $900 per fortnight to those aged 67 or older, a rate which is means-tested on the basis of current income and assets (including Super but excluding the primary residence). For those who own their own home and with assets of more than $280,000 ($419,000 for couples), every $1,000 in additional Super reduces the pension for which they are eligible by $3 per fortnight. This means those with assets of more than $609,250 ($915,500 for couples) are ineligible to receive a public pension.

[52] https://www.superguide.com.au/comparing-super-funds/super-funds-returns-financial-year

[53] See Mees (2017) for a history of the establishment of the Superannuation system.

B Eligibility

In Section 3, we compared those who withdrew with those who did not without regard for the eligibility conditions of the program. While the tax authority administering the program appears not to have engaged in any systematic compliance or enforcement activity, *ex ante* participants may have expected it to do so, or felt a moral obligation to conform to the rules. The application process generated very little friction, requiring the applicant to fill out a short online form but not requiring them to substantiate their eligibility in any way. The funds were then deposited into the applicant's bank account in just a few days.

During the application process, applicants had to nominate a criterion according to which they were eligible. In the first withdrawal, 48% stated a reduction in working hours, 19% each being unemployed and being eligible to receive a government benefit (government payments go to more than a quarter of the population, akin to the US Child Tax Credit), 9% being a sole trader shut down or with a reduction in turnover, 3% having been made redundant, and 2% being a visa holder facing hardship. We do not observe a withdrawer's true eligibility, nor whether they believed they were eligible. But because we observe weekly wage earnings and welfare payments, we can construct a proxy for eligibility which would seem to reasonably approximate the three largest and fifth-largest categories, covering 89% of applicants according to their self-nominated reason for eligibility. Consistent with the eligibility conditions listed in Appendix A, we define someone as eligible if they experienced a 20% decline in weekly wages or received a welfare payment between January 8 and June 25.

According to this measure, 70% of the population were eligible to withdraw Super.[54] While 17% of the working-age population withdrew Super, 20% of those eligible withdrew, and 11% of those ineligible withdrew. This suggests 18% of those who withdrew were ineligible—or, put differently, the compliance rate was 82%. One potential limitation is that we don't observe hours, only earnings. It is possible, however unlikely, that an observed reduction in wages is due to a reduction in the wage rate rather than hours, upon which eligibility was contingent. But while that

[54]An Australian National Audit Office audit of Australian Taxation Office (ATO) program administration notes that by mid-June 2020 the ATO was able to reach a high degree of confidence with respect to the eligibility of around 70% of applicants. By end-July the ATO had assessed that around 90% were eligibility to apply, and under 0.02% of applications were affected by fraud. See: `https://www.anao.gov.au/work/performance-audit/the-australian-taxation-office-management-risks-related-to-the-rapid-implementation-covid-19`.

means our measure could exclude some who are eligible, it also means it could include some who are ineligible. We also don't observe business turnover or visa status, which may have triggered eligibility for sole traders and foreign workers, respectively. But if we exclude the 11% claiming sole-trader or visa eligibility, the compliance rate is a little higher at 86%.

The main concern about eligibility is that it might explain the observed differences between those who withdrew and those who did not. For example, welfare recipients may both have lower liquidity than non-welfare-recipients and be overrepresented among the withdrawers not by choice but by eligibility. To assess the effect of eligibility, we recompute Table 1 but among the eligible (Table A2). Note that we are unable to construct our eligibility proxy in the bank transactions data, so can only reproduce the items relying on our administrative data. While there are differences in levels because the eligible differ from the broader population, all of the same patterns observed in the general population are present among the eligible. There are negligible differences in demographics. The eligible non-withdrawers have higher wages and rental incomes and lower interest incomes, dividends, and voluntary Super contributions than the non-eligible non-withdrawers. The eligible withdrawers have higher wages and slightly lower interest incomes, rental incomes, dividends, and voluntary Super contributions than the non-eligible withdrawers, and almost identical Super balances. But, critically, eligibility does not affect the relationship between liquidity and withdrawal—all of the differences have the same directions and similar magnitudes.

