A $100,000 marshmallow experiment: Withdrawal and spending responses to early retirement-savings access*

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Abstract

During the pandemic, Australia allowed the withdrawal of A$20,000 from mandatory retirement accounts (ordinarily inaccessible until retirement), forecasted to cost the modal-aged withdrawer A$120,000 in today's dollars at retirement. One in six withdrew 2% of GDP. Using administrative and transactions data, we find a marginal propensity to spend of 0.43–0.48 within eight weeks, with gambling the largest spending category. Poor financial health and gambling strongly predict withdrawal and spending. We develop a heterogeneous-agent model, demonstrating that while impatience under liquidity constraints reconciles the observed withdrawal behavior, only present bias reconciles the magnitude and frequency of the observed spending response.

JEL codes: E21, E63, E71, H31, H55, J32;
Keywords: Retirement savings, marginal propensity to consume, present bias

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1 Introduction

In the 1970s, researchers at Stanford University ran a series of lab experiments on children to study delayed gratification. The most famous involved leaving a child alone in a room with a marshmallow after having instructed them that if they did not eat the marshmallow, then they would be offered a second marshmallow or a pretzel stick (Mischel and Ebbesen, 1970). The researchers recorded the child's willingness to wait for a reward and how this was influenced by external stimulii. Follow-up studies indicated that those with a greater willingness to wait tended to have better later-life outcomes such as test scores, educational attainment, and a lower body mass index (Mischel, Shoda and Rodriguez, 1989; Shoda, Mischel and Peake, 1990; Ayduk, Mendoza-Denton, Mischel, Downey, Peake and Rodriguez, 2000; Schlam, Wilson, Shoda, Mischel and Ayduk, 2013). Recent work in economics has shown that, among children involved in such early experiments, survey measures over the life-cycle of self-regulation predict capital formation later in life (Benjamin, Laibson, Mischel, Peake, Shoda, Wellsjo and Wilson, 2020).

We study a 'natural' analog to the marshmallow experiment, but among an entire national working-age population and with a financial reward in the order of $100,000. During the COVID-19 pandemic, the Australian government for the first time allowed eligible people to withdraw early up to A$20,000 (US$13,600)[1] across two tranches from their mandatory private retirement savings accounts (called 'Superannuation' or 'Super'), theretofore inaccessible until retirement.[2,3] The liquidity of retirement savings was loosened in over 30 countries during the pandemic, though perhaps only Chile had a program quite so extensive as Australia's (OECD, 2021; Fuentes, Mitchell and Villatoro, 2023). This was a large, voluntary, one-off, and tax-free transfer directly to a withdrawer from their future self. The modal withdrawal ($20,000) by the modal-aged withdrawer (34) can be expected to reduce their balance at retirement by more than $120,000 in today's dollars.[4]

We ask: who chose to withdraw, what did they do with it, and why? We make

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

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

[3]We know of four papers on this program. Sainsbury, Breunig and Watson (2022) use the same admin data to study the effect of program participation on employment outcomes. Wang-Ly and Newell (2022) study the program using coarser and more limited data (e.g., monthly, no admin data) from an Australian bank, but find a similarly large spending response. Preston (2022) and Schneider and Moran (2024*b*) leverage household survey data to study the characteristics of the withdrawers.

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

three main contributions. The first is to describe in detail those who chose to withdraw their retirement savings when given the opportunity. The withdrawers differ strongly from the non-withdrawers along many dimensions, most notably in their poorer financial health and gambling expenditures. The second is to document a very large and sharp spending response among those who withdrew, with 90% occurring within just four weeks, concentrated mostly among non-durables, and with gambling the largest discernible category. We also document strong heterogeneity in this response, predicted by poor financial health and gambling. The third is to develop a model to distinguish formally the roles of liquidity constraints and preferences in determining selection into the program and the large and sharp spending response we observe. While liquidity constraints are necessary, they are not sufficient. And while heterogeneous exponential discounting (impatience) can explain selection into the program, it cannot explain the observed spending patterns. Heterogeneous hyperbolic discounting (present bias), on the other hand, is able to reconcile both selection into the program and spending conditional on withdrawal. To the best of our knowledge, our study is the first to verify present bias at a national scale, with one-quarter of all 34-year-olds participating in the program.

We begin by using administrative records on the full working-age population and a large panel of weekly bank transactions to study selection into the program. One in six working-age people (and one in four 34-year-olds) participated, withdrawing $38 billion (2% of GDP) in total. Among those who withdrew, the average withdrawal was 51% of the available balance. We find that most withdrawers remained constrained even after having accessed roughly half of median annual wage income in additional liquidity: five in six withdrew as much as possible, nearly half withdrew within the first 10 days of availability, and three quarters of those who had funds remaining after the first withdrawal chose to withdraw a second time.

Participation is strongly predicted by the characteristics of participants. Those in 'blue-collar' occupations and those located farther from cities were far more likely to withdraw. The highest-withdrawing occupation was construction and mining laborers (40% withdrew) and the lowest was teachers (6% withdrew). Looking across local areas, lower college attainment and greater socio-economic disadvantage are strongly predictive of withdrawal. Withdrawers had slightly lower wages, but this gap was persistent rather than transitory: wages were lower in the two months prior, the three years prior, and the working life to date. There was no discontinuity in weekly wages around the time of withdrawal among those who withdrew versus those who did not, suggesting that withdrawal was not motivated by a temporary loss of income. Withdrawers had far lower rates of saving and levels of savings, both immediately prior to withdrawal and in the three years prior, and they had substantially lower stock and investment property holdings. These differences were more pronounced among those who withdrew earlier or withdrew a second time.

Next, we study the spending response. Using modern difference-in-differences techniques (Callaway and Sant'Anna, 2021; Sun and Abraham, 2021) and exploiting the differential timing of withdrawal to compare spending changes across withdrawers and non-withdrawers, we find a high marginal propensity to spend (MPX) out of the first withdrawal of at least 0.43 over eight weeks.[5] By applying the estimator separately to each week of withdrawals, and then computing the MPX as a weighted average of these cohort-level estimates, we avoid well-known problems with two-way fixed-effects estimators with differential treatment timing (Goodman-Bacon, 2021). By employing two-way fixed effects, we control for all observed and unobserved confounders that vary across individuals but not over time (e.g., that withdrawers and non-withdrawers are different) or vary over time but not across individuals (e.g., pandemic-related panic buying). Identification then relies on a standard parallel-trends assumption, for which extremely stable pre-trends provide support. Furthermore, if selection into treatment were a major concern, one would expect to observe depressed spending among non-withdrawers at the time of withdrawal, but we do not. The fact that the majority of the spending response was on non-durables suggests against selection via re-timing.

The spending response (including ATM withdrawals but excluding debt repayments and external transfers) was spread broadly across categories, with 31 of the 40 discernible spending categories exhibiting increases that were statistically significant at the 90% level. Gambling was the largest discernible spending category, with more spent on gambling than on credit card repayments. On average, withdrawers spent an additional $4,000 over eight weeks, a nearly 50% increase. Of this, 71% occurred within the first two weeks and 90% within four weeks. During the first week following withdrawal, spending was on average nearly two-and-a-half times

[5] While a substantial share of the population participated in the program, our use of weekly bank transaction data and the sharpness of the observed spending response mitigates general-equilibrium effects of the policy on our estimates given prices and wages are sticky, taking time to adjust. Indeed, according to CPI data from the Australian Bureau of Statistics, quarter-to-quarter inflation in June 2020 and September 2020 was -1.9% and 0.7%, respectively.

its pre-withdrawal level. The MPX out of the second withdrawal, which two-thirds returned for, and which occurred after COVID-19 had effectively been eliminated and economic activity had recovered substantially, was 0.48 (0.01), even higher than in the first round. This suggests that, if anything, pandemic conditions dampened the observed spending response. Applying quantile difference-in-differences to spending changes, we find that the MPX was near-uniformly distributed up to 0.95 but with a long right tail (5% had an MPX of greater than one and 1% greater than 2.3). Among withdrawers, a higher propensity to spend was strongly predicted by several measures of poor financial health, as well as higher pre-withdrawal gambling and cash withdrawals. On average, the least-liquid 40% spent more than double that of (or $2,500 more than) the most-liquid 20%.

Finally, we develop a heterogeneous-agent model incorporating retirement, two assets (liquid savings and illiquid retirement savings), idiosyncratic income risk, borrowing constraints, and preference heterogeneity, enabling us to study the effects of the early superannuation withdrawal program on withdrawal and spending. We model the program as an announced temporary reduction in the (arbitrarily high) cost of withdrawing savings from the illiquid retirement account. We consider two versions of the model: one with time-consistent individuals with varying degrees of patience, as in Aguiar, Bils and Boar (2024); and another with time-inconsistent individuals, as in Laibson, Maxted and Moll (2024), but with varying degrees of present bias. We calibrate each version to match the pre-withdrawal liquidity distribution, program participation rate, and average withdrawal amount.

The observed withdrawal behavior (the propensity to withdraw, the average withdrawal amount, and the bunching of withdrawals near the maximum) can be explained by either version of the model using parameter values (discount factors and present-bias parameters) that are consistent with the recent literature (Aguiar et al., 2024; Ganong and Noel, 2019; Gerard and Naritomi, 2021). However, only present bias can account for the large and immediate observed spending response. Calibrated at the monthly frequency, impatient-but-liquidity-constrained withdrawers smooth their withdrawals excessively, even if at their borrowing limit. We obtain the best fit with naively present-biased households with an average present-bias parameter of $\beta = 0.63$, within the range of recent estimates across very different contexts (Ganong and Noel, 2019; Gerard and Naritomi, 2021). This version of the model also matches well the joint distribution of MPCs and liquidity.[6] The calibration frequency is critical. The exponential version of the model is able to match the data much more closely at a quarterly frequency than at a monthly frequency as the liquidity shock is a smaller multiple of wages and the predicted spending response is more dispersed.

On the face of it, there are a variety of preferences and market features with the potential to explain the propensity to withdraw or to spend. Australia's mandatory retirement saving rules might have led some to oversave, which they might rationally seek to reverse. Or some might rationally have been induced by the illiquidity of their retirement savings to rebalance their portfolios towards higher liquidity when given the chance. Our calibration exercise highlights that liquidity constraints play a crucial role but are not sufficient to explain the consumption and saving behaviors we observe. The underlying behavioral characteristics are critical to the model's ability to jointly match the liquidity distribution, withdrawal decisions, and spending responses. Time-inconsistency is key. The discount factors necessary for exponential discounting to generate a sufficiently large and sharp spending impulse imply far lower pre-withdrawal liquidity than is observed in the data. Hyperbolic discounting, by its nature, is able to reconcile these discordant stocks and flows.

We contribute to an emerging literature across diverse contexts in which high-frequency spending data reveals an excess sensitivity of spending to income that can only be rationalized by present bias. Ganong and Noel (2019) consider the exhaustion of unemployment benefits in the US, documenting a non-durables spending decline of 12%, consistent with a large share of recipients being present-biased ($\beta = 0.5$). Gerard and Naritomi (2021) consider São Paulo, Brazil, where workers laid off without cause are entitled to severance of 4.7 months' wages on average, estimating a spending jump of 35% despite a permanent-income decline of 14%, consistent with present bias ($\beta = 0.44$). Gelman (2022) finds that present bias ($\beta = 0.9$) best explains spending responses in the US to paychecks and tax returns. There is also an experimental literature on present bias. Augenblick and Rabin (2019), for example, offered participants a series of unpleasant transcription tasks in exchange for varying wages at different times, finding that $\beta = 0.83$. The relative novelty of our study is that we are able to verify present bias among a substantial

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

share of an entire national working-age population.

We also contribute to an empirical literature on early withdrawals, though at a smaller scale than we consider (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; Argento, Bryant and Sabelhaus, 2014). The closest precedent occurred in Denmark in 2009, studied by Kreiner, Lassen and Leth-Petersen (2019). There are some important differences between their setting and ours. The Danish system was relatively new and small: it was introduced 11 years earlier and had been closed to contributions for five years (vs 27 years earlier and ongoing for Super); contributions were 1% of earnings (vs 10.5% for Super); and all savings received the same, flat return subject to income taxes on withdrawal (vs no taxes on withdrawal and other tax concessions for Super). Consequently, after taxes, the average withdrawal was US$1,900, an order of magnitude lower than the modal Super withdrawal of US$13,600. The authors combine survey data on spending with administrative data on assets to establish a relationship between liquidity-constraint tightness, as measured by the marginal interest rate, and spending. In contrast, our focus—aided by rich, high-frequency administrative and bank 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 a literature on the optimal illiquidity of retirement savings. The key trade-off is between welfare gains from enabling people to smooth consumption and welfare losses by enabling people *not* to smooth consumption (Beshears, Choi, Clayton, Harris, Laibson and Madrian, 2022).[7] The Super withdrawal program generates a direct test of this, enabling us to ascribe selection into the program overwhelmingly to present bias rather than a rational desire to smooth consumption.[8] As a matter purely of retirement-saving policy, this implies illiquidity is welfare-improving in aggregate. This has implications for the design of retirement saving systems, which has gained renewed interest as the US Social Security trust funds approach insolvency. 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. And there have been recent proposals to allow people

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

[8]One potential reason why many did not withdraw despite a clear incentive to do so is that they may have considered Super to be a self-control mechanism (Attanasio, Kovacs and Moran, 2024).

to borrow against their future Social Security benefits.[9]

We also contribute to a vast macro literature documenting much larger responses to cash transfers than would be predicted under the permanent-income hypothesis (Friedman, 1957).[10] Scholars have proposed two explanations for this 'excess sensitivity': 'behavioral', considering deviations from rational, forward-looking behavior, such as present bias (Angeletos, Laibson, Repetto, Tobacman and Weinberg, 2001; Parker, 2017; Laibson et al., 2024);[11] and 'rational', considering liquidity constraints, taking as given rational and forward-looking behavior (Zeldes, 1989; Deaton, 1991; Carroll, 1997; Kaplan and Violante, 2014). To the best of our knowledge, the Super withdrawal program is unique in this literature in generating very large transfers to a broad swath of the population, *holding lifetime income constant*. Studies of stimulus checks consider transfers in the order of US$1,000, the responses to which may not differ markedly under different consumption models. Transfers from the Alaska Permanent Fund were larger (US$3,900 for an average family), but still far smaller than the Super withdrawals (up to US$27,000 for a family) and received regularly (Hsieh, 2003; Kueng, 2018). Lottery winnings are the largest transfers considered, but they increase lifetime income, often substantially (Imbens, Rubin and Sacerdote, 2001; Kuhn, Kooreman, Soetevent and Kapteyn, 2011; Fagerang, Holm and Natvik, 2021; Golosov, Greber, Mogstad and Novgorodsky, 2024).

