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Early pension withdrawal as stimulus

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Abstract

During the COVID-19 pandemic, the Australian government allowed eligible individuals to withdraw up to A$20,000 (around half median annual wage income) across two tranches from their retirement accounts, ordinarily inaccessible until retirement. Based on historical returns, the modal withdrawal by the modal-aged withdrawer can be expected to reduce their balance at retirement by more than $120,000 in today's dollars. One in six working-age people withdrew a total of $38 billion (on average, 51% of their balances). These transfers represented a liquidity shock and were much larger than those considered in the literature to date. Using administrative and weekly bank transactions data, we find a high marginal propensity to spend (MPX) given the size of the transfers of at least 0.43 within eight weeks, spread broadly across categories (including around half or more on non-durables) and across withdrawers. The response to the second withdrawal, which two-thirds returned for and which occurred after activity had recovered, was even larger at 0.48. Withdrawal and spending are predicted strongly by numerous measures of poor financial health, high pre-withdrawal rates of cash withdrawal and gambling, and younger age. The MPX of rational, forward-looking but liquidity constrained consumers can be expected to asymptote to zero as the transfer size rises, while that of present-biased consumers can be expected to remain high. Our findings overwhelmingly are consistent with the latter, suggesting roughly 80% of withdrawers were present-biased. In selecting strongly on the present-biased, the program presents a sharp trade-off between effective macroeconomic stimulus and suboptimal retirement saving policy.

Keywords: Stimulus, retirement saving, marginal propensity to consume present bias
JEL codes: E21, E63, E71, H31, H55, J32

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1 Introduction

There is a long literature documenting larger responses to cash stimulus than would be predicted under the permanent-income hypothesis (PIH) and Ricardian equivalence (Friedman, 1957; Ricardo, 1820; Barro, 1974).[1] Because of the future taxes necessary to fund it, cash stimulus is effectively a loan from the government. Recipients can be divided into three types depending on how they respond to such a loan: 1) 'Ricardian' types, who are rational, forward-looking, and liquid, have a marginal propensity to consume (MPC) of zero as they do not need the loan; 2) 'rational-hand-to-mouth' (RHTM) types, who are rational and forward-looking but liquidity constrained, have a positive MPC as the loan helps them smooth consumption (Kaplan and Violante, 2014); and 3) 'behavioral' types, who deviate from the PIH due to behavioral factors such as present bias, have a positive MPC as the loan allows them to consume more than a 'smooth' level (Laibson, Maxted and Moll, 2021).[2]

The targeting of cash stimulus away from group 1 to increase the fiscal multiplier is a common macro policy objective (Spilimbergo, Symansky, Blanchard and Cottarelli, 2008). But distinguishing between 2 and 3 matters, too: all other things equal, additional liquidity raises the welfare of group 2 but lowers the welfare of group 3, at least directly. And substantially—Maxted (2022), for instance, finds the welfare cost of present bias to be equivalent to a consumption tax of up to 17.2%. Our goal is to distinguish these types empirically.

To do so, we consider Australia's mandatory private retirement saving system (Superannuation or 'Super'), in which funds are inaccessible until retirement. During the COVID-19 pandemic, the Australian government allowed eligible individuals temporarily to withdraw up to A$20,000 (US$13,600)[3] across two tranches, the first time broad early access had been granted.[4,5] The modal withdrawal by the modal-aged withdrawer can be expected to reduce

[1]Examples include Agarwal and Qian (2014), Broda and Parker (2014), Parker, Soulless, Johnson and McClelland (2013), Sahm, Shapiro and Slemrod (2010), Shapiro and Slemrod (2009), and Johnson, Parker and Souleles (2006). These studies find high marginal propensities to consume or spend, out of fiscal transfers in the order of US$1,000, of 0.2–0.8 within six months. These responses tend to be higher among those on lower incomes, or those with low liquidity conditional on income. Recently, several studies have documented similarly 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]Campbell and Mankiw (1989) characterize the aggregate consumption response as being due to heterogeneity across Ricardian and hand-to-mouth types. Garcia, Lusardi and Ng (1997) present early empirical evidence of heterogeneity, pointing both to liquidity constraints and behavior as key factors. Kaplan and Violante (2014) follow earlier theoretical work by Zeldes (1989), Deaton (1991), and Carroll (1997), and Laibson et al. (2021) by Laibson (1997), O'Donoghue and Rabin (1999), and Angeletos, Laibson, Repetto, Tobacman and Weinberg (2001).

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

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

[5]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 similar association between poor pre-treatment financial health and withdrawal, and a large spending response.

their retirement balance by more than $120,000 in today's dollars.[6] One in six working-age people withdrew a total of $38 billion (on average, 51% of their balances). Given Australia's milder experience of the pandemic and the fact second withdrawals occurred after activity had recovered substantially, this setting offers a favorable basis for external validity outside the pandemic context.

The Super withdrawals had two unique features, which frame our analysis. First, they were optional.[7] This could generate a selection effect, altering the mix of types and thereby the consumption response relative to standard cash stimulus. The Ricardian types, for example, may be expected not to withdraw. However, as withdrawals could be re-contributed pre-income-tax, implying a subsidy of up to $7,528 depending on income and existing voluntary contributions, withdrawal presented an arbitrage opportunity to all types.[8] Moreover, withdrawals relieved the liquidity constraints of the RHTM types, allowing them to smooth consumption or rebalance their portfolios towards higher-liquidity assets. On the other hand, people may have faced frictions, such as the mechanism proposed by Attanasio, Kovacs and Moran (2020) in which RHTM types choose low liquidity as a commitment device. So it is unclear *ex ante* whether we should expect selection to have raised or lowered the MPC.

The second unique feature is size. Critically, the models generate distinct predictions of how the consumption response changes as transfer size increases: Kaplan and Violante (2014) predict that raising the transfer from $500 to $5,000 would lower the MPC of the RHTM types from 0.20 to 0.03, whereas Laibson et al. (2021) predict the MPC of the behavioral types would fall only from around 0.13 to 0.11.[9] Given a small transfer, the RHTM and behavioral types are co-located throughout the MPC distribution. But, as the transfer size rises, only the RHTM types bunch near zero, isolating a group of behavioral types at higher MPCs. As the modal Super withdrawal was nearly triple the amount in the example above, they generated substantial 'power' to distinguish between types. And because the withdrawals constituted a liquidity shock, holding lifetime income roughly constant, they served as a clearer test of the PIH than the only other large shock considered in the literature, lottery winnings.

Using administrative records covering Super, tax, payroll, and welfare on Australia's entire working-age population and a large panel of weekly bank transactions, we study two aspects of the program. First, selection into the program to gauge the intensity of desire for liquidity and its predictors. People had not only a choice of whether to withdraw, but how soon to withdraw and whether to withdraw again; we estimate how short- and long-term pre-treatment characteristics, including multiple measures of financial health, varied along

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

[7] Note, under pure Ricardian equivalence, cash stimulus and Super withdrawals have identical opportunity costs because cash stimulus can be contributed to Super and attract the same net return.

[8] People could contribute pre-income-tax an additional $25,000 per year at a concessional tax rate of 15%. More than 99% of people were below this cap. Above that amount, re-contributions could be made post-income-tax.

[9] Levels are not directly comparable across the two models.

these three dimensions. Second, the effect of the program, along with heterogeneity in that effect and its predictors, to discern motivations for accessing liquidity. We estimate the marginal propensity to spend (MPX) out of each round of withdrawals, how it varied across categories and across individuals, and its relationships with pre-treatment characteristics.[10]

On selection, we find the vast majority remained constrained even after having accessed almost half median annual wage income in additional 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 were far more likely to withdraw and the propensity to withdraw rose with distance from urban centers. Withdrawers had moderately lower wages, but this was persistent not transitory: wages were lower during the two months prior, the three years prior, and the working life to date, and there was no discontinuity in weekly wages at the time of withdrawal. But withdrawers had radically 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 withdrew a second time, indicating a high demand for liquidity.

On the treatment effect, using modern difference-in-differences techniques (Callaway and Sant'Anna, 2021; Sun and Abraham, 2021), we find a high MPX out of the first withdrawal of at least 0.43 (0.01) over eight weeks,[11] spread broadly across categories with around half of discernible spending on non-durables. Withdrawers raised spending by 129% for the first two weeks and by 46% over eight weeks, at which point spending returned to its pre-withdrawal level. The MPX out of the second withdrawals was even higher at 0.48 (0.01), which two-thirds returned for and which occurred after activity had recovered substantially. The MPX was broadly distributed between zero and one with no excess mass at zero. 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 withdrawals, and age. The least-liquid 40% spent more than double that of (around $2,500 more than) the most-liquid 20% over eight weeks.

The absence of a spike in MPXs at zero contra cash stimulus suggests the Ricardian types did not opt in despite the financial incentive, consistent with evidence of low take-up of employer 401(k) matches (Choi, Laibson and Madrian, 2011). This of course raises the question of whether such types exist at all. Put simply: those who did not want to spend did not withdraw. Further, if RHTM types made up a sizable share of the withdrawers, we would expect a very low MPX out of these very large transfers. Rather, we observe both very large

[10]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. Laibson, Maxted and Moll (2022) provide a theoretical mapping from MPCs to MPXs.

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

withdrawals and very high, rapid, and temporary spending, much of it on non-durables, clearly inconsistent with inter-temporal smoothing. The MPX is almost identical to that estimated out of $950 stimulus checks in Australia during the 2008 crisis (Leigh, 2012).

This average MPX will be higher for the absence of Ricardian types, and reflects the shares of RHTM and behavioral types. Consistent with the models' predictions, the conditional means of relevant pre-treatment variables decline precipitously at low MPXs and plateau at higher MPXs, which is indicative of a transition between types. This visual diagnostic suggests roughly 80% of withdrawers were behavioral types, translating into a conservative lower bound of one in five 34-year-olds in the population being behavioral types. This strong selection on the behavioral types implies the characteristics we observe among the withdrawers are also those exhibited disproportionately by the behavioral types: male, young, parents, single, in poor financial health, gamblers, blue-collar, outer-suburban or rural, etc.

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. Under the Super withdrawal program, the vast majority of those who were eligible chose not to access their retirement savings when given a seemingly one-off opportunity to do so, while the vast majority of those who did appeared to be driven by present bias. Accordingly, as a matter of retirement saving policy, illiquidity would seem welfare-improving in aggregate, perhaps explaining its ubiquity in retirement saving systems outside the US (Fadlon and Laibson, 2022).

On the other hand, we find the policy generated at least 0.8% of GDP in direct additional spending, almost entirely within a four-month period. Based on the government's 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 higher than with cash stimulus (Ramey, 2019).[12] 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". Given fiscal constraints (legal or political, as per Besley and Smart (2007)), self-financing can enable additional stimulus. This comes at the cost of the sub-optimal future consumption of the present-biased—though, that is also true of cash stimulus, which can be saved rather than spent. The question is 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

[12] 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 government policy, but we do not consider spillovers or other general-equilibrium consequences.

in generating very large transfers holding lifetime income roughly constant. Studies of stimulus checks consider transfers in the order of US$1,000, the responses to which should not differ markedly between the behavioral and RHTM types. Transfers from the Alaska Permanent Fund are larger (US$3,900 for an average family), but still far smaller than Super withdrawals (up to US$27,000 for a family) and received regularly (Hsieh, 2003; Kueng, 2018). The unemployment insurance supplements in the US during the pandemic were large cumulatively, but spread over time (Ganong, Greig, Noel, Sullivan and Vavra, 2022). Severance pay following a layoff generates simultaneous transitory and permanent income shocks (Gerard and Naritomi, 2021). Lottery winnings are the largest transfers considered in the literature, but they increase lifetime income substantially (Imbens, Rubin and Sacerdote, 2001; Kuhn, Kooreman, Soetevent and Kapteyn, 2011; Fagerang, Holm and Natvik, 2021).

The one direct policy precedent we know of occurred in Denmark in 2009, studied by Kreiner, Lassen and Leth-Petersen (2019). There are some important differences between their setting and the present one. 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.

We also contribute to the literature on present bias (Cohen, Ericson, Laibson and White, 2020). For example, in an observational setting covering more than 70% of the working-age population, we study the removal of a binding savings constraint—the opposite of the randomized controlled trial of Ashraf, Karlan and Yin (2006) in which people could *take up* such a constraint. And we 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 investigate the intensity of desire for liquidity 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. And in Section 6, we conclude by discussing our findings in the context of benchmark consumption models.

Table 1: 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.

2 Preliminaries

2.1 Policy

Australia has a compulsory, defined-contribution private retirement saving system called Superannuation (or 'Super').[13] All employers are required to contribute an additional 10.5% of pre-tax wages to their employees' Super accounts, inaccessible outside exceptional circumstances until age 58 if retired or 65 if still working. The median pre-withdrawal balance by age is shown in Table 1.