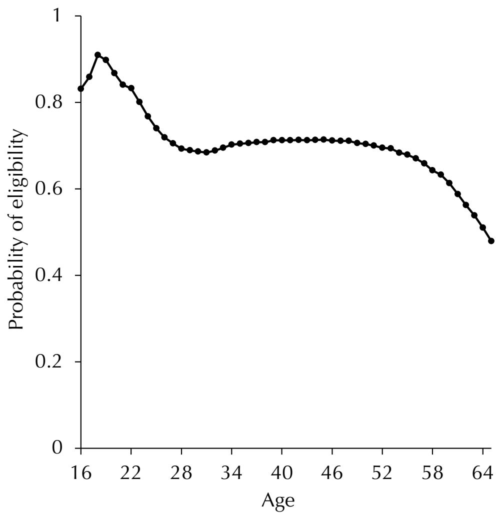
The relationship between age and withdrawal is only mildly affected by eligibility. In Figure A1a, the probability of eligibility is around 70% from the mid 20s to late 50s. Eligibility is significantly higher for those in their late teens and early 20s due to a greater probability of having a reduction in working hours or being in receipt of a government benefit (due to the prevalence of the Youth Allowance payment). As shown in Figure A1b, this translates into a probability of withdrawal among the eligible that has a very similar profile.

Table A2: Estimated differences in means between eligible withdrawers and non-withdrawers for first withdrawal

	Non-withdrawer	Withdrawer (difference)			
Controls	None	None	Wages	Plus age	Plus all
Demographics					
Age	40.09	-2.16	-1.67		
	(0.00)	(0.01)	(0.01)		
Female	0.50	-0.06	-0.09	-0.08	
	(0.00)	(0.00)	(0.00)	(0.00)	
Had spouse	0.54	-0.11	-0.09	-0.07	
	(0.00)	(0.00)	(0.00)	(0.00)	
Had kids	0.38	0.07	0.09	0.10	
	(0.00)	(0.00)	(0.00)	(0.00)	
Long-term financials					
Wages	57,973	-11,051			
	(18)	(40)			
Super balance	117,658	-57,560	-39,575	-31,669	-30,830
	(65)	(145)	(130)	(120)	(121)
Interest income	343	-253	-219	-203	-185
	(1)	(2)	(2)	(2)	(2)
Rental income	1,014	-461	-239	-213	-190
	(1)	(3)	(3)	(3)	(3)
Dividends	801	-632	-468	-418	-394
	(3)	(8)	(8)	(8)	(8)
Voluntary Super	1,807	-1,569	-1,412	-1,221	-1,109
	(4)	(9)	(9)	(9)	(9)
N = 10,675,214					

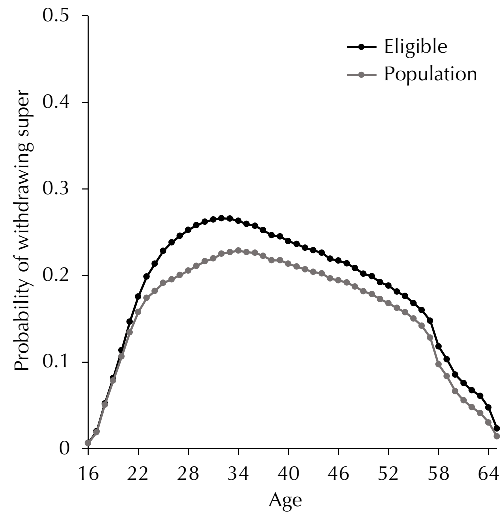
Data: Australian Taxation Office.
Notes: Results are from simple linear regressions of outcomes on a binary first-withdrawal indicator, controlling cumulatively for the 'Demographics' variables, and only among those eligible according to our proxy. Wage control for Demographics and Long-term Financials is average pre-tax wage income in the prior three years. Demographics are from the tax return in the financial year prior to withdrawal (July 1, 2018–June 30, 2019). Long-term financials except Super Balance and Voluntary Super are averages across the three prior tax returns (2016–17, 2017–18, and 2018–19). Super balance is as at June 30, 2019. Voluntary Super contributions are for the prior year (2018–19). Short-term wages are from Single-Touch Payroll records and cover average pre-tax wages in the month before withdrawal.

Figure A1: The relationship between eligibility and age



(a) Probability of eligibility conditional on age

Data: Ilion
Notes: First withdrawal. Individual-level MPXs estimated via simple difference in spending three weeks before and after withdrawal. Bin size is 0.05.