We also contribute to the literature on fiscal multipliers (Ramey, 2019). The early withdrawal program had two unique features: 1) the transfers were opt-in; and 2) the transfers were self-financed. Early withdrawals were used in 30 countries during the pandemic (OECD, 2021). We find that withdrawals generated at least 0.8% of GDP in direct spending, almost entirely within a four-month period. Based on the stated up-front fiscal cost of 0.06% of GDP, this implies a direct fiscal multiplier of 13.4, at least an order of magnitude larger than with cash stimulus.[12] By selecting on those with high MPXs, the program raised the numerator, and by being self-financed, it

[9] `https://www.rubio.senate.gov/rubio-releases-pro-family-framework-following-dobbs-decision/`

[10] Examples include Johnson, Parker and Souleles (2006), Shapiro and Slemrod (2009), Sahm, Shapiro and Slemrod (2010), Parker, Soulless, Johnson and McClelland (2013), Broda and Parker (2014), and Agarwal and Qian (2014). Several recent studies have documented large responses to transfers received during the COVID-19 pandemic, including Chetty, Friedman, Hendren and The Opportunity Insights Team (2024), Yannelis and Amato (2023), Karger and Rajan (2021), Kubota, Onishi and Toyama (2021), Baker, Farrokhnia, Meyer, Pagel and Yannelis (2023), Baker, Farrokhnia, Meyer, Pagel and Yannelis (2020), and Coibion, Gorodnichenko and Weber (2020).

[11] Pfäuti, Seyrich and Zinman (2024) offer a recent alternative behavioral explanation that a subset of people are persistently over-confident / over-optimistic, with similar behavioral implications to present bias.

[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 policy.

lowered the denominator. Under fiscal constraints, self-financing enables additional stimulus at the cost of the sub-optimal future consumption of the present-biased. In recent work, Schneider and Moran (2024*a*) highlight the distributional consequences of delivering stimulus in this way. An unanswered question remains 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.

Lastly, we contribute to the literature on gambling; in particular, to an emerging literature on online sports betting in light of its legalization in the US following a 2018 Supreme Court ruling (Baker, Balthrop, Johnson, Kotter and Pisciotta, 2024; Hollenbeck, Larsen and Proserpio, 2024; Taylor, McCarthy and Wilbur, 2024). An Australian parliamentary inquiry into online gambling heard from multiple 'problem gamblers' who withdrew their Super to fund online gambling (Parliament of Australia, 2023). The clinical psychology literature suggests a link between gambling and impulsivity (Maclaren, Fugelsang, Harrigan and Dixon, 2011). Relying on a large Australian household survey, Schneider and Moran (2024*b*) find that self-control issues are the strongest predictor of early Super withdrawals, with individuals in the top quintile of self-control issues 60% more likely to withdraw early than those in the bottom quintile. Because we observe gambling expenditures in our bank transactions data, we show that pre-withdrawal gambling strongly predicts withdrawal as well spending conditional on withdrawal. Moreover, gambling was the largest discernible use of the funds. Our findings establish a clear connection between the degree of present bias and gambling.

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

2 Preliminaries

2.1 Policy

Australia has a compulsory, defined-contribution private retirement saving system called Superannuation (or 'Super').[13] All employers are required to contribute an additional 10.5% of pre-tax wages to their employees' accounts, inaccessible outside exceptional circumstances until age 58 if retired or 65 if working. For most people, Super is tax-preferred over most other forms of saving: employer contributions are made pre-income-tax, then taxed at 15% on entering the fund (compared to a modal marginal income tax rate of 34.5% and a top rate of 47%); before retirement, cash returns are taxed at 15% and capital gains at 10%; and in retirement neither returns nor withdrawals are taxed. Voluntary top-ups can be made, up to a total of $27,500 per year before tax and $100,000 per year after tax, but less than 1% of people are above the pre-tax cap.[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. 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".[16] The government stated the fiscal cost as $1.2 billion, which reflected only foregone taxes over four years (not the $38 billion eventually withdrawn).[17] The government would later add broad eligibility conditions.[18,19] 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

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

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

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

[18]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.

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

a reason for withdrawing, self-assessing eligibility. Applications were processed in three days on average, at which time the withdrawal was deposited into the applicant's bank account.

Australia's experience of the pandemic was far milder than most other countries'. Australia closed its borders, with international travel virtually impossible for non-citizens and highly restricted for citizens for the first 18 months. From March 28, all international arrivals had to complete two weeks of quarantine. Public health measures were imposed locally until cases were eliminated. Australia's per-capita death rate was less than a tenth that in the US. Meanwhile, Australia had a large federal fiscal response at 17% of GDP (excluding 2% of GDP in Super withdrawn). The Super withdrawal program was part of the second of three rounds of fiscal support announced during March 12–30.[20] The other main supports were a broad wage subsidy, supplementary unemployment insurance, and cash transfers to government benefit recipients. While the initial outbreak generated a substantial reduction in mobility, this recovered rapidly as cases were eliminated (Figure 1a), and the decline in personal net income excluding Super withdrawals was negligible (Figure 1b). Through May and June, all domestic restrictions lifted nation-wide.[21]

2.2 Data

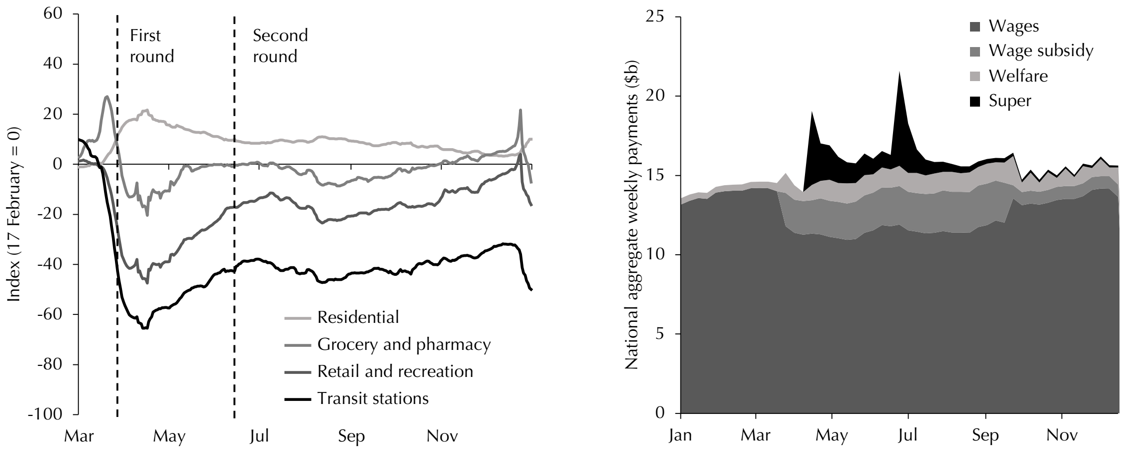
We rely on two data sources: administrative data covering all working-age Australians, which allow us to study the withdrawals; and bank transactions data covering a subset of people, which allow us to study how the withdrawals were used. We are not permitted to link individuals across the two datasets.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

We access the administrative data via the Australian Bureau of Statistics' Multi Agency Data Integration Project (MADIP), which links de-identified, individual-level datasets across agencies. For our purposes, these cover all working-age (16–65) Australians who reported positive Super balances in the financial year July 1, 2018

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

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

Figure 1: Economic activity indicators in Australia during 2020



(a) Mobility indicators by establishment

Data: Google trends
Note: Physical locations visited.

(b) Working-age payments by type

Data: Australian Taxation Office and Department of Social Services

to June 30, 2019 (2018-19), which yields a sample of 15.2 million (versus 25 million in the population). In MADIP, we observe age, sex, and location at the suburb level. Via personal tax records, we observe: occupation; a spouse indicator; the number of dependents; three years of tax returns, including the income derived from wages, interest, rent, and dividends; the Super balance at June 30, 2019; all Super contributions for 2018-19; and all information on the Super withdrawal program for all 4.5 million approved applications,[22] including the withdrawal amount, date, and reason. Via the Single Touch Payroll system, we observe all weekly pre-tax wages. Via the welfare payment system, we observe all weekly government benefits, including unemployment benefits and pandemic support payments. We also observe all fortnightly wage subsidies that each worker's employer received.

The bank transactions data are provided by Illion, one of Australia's three large credit bureaus. Data are collected during credit-check events initiated by Illion clients, including telcos, utilities, and financial institutions (more than 6,000 in total). When an individual triggers a credit-check event, Illion collects all bank transactions across their accounts, including transactions made with associated debit and credit cards over the prior 13 weeks. The original transactions data include the date and time the transaction was processed (usually within a few business days for

[22]3.05 million people lodged 4.78 million applications, of which 232,000 were rejected.

card purchases and instantly for transfers), description, transaction type (e.g., card payment, direct debit, external transfer), transaction value, and account. The dataset we use is an aggregated version of this transaction-level data. Transactions are classified by Illion into spending categories (e.g., supermarkets, retail, department stores) and income categories (e.g., wage income, Super withdrawal, welfare income) using the transaction description and type. External transfers are not counted towards spending. Single transactions are aggregated into weekly transaction amounts across all accounts for each spending and income category. This results in an individual-level panel of weekly spending and income by category, with a 13-week observation window for each individual.

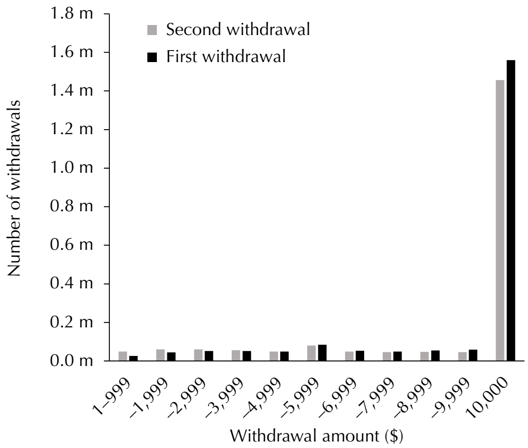
Observing a limited window for each individual introduces the possibility of assigning someone who withdrew before the start of their window as not having withdrawn. For the sample of withdrawers in the first round, we exclude those who: 1) did not have a recorded Super withdrawal, and 2) had an observation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

3 Withdrawal

3.1 Withdrawers v non-withdrawers

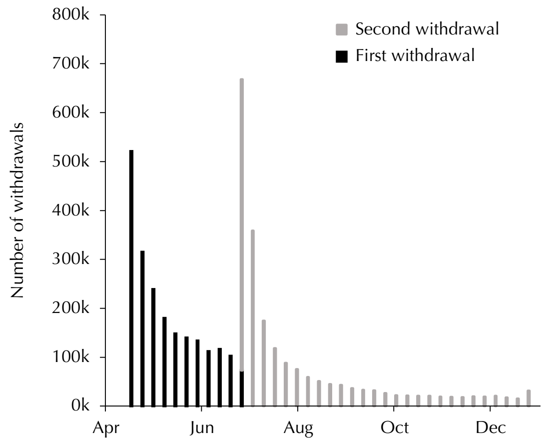
We begin with some descriptive facts about withdrawal. Around one in six (2.6 million) working-age people withdrew $37.8 billion (around 1% of assets or 2% of GDP) in total. Including those who did not withdraw, the average withdrawal was 9% of the balance; among only those who did withdraw, it was 51%. Around three quarters in each round withdrew the maximum $10,000, with the modal withdrawal across the two rounds $20,000 and the average $13,584 (Figure 2a). Among those who withdrew less than $10,000 in the first round, one third drained their account, suggesting they were constrained by their available balance. This

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

means only around one in six chose an 'interior' withdrawal amount. Around three quarters of those who still had a positive balance remaining after the first round withdrew again, with a similar pattern of withdrawals to the first round (Figure 2a). In the two rounds, 25% and 30% withdrew within the first three days and 40% and 48% within the first 10 days (Figure 2b).

Next, we compare the pre-withdrawal characteristics of those who withdrew and those who did not.[23] In the first column of Table 1, we present means among the non-withdrawers, and in the remaining columns we present differences in means between the withdrawers and non-withdrawers, with the third through fifth columns including controls for pre-treatment wages and then cumulatively for the variables listed under 'Demographics'. Those who withdrew were on average three years younger, five percentage points more likely to be male, 12 percentage points more likely to be single, and eight percentage points more likely to have dependents.