For most people, Super is tax-preferred relative to other forms of saving (except the primary residence): employer contributions are made pre-income-tax, and only taxed at 15% when they enter the fund (compared to a modal marginal income tax rate of 34.5% and a top rate of 47%); before retirement, Super cash returns are taxed at 15% and capital gains at 10%; and neither returns generated in retirement nor withdrawals are taxed at all. Additional voluntary contributions of $27,500 per year before tax and $100,000 per year after tax are allowed, but less than 1% of people are above the pre-tax cap.[14] 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.[15]

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.[16] Cash transfers were limited to

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

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

[15] People can choose between portfolios within their Super fund based on risk. Most people keep their savings in a 'growth' fund for most of their working life (generating higher average returns but greater volatility) and then switch to a more conservative fund near retirement.

[16] Leigh (2012) found that program generated an MPX of 0.41–0.42 out of $950 checks.

lump-sum payments for those receiving government benefits, alongside wage subsidies and supplemental unemployment insurance payments.

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".[17] The government stated the fiscal cost as $1.2 billion, which reflected only foregone taxes over four years (not the $38 billion eventually withdrawn).[18] The government would later add broad eligibility conditions.[19,20] 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.[21]

Australia's experience of the COVID-19 pandemic was in general milder than many comparable countries'.[22] Australia was early to close its borders and international travel remained virtually impossible for non-citizens—and highly restricted for citizens—for the first 18 months. From March 28, all international arrivals were required to complete two weeks of hotel quarantine. Public health measures were imposed in local areas until cases were eliminated. Australia's per-capita death rate was less than a tenth that in the US.[23]

Meanwhile, Australia had a relatively 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.[24] 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 had lifted nation-wide.[25] In this context, the Super withdrawal program can be viewed as 'additive' relative to pre-pandemic personal net income, supported as it was by public health

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

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

[19] 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.

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

[21] https://www.apra.gov.au/covid-19-early-release-scheme-issue-36

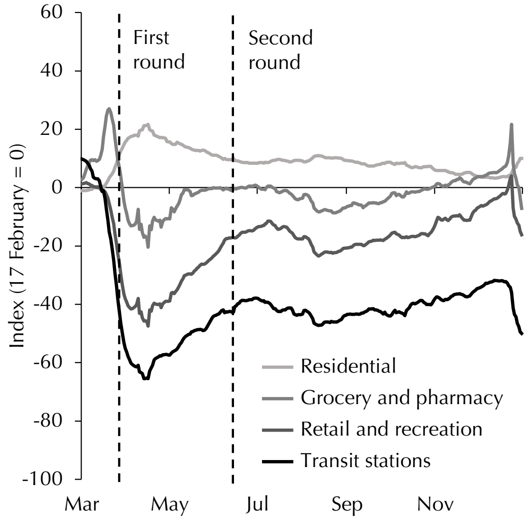
[22] For an overview of Australia's COVID-19 experience, see: https://independentcovidreview.com/wp-content/uploads/2022/10/FAULT-LINES-1.pdf.

[23] https://ourworldindata.org/covid-deaths

[24] Hamilton (2020) compares the Australian and US fiscal responses. Breunig and Sainsbury (2023) consider the distribution of all fiscal transfers. Bishop and Day (2020) and Watson, Tervala and Sainsbury (2022) find Australia's wage subsidy program had a substantial effect on employment.

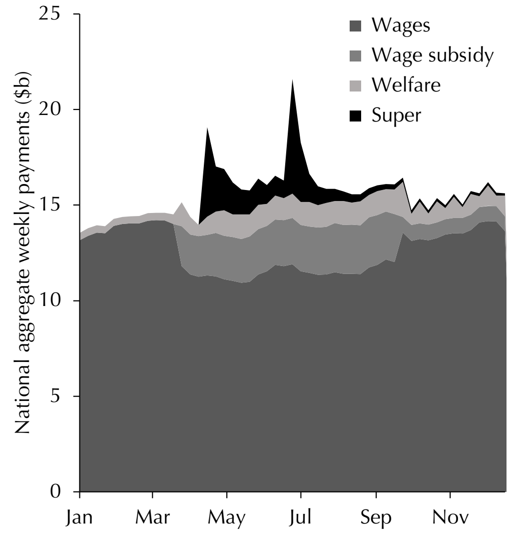
[25] https://www.aph.gov.au/About_Parliament/Parliamentary_Departments/Parliamentary_Library/pubs/rp/rp2021/Chronologies/COVID-19StateTerritoryGovernmentAnnouncements#_Toc52275795

Figure 1: Economic activity indicators in Australia during 2020



(a) Mobility indicators by establishment

Data: Google trends
Note: Google mobility data records physical locations people have visited.



(b) Working-age payments by type

Data: Australian Taxation Office and Department of Social Services

measures and fiscal transfers, pushing it well above its pre-pandemic level (Figure 1b).

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 working-age Australians, which allow us to study how the withdrawals were used. We are not permitted to link individuals across the two datasets, instead using each for different parts of the analysis, as noted. 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 the 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 before the pandemic (July 1, 2018 to June 30, 2019, or 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;[26] 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,[27] including the withdrawal amount, date, and the 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 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. In cases where multiple credit-check events are triggered by a single individual, the transactions data are stitched together to provide a longer transaction history. The original transactions data collected by Illion include the date and time the transaction was processed (usually within a few business days for card purchases and instantly for transfers), the description of the transaction, the type of transaction (e.g., card payment, direct debit, external transfer), the transaction value, and the account on which the transaction occurred.

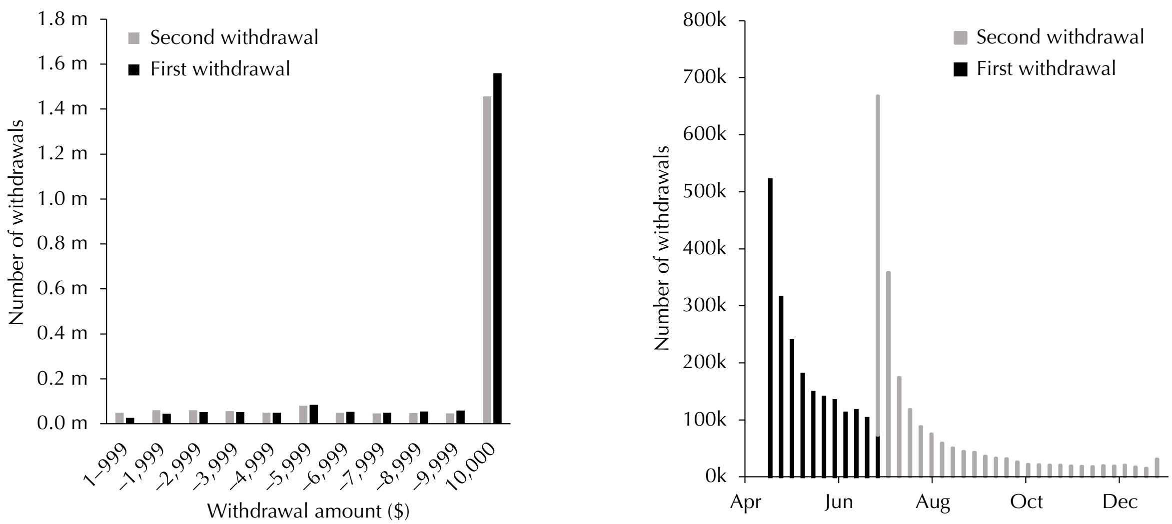
The dataset we use is an aggregated version of this transaction-level data. Transactions are classified by Illion into spending categories (e.g., supermarkets, retail, department stores) and income categories (e.g., wage income, Super withdrawal, welfare income) using the transaction description and type. External transfers initiated by the individual are not counted towards spending. For privacy reasons, 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 ending at their credit-check event.

Observing a limited window for each individual introduces the possibility of assigning someone who withdrew Super 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 Super 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

[26]Interest income is third-party reported by banks and financial institutions. Rental income is self-reported, though there may be strong incentives to report at least something because net rental losses may be claimed against other sources of income. Dividends are self-reported, though there is a strong incentive to report as tax paid at the corporate level may be credited against the personal tax liability on dividends.

[27]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

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.

3 Selection into withdrawal

3.1 Withdrawal

Around one in six (2.6 million) working-age people withdrew a total of $37.8 billion (around 1% of assets or 2% of GDP). Around three quarters in each round withdrew the maximum $10,000, with the modal withdrawal across both rounds $20,000 and the average withdrawal $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, so only around one in six chose an 'interior' withdrawal amount. Around three quarters of those who had a positive balance remaining after withdrawing in the first round withdrew again in the second round, with the pattern of withdrawal amounts similar to the first round (Figure 2a). Including those who didn't withdraw, the average withdrawal was 9% of balances; among those who withdrew, it was 51%. In the two rounds, 25% and 30% withdrew in the first three days and 40% and 48% in the first 10 days (Figure 2b). Collectively, these observations suggest the vast majority continued to be constrained even after having withdrawn up to $20,000 of their retirement savings (or $40,000 for a household with two withdrawing members).

In Table 2, we compare the withdrawers and non-withdrawers on the basis of a variety of pre-treatment variables.[28] In the first column, we present estimated means among the non-withdrawers, and in the remaining columns we present estimated 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.

We are interested primarily in people's financial circumstances, in both the short and long term. 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 discontinuity in weekly wages at the individual level coincident with withdrawal.[29] Withdrawers had around half the Super balances of non-withdrawers, reflecting lower wages over the working life to date.[30] Collectively, these observations indicate the difference in wages was persistent, being present in the month prior, the three years prior, and the working life to date.[31] Additionally, the Super balance and wage densities were compressed, with those in the middle over-represented among the withdrawers (Appendix E).

The differences in financial assets were even larger. In the month prior to withdrawal, the withdrawers had a 54% lower saving rate, 62% lower bank balances relative to spending, a 23% higher probability of having an overdrawn account, and 10% higher debt repayments relative to spending—all robust to wage differences. The pattern was similar over the longer term, with withdrawers having received 75% less interest income over the prior three years. Even controlling for wages, age, sex, and dependents, the difference remains large at 62%. Withdrawers received 24% less rental income and 60% less in dividends, and made 89% lower voluntary Super contributions in the year prior to withdrawal, reflecting that they were 10 percentage points (17.3% v 7.7%) less likely to have made a voluntary contribution, while those who did made 74% lower contributions on average ($3,327 v $12,766).

For some additional context on the withdrawers, in Table 3 we divide the Australian working-age population by occupation and location and calculate withdrawal rates within these divisions.[32] There was strong variation in withdrawal along both dimensions. Withdrawal was highest in 'blue-collar' professions and lowest in 'white-collar' professions, with the lowest-withdrawing occupation teachers at 6.3% and the highest construction and mining laborers at 40.2%. Withdrawal was also strongly, monotonically, and negatively related to the

[28]While withdrawal was subject to the eligibility criteria described earlier, eligibility was self-assessed. As discussed in Appendix B, we estimate at least 70% were eligible, and conditioning on eligibility doesn't alter meaningfully any of the patterns observed in Table 2.

[29]This is based on an event study on wages using the same method we apply to spending later (Appendix D).

[30]Compulsory Super contributions are a flat rate of wage earnings and voluntary contributions are small.

[31]This persistence of lower wages is consistent with Parker (2017), who relates it to the MPC.

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

Table 2: 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.

Table 3: 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.

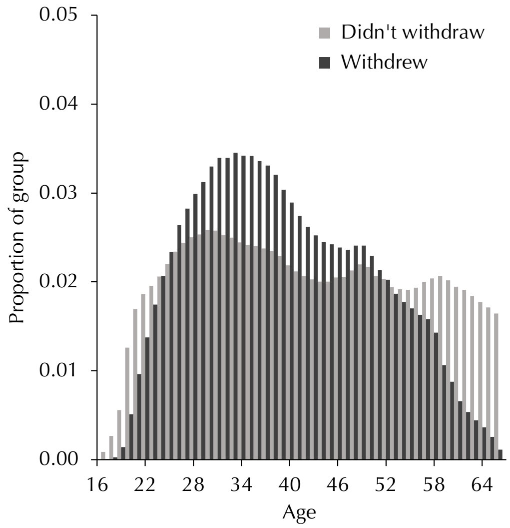
proximity to cities, with those in very remote areas 40% more likely to withdraw than those in major cities. In the most remote towns, more than half withdrew. The communities with the lowest rates of withdrawal were in Canberra (Australia's capital), in the 3–5% range.

Age is relevant to the distinction between rational and behavioral explanations in two ways. First, with a fixed mandatory contribution rate over the life cycle, younger workers may over-save early in their working life, and withdraw to offset this. If forward-looking, they would then spread those withdrawals over the remainder of their working life. Second, theory, experiments, and neurological imaging suggest a decrease in the rate of time discounting and present bias from adolescence to middle age (Sozou and Seymour, 2003; Read and Read, 2004; Whelan and McHugh, 2009; Samanez-Larkin, Mata, Radu, Ballard, Carstensen and McClure, 2011).[33] Relatedly, Lusardi, Mitchell and Curto (2010) find financial literacy to be low among younger people and Agarwal, Driscoll, Gabaix and Laibson (2009) find that the middle-aged make better financial decisions than younger people.

