WORKING PAPER

DANMARKS NATIONALBANK

21 SEPTEMBER 2020 — NR. 161

Spending when illiquid savings become liquid: evidence from Danish wage earners

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This paper offers new empirical evidence on the marginal propensity to consume out of an unanticipated liquidity shock. A Danish 2012 policy reform reduced the incentive to retire early in order to increase labour supply but at the same time the policy released a substantial amount of savings from an early retirement scheme that were locked in the scheme until the policy change. By exploiting the fact that cohorts were affected differentially by the policy, this paper uses a difference-in-differences specification to estimate an average increase in the propensity to spend of 43 per cent. The estimated spending patterns are consistent with the notion of *wealthy hand-to-mouth* behaviour.

Resume

I dette studie præsenteres ny empirisk evidens for den marginale forbrugstilbøjelighed ved et uventet likviditetsstød. En dansk reform fra 2012 reducerede incitamentet til tidlligere tilbagetrækning fra arbejdsmarkedet med henblik på at øge arbejdsudbuddet, men samtidig frigjorde reformen et betydeligt opsparet beløb, der hidtil havde været bundet i efterlønsordningen. Ved at udnytte, at nogle fødselsårgange blev ramt hårdere af reformen end andre, estimeres en gennemsnitlig forbrugstilbøjelighed på 43 pct. ved hjælp af en difference-in-differences-regressionsmodel. Den estimerede forbrugsadfærd er konsistent med ideen om *wealthy-hand-to-mouth*-adfærd.

Key words

Economic activity and employment; Household balance sheets; Public finances and fiscal policy.

JEL classification

D14; D15; E21; H31; E62.

Acknowledgements

Thanks to Søren Leth-Petersen and colleagues at Danmarks Nationalbank for valuable comments and discussions.

The author is responsible for any remaining errors.

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Abstract

This paper offers new empirical evidence on the marginal propensity to consume out of an unanticipated liquidity shock. A Danish 2012 policy reform reduced the incentive to retire early in order to increase labour supply but at the same time, the policy released a substantial amount of savings from an early retirement scheme that were locked in the scheme until the policy change. By exploiting the fact that cohorts were affected differentially by the policy, this paper uses a difference-in-differences specification to estimate an average increase in the propensity to spend of 43 per cent. The estimated spending patterns are consistent with the notion of wealthy hand-to-mouth behaviour.

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1 Introduction

Government policy reforms with a long-term policy goal, such as increasing labour supply, could also have short run implications for the business cycle. Many countries have administrative rules dictating that pre-retirement withdrawals from retirement schemes should be associated with a tax penalty or the funds may be completely inaccessible until retirement. The aim is to prevent leakage prior to retirement such that future retirees become more financially self-supporting (Poterba and Venti, 2001).

Access to illiquid savings could help savers smooth consumption if they are affected by a negative shock which they had not anticipated when they started contributing to the pension scheme. By reducing or completely removing the transaction cost of withdrawals we should see an increase in spending by consumers who would otherwise have been short on liquidity or have limited access to credit (Kaplan and Violante, 2014).

This paper offers a new empirical test of this prediction in a quasi-experimental setup. Access to longitudinal information at the individual level from Danish administrative registers enables us to track spending behaviour across time for cohorts that were differentially affected by a Danish 2012 retirement reform. The policy was aimed at reducing the incentive for retiring early but also led to the possible withdrawal of many years of savings which had been locked in an early retirement scheme. The reform implied that cohorts with a now-lower incentive to retire early would be more inclined to opt out of the early retirement scheme and cash out savings compared to cohorts that were less affected by policy change. The results indicate that 43 cents of each 1 dollar withdrawal were spent immediately, while the remaining part was saved in taxable savings accounts or individual retirement accounts.