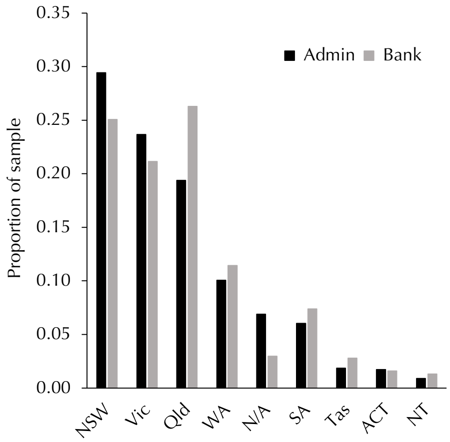


(b) Probability of withdrawal conditional on age

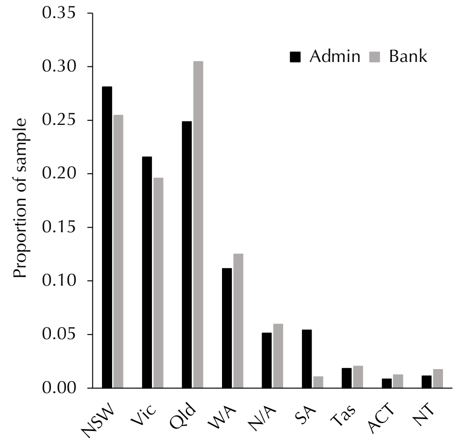
Data: Ilion
Notes: First withdrawal. Individual-level MPXs estimated via simple difference in spending three weeks before and after withdrawal. Bin size is 0.05.

C Data comparison

Figure A2: State distribution by sample



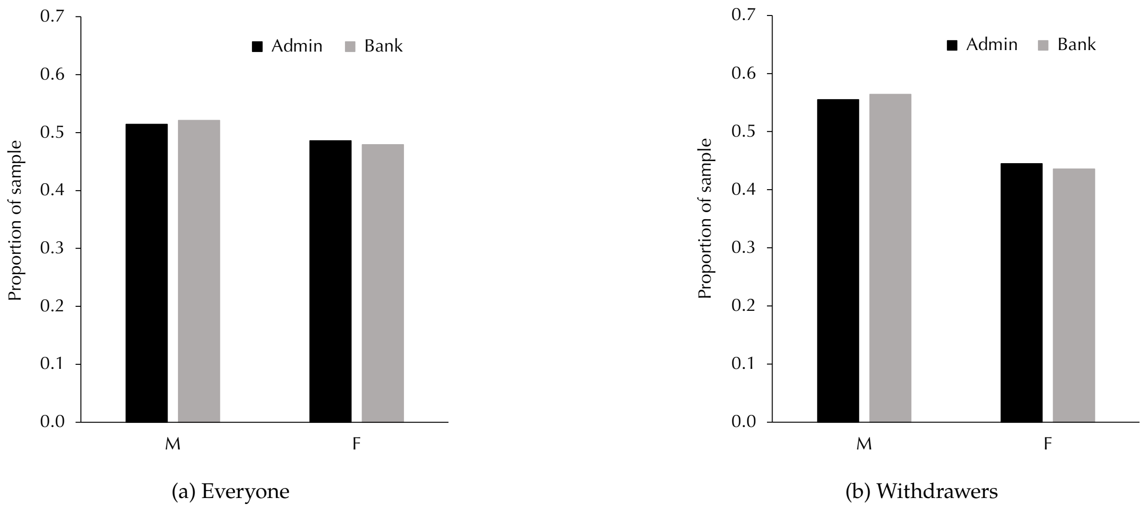
(a) Everyone



(b) Withdrawers

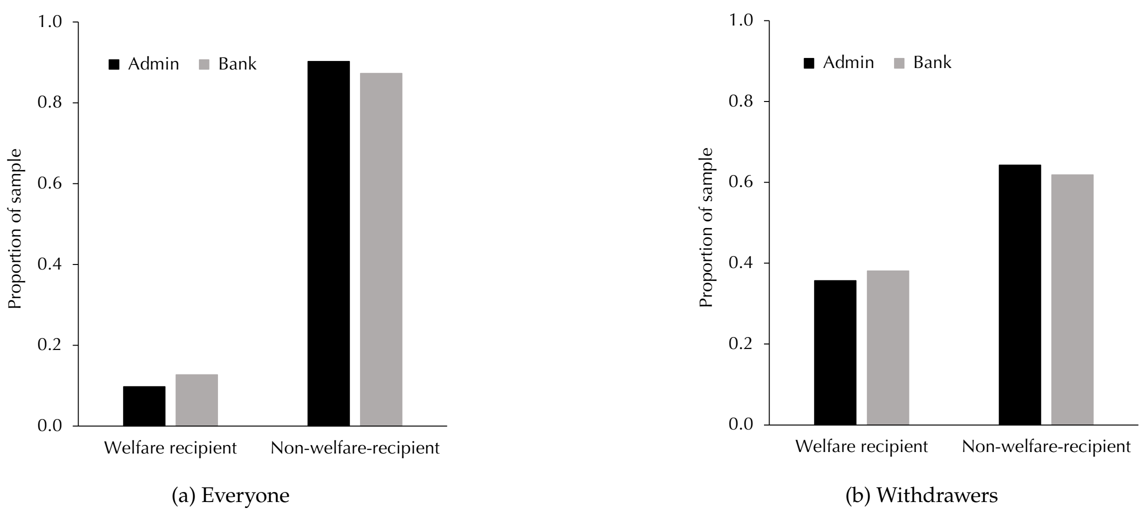
Data: Australian Bureau of Statistics and Illion
Note: Illion data are predicted based on transactions. Among the entire sample, location distributions are broadly similar, with Queensland overrepresented. Among the withdrawers, the two samples are more closely matched given Queensland is overrepresented among the withdrawers in the population.