To the extent withdrawal reflects behavioral factors, we might expect it to be more pronounced among younger people, who would then exhibit a greater propensity to spend. Note the two explanations predict greater withdrawals among younger people but they generate opposing predictions of how younger people will spend out of their withdrawals.

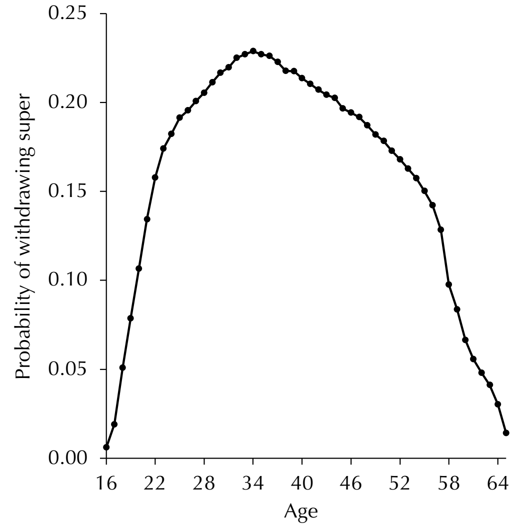
[33] In economics, Trestle and Taylor (2001) propose that time discounting varies over the life-cycle rationally due to the decreasing marginal utility of income in older age.

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.

The withdrawers were three years younger on average, but this masks a compression of the age distribution among the withdrawers (Figure 3a). The withdrawers were underrepresented up to age 23 and beyond age 51, with a modal age of 33.[34] In Figure 3b, we present the associated probability of withdrawal conditional on age. The maximum take-up rate occured at age 34 at 23%.[35] This was preceded by a rapid increase in take-up and succeeded by a gradual decline toward retirement. The drop in take-up at age 57 coincides with the age individuals could begin to access their Super if they were retired. Because those over 57 and retired could already access their Super, they had no reason to participate in the program.

3.2 Timing

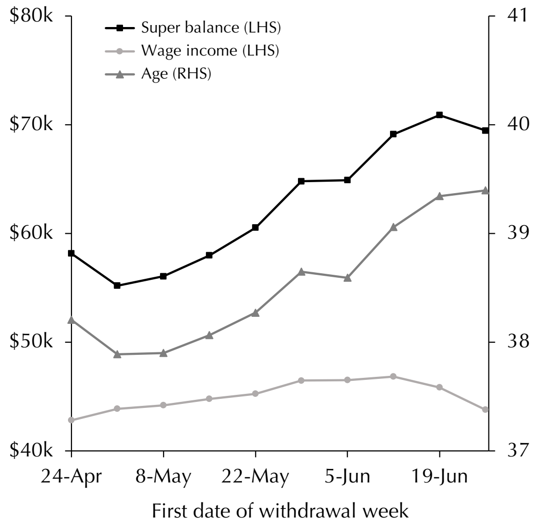
People faced not only a choice of whether to withdraw but also when to withdraw. The policy was announced around a month before applications opened, and funds were received within just a few days of application. In the first withdrawal, 25% of withdrawals occurred in the first three days, another 15% in the next seven days, and the remaining 60% over the following two months (Figure 2b).

The differences between the withdrawers and non-withdrawers observed in Table 2 are also reflected among those who withdrew earlier (Figure 4). Setting aside the first and last weeks,

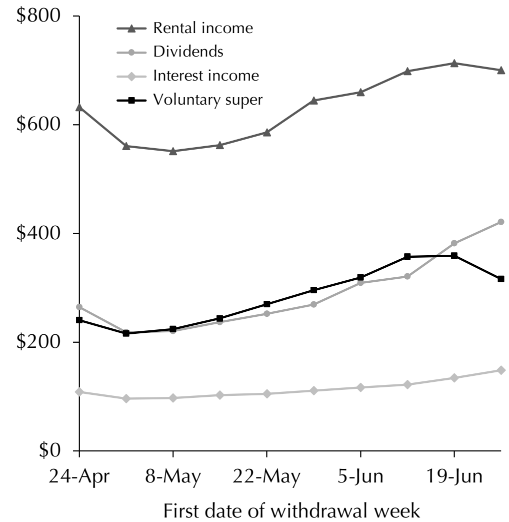
[34]The underrepresentation up to age 23 was not due to a lower probability of having a positive Super balance because the observed population is conditional on that, but it may be explained by lower balances or a lesser degree of salience or attention among the youngest.

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

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



Data: Australian Taxation Office
Notes: Outcomes as per 'Long-term financials' in Table 2.



Data: Australian Taxation Office
Notes: Outcomes as per 'Long-term financials' in Table 2.

which may be subject to idiosyncratic factors, the average Super balance increased by 28% over the intervening seven weeks, average wages by 4.5%, and average age by 1.5 years. Average pre-treatment interest, dividends, and rental income all increased with the withdrawal date, remaining statistically significant even after controlling for all of the same characteristics as those conditioned on earlier (Appendix G). The program offered temporary access to ordinarily inaccessible wealth, and the front-loading of withdrawals suggests a degree of urgency. The fact this urgency was strongly associated with asset levels (in particular, liquid assets) is consistent with our earlier findings for withdrawal generally. Overall, those with persistently less liquidity were more likely to seek additional liquidity—and to seek it sooner.

3.3 Second withdrawal

In addition to having a choice of whether and when to withdraw, people could also withdraw a second time, 72 days after applications for the first withdrawals opened. 54% withdrew in both rounds, 24% in only the first, and 21% in only the second. An advantage of observing the second withdrawal is that it came well after the initial pandemic shock, once it had become clear health and economic outcomes in Australia would be contained. During May and June 2020, domestic restrictions eased nation-wide. 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). Because people were given a second opportunity to withdraw, we can also measure the intensity of desire for liquidity.

Focusing on those who had a positive Super balance remaining after withdrawing in the

Table 4: 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 2. 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 2.

first round, we observe similar patterns among those who withdrew a second time as among those who withdrew at all or withdrew earlier (Table 4). Those who withdrew a second time did have higher wages, were slightly older, and had higher Super balances. But this is expected, as we removed those having drained their accounts in the first round, 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. When we condition on wages and age, these differences all increase substantially. Overall, those with persistently less liquidity were more likely to seek additional liquidity, to seek it sooner—and to seek it a second time.

4 Effect of withdrawal

4.1 Spending

Our spending measure includes all merchant debits associated with an individual's bank accounts (such as with a debit or credit card), cash withdrawals, and 'buy now, pay later' repayments. In Australia, electronic methods cover the vast majority of consumer payments, with cash transactions having fallen from 40% in 2007 to 10% in 2019,[36] compared to 22% in the US.[37] And, in a 2019 survey, just 12% of those who reported holding cash outside their wallet did so to save for a large purchase, suggesting cash withdrawals overwhelmingly are used for immediate spending.[38] Because we exclude all debt repayments and external transfers, which are sure to account for additional spending, our estimates can be viewed as conservative lower bounds on the true spending responses.[39]

We begin by plotting average income and spending among all units in the bank transactions data by 2020 calendar week (Figure 5). We also split these aggregates into those who did and did not withdraw Super. The timing of each round is clearly visible in both income and spending in the aggregate. There is a sharp spike in income coincident with the first week of withdrawals in both late April and early July, and concomitant but more diffuse increases in spending. This mirrors closely the timing profile of withdrawals we observed in the administrative data in Figure 1b. Split into withdrawers and non-withdrawers, Super withdrawal is even clearer.

There are two issues with this aggregate view. First, as noted earlier, there were a number of concomitant income (and thus spending) shocks, including supplementary unemployment insurance with relaxed eligibility requirements and mutual obligations, a large-scale wage subsidy program, cash transfers to government benefit recipients, and tax refunds distributed from July, which explain the concomitant spikes in non-withdrawer income. Second, withdrawals occurred over many weeks within 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. The method typically used for event studies, standard two-way fixed-effects (TWFE) regression, has come under scrutiny recently (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).[40] 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. A key problem with standard TWFE regression arises from the implicit weights used to aggregate treatment

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

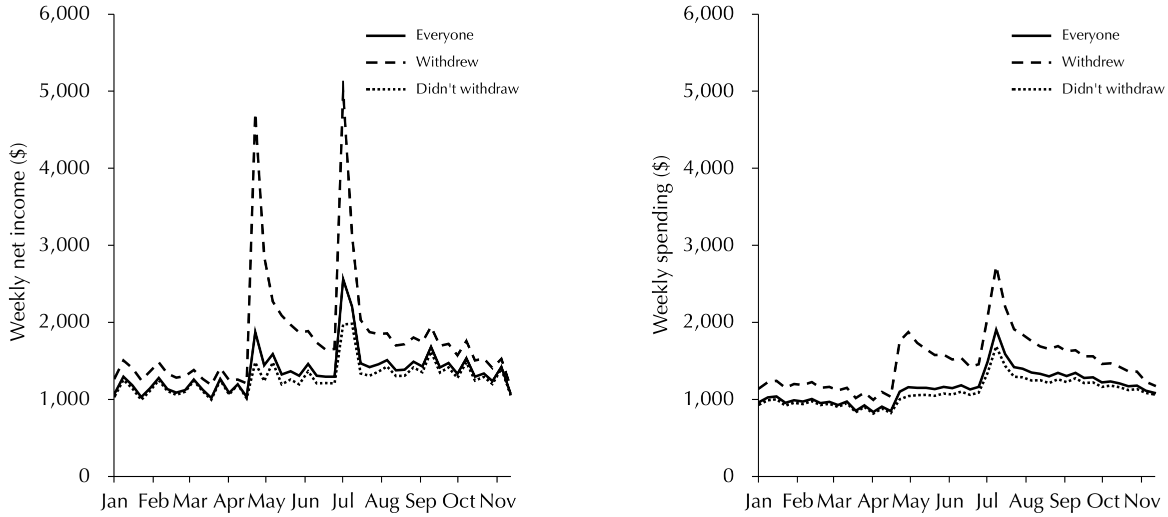
[37] `https://www.atlantafed.org/banking-and-payments/consumer-payments/survey-of-consumer-payment-choice`

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

[39] 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. Laibson et al. (2022) provide a theoretical mapping from MPCs to MPXs.

[40] 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

effects across cohorts, which can in some cases be negative (Goodman-Bacon, 2021).

New methods have been developed to ameliorate these concerns. The main thrust of two leading examples, Callaway and Sant'Anna (2021) and Sun and Abraham (2021), is to estimate average treatment effects on the treated (ATTs) separately for each cohort (those units treated at the same time) and then aggregate those ATTs across cohorts using non-negative weights.[41]

In Figure 6, we display cohort-specific event-study plots reflecting estimates of the following standard interacted TWFE model:

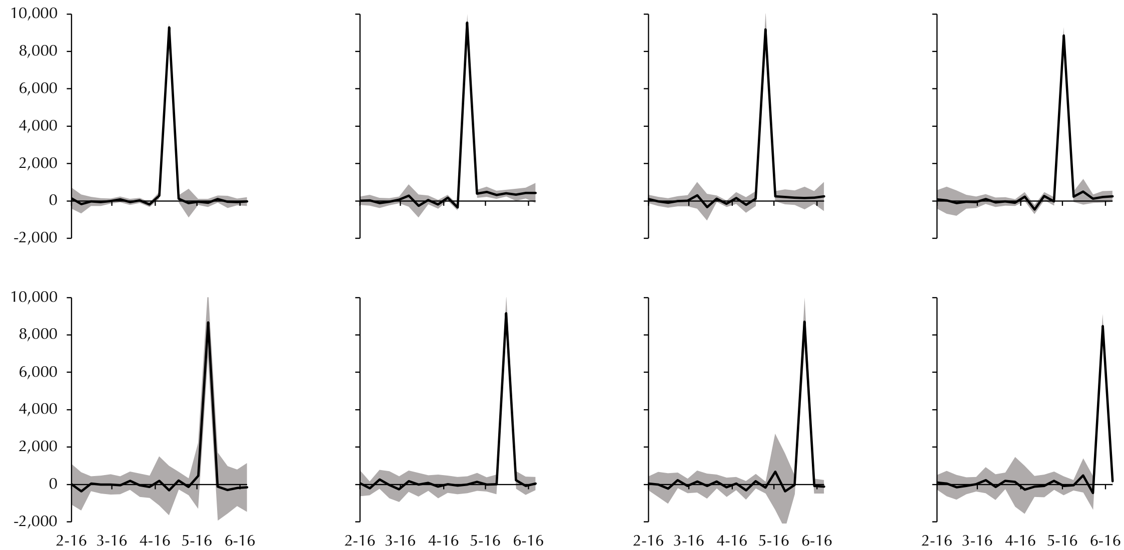
$$Z_{it} = \alpha_i + \lambda_t + \sum_{\ell \neq -1} \delta_{e\ell} \left(\mathbf{1}\{E_i = e\} \cdot D_{it}^{\ell} \right) + \varepsilon_{it},$$

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 effectively 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 prior to treatment, as in the canonical two-by-two difference-in-differences setup.