Few papers have estimated the behavioural response when changing the transaction cost of accessing liquidity locked in retirement plans. Chang (1996); Burman et al. (1999, 2012) estimate a reduction in the propensity to withdraw from retirement accounts as a 10 percentage point tax penalty was levied on pre-retirement cashouts in the US 1986 Tax Act. The tax penalty was aimed at reducing leakage from retirement accounts in the long run but, as a side effect, spending was likely to drop in the short term at the time when the penalty was introduced. Lack of data and appropriate research designs have, however, made it difficult to estimate behavioural changes—let alone MPCs—with sufficient statistical precision to draw firm conclusions about the spending response to this type of policy.[1]

A more recent, but also related paper, is Kreiner et al. (2019) which studies a Danish 2009 fiscal policy that allowed savers to withdraw savings from individual retirement accounts which had been completely inaccessible until the policy change. The aim of this policy was to increase demand in the short run following the 2007–2008 Great Recession, and the authors estimate an average spending propensity of 60 per cent. Other, and more traditional fiscal stimulus policies, have also been found to increase spending. This is the case irrespective of whether the stimulus comes from transfers from the governement (Parker et al., 2013; Broda and Parker, 2014; Kaplan and Violante, 2014) or tax reliefs (Souleles, 1999; Shapiro and Slemrod, 2009; Kaplan and Violante, 2014). These papers examine policies that were also enacted with an aim of raising demand after the 2007–2008 Great Recession and find spending propensities of 30–90 per cent. More recently, Chetty et al. (2020) use high frequency real-time data to show that stimulus payments during the COVID-19 crisis in 2020 increased spending, particularly for low income households. Similarly Karger and Rajan (2020) use transaction-level debit card data to show that the US COVID-19 stimulus payments increased spending corresponding to an average MPC of 48 per cent. Another recent study uses US consumer survey information to show that 40 per cent of transfers from the CARES Act were spent immediately or were planned to be spent in the near future (Coibion et al., 2020).

The Permanent Income Hypothesis predicts that consumption should increase when income increases unexpectedly, e.g. from government stimulus transfers. Consumption should remain unchanged, however, where policies grant consumers access to their own savings in illiquid accounts. Only if consumers are constrained by a lack of access to liquidity should they increase spending once this constraint is lifted (Zeldes, 1989). More recent theoretical refinements of this idea are the modelling of wealthy hand-to-mouth consumers described by Kaplan and Violante (2014), where agents hold limited or no liquid wealth but own substantial amounts of less

[1]A range of papers examine the characteristics of savers who withdraw retirement savings prior to retirement. Poterba and Venti (2001) find that larger withdrawals are rolled over to other savings accounts, while smaller cashouts were not. Moreover, the rollover probability was found to be smaller for young savers than for their older counterparts. These results are supported by Sabelhaus and Weiner (1999) who also find higher withdrawal propensities for low-income groups, and Hurd and Panis (2006) who present a correlation between the timing of withdrawals and divorce or widowhood. Finally, the propensity to perform pre-retirement withdrawals is found to coincide with jobloss (Amromin and Smith, 2003) and business cycles (Argento et al., 2015).

liquid assets, such as housing or pension wealth. These consumers should exhibit large propensities to spend out of withdrawals from otherwise illiquid accounts when they become able to withdraw these assets or when the associated cost of doing so declines.

The empirical evidence in this paper differs from what we know already by measuring the propensity to spend out of a liquidity shock that was not directly inteded to increase spending. In fact, the shock was a second order effect of a policy reform with a completely different and long-term aim. To the best of the author's knowledge, no other empirical papers have addressed the possible stimulus effect in the short run of a policy reform that aimed to improve fiscal sustainability for public finances in the long run.

This paper exploits a Danish 2012 policy that generated variation in the incentive to withdraw savings which had been locked in an illiquid retirement scheme to test the prediction of wealthy hand-to-mouth behaviour. Despite the fact that the policy was entirely aimed at raising the retirement age and improving fiscal sustainability in the long run, the reform released a substantial amount of individual savings to wage earners, which had been illiquid to them until the reform. This discretionary increase in liquid resources was not necessarily targeted for increasing demand but, as it turned out, the policy stimulated spending in a similar fashion to a stimulus policy.