In probing the motivation for withdrawal, we are particularly interested in withdrawers' financial circumstances. Those who withdrew had lower wages, both during the month before withdrawals commenced (3% lower) and during the three years before (8% lower). We find no change in individual-level weekly wages coincident with withdrawal.[24] Withdrawers had around half the Super balances

[23]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 1.

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

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

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

Data	N
A: Administrative data	15,249,488
B: Bank transactions data	336,809

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

of non-withdrawers, which mechanically reflects lower wages over the working life to date.[25] Collectively, these results indicate that the difference in wages was persistent, being present in the month prior, the three years prior, and the working life to date. Because the withdrawers were not on average suffering a temporary wage shortfall, this does not seem to be a motivation for withdrawal.

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

For further context, in Figure 3 we present the age distributions of the withdrawers and non-withdrawers along with the associated probability of withdrawal by age. As noted earlier, the withdrawers were three years younger on average, but this masks a compression of the age distribution among the withdrawers. The withdrawers were underrepresented up to age 23 and beyond age 51, with a modal age of 33. The maximum take-up rate was among those aged 34, at 23%.[26,27]

Finally, in Table 2, we divide the Australian working-age population by occupation and location and calculate withdrawal rates among these divisions.[28] There was strong variation in withdrawal along both dimensions. Withdrawal was highest in 'blue-collar' professions and lowest in 'white-collar' professions, with teachers the lowest-withdrawing occupation at 6.3% and construction and mining laborers the highest at 40.2%. Withdrawal was also strongly, monotonically, and negatively related to the proximity to cities, with those in very remote areas 40% more likely to withdraw than those in major cities. In the most remote locations, more than half of people withdrew, while in Australia's capital just 3–5% withdrew. Across loca-

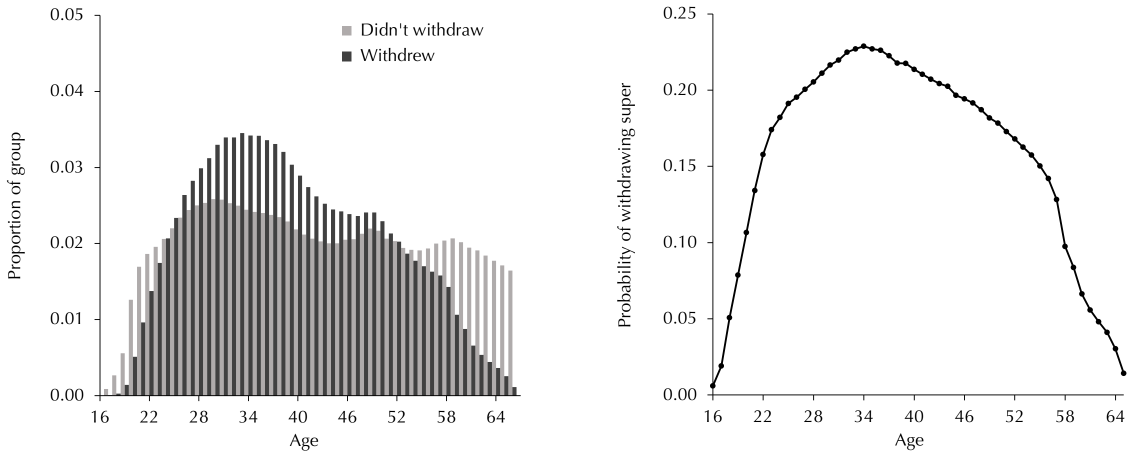
[25]Additionally, the Super balance and wage densities among the withdrawers were compressed. (Appendix E).

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

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

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

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.

tions, withdrawal is strongly predicted by college attainment and socio-economic disadvantage (see Appendix H).

3.2 Timing

People faced not only a choice of whether to withdraw but also how soon to withdraw. The differences between the withdrawers and non-withdrawers in Table 1 were greater the earlier the withdrawal (Figure 4). Setting aside the first and last weeks, which may be subject to idiosyncratic factors, the average Super balance of a withdrawer increased by 28% over the intervening seven weeks, average wages by 4.5%, and average age by 1.5 years. Average pre-withdrawal interest, dividends, and rental income all increased with the withdrawal date, a relationship that remained statistically significant even after controlling for the characteristics conditioned on earlier (Appendix G). With the program having offered temporary access to ordinarily inaccessible wealth, the front-loading of withdrawals suggests urgency.

3.3 Second withdrawal

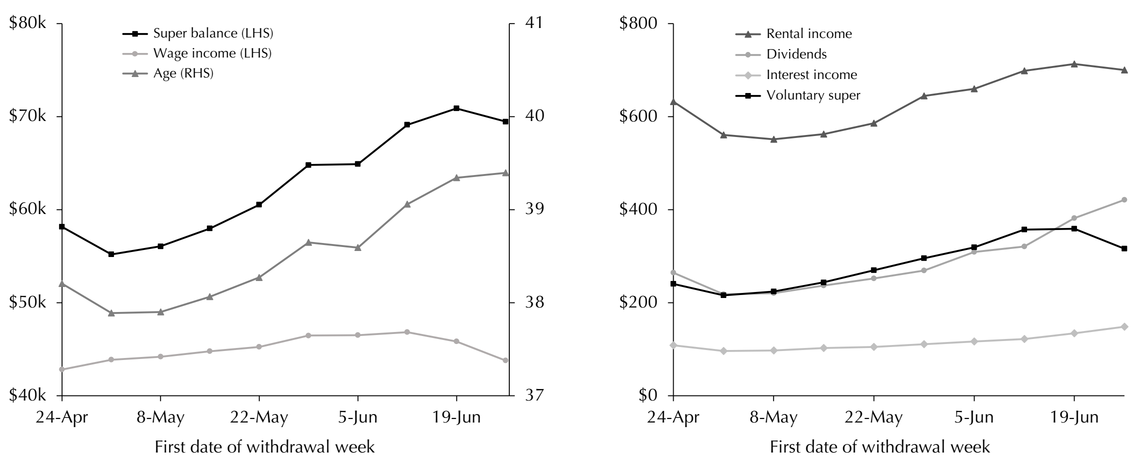
In addition to having a choice of whether and how soon to withdraw, people could also choose to withdraw a second time, 72 days after applications for first

Table 2: Withdrawal rates by occupation and location

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

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

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



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

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

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

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

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

withdrawals opened. One advantage of observing the second withdrawal is that it came well after the initial pandemic shock. While both rounds were announced in late May, the first round opened at the peak while the second round opened after COVID-19 had effectively been eliminated and activity had recovered substantially (Figure 1a). Observing a second opportunity to withdraw also allows us to gauge the intensity of the desire for liquidity.

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

4 Spending

4.1 Marginal propensity to spend

Our spending measure includes all debits associated with an individual's bank accounts (such as with a debit or credit card), cash withdrawals, and 'buy now, pay later' repayments. It excludes all debt repayments and external transfers. In Australia, cash transactions fell from 40% in 2007 to just 10% in 2019 (compared to 22% in the US).[29,30] In a 2019 official survey, 12% of those who reported holding cash outside their wallet did so to fund a large purchase, suggesting cash withdrawals overwhelmingly are used for immediate consumption.[31] Because we exclude all debt repayments and external transfers, which account for some spending, our measure can be viewed as a conservative lower bound on true spending.[32]

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

Accordingly, we consider the effect of withdrawal on income and spending in an event study, with non-withdrawers serving as our comparison group (we discuss identification later). We take the approach of Callaway and Sant'Anna (2021) and Sun and Abraham (2021), estimating average treatment effects on the treated (ATTs) *separately* for each cohort (those who withdrew in a given week) then

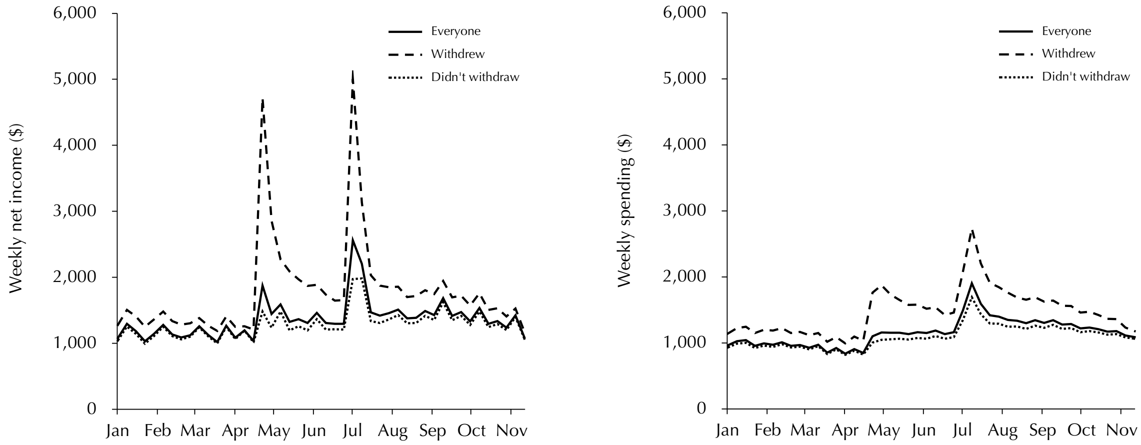
[29] https://www.rba.gov.au/publications/bulletin/2020/mar/pdf/consumer-payment-behaviour-in-australia.pdf

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

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

[32] 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.

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



(a) Income

Data: Illion

(b) Spending

Data: Illion

averaging across cohorts weighted by size. This addresses several problems with the two-way fixed-effects estimator commonly used in event studies (de Chaisemartin and D'Haultfœuille, 2020; Callaway and Sant'Anna, 2021; Goodman-Bacon, 2021; Sun and Abraham, 2021; Borusyak, Jaravel and Spiess, 2024; Wooldridge, 2023).[33]

Specifically, in Figure 6, we display cohort-specific event-study plots reflecting estimates for the following interacted TWFE model:

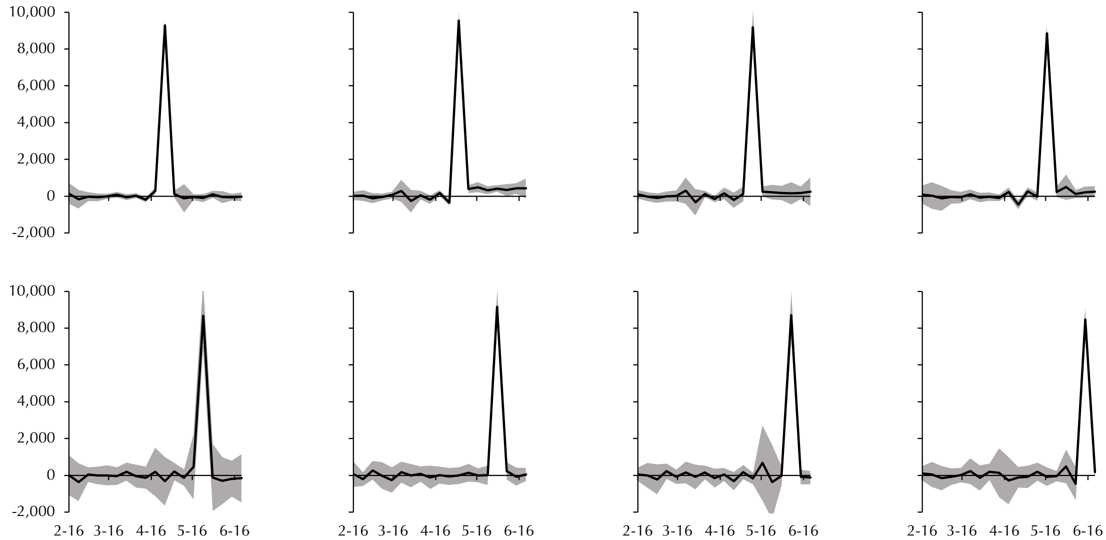
$$Z_{it} = \alpha_i + \lambda_t + \sum_{\ell \neq -1} \delta_{e\ell} \left(\mathbf{1}\{E_i = e\} \cdot D_{it}^{\ell} \right) + \varepsilon_{it},$$

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

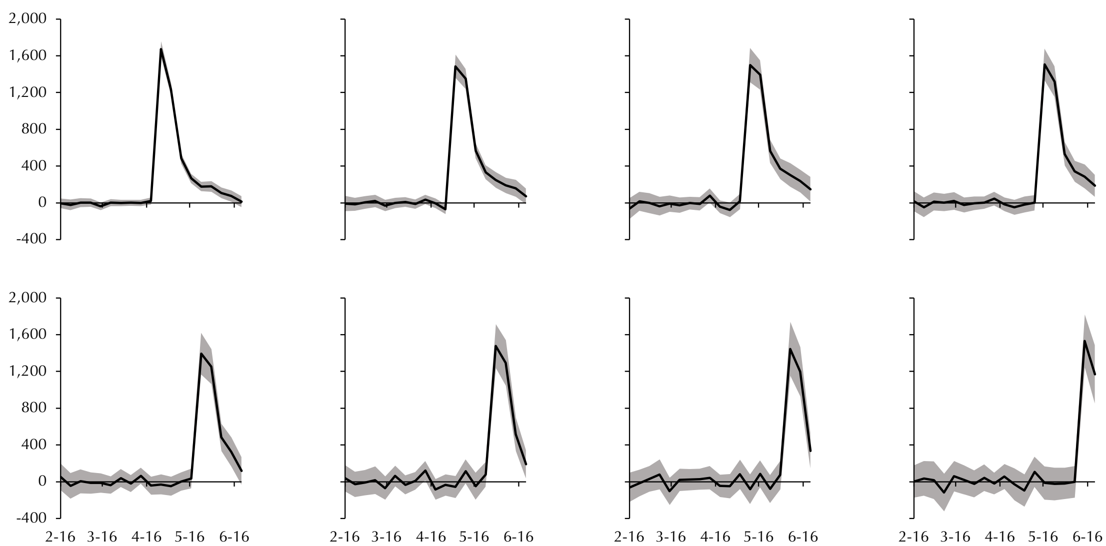
In Figure 7, we display event-study plots aggregated using cohort share weights,

[33]Of concern are situations in which: different units are treated at different times; there is no 'never-treated' group; there are heterogeneous treatment effects across cohorts; or time-varying controls are used. For a review of these developments, see Roth, Sant'Anna, Bilinski and Poe (2023).