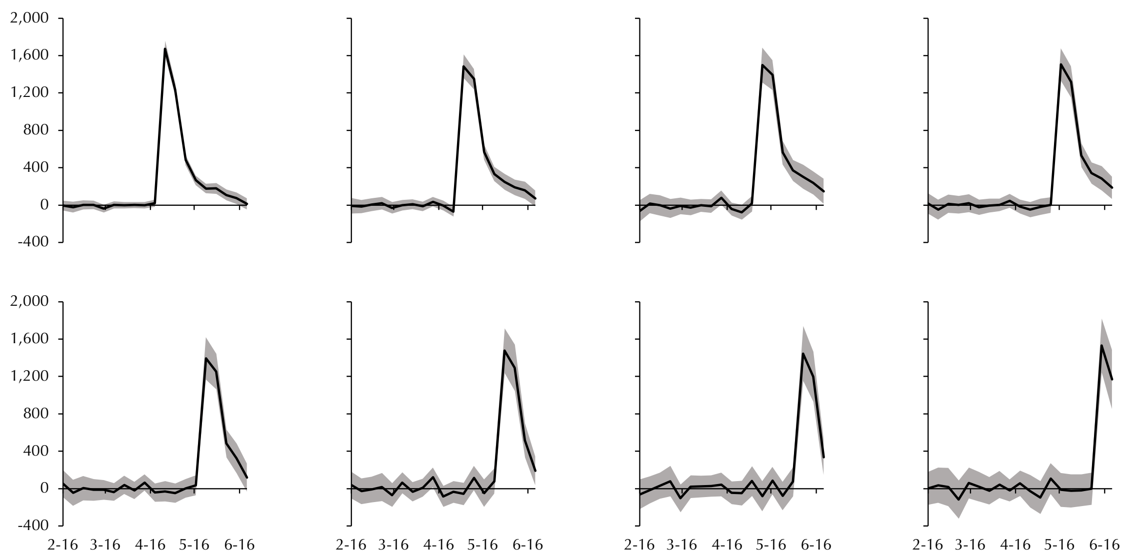
These weekly cohort ATTs may then be aggregated across cohorts, to produce a single

[41]In contexts such as ours, in which there is a never-treated group and an unconditional parallel trends assumption is made, these two methods are equivalent, as both pairs of authors note.

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.

ATT for each period relative to treatment, using the cohort shares as weights, $\text{ATT}_\ell = \sum_e \delta_{e\ell} \cdot \Pr[E_i = e]$. Estimates are displayed in Figure 7.[42] We then sum our ATTs across the post-treatment periods for which we observe a positive treatment effect on spending, which based on Figure 7b is eight weeks.[43] To construct the MPX, we need to divide our estimated cumulative aggregate spending ATT by the withdrawal amount.[44] But because non-super-withdrawal income may vary post-treatment between the withdrawers and non-withdrawers, instead we divide by the income analogue of our cumulative aggregate spending ATT:

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

where X is spending and Y is income.[45,46]

Our estimates are displayed in Table 5. The first round of withdrawals raised income by 93% on average over eight weeks. In the month before withdrawal, the withdrawers spent $1,107 per week on average. Our estimates indicate spending rose by 46% over eight weeks, and was 129% higher over the first two weeks. Our estimated MPX of 0.43 over eight weeks is high in absolute terms but particularly in the context of those found in studies of far smaller cash transfers. Leigh (2012), for example, estimated 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 all debt repayments and external transfers, this is likely to be conservative.

Observing spending out of the second withdrawals, which occurred from July 1, allows us to gauge the intensity of the desire to spend and to assess the spending impact in a more 'normal' environment. By July, public health restrictions had lifted nation-wide and case numbers had been reduced to zero, in stark contrast to the experiences in most comparable countries. This should go some way to alleviating concerns the large observed spending impact of the first withdrawals was driven by panic or early pandemic-related spending.

The spending profile following the second withdrawal was similar to that following the

[42]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 issues with the US Great Recession stimulus literature raised by Orchard, Ramey and Wieland (2023).

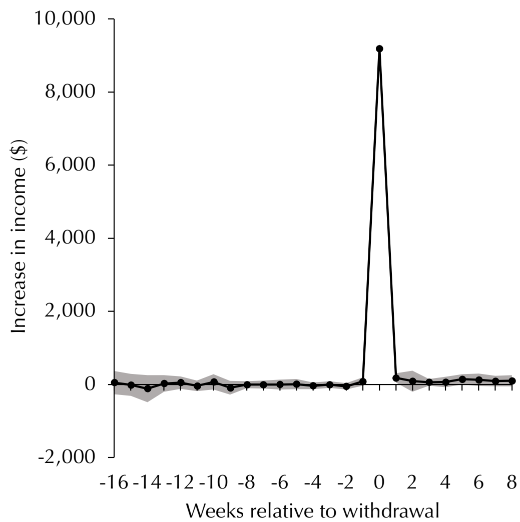
[43]Appendix H includes estimated weekly cohort ATTs for income and spending.

[44]This is similar to Flynn and Smith (2022), who estimate the effect of Clean Water Act funding on capital investment by dividing the effect on average capital investment by the effect on average grant funding.

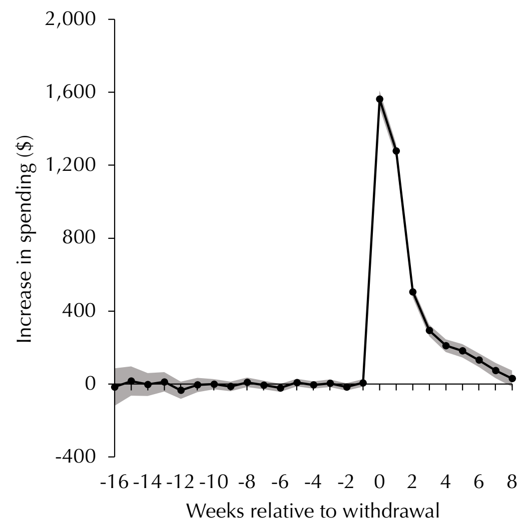
[45]While the 95% confidence interval for the estimated income ATT shown in Figure 7 contains zero, the weekly point estimates are modestly but consistently positive—evidently driven by a small but statistically significant and persistent increase in income for the second cohort, as can be seen in Figure 6a. Attributing all additional post-treatment spending to Super would upwardly bias our MPX estimate. But note that $9,191, or 94%, out of a total income increase of $9,940 over eight weeks is Super.

[46]Because we are dividing one estimate by another, we estimate standard errors via a bootstrap procedure. For a single bootstrap iteration, we construct a bootstrap sample by resampling individuals with replacement, then perform the difference-in-differences estimation procedure described on each subsample. Standard error estimates are the standard deviations of our estimates across 1,000 bootstrap iterations.

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.

Table 5: 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.

first (Appendix I). And the estimated MPX for the second round was even higher at 0.48. Recall three quarters of those who had a positive balance remaining after withdrawing in the first round made a second withdrawal. If our spending estimates are representative of all withdrawals, the first and second withdrawals together generated 0.8% of GDP in additional direct spending, almost entirely within May–August 2020.

4.2 Identification

In order for our ATTs to identify causal effects, we must make a parallel trends assumption: that, post-treatment, average outcomes among the treated units would have evolved similarly to those among the never-treated units had the treated not received the treatment. This does not require randomization nor even the treatment and comparison groups to have been similar. There are two general threats to identification: that treated and never-treated outcomes were on different trends before and after treatment or that only the outcomes of treated or never-treated units were subject to a confounding effect post-treatment. Note that, under our TWFE specification, we are making the comparison between treated and never-treated units conditional on all time-invariant characteristics, observed or not. And recall that we have addressed bias due to differential concomitant income shocks by dividing by our estimated cumulative aggregate income ATTs rather than the withdrawal amount.

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

Next, in Figure 6, there does not appear to have been a post-treatment confounder that, in calendar time, applied only to either the treated or never-treated groups. Moving between cohorts, the treatment date advanced by one week, each income spike advanced by one week, and each spending spike also advanced by one week. The dynamic paths of income and spending were similar across cohorts, with spending consistently elevated over the first two weeks and then having tapered down similarly over the subsequent six weeks. This suggests against the occurrence of a confounding event affecting all cohorts on a given calendar date.

There remains the potential for a confounding effect only on treated or never-treated units at the time of treatment for each cohort; that is, in event rather than calendar time. Reverse causality is one possibility: that the spending caused withdrawal rather than the reverse. This is a form of selection bias, as never-treated units switch to the treated group at the time of treatment. In order for this to be a major concern, given the large share of the treated group in the population and the large treatment effect among that group, it would have to have resulted in a noticeable decline in spending among the never-treated group as those whose spending would have been elevated even in the absence of treatment selected out of the never-treated group, lowering spending there.

But the aggregate spending in Figure 5 suggests this was not the case. Not only did spending among the never-treated not decline at the time of the first withdrawals, but spending *in aggregate* across all units (uncontaminated by selection bias as all units remain in the group at all times) rose at a rate greater than would be explained by Super withdrawals alone. This was also the case at the time of the second withdrawals, addressing concerns this may have been due to early pandemic-related spending (e.g., panic buying or home office purchases) across both treated and never-treated groups. For additional confidence, 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 nil effect concomitant with treatment.

There is also the question of external validity. It is worth repeating our estimates are average treatment effects on the *treated*—those who withdrew. As shown earlier, the withdrawers differed markedly from the non-withdrawers; in particular, having had persistently poorer financial health. While this need not threaten identification, to the extent it explained selection, it may still have affected the spending induced by the program. This effect is *ex ante* theoretically ambiguous because liquidity-constrained withdrawers may have accessed Super to finance spending or to rebuild liquidity. The very high spending we observe is consistent with the former (as is further evidence to come). But there is no suggestion this estimated MPX applies to the population broadly.

4.3 Categories

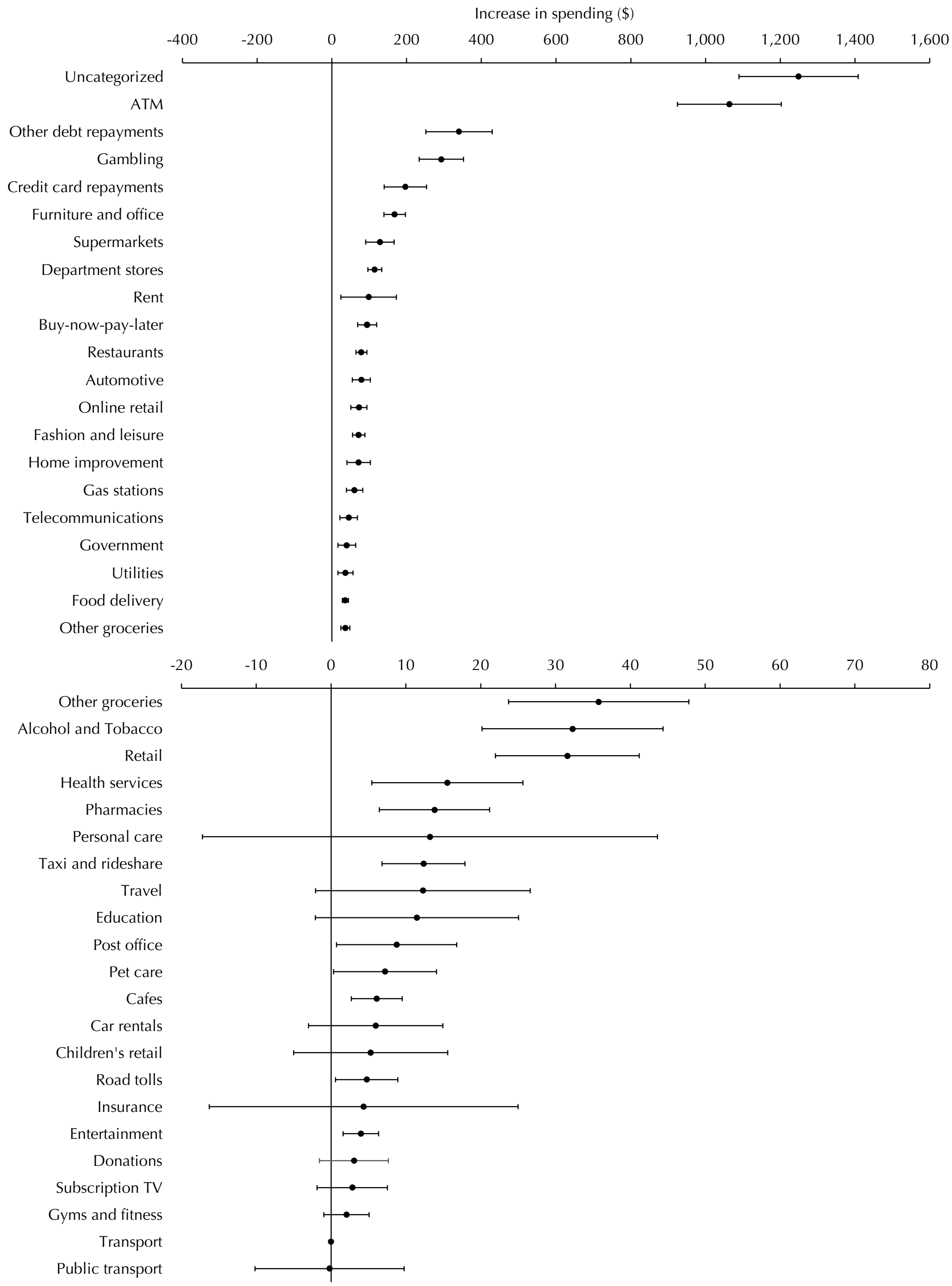
In Figure 8, we present estimated cumulative aggregate ATTs for each spending category we observe.[47] Spending categories are defined based on the merchant name (such as a large national chain or the merchant name containing a recognizable category). The largest share of spending was 'uncategorized'—in reality, this will have been spread across the other categories. ATM withdrawals constituted the other large category. Recall Australia is mostly a cashless society, with cash accounting for around 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 may suggest cash withdrawals were not only likely predominantly used for spending but spending predominantly on non-durables.[48]

All other categories had spending impacts below $400, or less than 5% of the total. We

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

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

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.

have included debt repayments for reference, but these are not included in our spending estimates. Spending was notably 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, above credit card repayments.