Figure A3: Sex distribution by sample



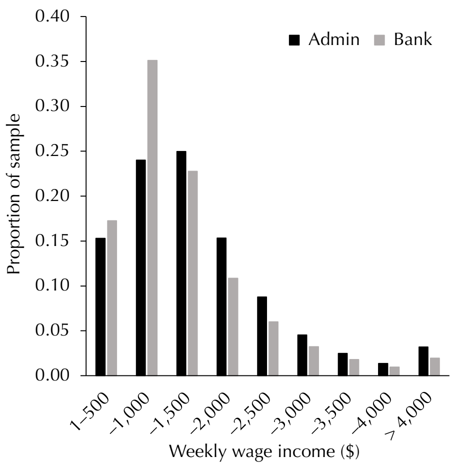
Data: Australian Bureau of Statistics and Illion
Note: Illion data are predicted based on transactions. Among the entire sample, the sex shares are similar (the self-employed, who are more likely to be male, are less likely to have Super and thus not to be in the population). Among the withdrawers, the shares continue to be similar despite the skewing towards men in the population.

Figure A4: Welfare receipt distribution by sample

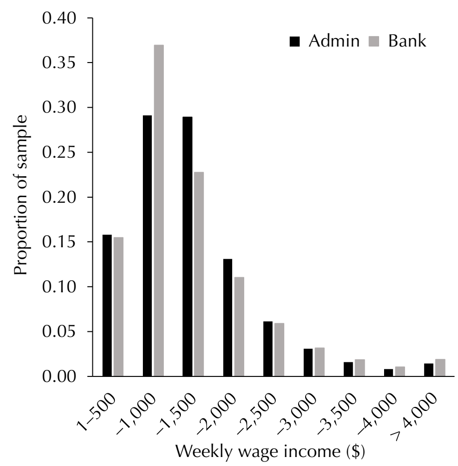


Data: Department of Social Services and Illion
Note: Welfare receipt defined as having been observed receiving a 'JobSeeker' payment. Among the entire sample, the welfare shares are similar, with a slight overrepesentation in the Illion data. Among the withdrawers, the shares are more similar despite the skewing towards welfare recipients in the population.

Figure A5: Wage distribution by sample



(a) Everyone

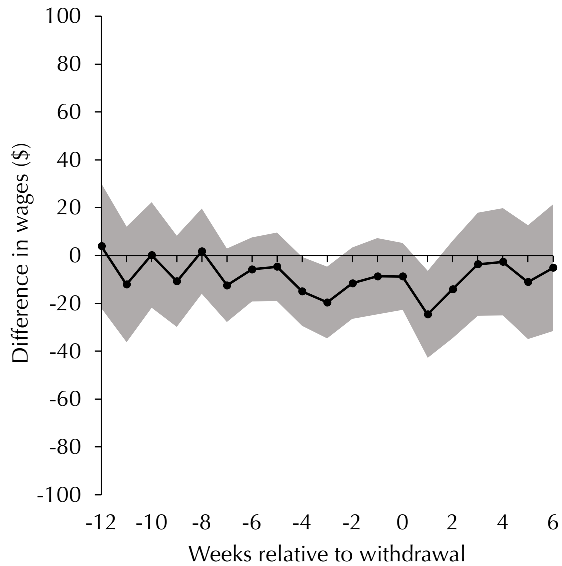


(b) Withdrawers

Data: Australian Taxation Office and Illion
Note: Average weekly wages in March 2020. ATO wages are pre-tax and Illion wages are post-tax, so the Bank wage distribution is shifted left by at least 18% for all weekly wage amounts in excess of $350 (tax-free threshold). Among the entire sample, the Bank data are less right-skewed, being more concentrated around the mode. Because this is true for the withdrawers in the population, the withdrawer distributions are quite closely matched.