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



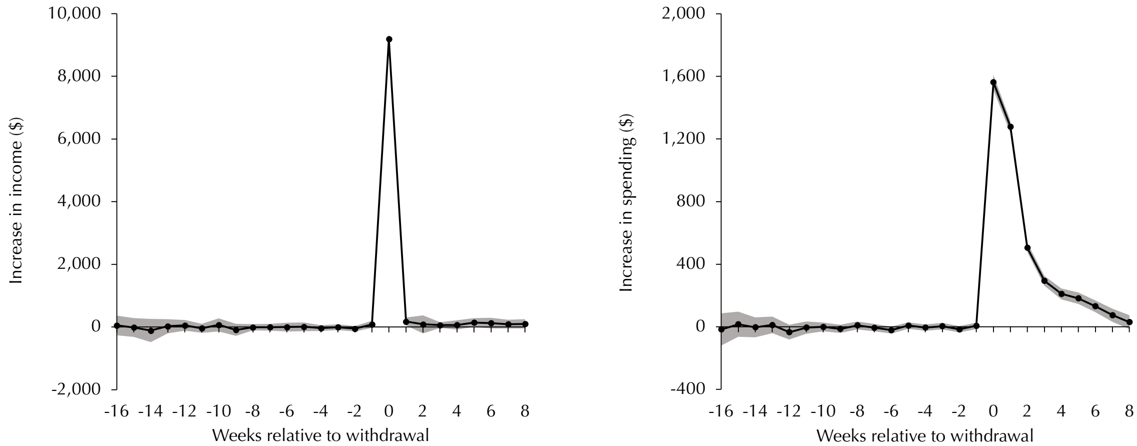
(a) Income



(b) Spending

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

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



(a) Income (b) Spending

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

$\text{ATT}_\ell = \sum_e \delta_{e\ell} \cdot \Pr[E_i = e]$.[34] We then sum these aggregate ATTs across the post-treatment periods for which we observe a positive treatment effect on spending, which based on Figure 7b is eight weeks.[35] To construct the MPX, we need to divide this by the withdrawal amount. But because non-super-withdrawal income may vary post-treatment between the withdrawers and non-withdrawers, we divide by the income analogue of our spending estimate:

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

where X is spending and Y is income.[36] We estimate standard errors via bootstrap.

In Table 4, we display the resultant estimates. Our estimated MPX of 0.43

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

[35]Appendix I includes estimated weekly cohort ATTs for income and spending.

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

Table 4: Estimated cumulative aggregate ATTs

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

Data: Illion
Notes: Results are based on cohort ATTs estimated via the R package 'fixest' (estimates in Appendix I), 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.

over eight weeks is high in the context of estimates of the effects of far smaller cash transfers—Leigh (2012), for example, estimates an almost-identical MPX of 0.41–0.42 out of $950 stimulus checks distributed in Australia during the Global Financial Crisis. Because we exclude debt repayments and external transfers, our estimate should be seen as a conservative lower bound on the true spending impact. The income and spending shocks are also large relative to their pre-withdrawal levels. The first round raised income by 93% over eight weeks. In the month before withdrawal, the withdrawers spent $1,107 per week on average. Our estimates indicate spending was 129% higher over two weeks and 46% over eight weeks.

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

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

4.2 Identification

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

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

In Figures 6 and 7, one can observe tightly parallel pre-trends in every cohort and all withdrawers overall, even without conditioning on additional time-varying covariates. There is also no evidence of anticipation by any cohort or overall despite the fact that the ability to withdraw was announced a month before the first withdrawals and almost half of withdrawals occurred in the first 10 days.

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

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

The aggregate spending in Figure 5 suggests this was not the case. Not only did spending among non-withdrawers not decline upon withdrawal, but aggregate spending (uncontaminated by selection bias as all units are present at all times) rose at a rate greater than would be explained by withdrawals alone. This was also the case with the second withdrawals, addressing concerns this may have been due to early pandemic-related spending (e.g., panic buying or home office purchases).[37] A remaining possibility is that the counterfactual non-withdrawer spending would have been dispersed across individuals across time and thus withdrawal under the program would not have resulted in a concentrated depression in non-withdrawer spending at the time of withdrawal. However, the fact that the majority of spending was on non-durables suggest against this kind of re-timing story.

Lastly, there is the question of external validity. It is worth reiterating that our estimates are average treatment effects on the *treated*. As shown earlier, the withdrawers differed markedly from the non-withdrawers; in particular, having had persistently poorer financial health. While this in and of itself does not threaten identification, to the extent it explains selection it may still have mediated the spending induced by the program, and thus have implications for the external

[37] For additional robustness, in Appendix D we present the result of an event study on weekly wages, which one might not expect to be affected by withdrawal but may have been associated with selection, showing no concomitant effect.

validity of the estimates. This effect is *ex ante* theoretically ambiguous because liquidity-constrained withdrawers may have withdrawn to finance spending or to rebuild liquidity. *Ex post*, the high spending we observe is consistent with the former. But there is no suggestion our MPX estimates apply to the broader population.

4.3 Spending categories

In Figure 8, we present estimated MPXs for each observed spending category, defined based on the merchant name.[38] 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. As noted earlier, Australia is almost a cashless society, with cash accounting for just 10% of spending. In a 2019 survey, just 12% of those holding cash outside their wallet did so to save for a large purchase, which suggests cash withdrawals were not only likely predominantly used for spending but spending predominantly on non-durables.[39] Also, we include debt repayments for scale, though these are not included in our aggregate spending estimates.

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

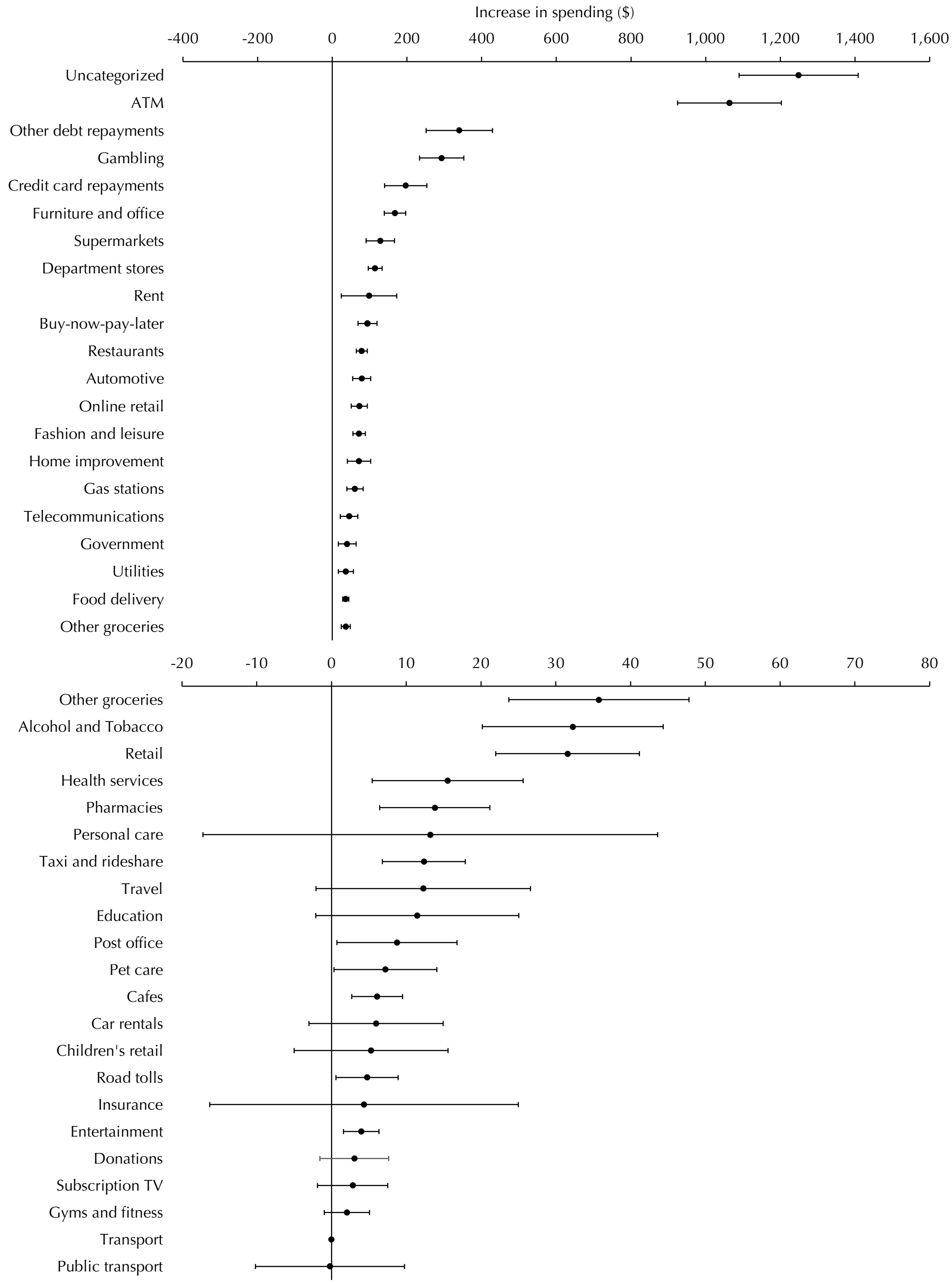
We cannot distinguish perfectly between durables and non-durables, but many of the spending categories that had highly statistically significant spending impacts are clearly non-durables (e.g., food delivery and supermarkets). Categories that are clearly non-durables sum to 60% of the spending impact across all discernible categories.[40] This is conservative as the other categories (e.g., retail) will also have included non-durables. If we assume the same non-durables share of uncategorized and ATM withdrawals, then our estimates imply a lower bound on the marginal propensity to consume (MPC) out of the first withdrawal of 0.26 (0.29 for the second).

[38] A corresponding table of results can be found in Appendix K.

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

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

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



Data: Illion

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

5 Heterogeneity

5.1 MPX distribution

In our bank transactions data, we observe individual income and spending before and after withdrawal, allowing us to estimate individual-level treatment effects. Specifically, for each treated unit we compute the difference in average spending between the three weeks before and the three weeks after withdrawal, then divide by the withdrawal amount (Figure 9b).[41,42] As is evident in Figure 9b, these estimates are subject to error. But because we observe a pre-period for both the withdrawers and non-withdrawers, we can remove its time-invariant component specific to the withdrawers and its time-varying component common to both groups.

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

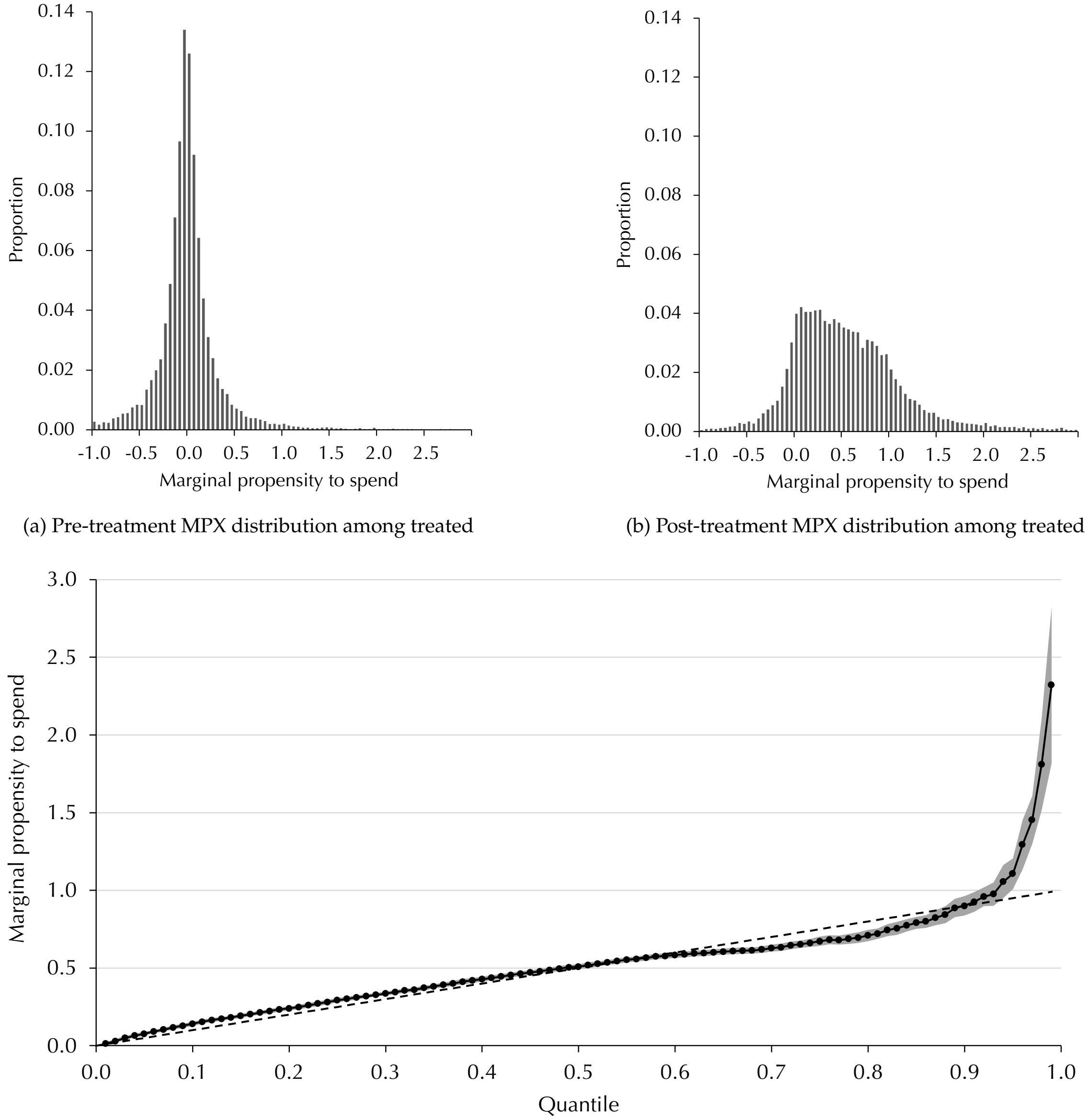
These spending differences then serve as the four quadrants in a two-by-two difference-in-differences setup, but applied to quantiles rather than means; i.e., quantile difference-in-differences (Athey and Imbens, 2006). This differs from the quantile-regression approach taken in previous studies (Misra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

This distribution has two notable features. First, for the lowest 95%, the distribution is near-uniform (the dashed 45-degree line). This near-uniformity is also present in the density estimated by Karger and Rajan (2021) for the US pandemic stimulus, though that density had a spike at zero. Second, the distribution has a long right tail, with 5% having an MPX above one (and the top 1% above 2.3).