The 2012-reform reduced the incentive to be a member of the voluntary early retirement pension (VERP) scheme differentially across birth cohorts. Wage earners would have the option to retire early if they had been member of the scheme since its launch in 1999 and contributed annually the fixed members' fee, about DKK 5,400 (USD 850) in 2011, which is adjusted for inflation in each year. After 30 years of membership, wage earners could retire up to five years before reaching the statutory state pension age. This paper focuses on individuals born between 1 July and 31 December 1960. The reform reduced the time eligibility for early retirement benefits of this cohort from five to three years and postponed the early retirement age by two years. Additionally, this group of individuals became subject to asset testing in 2012, meaning that monthly benefits would be reduced by some factor according to the amount of savings in privately funded retirement schemes that the recipient had saved before age 60. In a quasi-experimental research design, the spending behaviour of this cohort is compared to the behaviour of those born between 1 July and 31 December 1955. The only change in retirement rules in 2012 for this group was a two year postponement of the early retirement age. Both groups had contributed to the VERP scheme since 1999, implying that the accumulated contributions from 1999 to 2011 were identical, about DKK 60,000 (USD 9,500). The basic idea behind the empirical setup is that the 1960 cohort would be more inclined to opt out of the VERP scheme in 2012 compared to the 1955 cohort as their option value of retiring early had been reduced to a relatively greater extent by the policy change. This implies that the 1960 cohort was also more likely to have its past VERP contributions repaid in 2012, generating a larger shock to disposable resources relative to that of the 1955 cohort. Repayments of past contributions happened automatically if the saver opted out of the scheme and the withdrawals were not taxed. Such discretionary shock to liquidity should not affect deep preferences for spending at the individual level if we are willing to assume that consumers' unobserved spending preferences are fixed across their lifetime. In other words, the 2012-reform is arguably a source of exogenous variation in liquidity—or spending opportunities—when comparing cohorts who were influenced differentially by the policy.

A range of individual information from the Danish administrative registers has been combined for this paper, containing all members of the early retirement pension scheme since its launch in 1999. Age is cut at 59—the year before the pre-reform early retirement age—which provides a panel covering 1999–2014. Access to wealth information allows for measuring crowding out in other savings accounts and calculating an imputed measure of consumption by following Browning and Leth-Petersen (2003). Unique personal identifiers allows for linking individuals to their spouses and for each partner, birth dates, VERP withdrawals, income, and wealth can be looked up. This particular feature of the data is useful as retirement decisions are often determined at the family level and are conditional on the partner's retirement opportunities (Hurd, 1990; Blau, 1998). The information is reliable and considered to be of high quality as it is third-party reported and audited by the tax authorities with limited risk of selfreporting bias.

The empirical design constitutes a comparison of saving and spending flows over time between cohorts that were differentially affected by the policy. Parallel pre-trends across cohorts can be documented up until the reform implementation which points to the fact that saving and spending behaviour would have developed identically over time in the absence of the reform. A standard difference-in-differences estimator measures how much of the liquidity provided by the policy is spent immediately and how much is saved in alternative accounts. The detail of the data allows for estimating the spending propensities across subgroups in the sample, which uncovers important heterogeneity in the spending response.

The results show that 43 per cent of the withdrawals were spent within the same calendar year as the payout took place. This number is robust to controlling for a broad range of individual observables as well as unobserved factors at the individual level that remain constant over time. Proxy variables that potentially point out consumers with limited cash on hand who still own substantial wealth predict the increased spending propensities. This indicates that the increase in spending was driven by wealthy hand-to-mouth consumers.

The main contribution of this paper is to measure the stimulating effect of granting access to otherwise illiquid assets locked in an early retirement scheme. This is done in the context of a policy that reduced the incentive to be a member of a voluntary early retirement pension scheme, where opting out of the scheme generated a substantial shock to liquid resources. Measuring the stimulating effect of such a policy is interesting because the long-term policy goal was to reduce the incentive to retire early. The results show that such a policy can have important short run fiscal implications by stimulating spending, in particular for the segment of the population which is short on liquidity.