D Wage event study

Figure A6: Relationship between wages and withdrawal timing

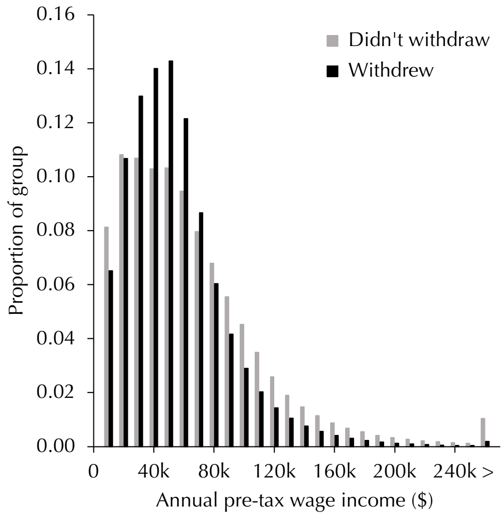


Data: Australian Taxation Office

Notes: Results are averages of cohort ATTs in Figure 6 weighted by cohort size, estimated via the R package, 'did', which implements Callaway and Sant'Anna (2021). Comparison group is the never-treated. Estimation is 'doubly-robust', with standard errors computed using the bootstrap procedure of Callaway and Sant'Anna (2021). Confidence intervals are at the 95% level.

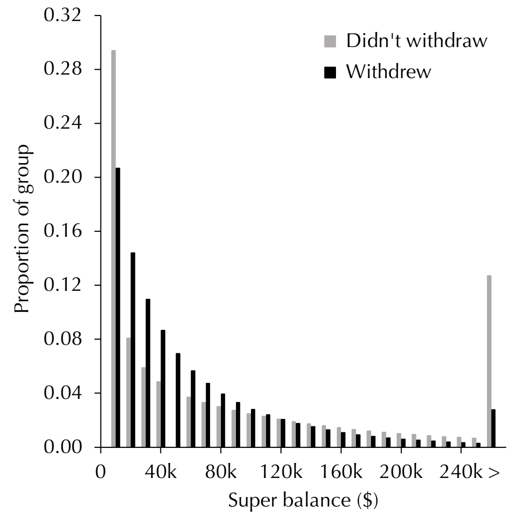
E Wage and Super balance densities

Figure A7: Histograms of Super and wages by withdrawal status



(a) Wage histogram by withdrawal status

Data: Australian Taxation Office
Notes: Wage income is average of the prior three financial years (2016–17, 2017–18, and 2018–19). Bin size is $10k, first bin includes $0, top bin is >$250k.



(b) Super histogram by withdrawal status

Data: Australian Taxation Office
Notes: Super balance is as at June 30, 2019. Bin size is $10k, first bin includes $0, top bin is >$250k.

F Withdrawal rate by occupation

Table A3: Withdrawal rate by occupation

Occupation	Withdrew (%)
Construction and Mining Labourers	40.2
Mobile Plant Operators	36.7
Factory Process Workers	35.0
Machine and Stationary Plant Operators	32.9
Other Labourers	32.6
Food Trades Workers	32.4
Storepersons	32.0
Road and Rail Drivers	30.1
Construction Trades Workers	29.0
Cleaners and Laundry Workers	27.2
Automative and Engineering Traders Workers	26.4
Other Technicians and Trades Workers	26.2
Hospitality, Retail and Service Managers	25.3
Health and Welfare Support Workers	24.7
Sports and Personal Service Workers	24.6
Hospitality Workers	23.7
Carers and Aides	23.1
Farm Forestry and Garden Workers	22.4
Sales Representative and Agents	22.3
Skilled Animal, Agricultural and Horticultural Workers	21.4
Inquiry Clerks and Receptionists	20.4
Sales Support Workers	19.9
Sales Assistants and Salespersons	18.9
Food Preparation Assistant	18.7
Other Clerical and Administrative Workers	18.5
Electrotechnology and Telecommunications Traders Workers	17.8
Outside Labour Force	17.6
Clerical and Office Support Workers	17.0
Protective Service Workers	16.7
Farmers and Farm Managers	16.6
Engineer, ICT and Science technicians	16.0
Arts and Media Professionals	14.5
Numerical Clerks	14.4
Chief Executives, General Managers and Legislators	14.2
General Clerical Workers	14.1
Office Managers and Program Administrators	13.9
Specialist Managers	13.4
Personal Assistants and Secretaries	13.2
Business, HR and Marketing Professionals	11.8
Health Professionals	10.5
Legal, Social and Welfare Professionals	9.4
Design, Engineering, Science and Transport Professionals	8.9
ICT Professionals	7.2
Education Professionals	6.3

Data: Australian Taxation Office
Note: Occupation based on tax return in prior financial year.