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

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

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



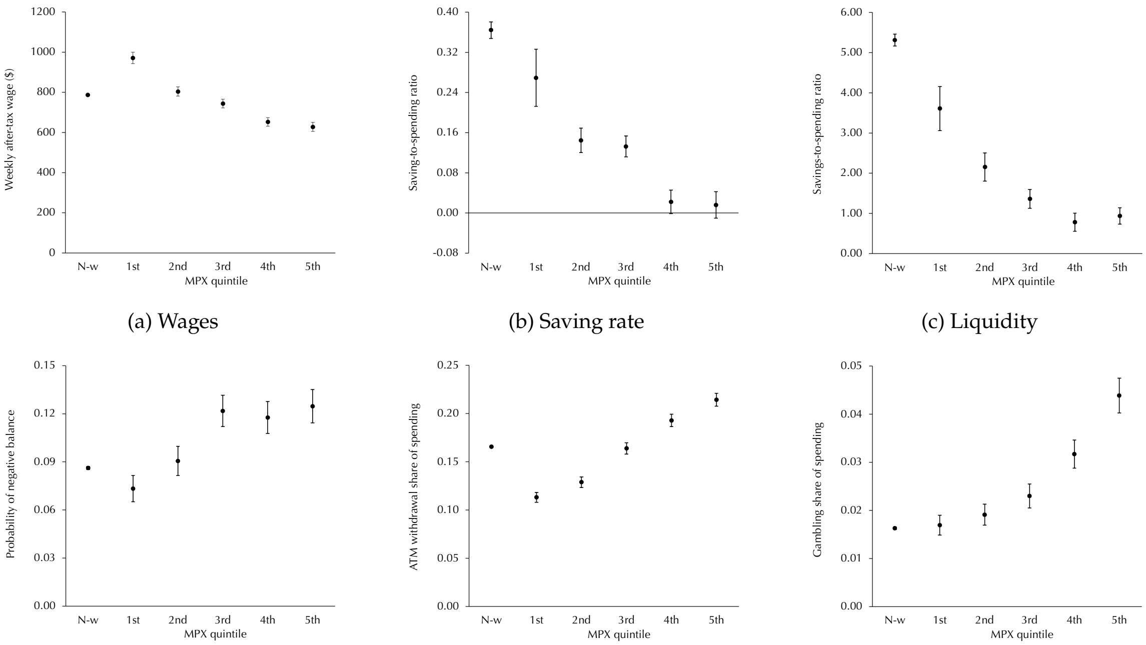
(a) Pre-treatment MPX distribution among treated

(b) Post-treatment MPX distribution among treated

(c) Quantile difference-in-differences of MPX

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

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



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

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

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

5.2 MPX predictors

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

Poorer pre-treatment financial health strongly predicts greater spending, consistent with the literature on substantially smaller transfers (Johnson et al., 2006; Broda and Parker, 2014; Kreiner et al., 2019). Wages vary modestly across the MPX distribution, falling 35% between the first and fifth quintiles, with half this decline between the first two quintiles. The variation in financial health is more dramatic. Between the first and second MPX quintiles, the saving rate halves and liquidity almost halves. The 40% with MPXs exceeding 0.63 (around $6,000 of additional spending over eight weeks) were saving nothing and had savings at or below monthly spending. Even the 40% with MPXs between 0.09 and 0.63 had savings to cover no more than an additional month of spending, totalling less than 15% of outlays. The 60% with MPXs of 0.34 or more were nearly twice as likely to have an overdrawn account than the 20% with MPXs of 0.09 or less.

We also include two pre-treatment spending categories that were strongly related to the MPX: ATM withdrawals and gambling. As noted earlier, Australia is predominantly a cashless society, though cash is ubiquitous in illicit markets. Recall these two categories were the largest and third-largest discernible uses of withdrawn funds. Both ATM withdrawals and gambling prior to withdrawal were strongly predictive of the MPX. Indeed, between the lowest and highest MPX quintiles, the rate of pre-treatment gambling more than doubled.

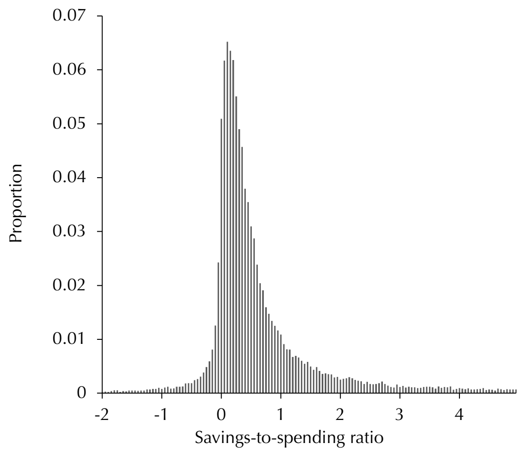
Comparing to the non-withdrawers is informative. There was a modest difference in wages between the withdrawers and non-withdrawers, but low spenders had higher wages than the non-withdrawers and high spenders had lower wages. This same pattern applied to cash withdrawals. Contrast this with the saving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

5.3 Liquidity

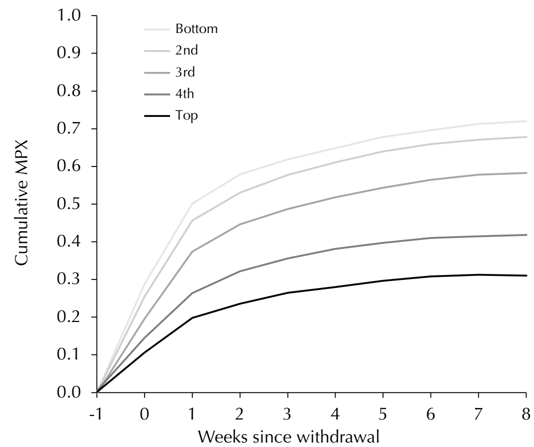
To assess the relationship between financial health and the MPX in greater detail, we focus on the ratio of savings to spending (a measure of liquidity) just prior to withdrawal. In Figure 11a, we present the estimated density of liquidity among the withdrawers. We then divide the liquidity distribution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

The results are displayed in Figure 11b, and indicate a substantial and monotonic increase in MPX, for every week post-treatment, as liquidity declines. The MPX among the bottom two liquidity quintiles is similar, with the least-liquid 40% having

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.

an MPX (0.68–0.72) more than double that among the most liquid 20% (0.31). This translates to additional spending of around $2,500 over eight weeks—equivalent to almost an entire month's post-tax wages for the former group.

6 Mechanism

On the face of it, there are a variety of preferences and market features that might explain a propensity for individuals to withdraw early from their retirement accounts when given the opportunity, and to spend their withdrawals upon receiving them. For example, by providing additional liquidity to those facing exogenously imposed credit constraints, the program might have allowed rational, time-consistent, and patient individuals to achieve their desired consumption and saving levels. Alternatively, the observed withdrawal and spending patterns might reflect liquidity constraints binding on participants due to their personal characteristics, such as a high degree of impatience or present bias. Critical to distinguishing between the various potential drivers of the responses is to account jointly for participants' decisions to withdraw and their decisions to spend conditional on withdrawal.

To do so, we develop a heterogeneous-agent continuous-time model featuring retirement, two assets, idiosyncratic income risk, credit constraints, and preference heterogeneity. We use this model to study the effects of the early withdrawal program on withdrawal and spending. We consider two versions of the model: one in which individuals have time-consistent preferences (exponential discounting, impatient) and differing degrees of impatience; and another in which individuals have time-inconsistent preferences (hyperbolic discounting, present-biased) with differing degrees of present bias. We calibrate each version to match the pre-withdrawal liquidity distribution, the program participation rate, and the average withdrawal amount.

Our goal is to use this structural model to discern the combination of market features and preferences that best rationalizes the following empirical observations: i) the households that withdrew their retirement savings are significantly more liquidity constrained than non-withdrawers; ii) most households withdrew the maximum amount they could; iii) there is significant heterogeneity in withdrawers' liquidity and marginal propensities to spend; and iv) 90% of the spending response occurred within the first four weeks (and 100% within eight weeks).[43]

6.1 Two-asset heterogeneous-agent model

As in Kaplan, Moll and Violante (2018), time is continuous and households maximize the present discounted value of utility. We incorporate retirement following Blanchard (1985). With probability λ_R a household retires and exits the labor market. The retirees obtain a pension income that is a combination of a fixed pension benefit y_R and an annuity from the savings accumulated throughout their lifetime (both liquid and illiquid). For simplicity, we assume retirees are effectively hand-to-mouth, consuming their income flow each month. Once retirees exit the labor market, we replace them with newborn households with identical liquid assets.

[43]Our analysis abstracts from the general-equilibrium effects of the policy to focus on the role of households' liquidity and preferences in explaining the short-term spending patterns. We also assume that the macroeconomic environment did not play a role in explaining households' consumption behavior. This is supported by the fact that spending patterns in April 2020, at the height of the COVID-19 outbreak in Australia, were very similar to those in July 2020, when the virus had effectively been eliminated and much activity had returned to normal. Unlike most other countries, Australia faced very limited deaths and COVID cases came down to zero in May 2022. Moreover, there was a strong federal response that maintained households income stable throughout 2020. Therefore, our model abstracts from income shocks or uncertainty shocks in shaping the consumption responses. Furthermore, the marginal propensities to consume we match were estimated using time fixed effects, holding constant any general economic shock that affected both the withdrawers and non-withdrawers. And, lastly, our evidence shows that temporary income shocks do not appear to play a role in the withdrawal and spending decision. Instead, permanent differences in liquidity are key, which in our model are shaped by preferences.

Working households receive stochastic labor income wz (in which z represents labor productivity and w is the wage), where labor productivity follows a Poisson process with transition probability λ. Households accumulate liquid assets b and deposit a fraction ξ of labor income into the illiquid retirement savings (a) account at no cost. Households can deposit (withdraw) $d > 0$ ($d < 0$) voluntarily into (from) the retirement savings account by incurring a monetary cost $\chi(d, a)$. The return on liquid savings is r^b, the wedge on liquid asset borrowing ($b < 0$) is ϕ, the return on illiquid pension savings is r^a, and the borrowing limit is $\underline{b}$. Non-durables consumption is c. We focus on non-durables as we cannot discern durables precisely in our data, and the non-durables MPX is closer to the theoretical MPC concept.[44]

The recursive formulation of the problem for an exponential household is:

$$\begin{aligned}\rho V(a, b, z) = \max_{c,d} u(c) + V_b(a, b, z)\dot{b} + V_a(a, b, z)\dot{a} \\ + \sum_{z'} \lambda(z, z') \left(V(a, b, z') - V(a, b, z)\right) \\ + \lambda_R \left(V^R(a, b) - V(a, b, z)\right),\end{aligned} \tag{1}$$

where $V^R(a, b) = u\left(\bar{r}(a + b) + y_r\right)/\rho$ represents the continuation value of a retiree, subject to:

$$\dot{b} = (1 - \xi)\, wz + \left(r^b + \phi \cdot \mathbf{1}(b < 0)\right) b - d - \chi(d, a) - c \tag{2}$$

$$\dot{a} = r^a a + \xi wz + d \tag{3}$$

$$a \geqslant 0 \tag{4}$$

$$b \geqslant \underline{b} \tag{5}$$

$$\chi(d, a) = -\chi_0 \cdot d^- + \frac{\chi_1}{2}\left(\frac{d^-}{a}\right)^2 a + \chi_2 \cdot d^+ \frac{\chi_3}{2}\left(\frac{d^+}{a}\right)^2 a, \tag{6}$$

where $d^- = \min\{d, 0\}$ and $d^+ = \max\{d, 0\}$.