The following section explains the institutional setting and the Danish 2012 early retirement pension reform. Section 3 presents the data set, while section 4 explains the empirical design and regression model. Section 5 concludes the paper.

2 The 2012 early retirement pension reform

The voluntary early retirement pension was introduced in 1979 and the administrative rules surrounding the scheme were reformed several times hereafter, notably in 1998, as participants became obliged to join an unemployment insurance scheme for at least 25 out of the past 30 years before turning 60 years old. The state pension age was 65 but members of the early retirement pension scheme could retire at age 60 if they were eligible. From 1999, scheme membership was conditional on paying a annual fee, about DKK 5,400 (USD 850) in 2011. This amount was adjusted for price developments over the years in a similar fashion to adjustments to benefits, and wage earners could subtract the fee from their salary before paying taxes. Early retirment benefits were up to DKK 255,000 (USD 41,000) per year, in 2011, and taxed as ordinary income. Prior to 2012, the early retirement option was considered very generous for wage earners as the discounted sum of early retirement benefits clearly surpassed the sum of annual contributions over 30 years of employment. Contributions could be extracted by giving up the early retirement option and pay a 30 per cent penalty rate when cashing out the balance. Very few members did this given the generousity of the scheme.[2]

The 2012 reform constituted three particularly important changes which were phased in differentially across wage earners' birth dates. First, the early retirement age and the state pension age were postponed by a similar number of years for individuals born from 1 January 1954 to 31 December 1955. Second, the early retirment age was postponed by some number of years X for individuals born 1 January 1956 or later, but for these people the state pension age was postponed by $Z < X$, implying that the time spent receiving VERP benefits was reduced.[3] Postponements and reductions in the time eligibility for VERP benefits for all cohorts are presentend in Appendix Table 6. Finally, early retirement benefits were asset tested such that wage earners with large balances in private or occupational retirement accounts would have their benefits reduced by some determined factor. This would potentially affect a large part of wage earners as occupational pension schemes were widely used in Denmark with a labour market coverage of about 80 per cent.[4] The policy changes implied that the early retirement age could be postponed by up to three years, the time eligibility for early retirement benefits could be reduced by up to two years and the early retirement benefits could effectively be reduced to zero for high pension wealth savers.[5]

[2]The VERP scheme was introduced in an attempt to increase employment for younger generations at the expense of the labour participation of older generations. The comprehensive rollback of the scheme in 2006 and 2012 was rooted in the acknowledgement that long term employment and long term labour supply are closely (positively) related and the objective was to raise the long term labour supply and GDP and improve the sustainability of public finances in the face of an ageing population.

[3]In 2006, the Danish parliament decided to increase the early retirment age for individuals born in 1959 or later. The 2012-reform was partly a revision of the 2006-decision so that the retirement ages would already start to increase for 1954-cohorts. Additionally, the 2006 reform implied that the retirement age would increased over time identical to developments in life exectancy.

[4]The majority of wage earners had contributed to employer-paid retirement schemes since the 1990s and had thus accumulated considerable pension wealth since then.

[5]In addition, wage earners could earn a tax free premium by being a member of the early retirement pension scheme but not utilizing their right to retire early once they reached the early retirement age. Before 2012, this was possible for all members if they postponed early retirement two years after reaching

The empirical design in this paper relies on comparing individuals born between 1 July and 31 December of 1960, the *treatment* group, to those born between 1 July and 31 December of 1955, the *control* group. Early retirement ages was postponed from 62 to 64 for the treatment group and from 60 to 62 for the control group, i.e. a two-year postponement for individuals in both groups. The state pension age remained unchanged for the treatment group such that old age pension benefits started at age 67 both before and after the reform. The control group was on the other hand subject to a similar two-year postponement of the state pension age from 65 to 67. The implication was that individuals in the treatment group had their time eligibility for VERP benefits reduced by two years more than individuals in the control group. In addition, the treatment group became subject to asset testing in 2012, which was not the case for the control group. This implied that, not only did the time receiving VERP benefits drop for the treated group, the value of benefits also decreased in pre-retirement pension wealth.