While we cannot distinguish precisely between durables and non-durables,[49] there are many categories with highly statistically significant spending impacts that we can infer to be non-durables (e.g., food delivery); these sum to around half of additional spending in discernible categories. Some of the spending in the other discernible categories (e.g., 'retail'), uncategorized, ATM withdrawals, and external transfers will also have been on non-durables. So the high observed spending out of Super-withdrawals does not appear to have been accounted for overwhelmingly by durables.

5 Heterogeneity

5.1 Marginal propensity to spend

The panel nature of the bank transactions data enable us to estimate individual-level treatment effects. For each treated unit, we compute the difference in average spending between the three weeks before and the three weeks after withdrawal (Figure 9b).[50,51] Naturally, these individual-level treatment effect estimates are subject to error, as spending may vary across three-week periods for reasons other than Super withdrawal. To get a sense of this underlying variation, among the same individuals we estimate a placebo distribution by taking the average difference in spending between the three weeks prior to withdrawal and the three weeks prior to that, and then dividing by the subsequent withdrawal amount (Figure 9a).

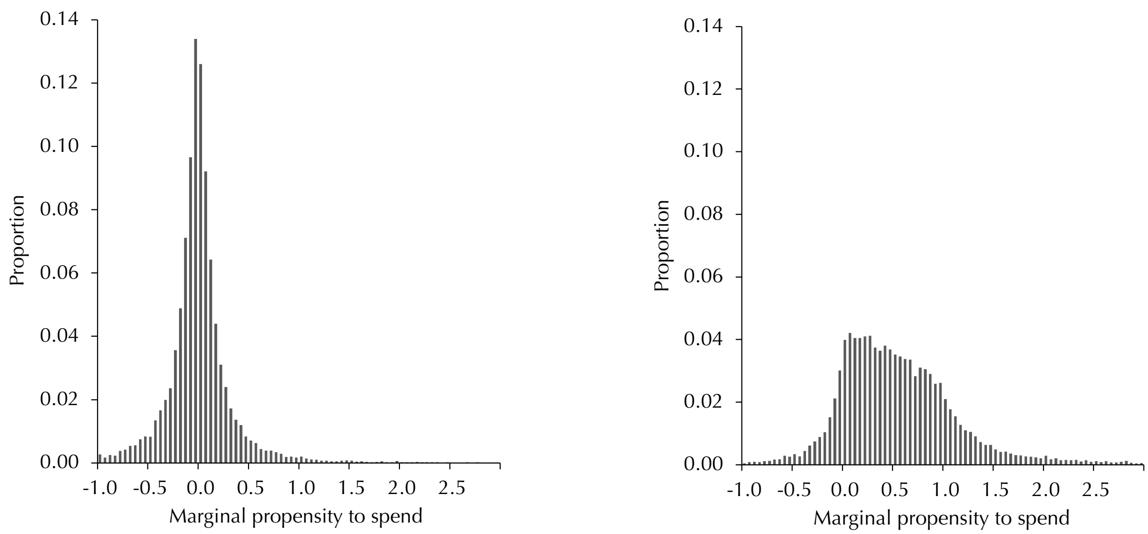
With fewer degrees of freedom, this method cannot address the same concerns as our main difference-in-differences procedure; for example, it does not account for trends in spending or factors affecting spending other than Super withdrawals. Four observations offer confidence. First, were we to use the non-treated average as a counterfactual for each unit, the observed distribution would shift left or right with no change in shape. Second, in Figure 7, one can see there were no pre-trends or anticipation, ruling out one concern. Third, in Figure

[49]The US Department of Commerce defines durables as those lasting three years or more.

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

[51]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: Histograms of estimated individual-level MPX estimates



(a) Placebo period

(b) Treatment period

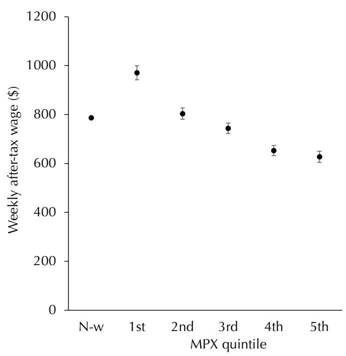
Data: Illion
Notes: Individual-level MPXs estimated via simple difference in spending across consecutive three-week periods among all withdrawers. Treatment period compares three weeks either side of withdrawal; placebo period compares three weeks before treatment and the three weeks before that. Bin size is 0.05.

9b, one can see the slope of the estimated density is discontinuous at MPXs of around zero and one, consistent with a reasonable prior. Fourth, the average MPX implied by the mass in the unit interval is 0.435, compared to 0.431 with the difference-in-differences procedure.

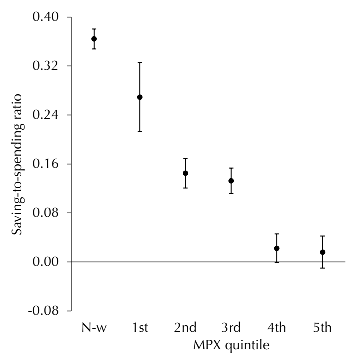
Given the error displayed in Figure 9a, the MPX density in Figure 9b appears close to uniformly distributed between zero and one, but with an approximately linear slope of -0.33. The distribution is neither bimodal (with excess mass at zero and one) nor strongly right-skewed (with excess mass only at zero). This uniformity is present in the density estimated by Karger and Rajan (2021)) for the US pandemic stimulus, in which US$1,000 checks were distributed broadly, but that density had a large spike at zero.

Possessing individual-level MPX estimates, next we investigate their correlates. Earlier, we found withdrawers had substantially worse financial health prior to withdrawal; now, we investigate how that covaries among withdrawers with their propensity to spend. In Figure 10, having split the withdrawer sample into quintiles of the estimated MPX, we display estimated means of pre-treatment variables across those quintiles. Given the near-uniformity of the MPX distribution and the random noise introduced by fluctuations in spending, the conversion to quantiles can be seen as an approximate normalization of MPXs to the unit interval. As a benchmark, we present on the left the corresponding estimated mean among those who did not withdraw (a group five times larger than the withdrawers).

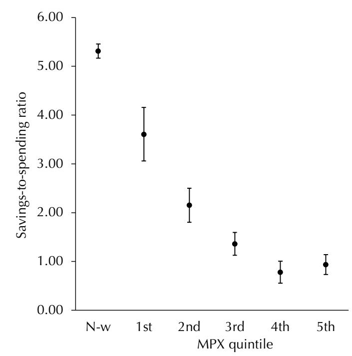
Figure 10: Estimated pre-treatment variable means by estimated MPX quintile vs non-withdrawers



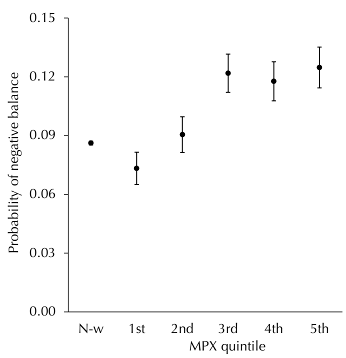
(a) Wages



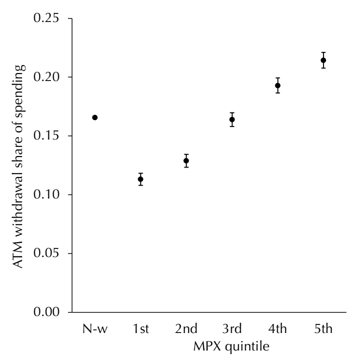
(b) Saving rate



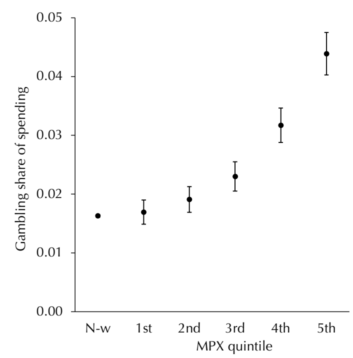
(c) Liquidity



(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.

All of the pre-treatment variable mean estimates differ with the estimated MPX in the expected directions: poorer pre-treatment financial health strongly predicts greater spending, consistent with the existing literature on substantially smaller transfers (Johnson et al., 2006; Broda and Parker, 2014; Kreiner et al., 2019). Wages varied 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 was far more dramatic. Between the first and second MPX quintiles, the saving rate halved and liquidity nearly halved. The 40% with MPXs exceeding 0.63 (around $6,000 of additional spending over eight weeks after the first round alone) 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.

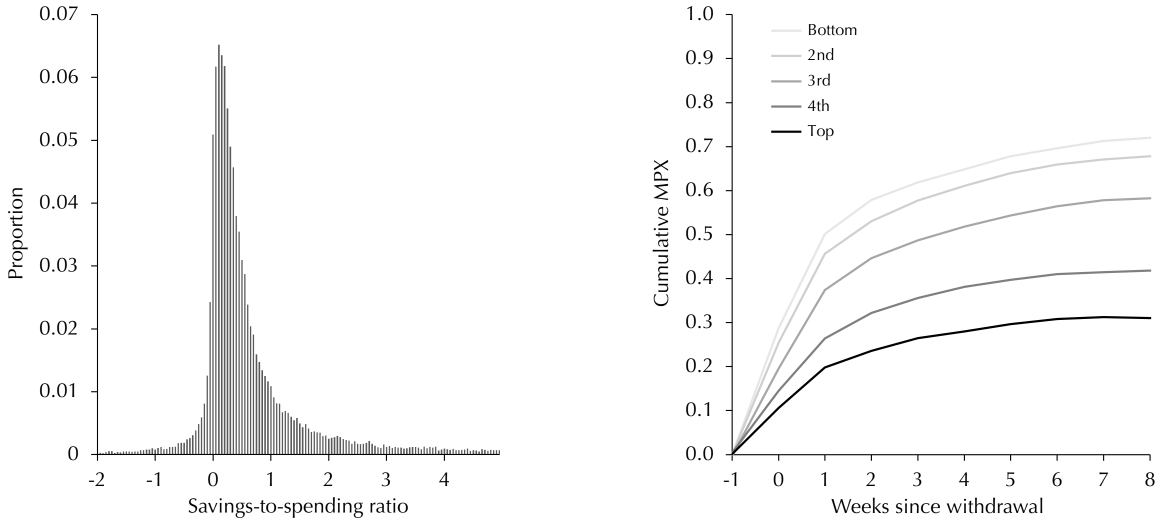
We also identify two pre-treatment spending categories that were strongly related to the MPX: ATM withdrawals and gambling. As noted earlier, Australia is predominantly a cashless society, at least in formal markets. And gambling is more lightly regulated than in the US (online betting is legal, for example), with higher average losses.[52] Recall these two categories were the largest and third-largest discernible uses of withdrawn funds. Among those with higher MPXs, both pre-treatment ATM withdrawals and gambling were substantially higher, but rates of gambling increased at an increasing rate—between the lowest and highest spenders, 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 clearly divisible: roughly 20% had MPXs below 0.09 and financial health that was not too dissimilar from the non-withdrawers; the remaining roughly 80% had MPXs above 0.09 and sharply worse financial health than the non-withdrawers. Various measures decline rapidly with the MPX and then plateau. The top 40–60% of spenders were strongly homoegeneous, being statistically indistinguishable along multiple measures.