G Week of withdrawal regressions

Table A4: Relationships between observed variables and week of withdrawal during the first round

	Withdrew 1 May	Average change per week of withdrawal delay			
Controls	None	None	Wages	Plus age	Plus all
Wages	43,975	427			
	(55)	(15)			
Age	37.76	0.20	0.19		
	(0.02)	(0.00)	(0.00)		
Super balance	54,299	2,337	1,919	1,348	1,357
	(130)	(35)	(32)	(29)	(29)
Interest income	93.64	4.89	4.70	3.88	3.54
	(1.50)	(0.40)	(0.40)	(0.40)	(0.40)
Rental income	537.99	24.27	17.49	13.89	11.92
	(5.07)	(1.36)	(1.34)	(1.34)	(1.34)
Dividends	203.37	20.91	20.10	16.68	16.52
	(7.39)	(1.99)	(1.99)	(1.99)	(1.99)
Voluntary Super	225.86	24.83	22.96	19.62	19.90
	(4.20)	(1.13)	(1.13)	(1.13)	(1.13)
N = 1,172,034					

Data: Australian Taxation Office
Notes: Results are from simple linear regressions of listed outcomes on a category variable for withdrawal week during the first round (May 1 is zero and for subsequent weeks the indicator increases by one), controlling cumulatively for the wages and 'Demographics' variables listed in Table 1. The first and last weeks of the first withdrawals are excluded. This estimates the average increase by withdrawal week in pre-treatment characteristics controlling for wages and demographics; that is, whether the upward-sloping lines in Figure 4 remain upward sloping when we condition on wages and demographics. Note all coefficient estimates are positive and statistically significant at the 99% level or above. Variable definitions as per 'Demographics' and 'Long-term financials' in Table 1.

H Estimated average treatment effects on the treated

Table A5: Estimated ATTs on income of the first withdrawal

Week	Cohort								
	1	2	3	4	5	6	7	8	All
0	9,311.75	9,508.25	9,148.29	8,867.91	8,717.52	9,074.77	8,588.43	8,470.03	9,187.90
	(70.61)	(109.90)	(246.41)	(134.49)	(438.35)	(259.33)	(338.74)	(187.82)	(56.36)
1	121.26	345.18	238.20	219.74	-8.21	121.38	-205.44		166.35
	(56.10)	(64.98)	(81.01)	(83.84)	(280.05)	(130.61)	(157.48)		(36.10)
2	-127.72	402.63	157.62	463.05	-343.39	-124.70			54.44
	(170.12)	(77.41)	(97.77)	(178.25)	(314.98)	(135.81)			(88.37)
3	-71.44	191.60	161.68	46.11	-293.86				6.65
	(47.48)	(66.47)	(107.89)	(61.50)	(279.96)				(36.17)
4	-127.95	295.55	89.81	92.11					17.50
	(62.42)	(55.75)	(154.85)	(85.98)					(41.58)
5	28.69	196.57	79.46						77.92
	(61.06)	(85.19)	(102.71)						(45.02)
6	-126.59	235.86							-16.98
	(98.42)	(95.40)							(74.96)
7	-151.02								-151.02
	(63.47)								(63.47)
Pr	0.43	0.19	0.10	0.09	0.06	0.05	0.04	0.03	1

Data: Illion
Notes: N = 337,223. Results are cohort ATTs estimated via the R package 'fixest', which implements Sun and Abraham (2021). Comparison group is the never-treated. Base period is the period immediately prior to treatment. Standard errors computed via a standard bootstrap procedure. Probabilities listed are cohort shares, which for each week are used to compute the weighted averages across cohorts in the right-most column.