3 Data

The data used in this paper draw on a comprehensive set of information from public administrative registers and the income tax register compiled by Statistics Denmark. The information is merged at the individual level using unique personal identifiers and covers the population of Danish residents who had been members of the VERP scheme since 1999. Age is cut at 59, shortly before the earliest possible early retirement age, which provides a panel dataset covering 1999–2014 and 216,406 observations. Further selection constitutes the omission of self-employed as the administrative registers do not separate personal income from business income. Also, individuals engaging in home purchases are removed because such activity cause substantial volatility in the spending measure.

The income tax register contains information about incomes for each calendar year as well as the end-of-year values of assets and liabilities. Assets cover savings in taxable accounts such as deposits, the value of stocks and bonds and the value of

the early retirement age. After 2012, members could earn this premium from the day they reached the early retirement age. This rule change applied only to cohorts born 1 January 1956 or later. The rule change implied that, although the early retirement age was postponed and the time eligibility for benefits was reduced, wage earners retained the possibility to earn a tax free premium from not exercising their right to receive early retirement benefits depite being eligible. This was another incentive-based measure to increase labour supply for individuals close to the retirement age.

housing assets, while liabilities constitute mortgage debt and other debt in banks. Our measure of spending is imputed by subtracting all changes in net wealth, S, and pension contributions, P, from disposable income, I, for each individual in each year, $C_{i,t} = I_{i,t} - \Delta S_{i,t} - P_{i,t}$. Although this measure is inherently noisy, imputed spending is found to correlate closely with consumption elicited in surveys (Browning and Leth-Petersen, 2003). In order to reduce the noise we censor at the 5th and 95th percentiles. Later in the paper we show that the results do not change in any important way when censoring at less restrictive cutoffs, which produce more noisy estimates.

Table 1 presents the summary statistics of the full sample used in the regressions and figures. Mean disposable income is DKK 217,000 per year and pension contributions are about 10 per cent. This illustrates the well-developed occupational pension system in Denmark with relatively large mandatory contribution rates set by agreements between workers' unions and employers' associations. The balance in the early retirement pension scheme is not included in these numbers. Deposits and financial assets are DKK 66,000 and 21,000, respectively, and housing assets are DKK 621,000 per individual. Total liabilities are about DKK 435,000 with 2/3 in mortgages. There is a slight overweight of females at 58 per cent and an average tenure on the labour market of 24 years. Almost seven out of ten are married or cohabiting with their partner and about equally many are homeowners.

The control and treatment groups contain 85,651 and 130,755 observations, respectively. Clearly, the treatment group is five years younger than the control group. To account for the fact that two cohorts are compared over time on a set of observable measures we control for differences that can be explained solely by developments across calendar years. The columns labelled *Control* and *Treatment* in Table 1 present the predicted values of each variable shown in the table when controlling for time fixed effects. This implies that each variable is regressed on year dummies and the predicted values of each regression are stored and displayed in the table as means and standard deviations. This exercise allows us to assess the balancing of covariates between the control group and the treatment group. As it turns out the groups seem very similar on observables conditional on the age gap of five years. This indicates that a comparison of spending behaviour between these two cohorts is not likely to be suffering from selection bias.