[52] 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

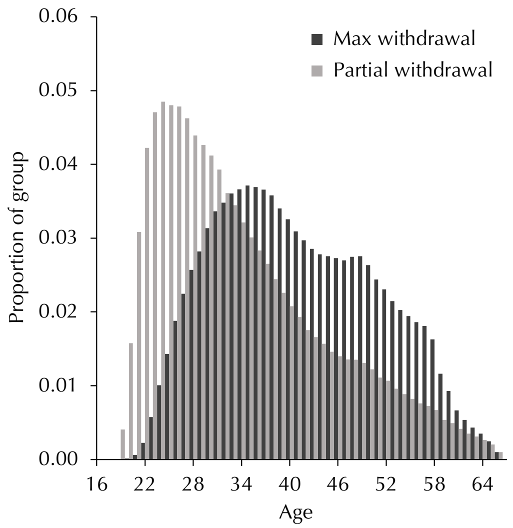
Data: Illion
Notes: Liquidity is savings divided by spending on average during the month prior to withdrawal. For each liquidity quintile, we estimate cohort ATTs via the R package, 'did', which implements Callaway and Sant'Anna (2021). We compute weekly aggregate ATTs by averaging across cohorts weighted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

5.2 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.

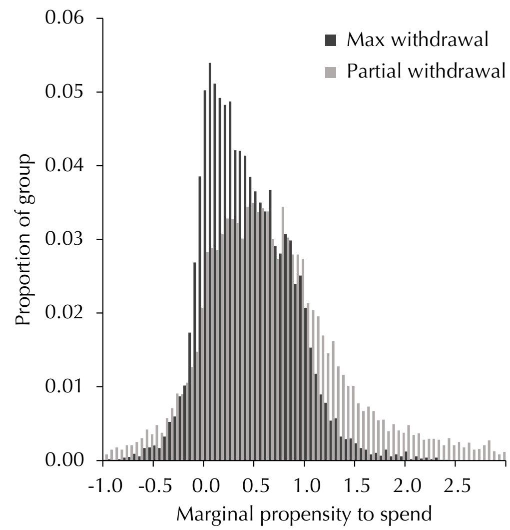
The results are displayed in Figure 11b, and indicate a substantial and monotonic increase in the MPX, for every week post-treatment, as liquidity declines. The MPX among the bottom two liquidity quintiles is similar, with the least-liquid 40% having an MPX (0.68–0.72) more than double that among the 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.

Figure 12: The relationship between age, withdrawal amount, and spending



(a) Age histogram by withdrawal amount

Data: Australian Taxation Office
Notes: First withdrawal. Only working-age people with positive Super balances includes.



(b) MPX histogram by withdrawal amount

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.

5.3 Age

Recall the withdrawers were three years younger on average, but both younger and older people were underrepresented. Unfortunately, we do not observe age directly in our bank transactions data, so we cannot estimate directly the relationship between age and the MPX. Instead, we construct a proxy for age. Super balance is highly correlated with age because mandatory contributions are a fixed percentage of wage income. Further, partial withdrawals (less than $10,000) are substantially explained by the withdrawer having an available balance less than $10,000, which is concentrated among the young (Appendix K). This generates a strong relationship between partial withdrawal and age. While we do not observe age or Super balance in our bank transactions data, we do observe the withdrawal amount.

In Figure 12a, relying on the administrative data, we present age densities among those who withdrew $10,000 and those who withdrew less, which reflect this relationship. In Figure 12b, relying on our bank transactions data, we present the estimated MPX densities among those who withdrew $10,000 and those who withdrew less. Rather than being roughly uniformly distributed between zero and one, as is the case among the partial withdrawers, the mass of full withdrawers (the older group) is tilted towards zero. Using the same method as for our main estimates, we estimate an MPX for the full-withdrawers of 0.40 (0.01) and for the partial-withdrawers of 0.51 (0.04), indicating the younger group spent significantly more.

6 Discussion

6.1 Models

Heterogeneity has for decades been central to our understanding of the consumption response to fiscal stimulus (Campbell and Mankiw, 1989). Deepening this understanding can help us improve the design of stimulus programs, as well as government policies in a wide range of other areas. The Super withdrawal program offers a unique setting in which to study these responses, being particularly helpful to disentangling their heterogeneity. We interpet our findings in the context of the heterogeneity considered in the literature to date.

Broadly, this includes three types. 'Ricardian' types are rational, forward-looking, and liquid; as a result, they conform to the permanent-income hypothesis (PIH) (Barro, 1974; Friedman, 1957; Ricardo, 1820). 'Rational-hand-to-mouth' (RHTM) types, who may be 'poor' (i.e., have little to no net wealth) or 'wealthy' (i.e., have substantial net wealth), are rational and forward-looking but liquidity constrained; as a result, they deviate from the PIH (Kaplan and Violante, 2014). 'Behavioral' types have time-inconsistent preferences, such as present bias; as a result, they too deviate from the PIH (Laibson, 1997; Laibson et al., 2021).[53]

The RHTM and behavioral types are distinguished by their form of discounting. The former exhibit standard exponential discounting, which in the absence of liquidity constraints generates consumption smoothing. Kaplan and Violante (2014) present the following standard short-run Euler equation for these types:

$$u'\left(c\left(x_t\right)\right) = r_{t+1} \cdot \delta \cdot u'\left(c\left(x_{t+1}\right)\right),$$

where x is liquid wealth, c is consumption, r is the rate of return on liquid wealth, and δ is the exponential discount factor. Among present-biased types, this Euler equation becomes:

$$u'\left(c\left(x_t\right)\right) = r_{t+1} \cdot \left[c'\left(x_{t+1}\right) \cdot \beta \cdot \delta + \left(1 - c'\left(x_{t+1}\right)\right) \cdot \delta\right] \cdot u'\left(c\left(x_{t+1}\right)\right),$$

where the discount factor is now a weighted average of the quasi-hyperbolic discount factor in the short run, $\beta \cdot \delta$, and that in the long-run, δ (Harris and Laibson, 2001). If the consumer expects their MPC to be high in future, perhaps because they exhaust available liquidity, then their discount factor is closer to $\beta \cdot \delta$, driving up current consumption.

The RHTM types incur a transaction cost of drawing on (or contributing to) their high-return illiquid wealth, which makes them willing to incur a sizable drop in consumption in response to a temporary drop in income. Conversely, a temporary increase in current income may temporarily boost their consumption as it allows them to get closer to their 'smooth' target level. Given a sufficiently large income shock (or a sufficiently low transaction cost),

[53] There are other behavioral models, considered briefly later. Here, we focus on predictions under present bias.

RHTM types will be willing to incur the transaction cost, become unconstrained, and then behave like Ricardian types, smoothing consumption over time.

In practice, both RHTM and behavioral types will tend to have low liquidity, just for different reasons. And while they may respond similarly to a small transfer, the welfare implications are very different: additional liquidity raises the welfare of the RHTM types, but it may lower that of the behavioural types. So distinguishing between them—for example, to design stimulus that targets one but not the other—is of central importance.

6.2 Predictions

The Super withdrawals have two features that distinguish them from cash stimulus. First, under Ricardian equivalence, cash stimulus is a mandatory loan from the government, whereas the Super withdrawals were optional and self-financed. The former implies higher future taxes, while the latter implies lower future retirement balances. This opt-in feature created the potential for a selection effect. All other things equal, Ricardian types should not have participated, eliminating the spike in MPCs at zero typically observed for cash stimulus. The RHTM types may have participated, to relieve their liquidity constraint or rebalance their portfolio towards higher-liquidity assets. The present-biased types should have participated to increase current consumption.

But because subsequent re-contributions were not only possible but subsidized (up to \$7,528 for those on the top marginal tax rate) depending on income and existing voluntary contributions), it would have been rational for many Ricardian and RHTM types to have withdrawn.[54] On the other hand, they may be subject to frictions such as insalience, status-quo bias, or a self-control mechanism as per Attanasio et al. (2020), where RHTM types choose low liquidity as a commitment device given temptation preferences. This would be consistent with Choi et al. (2011), who find a lack of take-up of employer 401(k) matches despite the financial incentives (and no liquidity disadvantage) of doing so. So, *ex ante*, it is unclear what type-mix among the withdrawers we should have expected and thus how selection would affect the MPC.

The second feature was the size of the transfers—at least an order of magnitude larger than standard cash stimulus. This is critical because the models generate distinct predictions of how the consumption response evolves as transfer size increases (Kaplan and Violante, 2014; Laibson et al., 2021). Debate in the literature has centered on the proposition that,

[54]In each of the 2020 and 2021 tax years, people were entitled to make \$25,000 in voluntary contributions to their Super account pre-income-tax and \$100,000 post-income-tax. This means someone who otherwise would have made \$13,235 or less in voluntary contributions would have been allowed to contribute the additional \$11,765 in each tranche necessary to exactly offset two \$10,000 super withdrawals (after incurring the flat 15% Super contribution tax). Because the contributions would have been made pre-income-tax, post-tax personal income would have fallen by \$6,236 for those on the top marginal tax rate of 47% or \$7,706 for those on the modal marginal tax rate of 34.5%—implying net subsidies of \$3,764 and \$2,294, respectively, for each round.

for the modest cash transfers we typically observe, behavioral motives like present bias are not necessary to expect sizable consumption responses. This is because the transfers act as loans that allow RHTM types to get closer to their 'smooth' target consumption level without drawing on their illiquid wealth. Under a modest transfer, the RHTM and behavioral types may behave similarly, being co-located throughout the MPC distribution.

As the size of the transfer rises, however, additional liquidity provides no additional benefit to the RHTM types now smoothing, driving down rapidly the average MPC. The behavioral types, on the other hand, continue to benefit from additional current consumption. Accordingly, Kaplan and Violante (2014) predict that raising the transfer from $500 to $5,000 would lower the MPC of the RHTM types from 0.20 to 0.03, while Laibson et al. (2021) predict the MPC of the behavioral types would fall only from around 0.13 to 0.11.[55] The modal Super withdrawal was almost triple that amount.

Given a sufficiently large transfer, the models generate two straightforward predictions: 1) conditional on selection, the average MPC should be lower the greater is the share of RHTM types; and 2) the types should sort by MPC, with the RHTM types bunched near zero and the behavioral types continuing to be distributed across MPCs above that. As illustrated in Figure 13a, this isolates a group of behavioral types at higher MPCs. While this separation might not be discernible in the observed MPC density, it generates a level-shift in conditional expectations of variables that differ across types (Figure 13b). This suggests a simple visual diagnostic for determining the share and characteristics of behavioral types.

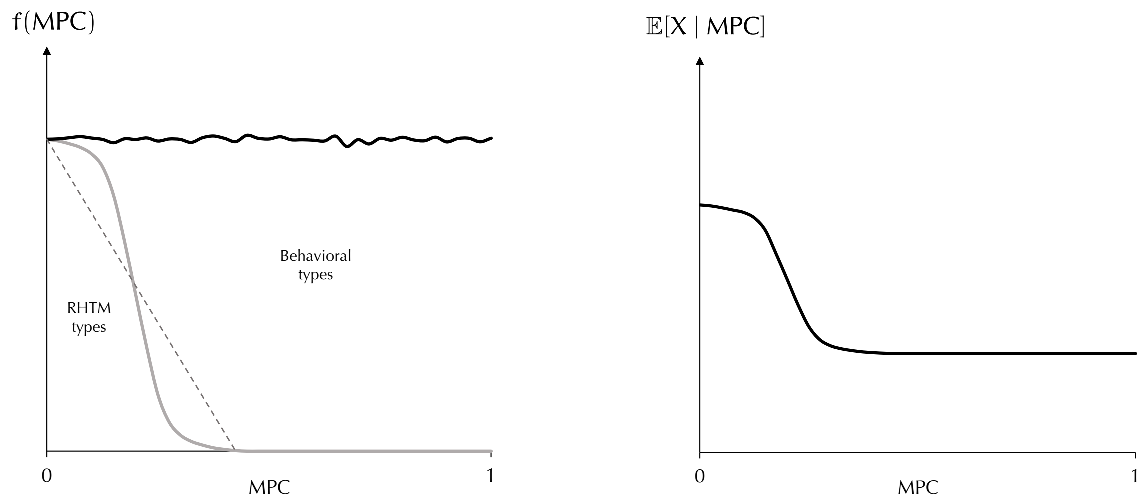
6.3 Interpretation

While 83% of working-age people did not withdraw Super, those who did had a high intensity of demand for liquidity. They tended to withdraw as soon as possible, as much as possible, and twice if funds were available, suggesting the vast majority remained constrained even after having withdrawn around half median annual wage income. On average, they withdrew more than half their Super balances, constrained by the maximum withdrawal amount. That so few withdrawers chose an 'interior' solution suggests they were predominantly behavioral types, with many Ricardian and RHTM types facing frictions in accessing Super for consumption smoothing or portfolio rebalancing.