6.1.1 Present-biased households

We also accommodate present-biased households, following Laibson et al. (2024), which exhibit quasi-hyperbolic discounting with a naive perception that their future

[44] Laibson, Maxted and Moll (2022) demonstrate the relationship between the MPX and MPC. Intuitively, unlike non-durable consumption, the purchase of durable goods does not directly translate into current consumption. Instead, durable goods provide a flow of consumption services over time, which need to be discounted in the current period.

selves will behave as rational households. More precisely, for these households, we assume the following instantaneous-gratification discounting function:

$$D(t) = \begin{cases} 1 & \text{if } t = 0 \\ \beta e^{-\rho t} & \text{if } t > 0 \end{cases}, \tag{7}$$

where ρ is the discount rate and β is the present-bias parameter. The household values instantaneous utility flows and discounts all future utility flows by β. When $\beta = 1$, we recover the exponential discounting case. The naive household expects that all future selves will discount utility with exponential discounting ($\beta = 1$).[45]

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

For present-biased households, the recursive formulation becomes:

$$W(a, b, z) = \beta V(a, b, z), \tag{8}$$

where $W(a, b, z)$ is the current-value function. The instant after the current period is discounted by β, but for all the subsequent periods the agent perceives her decisions as being given by $V(a, b, z)$ in Equation 1 ($\beta = 1$).

As demonstrated by Laibson et al. (2024), the consumption policy rule for constant relative risk-aversion preferences is:

$$c(a, b, z) = \begin{cases} \beta^{-\frac{1}{\gamma}} \hat{c}(a, b, z) & \text{if } b > \underline{b} \\ \min\left\{\beta^{-\frac{1}{\gamma}} \hat{c}(a, b, z), (1-\xi)wz + (r^b + \phi)\underline{b}\right\} & \text{if } b = \underline{b} \end{cases}, \tag{9}$$

where γ is the intertemporal elasticity of substitution in consumption and $\hat{c}(a, b, z)$ is the consumption policy function of an exponential household, which is the policy rule that the naive household considers it will adopt in the future.

[45]Laibson et al. (2024) choose this tractable naive version of preferences to match the observed procrastination in refinancing behavior in the data. Our evidence in Figure 2b also points to procrastination in the withdrawal of pension savings.

6.1.2 Early withdrawal program

We solve for the stationary equilibrium of the model and then simulate the early withdrawal program as follows. Prior to the program, the cost of withdrawal is arbitrarily large such that no early withdrawals occur (consistent with the rules). We then impose an announced temporary decline in the cost of withdrawing funds from the retirement account. In particular, the cost of withdrawing funds is zero for withdrawals above a threshold $\underline{d}$, with $\underline{d} < 0$. The adjustment cost during the duration of the early withdrawal program becomes:

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

in which $\underline{d}$ is the withdrawal limit, $d^{<\underline{d}} = \max\{0, \underline{d} - d\}$, $d^{\underline{d}\leqslant d\leqslant 0} = \max\{\min\{d, 0\}, \underline{d}\}$, and $d^+ = \max\{d, 0\}$.

Using the household withdrawal amount at the implementation of the policy, we then calculate the marginal propensity to consume as:

$$\text{MPC} = \frac{c(a', b', z) - c(a, b, z)}{|d^p_{T-\Delta}|}, \tag{10}$$

where $d^p_{T-\Delta}$ is the withdrawal amount at the implementation of the policy, which directly increases households' liquidity by $\dot{b} = b' - b$. Hence, $c(a', b', z) - c(a, b, z)$ describes the change in consumption after the portfolio reallocation.

6.2 Calibration

We calibrate the model at a monthly frequency. We perform a two-step procedure to choose the parameters. In the first step, we calibrate a set of parameters using external sources (data, studies, and policy details). In the second step, we choose parameters to match two sets of moments in the data—one among the entire population and the other among the subset of withdrawers. Among the population, we attempt to match both the average-net-liquid-asset level and the fraction participating in the program. Among the subset of withdrawers, we attempt to match the distribution of net liquid assets and the average withdrawal amount.

Table 5: Externally calibrated parameters

Parameter	Description	Value	Source / Target
Preferences			
γ	Risk aversion	2	Standard
Assets			
ϖ	Interest rate wedge	0.75%	Reserve Bank of Australia
r^b	Liquid asset return	0.18%	Kaplan et al. (2018)
r^a	Illiquid asset return	0.47%	Australian Super
ξ	Share of income automatically deposited	10.5%	Australian regulation
χ_0	Adj. cost linear component withdrawals	1.1	Arbitrarily high (policy)
χ_1	Adj. cost convex component withdrawals	12	Arbitrarily high (policy)
χ_2	Adj. cost linear component deposits	0.002	Arbitrarily low (policy)
χ_3	Adj. cost convex component deposits	0.01	Arbitrarily low (policy)
$\underline{d}$	Withdrawal limit	$3 \cdot w$	Policy
Income process			
z_1, z_2	Income states	0.94, 1.06	Guvenen et al. (2023)
λ_1, λ_2	Income jumps	0.887	Guvenen et al. (2023)

6.2.1 Externally calibrated parameters

In Table 5, we report the externally calibrated parameters in our model. We set the monthly interest rate on illiquid assets using the observed returns on Super savings. The median return (net of fees and taxes) on Australian Super savings for the last 29 years is 8.3%, which implies an annual real return, net of average inflation (2.58%), of 5.72% (0.47% per month). The monthly real return on liquid assets is 0.18% (approximately 2% per annum) as in Kaplan et al. (2018). The monthly wedge on borrowing ϕ is 0.75%, which we obtain from the 14.18% interest rate on unsecured personal loans from the Reserve Bank of Australia, net of average inflation.

The share of income that is automatically deposited into the illiquid retirement saving account ξ is 10.5% as per regulation. We set the linear and the convex adjustment cost of withdrawing from the illiquid account, χ_0 and χ_1, to be sufficiently high such that households do not withdraw in equilibrium. On the other hand, the linear and the convex adjustment costs of depositing into the illiquid asset, χ_2 and χ_3, are set to be very low to allow for non-costly voluntary Super saving.

The income process follows Achdou, Han, Lasry, Lions and Moll (2022), adapted to a monthly frequency. We discretize an AR(1) process, with annual persistence

$\rho_z = 0.9$ and standard deviation $\sigma_z = 0.2$ (Guvenen, Kambourov, Kuruscu, Ocampo and Chen, 2023), into a monthly two-state Poisson process with income states $z_1 = 0.94$ and $z_2 = 1.06$, and a jump probability of 0.88.

The probability of retiring λ_R is assumed to be $1/(40 \cdot 12)$, which implies an average of forty years in the labor force.

6.2.2 Internal calibration

In our internal calibration, we calibrate two versions of the model: one with exponential discounting ($\beta = 1$) with heterogeneous discount factors ρ; and the other with present bias ($\beta < 1$) with heterogeneous present-bias parameters β. Our key untargeted moments are the average MPCs within each liquidity quintile.

For the exponential version of the model, we calibrate the borrowing limit, the population discount factor, and the discount factor for each of the liquidity quintiles of the withdrawers to match seven moments in the data: the average net-liquid-assets-to-income ratio of the population, the percentage of households that withdrew under the program, and the average liquidity for each of the liquidity quintiles of the withdrawers.[46] In this calibration we leave the amount withdrawn untargeted. However, given the nature of the policy, the degree of impatience required to match the moments naturally generates bunching of households at the maximum withdrawal amount allowed under the program, as observed.

For the present-bias version of the model, we calibrate the borrowing limit, the population discount factor, the withdrawers' discount factor, and the present-bias parameters for each of the liquidity quintiles of the withdrawers to match eight moments in the data: the average net-liquid-assets-to-income ratio of the population, the percentage of households that withdrew under the program, the average liquidity for each of the liquidity quintiles of the withdrawers, and the average withdrawal amount. We first choose the discount rate ρ that generates bunching at the maximum withdrawal amount. Then we choose the present-bias

[46] To obtain the net-liquid-assets-to-income ratio we transform the savings-to-spending ratios in Figure 11a as follows. First, we use the saving rate to obtain the ratio of total savings to income. Second, we use HILDA to convert our empirical gross-asset-to-income ratio into the net-assets-to-income ratio. To this end, we use the information reported in Lwin (2020) on the net-liquid-asset-to-income ratio of Australian households (156%) as a baseline. Third, for each quintile's gross-savings-to-income ratio, we preserve the difference with respect to the previous quintile, and then subtract that from the HILDA average. For example, the difference between the fifth quintile of liquidity and the non-withdrawers' liquidity is subtracted from the average 156% to obtain the average net-liquid-assets-to-income ratio of the fifth quintile. For the fourth quintile, we obtain the difference between the gross-liquid asset-to-income ratio of the fifth and fourth quintiles and then subtract that difference from the adjusted net-liquid-asset-to-income ratio of the fifth quintile. We do the same for the third, second, and first quintiles.

parameters that match the distribution of liquid assets among the withdrawers.

In Table 6, we present the first set of calibration results. Although we calibrate unique parameter values for each liquidity quintile, for a general sense of model fit, ready comparison with the existing literature, and ease of exposition, we present these first results as weighted averages (across liquidity quintiles) of the various moments and parameters of interest.

We begin with withdrawal. In column 2, we present the results for exponential households. An average monthly discount factor of 3%, equivalent to an annual subjective discount factor of 0.7, and a borrowing limit of four times monthly average labor income can simultaneously match the average liquid-asset-to-income ratio among the withdrawers and the typical withdrawal amount (with households bunching at the maximum). That is, with a discount factor similar to that in Aguiar et al. (2024) for impatient US households (0.72), exponential discounting is able to reconcile the withdrawal behavior we observe in practice. In column 3, we present the results for present-biased households. A monthly discount factor of 0.8%, equivalent to an annual subjective discount factor of 0.91, delivers bunching of households at the maximum withdrawal, and an average present-bias parameter of 0.63 matches the average liquid-assets-to-income ratio of the withdrawers. This value of the present-bias parameter lies within the range of monthly values estimated by Ganong and Noel (2019) ($\beta \in [0.5, 0.9]$) for the case of unemployment insurance exhaustion in the US and Gerard and Naritomi (2021) ($\beta = 0.44$) for the case of severance pay in Brazil. So either form of discounting predicts the observed withdrawal behavior given parameter values consistent with recent work.

However, they do not perform equally well in predicting the observed spending response among those who withdrew.[47] In the final row of Table 6, we present the average MPC predicted by each version of the model, with the average MPC observed in the data (28%) in column 1. The present-bias version of the model comes quite close to matching the average spending response in the data with an MPC of 24%. The exponential version of the model, however, predicts a much smaller spending response, with an MPC of 10%, roughly a third of that observed in the data. In order for the exponential version of the model to generate MPCs that are as high as under present bias, the withdrawers would need to have liquid asset

[47]To calculate the withdrawal MPC we use the four-weeks MPX from Figure 11b and weight it by the fraction of discernible non-durable goods spending.

Table 6: Calibration results (weighted averages across liquidity quintiles)

Parameter/Moment	(1) Data	(2) Exponential impatient	(3) Present-biased
Preferences			
ρ (population)	-	0.5%	0.5%
ρ (withdrawers)	-	3%	0.8%
β (withdrawers)	-	1	0.63
Liquidity			
b/w (population)	156%	155%	155%
b/w (withdrawers)	-286%	-286%	-286%
Withdrawal			
Withdrawer percentage $(\mathbf{1}_{d<0})$	17%	18%	18%
Withdrawal amount $(\mathbf{1}_{d<0})$	$-3 \cdot w$	$-3 \cdot w$	$-3 \cdot w$
Spending			
Average MPC	28%	10%	24%

Note: In this table, we report the calibrated parameter values (weighted average among withdrawers' liquidity quantiles) along with the model-implied moments. In column (1), we report the model with exponential discounting and heterogeneous discount factors. In column (2), we show the parameter values and the moments from a model with naive present bias households. We obtain the average MPC by averaging the MPX of withdrawers liquidity quantiles, weighting by the fraction of non-durable spending (60%).

levels that are far lower than those observed in the data and discount factors that are far lower than in the existing literature.

6.2.3 Heterogeneity

Earlier, we estimated substantial heterogeneity among withdrawers in their propensity to spend out of their withdrawals and strong relationships between this spending propensity and various pre-withdrawal characteristics, the most notable of which was liquidity. In recognition of this, we calibrate our model by splitting the withdrawers into pre-withdrawal liquidity quintiles and then generate the quintile-average parameter values necessary to match the liquid-assets-to-income ratio within each quintile. Now we assess the ability of each version of the model to match the joint distributions of liquidity and spending.

In Table 7, we display the calibrated parameter values for each liquidity quantile. The exponential model with heterogeneous discount factors requires the following

Table 7: Calibration results (by liquidity quintile)

	(1)	(2)	(3)	(4)	(5)
Liquidity quintile	1st	2nd	3rd	4th	5th
Calibration target					
Liquid assets to income	-355%	-325%	-317%	-285%	-152%
Exponential impatient					
ρ	8.2%	4.1%	3.6%	2.5%	1.4%
β	1	1	1	1	1
Liquid assets to income	-355%	-325%	-317%	-285%	-152%
Present-biased					
ρ	0.8%	0.8%	0.8%	0.8%	0.8%
β	0.48	0.59	0.62	0.68	0.72
Liquid assets to income	-355%	-325%	-317%	-285%	-152%

Note: In this table, we report the calibrated parameter values along with the model-implied moments for two versions of the model, for each of the quantiles of the distribution of withdrawers' liquid assets (Q1-Q5). Column 1 to 5 display the calibrated parameter values and model-implied moments for the five quantiles of the withdrawers liquidity distribution.