Table 1: Summary statistics

	Full sample		Control		Treatment	
	Mean	SD	Mean	SD	Mean	SD
Disposable income	216,892	65,456	216,813	30,864	216,944	30,876
Pension contributions	22,681	17,543	22,670	5,760	22,688	5,752
Deposits	65,948	108,208	65,884	25,488	65,991	25,516
Stocks/bonds	20,987	70,158	20,966	7,263	21,001	7,254
Debt	95,054	131,341	95,031	23,100	95,068	23,071
Mortgage debt	340,531	361,421	340,389	52,427	340,624	52,400
Real assets	621,039	629,295	620,914	143,958	621,121	143,615
Age (Years)	48	4	51	4	46	4
Work experience (Years)	24	7	27	7	23	6
Female (%)	58	49	56	50	60	49
Married/co-habiting (%)	69	46	70	46	68	47
Homeowner (%)	67	47	67	47	67	47
Observations	216,406		85,651		130,755	

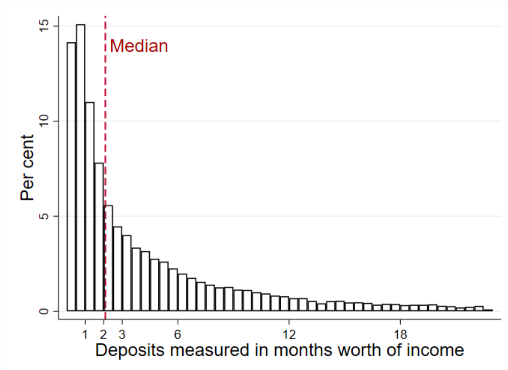
Notes: The *Full sample* columns present actual values, while the *Control* and *Treatment* columns show predicted values when controlling for year fixed effects obtained from running the regression $y_{i,t} = \alpha + \omega_t + \varepsilon_{i,t}$ for each outcome $y_{i,t}$, where α is a constant, ω_t are year specific dummies and $\varepsilon_{i,t}$. Source: Own calculations based on Statistics Denmark's administrative registers.

3.1 Liquidity constraint indicators

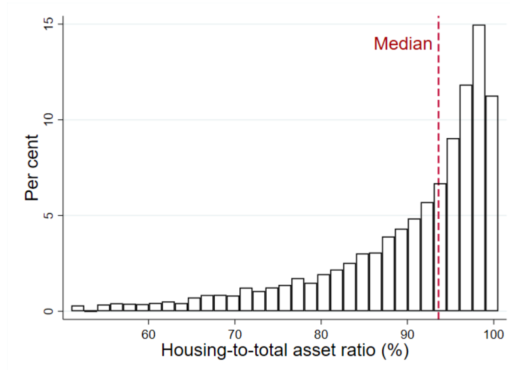
The wealth and income information available in the registers is used to generate two key indicators for whether individuals are constrained by a lack of access to liquid wealth. Ideally, we would want each individual to be credit rated by a financial institution. This type of information does not exist in the Danish setting. The closest alternative is the individual marginal interest rate, which captures the extra amount of money each borrower would need to pay for one additional dollar of credit. Kreiner et al. (2019) compute the marginal interest rate at the individual level using account-level information from the Danish tax authorities containing the universe of deposit accounts and loans in Denmark. They find that the marginal interest rate predicts the propensity to spend out of transitory income shocks but they also document that the marginal interest rate is closely correlated to the deposit-to-disposable income ratio, a measure available in our data set too.

The left panel in Figure 1 presents a histogram of the deposit-to-disposable income ratio by the end of 2011. The red dashed line indicates the median and every two bars represent one month's worth of deposits. The median individual had just over two months' worth of income in cash shortly before the 2012-reform was implemented. This cash-on-hand indicator is found to be a strong predictor of liquidity constraints (Gelman, 2020) and in the empirical section we use this piece of information to explore heterogeneity in the spending response.

Figure 1: Distributions of liquidity indicators



(a) Cash-on-hand



(b) Illiquid share of total wealth

Notes: The left panel shows the distribution of the cash-on-hand ratio. This is measured by dividing total deposits by the end of the year by one twelfth of the annual disposable income. The right panel shows the distribution of the housing-to-total asset ratio, measured by the total real assets divided by the sum of real and financial assets. The red dashed lines show the sample median.
Source: Own calculations based on Statistics Denmark's administrative registers.