There were also stark differences between those who chose to withdraw and those who did not. The withdrawers were younger, far more likely to be in a blue-collar profession, and more likely to live farther from the major cities. Withdrawers had only moderately lower wages, but this difference was persistent not transitory: wages were lower immediately prior to withdrawal, in the three years prior, over the working life to date, and there was no drop in wages around the time of withdrawal. Despite having only moderately lower wages, the

[55] Levels are not directly comparable across the two models.

Figure 13: Hypothetical illustration of predicted PDF and CEF under a large withdrawal



(a) PDFs for the RHTM types and all withdrawers

(b) Conditional expectation function of observed variable

Notes: Hypothetical example with 80% behavioral types and transfer size sufficient to generate bunching of RHTM types near zero. In the example, almost all behavioral types have MPCs above 0.2, wherein they are uniformly distributed. The plateauing range of a conditional expectation function of a constant with a higher value among the RHTM types covers top 60% of MPCs. Linear approximation divides lower 40% in half to estimate 80% behavioral types. This approximation is conservative given RHTM density may be convex at low MPCs.

withdrawers had radically lower saving rates, savings, and levels of financial assets, and these differences too were persistent. They were particularly pronounced among those who withdrew earlier or withdrew a second time.

This indicates strong selection into withdrawal, but it is difficult to distinguish clearly between types on the basis of lower wages and liquidity alone. However, the *persistence* of lower wages and liquidity does suggest a behavioral explanation (Parker, 2017). In models of rational, forward-looking people, liquidity constraints typically do not bind indefinitely as declines in permanent income are matched by declines in permanent consumption. One possible rationalization of a permanently binding liquidity constraint is the Super system's mandatory, flat contribution rate over the life cycle, which likely encourages over-saving early in the working life. This could make the constraint bind over many years as younger workers desire to draw on their future incomes. But if this were the motive for Super withdrawal, what withdrawers did with their withdrawals would then serve as a test of time-consistency: when the constraint is loosened, they should smooth their withdrawal over many years.

Yet, we find a very high propensity to spend given the size of the transfers of at least 0.43—in the order of spending responses to cash transfers an order of magnitude smaller (e.g., Leigh (2012) estimated an MPX of 0.41–0.42 out of $950 cash transfers distributed in Australia in 2009). This burst of spending—up 129% over two weeks and 46% over eight weeks—was short-lived, with spending returning to normal within eight weeks. Spending was broadly allocated across categories, with roughly half of discernible categories clearly non-durables like restaurant meals. There was an even larger response to the second withdrawal at 0.48, which three quarters returned for and which occurred after activity had recovered substantially. The high propensity to spend was not concentrated among outliers, with responses distributed broadly across the unit interval. Those in the worst financial health—with the lowest wages, the lowest rates of saving, the least liquidity, the greatest likelihood of being overdrawn—spent by far the most. Rates of spending were also strongly positively related to pre-withdrawal gambling and cash withdrawals.

The high observed MPX is a function both of selection into withdrawal and the size of the transfers. The characteristics that predicted high MPXs also predicted withdrawal (and earlier withdrawal and second withdrawal), suggesting selection raised the average MPX. For example, in addition to those with low liquidity, the program selected heavily on those from their late twenties to their early forties, and younger people appeared to spend more out of their withdrawals, consistent with evidence that younger people make worse financial decisions than middle-aged people (Lusardi et al., 2010; Agarwal et al., 2009). The absence of a spike in MPXs at zero, as is common with standard cash stimulus, suggests the Ricardian types tended not to select into the program, raising the average MPX. The distribution of responses among RHTM consumers is predicted to be strongly right-skewed; the observed distribution being close to uniform suggests those who did not want to spend simply did not withdraw. The fact the MPX was similar to those out of cash transfers an order of magnitude smaller suggests behavioral types participated disproportionately.

For a sense of degree, consider our estimates of pre-treatment conditional means by MPX quintile (Figure 10) in the context of the models' predictions of the sorting of types by MPC under a very large transfer (Figure 13). Along multiple dimensions, there appeared to be a clear level shift, with values declining rapidly at low MPXs and then plateauing at higher MPXs. We might expect characteristics to vary across MPXs due to within-type variation. But the sharpness of the level shifts in a number of variables seems more consistent with a transition between types, as the models predict. Notwithstanding such a type transition, there was also strong variation within the behavioral types across MPXs, with pre-treatment gambling increasing at an increasing rate and wages and cash withdrawals varying linearly.

This simple visual diagnostic suggests roughly 80% of withdrawers were behavioral types, translating into a conservative lower bound of around 20% of all 34-year-olds in the population being behavioral types (and at least 5% being RHTM types). This is a lower bound because we would also expect a portion of those who did not withdraw also to be behavioral types. Given Kaplan and Violante (2014) estimate roughly a third of US households are RHTM types and assuming a similar share applies in Australia, this suggests such types chose overwhelmingly not to participate in the program despite the substantial financial incentives available. The diagnostic implies the average MPX out of the first withdrawal was around 0.54 among the behavioral types and around 0.08 among the RHTM types.

6.4 Implications

We contribute to a recent and active literature generating empirical evidence in favor of a behavioral explanation for the sensitivity of consumption to fiscal transfers. Parker (2017) presents survey evidence on the 2008 US stimulus checks, the consumption response to which is predictable based on income several years prior, and is associated with sophistication and planning, and impatience. Ganong and Noel (2019) study the predictable exhaustion of unemployment insurance (UI) in the US, observing a lack of the anticipation we would expect among rational, forward-looking (but liquidity-constrained) people. Their evidence suggests there is variation in the degree of present bias across recipients, consistent with our finding of a broadly distributed MPX. And Ganong et al. (2022) find the supplementary pandemic UI payments in the US raised consumption substantially, consistent with our finding of consumption rising by 136% over the first two weeks.

Something we do not do is detail exactly what it is about the recipients as people that led them to engage in time-inconsistent behavior. Our evidence on pre-treatment ATM withdrawals and gambling, and age was striking—based on the literature in clinical psychology, this suggests a link between impulsivity and the MPX.[56] The survey evidence of Parker (2017), that the consumption response is associated with sophistication and planning, and impatience, seems consistent with this, and points to a role for better financial literacy (Lusardi and Mitchell, 2014; van Rooji, Lusardi and Alessie, 2012). Moreover, our settling highlights that even the 83% who did not participate, many of who we might otherwise think of as rational and forward-looking, were evidently subject to behavioral frictions, perhaps in the form of the commitment device proposed by Attanasio et al. (2020). Overall, deeper investigation of the mechanisms behind the behavioral types' responses to fiscal transfers is needed.

The Super withdrawal program serves as a cautionary tale for the design of private retirement saving systems, which have gained renewed interest as the US Social Security trust funds near depletion.[57] The US 401(k) system is unique globally in its high degree of liquidity (Beshears et al., 2015), loosened even further during the pandemic, with Vanguard reporting early withdrawals reached 2.8% in 2022, compared to a pre-pandemic average of 2%.[58] Under

[56] Maclaren, Fugelsang, Harrigan and Dixon (2011) conduct a meta-analysis of 44 studies in clinical psychology, noting a link between urgency or impulsivity and gambling. Canale, Vieno, Griffiths, Rubaltelli and Santinello (2015) use experimental and survey evidence to establish a link between age, urgency, and gambling.

[57] `https://thehill.com/policy/finance/3843718-pence-weve-got-to-have-a-conversation-about-reforming-social-security/`

[58] `https://www.wsj.com/articles/short-on-cash-more-americans-tap-401-k-savings-for-emergencies-11675305976`

the Super withdrawal program, the vast majority of people who were eligible chose not to access their retirement savings when given a seemingly one-off opportunity to do so, and the vast majority of those who did appeared to be driven by present bias. Accordingly, as a matter of retirement saving policy, it is difficult not to view illiquidity as welfare-improving in aggregate—consistent with its ubiquity in retirement saving systems outside the US (Fadlon and Laibson, 2022).

As macro-stabilization policy, the welfare impact is not so clear cut. To the extent it overcame fiscal constraints, it improved macro-stability, increasing welfare—including among the withdrawers. 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. The question is whether macro-stability is funded by the lower future retirement incomes of the few or higher future taxes on the many, the consequences of which depend on the tax and transfer system. To the extent withdrawers are net-beneficiaries of that system, they may have been relatively worse off under the program.

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 under traditional cash stimulus—the Super withdrawal program also highlights a perversity of cash stimulus generally. The greater the present bias, the more effective is cash stimulus (Laibson et al., 2021). But, by definition, those induced to spend are making themselves worse off, at least directly, by doing so—and others better off (Auclert, Rognlie and Straub, 2023). Their error will be felt when eventually they have to pay the taxes necessary to fund that macro-stability. How this all nets out depends on the distributional properties of the tax and transfer system. The application of a comprehensive welfare frame to stimulus under present bias could provide a valuable guide to 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'.[59] 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.[60] The median balance by age in 2019 is shown in Table 1.[61]

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.[62] 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,[63] 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 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

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

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

[61] 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.

[62] 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/`

[63] 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).

inherited assets.[64]

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.[65] 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 8.3%, with four years posting losses (the largest of which was 21.5% in 2008).[66] 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.[67] 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.

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

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

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

[67]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.

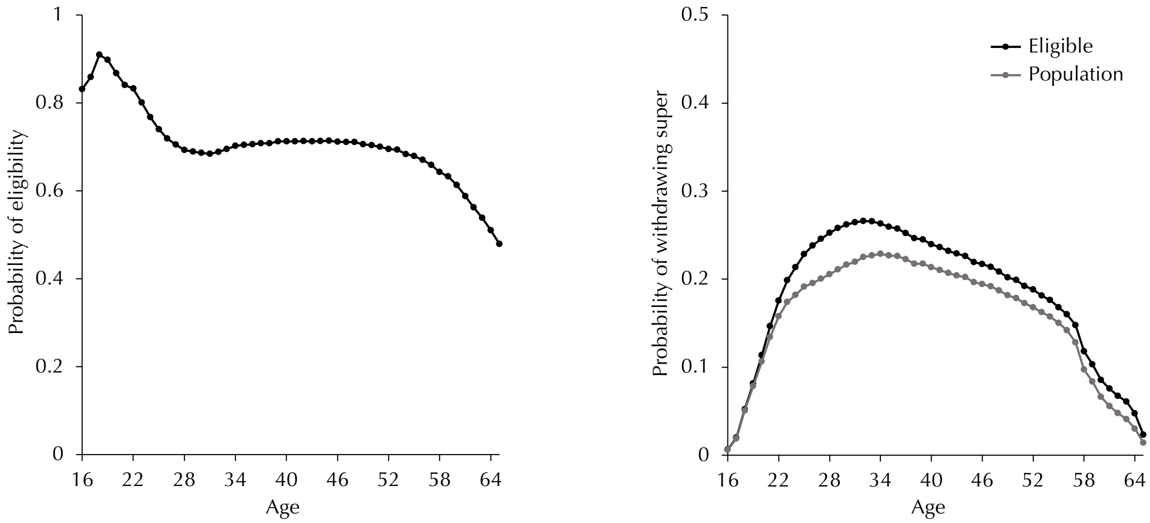
During the application process, applicants had to nominate a criterion according to which they were eligible. In the first withdrawal, 48% stated a reduction in working hours, 19% each being unemployed and being eligible to receive a government benefit (government payments go to more than a quarter of 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.[68] 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 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

[68]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`.

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.

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 2 but among the eligible (Table A1). 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.

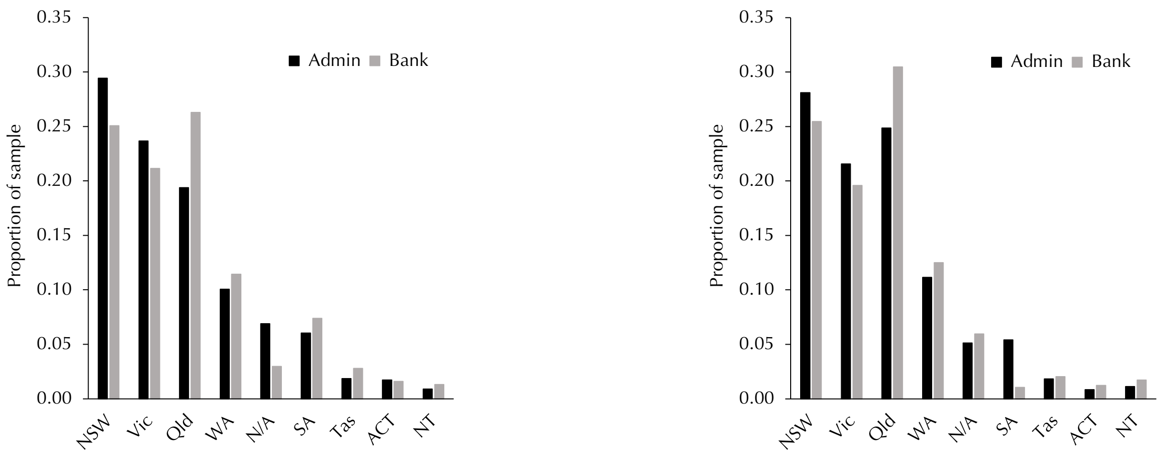
Table A1: Estimated differences in means between eligible withdrawers and non-withdrawers for first withdrawal

	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.