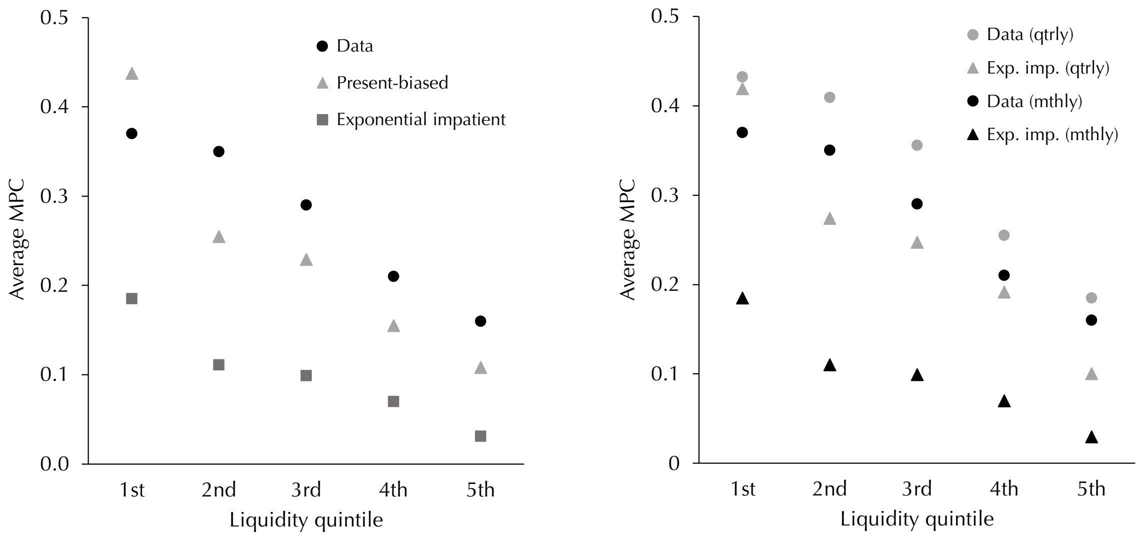
values of the monthly discount factor: 1.4%, 2.5%, 3.6%, 4.1%, and 8.2%.[48]. The present-bias model with heterogeneous degrees of present bias requires the following values of the present-bias parameter: 0.72, 0.68, 0.61, 0.59, and 0.48. The implied discount factors for the third through fifth quintiles are substantially smaller than the values documented in the existing literature, whereas the monthly present-bias parameters are within the range of values documented in the literature. In Figure 12a, we plot the relationship between liquidity and spending for the two models. There is one clear take away. As with the calibrated averages presented earlier, the present-bias version of the model provides a good fit of the withdrawers' MPCs. On the other hand, the exponential version of the model fails to generate MPCs that resemble those in the data.

6.2.4 Importance of high-frequency data

Among the most striking features of the empirical evidence presented earlier is the sharpness of the observed spending response, with 39% having occurred within a week of withdrawal, 71% within two weeks, 83% within three weeks, 90% within

[48]These discount factors at monthly frequency are equivalent to the following values of the annual subjective discount factor: 0.85, 0.74, 0.65, 0.62, and 0.39

Figure 12: Calibration predictions of the average MPC by liquidity quintile



(a) Present-biased v exponential impatient

(b) Exponential impatient, quarterly v monthly

Note: On the vertical axis, we report the average MPCs of the corresponding liquidity quintiles on the horizontal axis. To obtain the average MPC, we use the weights from the stationary distribution of assets.

four weeks, and 100% within eight weeks. In recognition of this, we calibrated the model at a monthly frequency, though it is common for calibration exercises of this kind to be performed at a quarterly frequency.

To illustrate the importance of calibrating a frequency that matches the speed of the observed spending response, we repeat the same calibration exercise but using a quarterly rather than monthly frequency. To calculate the quarterly MPCs we use the 8-weeks MPX by liquidity and weight it by the fraction of discernible non-durables spending (60%). The implied annual subjective discount factors are: 0.84, 0.76, 0.71, 0.68, and 0.53. In Figure 12b, we plot the monthly and quarterly average MPCs across the liquidity distribution, for both the data and the exponential model. Compared to the monthly calibration, the model with exponential households calibrated at the quarterly frequency comes much closer to matching the observed spending.

However, this calibration is clearly inappropriate, for two reasons. First, the vast majority (90%) of the spending response occurred within the first four weeks after withdrawal. Thus, a calibration that targets the quarterly MPC implicitly imposes a counterfactually "smoother" spending impulse. Second, at a quarterly frequency, the relative size of the liquidity injection is three times smaller than at a monthly frequency. While the first (or second) pension withdrawal represents a liquidity injection of between two thirds to one average quarterly wage, the first (or second) withdrawal represents between two to three months of wages. This is an important consideration as the spending responses in the models considered here decrease with the size of the transfer.

7 Conclusion

Our quantitative model demonstrates that liquidity constraints are necessary but not sufficient to generate the withdrawal and spending behavior we observe. The behavioral factors shaping households' liquidity—in this case, present bias or impatience—are what determine the model's ability to jointly match the liquidity distribution, withdrawal decisions, and spending responses. Time inconsistency is key. By its nature, only the present-bias version of the model is able to reconcile the discordant stocks and flows we observe in the data. The extreme degree of impatience necessary for withdrawers to spend enough of their withdrawals quickly enough implies a far lower level of liquid assets than we observe in practice. Our high-frequency evidence is crucial. The front-loaded spending within the first four weeks after withdrawal, in the face of a large liquidity shock, is what allows us to distinguish present bias and impatience as drivers of the observed behavior.

The Super withdrawal program gave people a one-time chance to withdraw $20,000 from their retirement accounts before retirement at an expected cost to their future selves of $120,000 in today's dollars. Those who chose to withdraw were highly idiosyncratic: asset-poor, far lower-saving, more likely to be in blue-collar occupations and to live in rural or remote areas, less likely to be college-educated, and with much greater spending in cash and on gambling. And, overwhelmingly, they did not use their withdrawals to repair their financial circumstances—indeed, the worse their circumstances, the less likely they were to do so. Given the broad swath of the population—roughly one in four 34-year-olds—that withdrew, our evidence suggests a high lower bound on the number of present-biased people in the population. By their having withdrawn, we now observe a 'tag' (Akerlof, 1978) that will enable further study of present bias.

A natural question is: what is the source of withdrawers' time-inconsistency? The clinical psychology literature suggests a link between gambling and impulsivity, implying a link between impulsivity and present bias in our setting (Maclaren et al., 2011). Relying on a large Australian household survey, Schneider and Moran (2024*b*) find that self-control issues are the strongest predictor of early Super withdrawals, with individuals in the top quintile of self-control issues 60% more likely to withdraw early than those in the bottom quintile. Financial literacy, based on their answers to financial questions, was another important factor (though less than self-control), consistent with the retirement literature (van Rooji, Lusardi and Alessie, 2012).

The key policy trade-off posed by the program is between the welfare losses to those who participated and the welfare gains to society more broadly due to greater macro-stability. The present-biased will pay in sub-optimal future consumption—but this is also true of stimulus checks, which can be saved rather than spent. By connecting directly the individual-level costs and benefits of using transfers to stabilize the economy (as opposed to an indirect connection via higher future taxes), the program highlights a quirk of all stimulus transfers generally. The greater the present bias, the more effective it is (Laibson et al., 2024). But those induced to spend make themselves worse off (and others better off, as per Auclert, Rognlie and Straub (2023)) by not saving to offset future taxes. The net welfare impact depends on the properties of the tax and transfer system (Schneider and Moran, 2024*a*). The application of a comprehensive welfare frame to stimulus under present bias is critical to designing optimal macro-stabilization policy (Maxted, 2022). Channelling Tobin (1977): just how many Harberger triangles does it take to fill an Okun's gap?

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Supplemental appendices

A Detailed description of the Super system

Australia has a compulsory, defined-contribution private retirement saving system, called 'superannuation'.[49] 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.[50] The median balance by age in 2019 is shown in Table A1.[51]

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.[52] 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,[53] 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

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

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

[51]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.

[52]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/`

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

Table A1: Median Super balance by age

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

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

full marginal personal rates (more than two times the Super tax rate for the median taxpayer and more than three times for those in the top 3.6%). This includes interest, dividends, and rental income from investment properties. All capital gains (held for more than a year and excluding the primary residence) are taxed at half the marginal income tax rate (17.25% for the median individual and 23.5% for those in the top 3.6%) upon realization. Australia does not have a step-up in basis for inherited assets.[54]

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.[55] 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

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

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

8.3%, with four years posting losses (the largest of which was 21.5% in 2008).[56] 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.[57] 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.

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

[57] 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.[58] 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

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

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

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

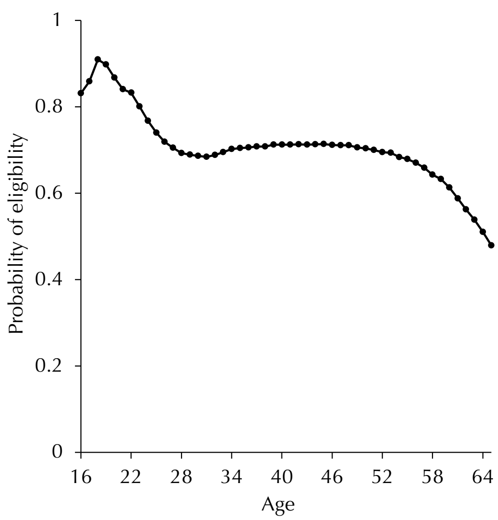
The relationship between age and withdrawal is only mildly affected by eligibility. In Figure A1a, the probability of eligibility is around 70% from the mid 20s to late 50s. Eligibility is significantly higher for those in their late teens and early 20s due to a greater probability of having a reduction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

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

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

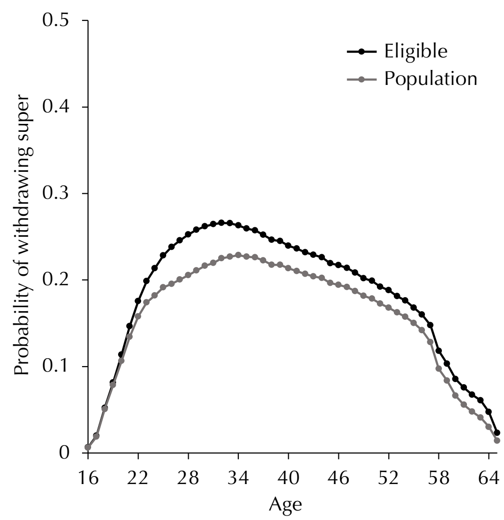
Data: Australian Taxation Office.
Notes: Results are from simple linear regressions of outcomes on a binary first-withdrawal indicator, controlling cumulatively for the 'Demographics' variables, and only among those eligible according to our proxy. Wage control for Demographics and Long-term Financials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

Figure A1: The relationship between eligibility and age



(a) Probability of eligibility conditional on age

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

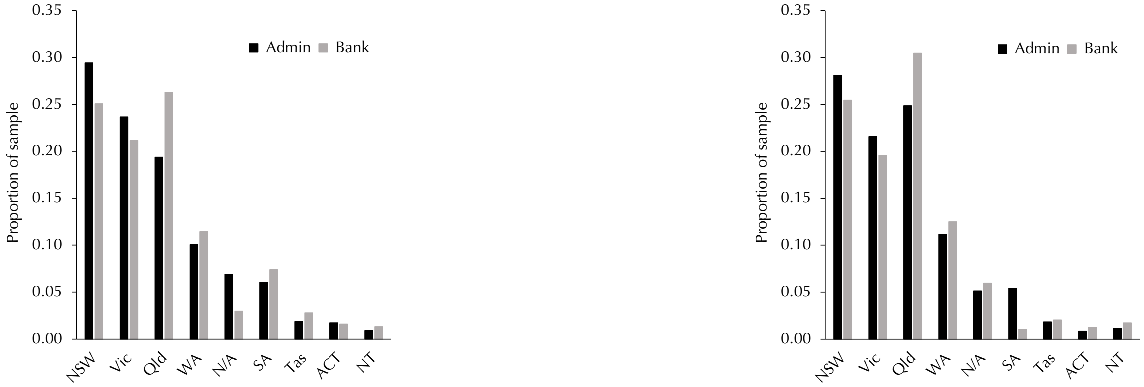


(b) Probability of withdrawal conditional on age

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

C Data comparison

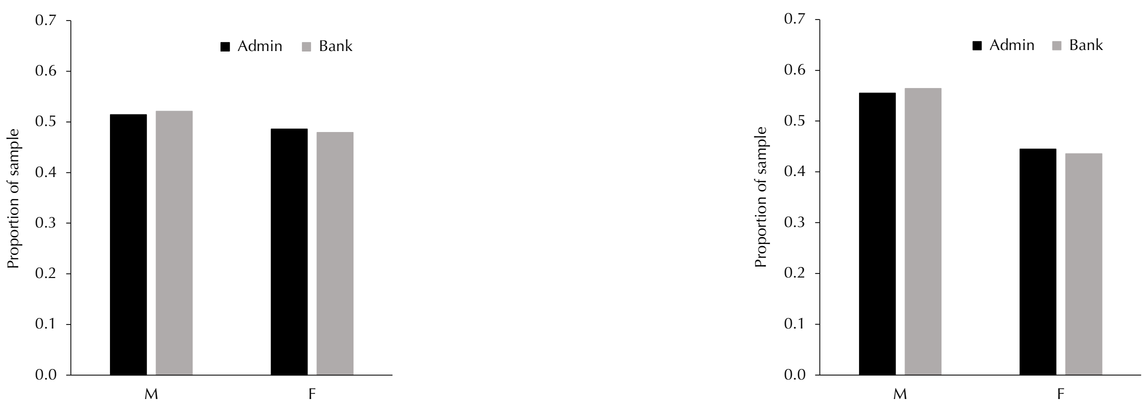
Figure A2: State distribution by sample



(a) Everyone (b) Withdrawers

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

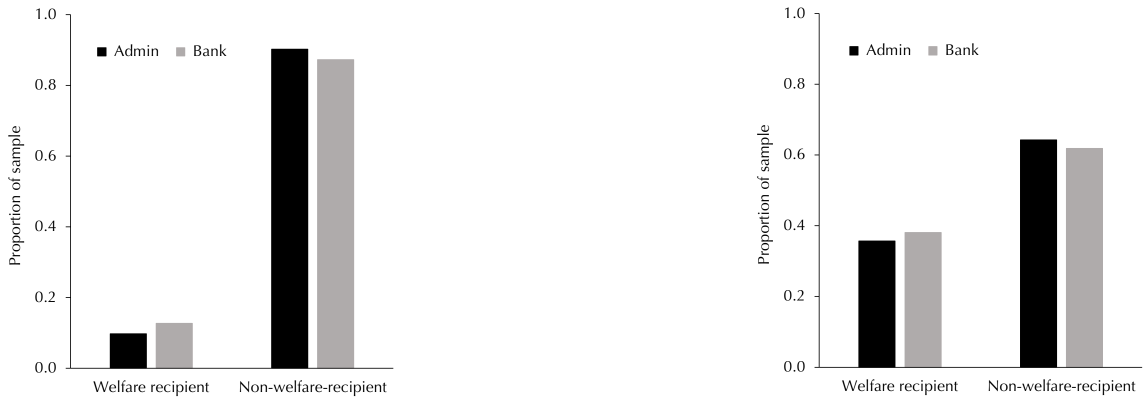
Figure A3: Sex distribution by sample



(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.