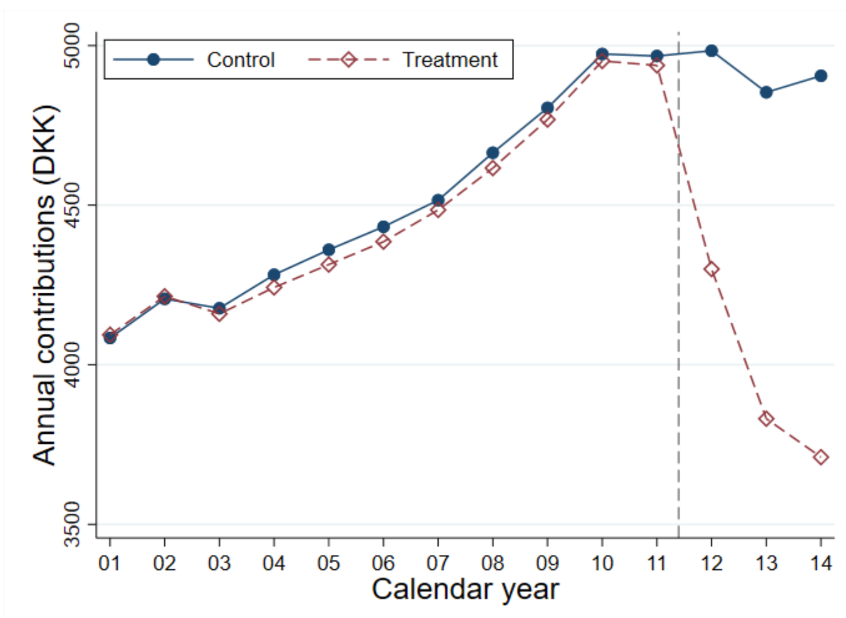
The right panel in Figure 1 shows the illiquid asset share of total wealth by the end of 2011. Illiquid assets are defined as real assets and total wealth includes the value of real assets, deposits, stocks and bonds. The median is about 93 per cent, again illustrated by the red dashed line. In other words, half of the individuals in the sample have less than 7 per cent of their total wealth placed in financial accounts or cash deposits. These individuals are considered wealthy as they own considerable wealth but they hold only limited liquid wealth. Homeowners can borrow against the value of their home in Denmark by using the well-established market for mortgage loans. Home equity withdrawal is, however, considered relatively more expensive in terms of transaction cost compared to for example withdrawals from bank accounts, stocks and bonds.

4 Empirical analysis

This section explains the research design and regressions used for estimating spending propensities. Individuals have been allocated in the treatment or control group based on their date of birth. The 2012 retirement reform implied that the treatment group was more likely to opt out of the VERP scheme and cash out prior contributions relative to the control group. For this reason, average contributions to the VERP scheme should decline substantially for the treated savers, while their untreated peers should continue to contribute to about the same extent.

Figure 2 shows the average contributions to the early retirement pension scheme across calendar years, split in control and treatment groups. By mid-May 2011, the government announced that wage earners could opt out of the voluntary early retirement scheme and cash out prior contributions free of taxation between April and October 2012. The illustration shows how contributions of the two cohorts were parallel prior to the policy change. By 2012, average contributions dropped substantially for the treatment group, implying that many of these savers decided to leave the scheme. For the control group, average contributions were changed to a much smaller extent and broadly remained constant in the following years. The illustration presents the policy impact on VERP contributions based solely on the wage earners' date of birth.[6]

Figure 2: Annual contributions to Voluntary Early Retirement Pension (VERP)