Table A6: Estimated ATTs on spending of the first withdrawal

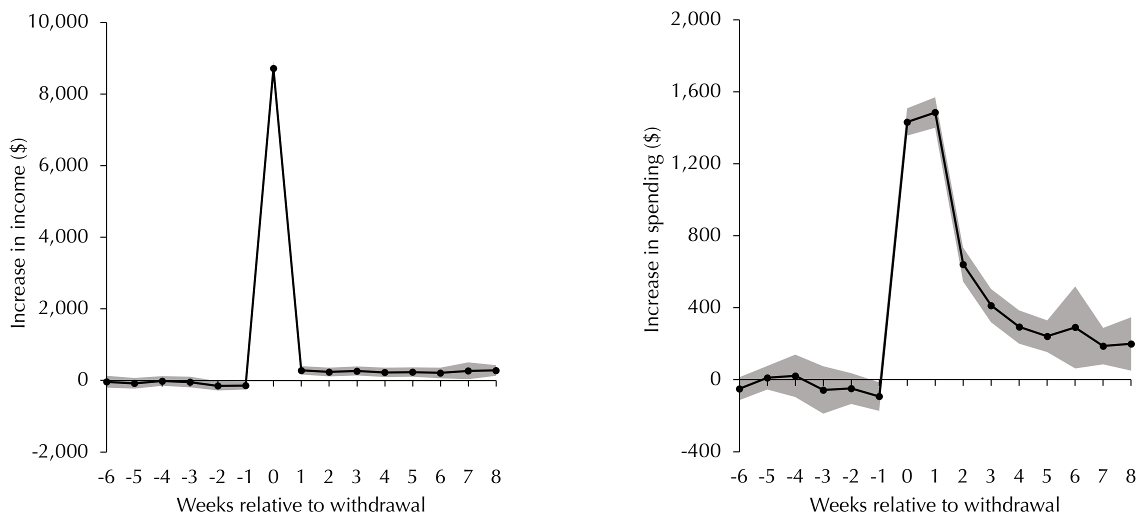
Week	Cohort								
	1	2	3	4	5	6	7	8	All
0	1,676.14	1,488.76	1,502.21	1,508.68	1,414.70	1,498.56	1,471.85	1,545.65	1,569.85
	(24.98)	(34.33)	(50.44)	(47.27)	(62.42)	(68.94)	(78.12)	(82.06)	(16.10)
1	1,229.40	1,344.43	1,384.28	1,313.06	1,252.23	1,295.65	1,217.15		1,279.86
	(19.58)	(30.81)	(46.20)	(43.88)	(54.07)	(66.72)	(71.88)		(13.26)
2	472.92	544.20	551.52	519.32	476.52	499.47			501.74
	(14.94)	(23.50)	(33.98)	(34.52)	(43.07)	(51.34)			(10.53)
3	247.78	303.65	346.69	306.09	284.20				278.13
	(13.36)	(21.91)	(32.48)	(31.10)	(42.99)				(9.77)
4	150.68	212.25	244.18	238.27					184.19
	(14.99)	(22.61)	(35.28)	(37.66)					(11.24)
5	134.43	133.79	160.12						137.34
	(15.43)	(24.13)	(36.34)						(12.35)
6	55.69	89.95							66.05
	(16.34)	(25.38)							(13.51)
7	15.58								15.58
	(16.92)								(16.92)
Pr	0.43	0.19	0.10	0.09	0.06	0.05	0.04	0.03	1

Data: Illion

Notes: N = 337,223. Results are cohort ATTs estimated via the R package 'fixest', which implements Sun and Abraham (2021). Comparison group is the never-treated. Base period is the period immediately prior to treatment. Standard errors computed via a standard bootstrap procedure. Probabilities listed are cohort shares, which for each week are used to compute the weighted averages across cohorts in the right-most column.

I Second withdrawal event study graph

Figure A8: Estimated ATT of the second withdrawal by event week



(a) Income

(b) Spending

Data: Illion
Notes: Results are averages of cohort ATTs weighted by cohort size, estimated via the R package, 'did', which implements Callaway and Sant'Anna (2021). Comparison group is the never-treated. Estimation is 'doubly-robust', with standard errors computed using the bootstrap procedure of Callaway and Sant'Anna (2021). Confidence intervals are at the 95% level. The 'did' package, by default, uses a 'varying' base period when estimating cohort ATTs for the pre-treatment periods, where the base period is the period immediately prior. This is why estimates for the period immediately prior to withdrawal (and their confidence-interval estimates) are not zero. For all post-treatment estimates, the period immediately prior to treatment is used as the base period. There was a permanent increase in income post-withdrawal. Because withdrawals occurred from July 1, they coincided with the start of the new financial year in Australia, typically when people receive a pay rise, a higher government benefit linked to inflation, or a tax refund. This income difference may reflect differences between withdrawers and non-withdrawers on these bases. By dividing the estimated spending impact by the estimated income impact when calculating the MPX, we remove upward bias driven by these other sources of income.