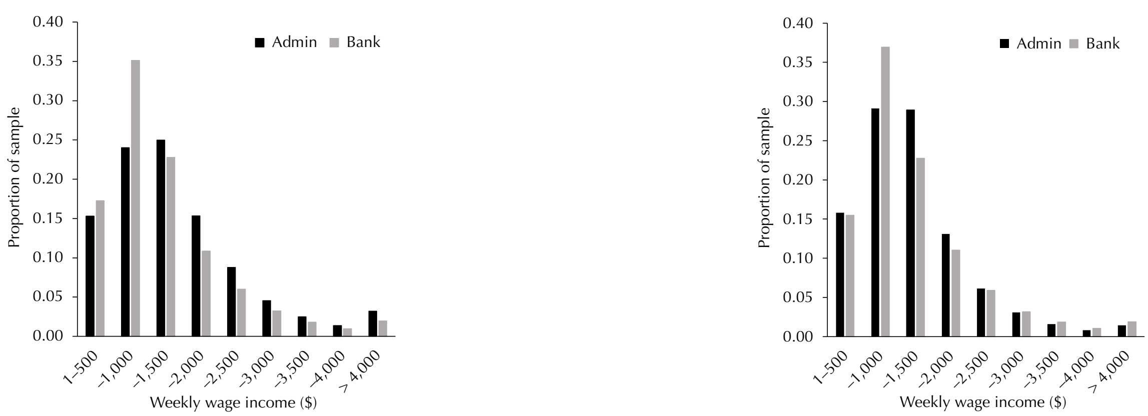
Figure A4: Welfare receipt distribution by sample



(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

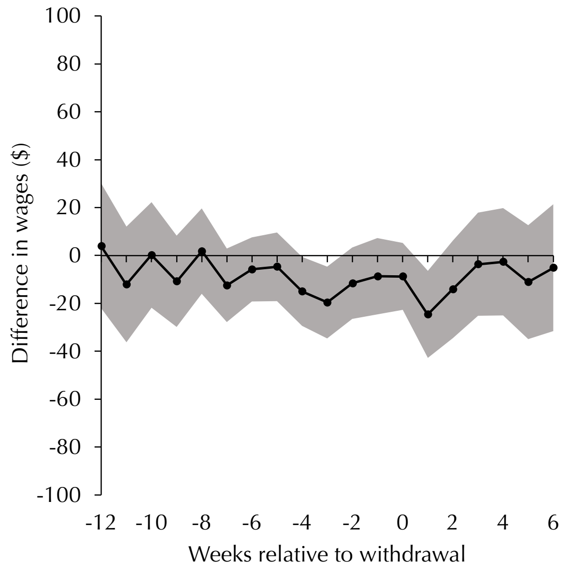


(a) Everyone (b) Withdrawers

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

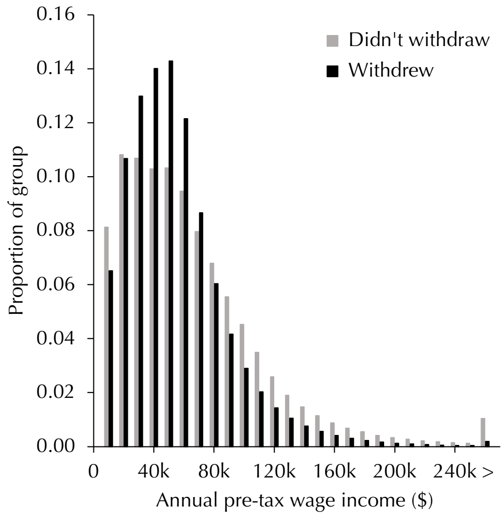
Figure A6: Relationship between wages and withdrawal timing



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.

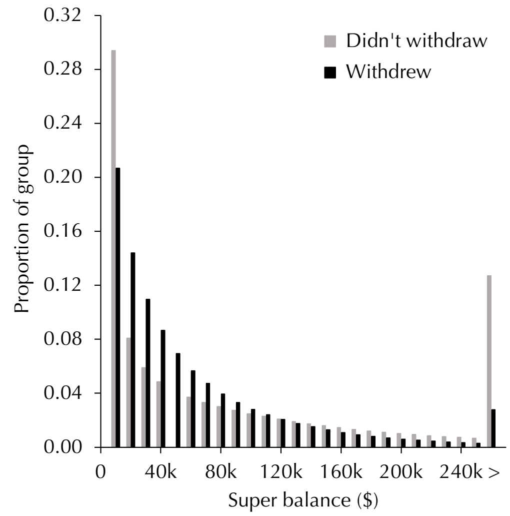
E Wage and Super balance densities

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 A3: Withdrawal rate by occupation

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

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

G Week of withdrawal regressions

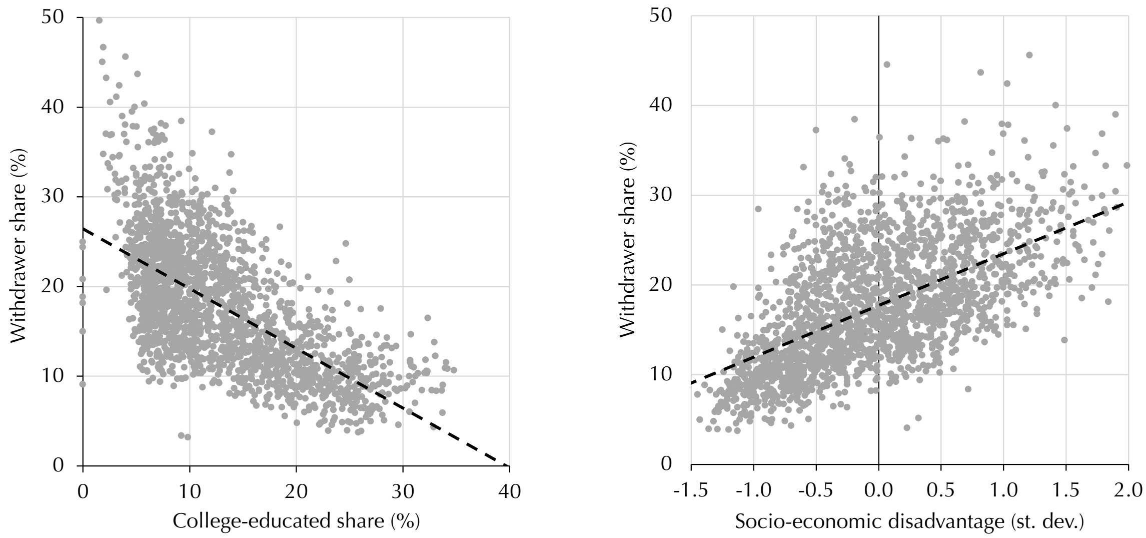
Table A4: Relationships between observed variables and week of withdrawal during the first round

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

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

H Across-location predictors of withdrawal

Figure A8: Across-location predictors of withdrawal



(a) College-educated share (b) Socio-economic disadvantage

Data: Australian Bureau of Statistics
Note: Each dot represents an ABS SA2 statistical area (2,310 in total). College-educated share is based on responses from the 2016 census. Socio-economic disadvantage is given by the ABS's 'Index of Relative Socio-Economic Disadvantage', derived from the 2016 census, which is a general socio-economic index that summarizes a range of information about the economic and social conditions of people and households within an area. For the latter, the data are displayed as standard deviations from the average level of socio-economic disadvantage across all local areas.

I Estimated average treatment effects on the treated

Table A5: Estimated ATTs on income of the first withdrawal

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

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

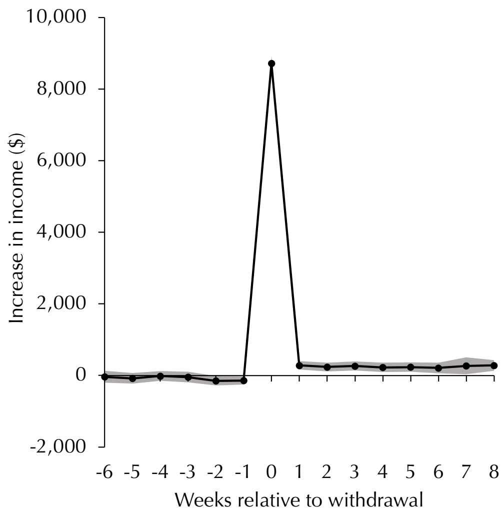
Table A6: Estimated ATTs on spending of the first withdrawal

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

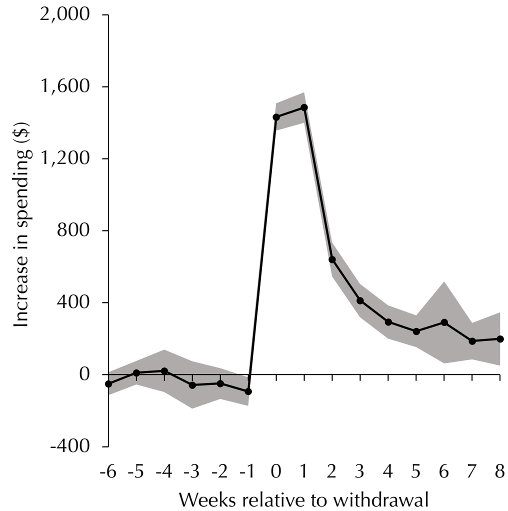
Data: Illion
Notes: N = 337,223. Results are cohort ATTs estimated via the R package 'fixest', which implements Sun and Abraham (2021). Comparison group is the never-treated. Base period is the period immediately prior to treatment. Standard errors computed via a standard bootstrap procedure. Probabilities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

J Second withdrawal event study graph

Figure A9: Estimated ATT of the second withdrawal by event week



(a) Income



(b) Spending

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

K Category regression table

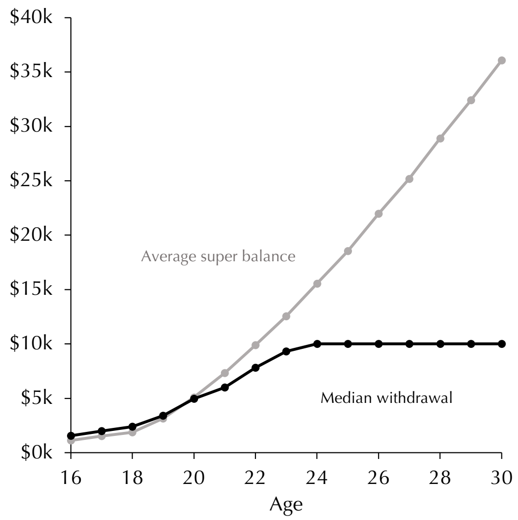
Table A7: Estimated cumulative aggregate ATTs (CAATTs) of the first withdrawal by category

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

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

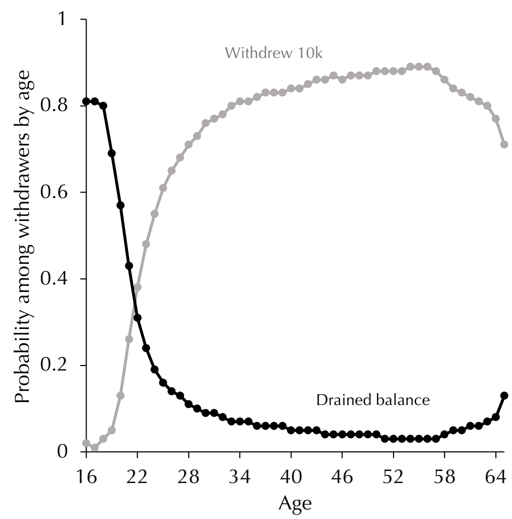
L Withdrawal and age

Figure A10: 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.