C Data comparison

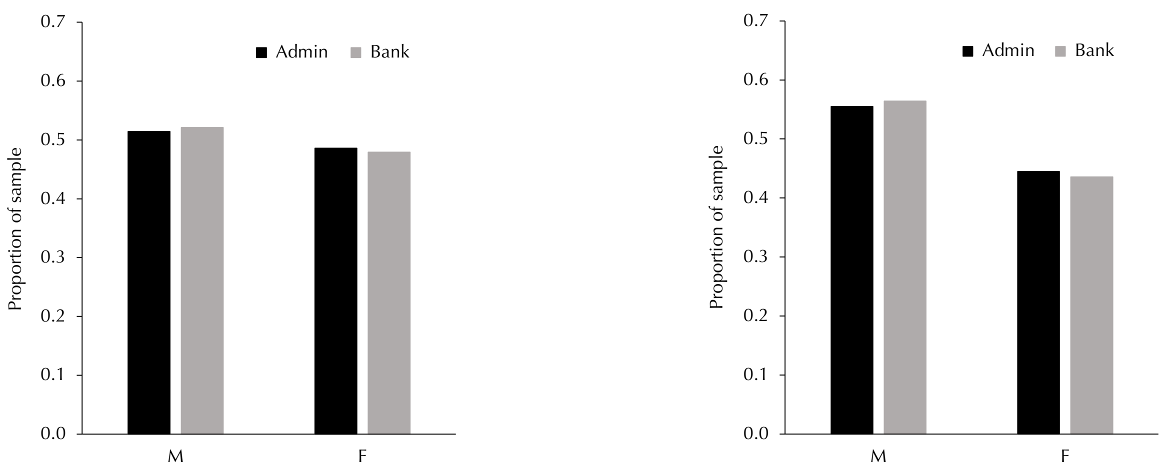
Figure A2: State distribution by sample



(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.

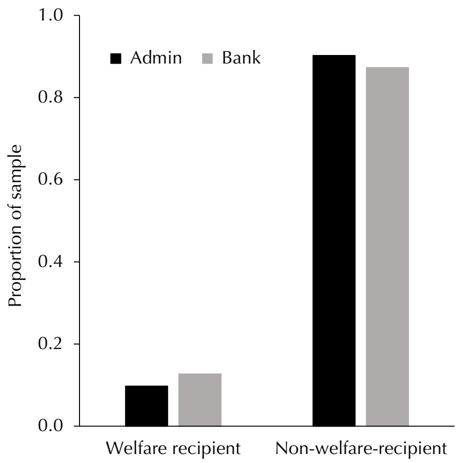
Figure A3: Sex distribution by sample



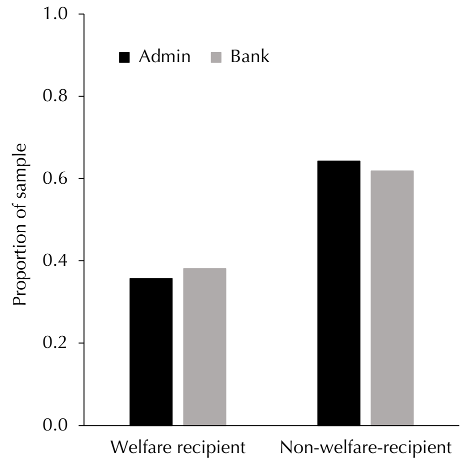
(a) Everyone (b) Withdrawers

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



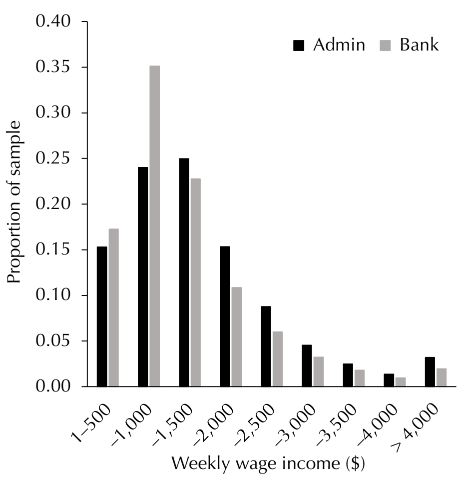
(a) Everyone



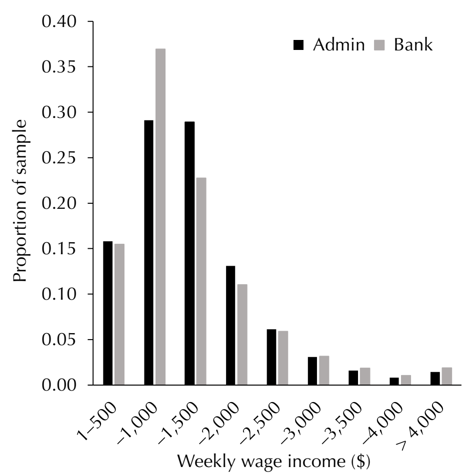
(b) Withdrawers

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

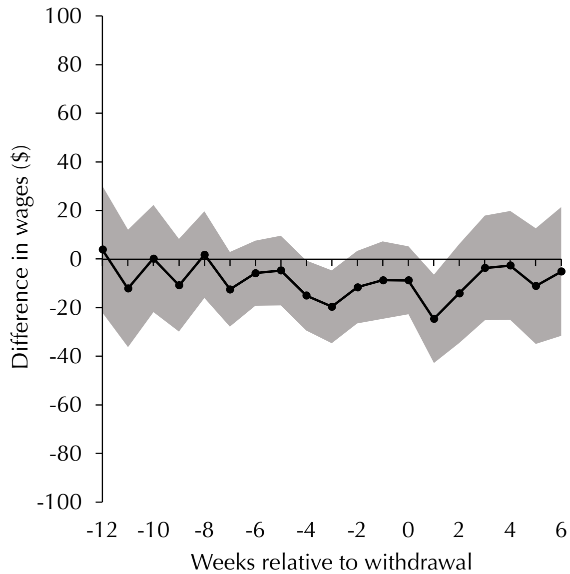


(b) Withdrawers

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

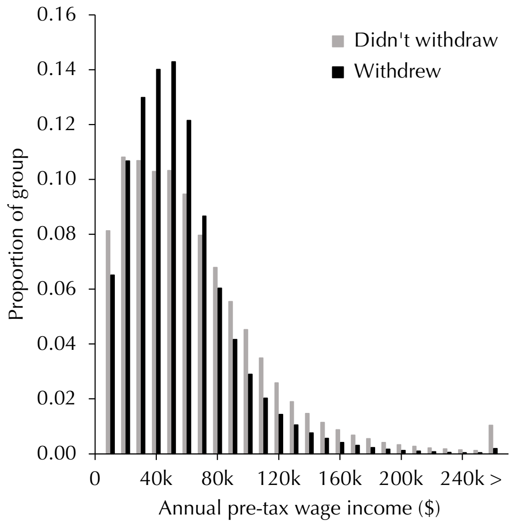


Data: Australian Taxation Office

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.

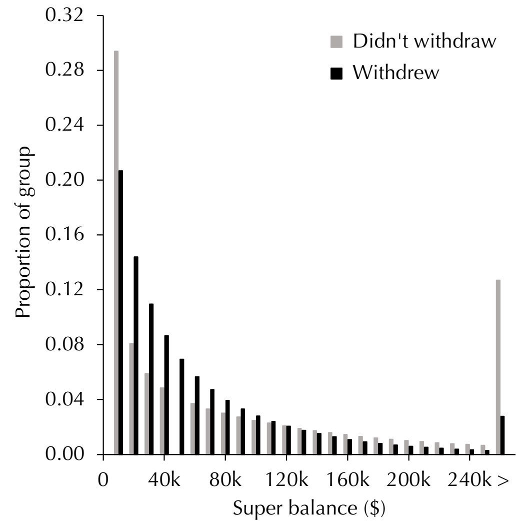
E Wage and Super balance densities

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 A2: 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 A3: 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 2. 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 2.

H Estimated average treatment effects on the treated

Table A4: 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.

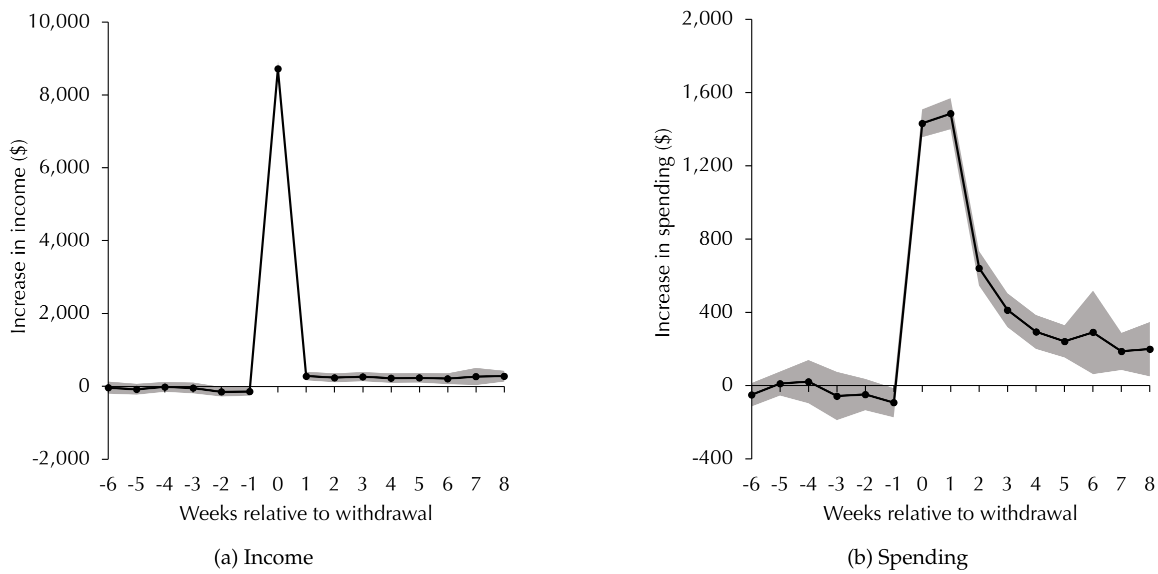
Table A5: Estimated ATTs on spending of the first withdrawal

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



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

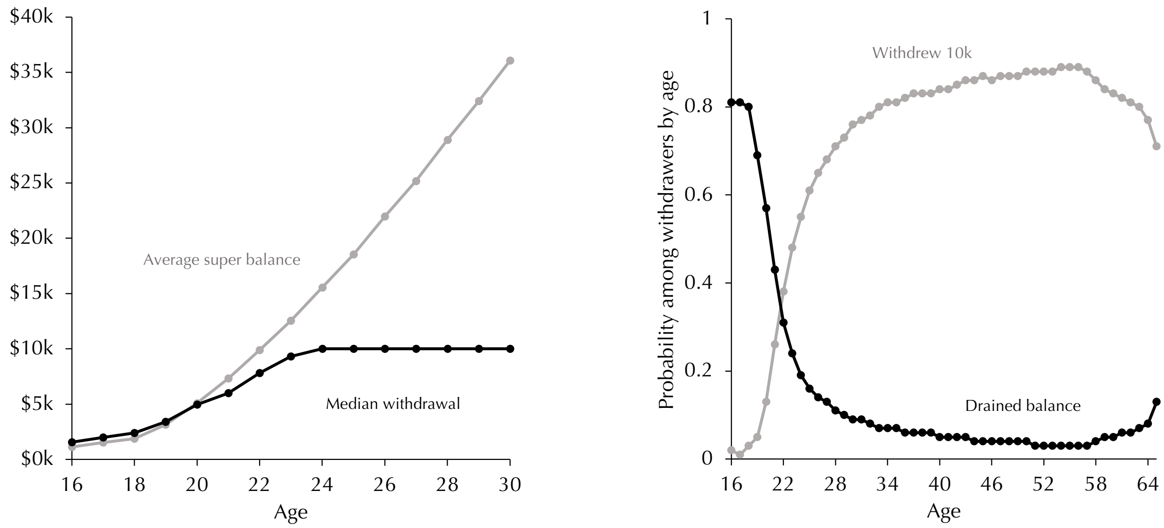
Table A6: Estimated cumulative aggregate ATTs (CAATTs) of the first withdrawal by category

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.