J Category regression table

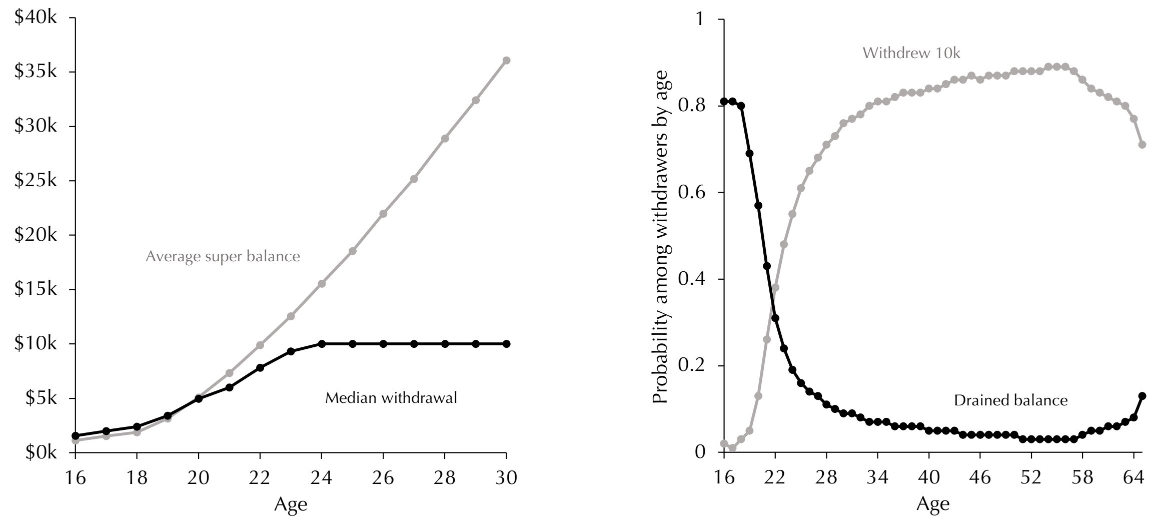
Table A7: Estimated cumulative aggregate ATTs (CAATTs) of the first withdrawal by category

Category	CAATT	Category	CAATT
Uncategorized	1,248.87***	Alcohol and Tobacco	32.27***
	(81.41)		(6.18)
ATM	1,063.88***	Retail	31.58***
	(71.04)		(4.90)
Other debt repayments	339.95***	Health services	15.54***
	(45.37)		(5.16)
Gambling	292.98***	Pharmacies	13.82***
	(30.21)		(3.76)
Credit card repayments	196.70***	Personal care	13.21
	(28.87)		(15.52)
Furniture and office	168.06***	Taxi and rideshare	12.35***
	(14.59)		(2.82)
Supermarkets	128.80***	Travel	12.27*
	(19.45)		(7.32)
Department stores	114.73***	Education	11.47*
	(9.48)		(6.93)
Rent	98.51***	Post office	8.75**
	(37.88)		(4.10)
Buy-now-pay-later	94.49***	Pet care	7.21**
	(12.99)		(3.51)
Restaurants	79.02***	Cafes	6.11***
	(7.46)		(1.74)
Automotive	78.82***	Car rentals	5.97
	(12.25)		(4.58)
Online retail	72.29***	Children's retail	5.29
	(10.86)		(5.25)
Fashion and leisure	71.67***	Road tolls	4.75**
	(8.47)		(2.12)
Home improvement	71.42***	Insurance	4.34
	(15.90)		(10.53)
Gas stations	60.78***	Entertainment	3.98***
	(11.21)		(1.21)
Telecommunications	44.88***	Donations	3.06
	(11.81)		(2.35)
Government	39.95***	Subscription TV	2.84
	(12.17)		(2.40)
Utilities	36.26***	Gyms and fitness	2.06
	(10.35)		(1.55)
Food delivery	35.80***	Transport	-0.02
	(4.14)		(0.05)
Other groceries	35.76***	Public transport	-0.21
	(6.15)		(5.08)
N = 337,223			

Data: Illion
Notes: *** p < 0.01; ** p<0.05; * p <0.1. Results are based on cohort ATTs estimated via the R package 'fixest', which implements Sun and Abraham (2021). We compute weekly aggregate ATTs by averaging across cohorts weighted by cohort share. We then accumulate over the first eight post-treatment weeks. Standard errors are computed analytically as per Sun and Abraham (2021).

K Withdrawal and age

Figure A9: The relationship between age and withdrawal amount



(a) Super balance and withdrawal amount by age

Data: Australian Taxation Office
Notes: First withdrawal. Average Super balance as at June 30, 2019.

(b) Withdrawal cap and balance as constraints by age

Data: Australian Taxation Office
Notes: First withdrawal. Drained balance if first withdrawal amount equal to Super balance on June 30, 2019.