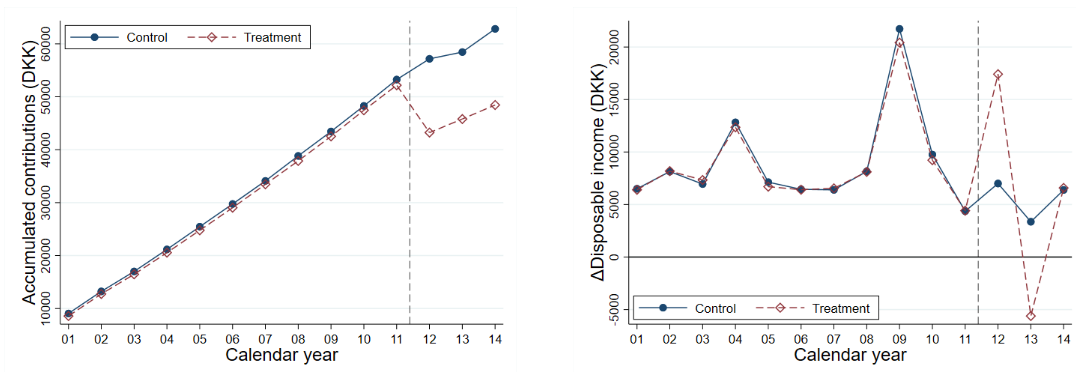


Notes: Annual contributions are measured as the average contribution to the Voluntary Early Retirement Pension (VERP) scheme for each group within each calendar year. The information is third-party reported as unemployment benefit schemes send individual information to the tax authorities as contributions can be deducted from the taxpayers' income before paying taxes. Source: Own calculations based on Statistics Denmark's administrative registers.

[6]Appendix Figure 5 illustrates the participation rate, showing an identical pattern. Participation is defined as having contributed to the VERP scheme continuously since 1999 until the year that the contribution to the scheme is reported to be zero.

The next step is to quantify how much more liquidity the treatment group obtained in 2012 relative to the control group. The left panel in Figure 3 shows the accumulated balance within the early retirement scheme across years. The substantial drop in 2012 for the treatment group relative to that of the control group measures the mean difference in payouts between the two groups. This drop in VERP balances is interpreted as the amount of liquid resources that the treated individuals gained access to after implementation of the reform compared to individuals in the control group.

Figure 3: Liquidity effect of Withdrawals from VERP



(a) VERP balance

(b) ΔDisposable income (incl. VERP)

Notes: The left panel shows the accumulated contributions to the Voluntary Early Retirement Pension (VERP) scheme for each group across calendar years. An increase in the balance indicates a net contribution, on average, to the VERP scheme and a decline indicates a net withdrawal. The right panel shows the first difference in disposable income for each group across calendar years. Dispoable income comprises total income net of tax payments, interest payments, pension contributions, early withdrawals from individual pension accounts, mandated pension accounts (Den Særlige Pensionsordning and Lønmodtagernes Dyrtidsfond), and withdrawals from the VERP scheme—all withdrawals are net of taxes.
Source: Own calculations based on Statistics Denmark's administrative registers.

The right panel in Figure 3 shows the first difference in disposable income for the two groups, including liquidity obtained from pre-retirement VERP withdrawals. This graph serves as an important piece of evidence in support of the identifying assumption; namely that disposable resources would have developed similarly for the treatment and control groups in the absence of the 2012-reform. This is due to the fact that pre-trends are developing identically up until the time of reform implementation. In the reform year, disposable income increases by an amount corresponding to the withdrawal of VERP assets shown in the left panel in Figure 3. The shock is transitory, which explains the substantial drop in 2013 and therefore, we focus solely on the behavioural effect in 2012 relative to all pre-reform years in the remaining part of the empirical exercise.

4.1 Empirical model and results

The discretionary liquidity shock that was potentially caused by the 2012-reform is quantified using a standard difference-in-differences estimator. The reduced form model in mind is given by

$$\Delta VERP_{i,t} = \alpha + \beta_1 POST_t + \beta_2 TREAT_i + \beta_3 POST_t \times TREAT_i + \omega_t + \gamma X_{i,t-2} + \varepsilon_{i,t}, \tag{1}$$

where $\Delta VERP_{i,t}$ is the first difference in VERP balances, depicted in the left panel in Figure 3, for individual i at time t. $POST_t$ takes the value 1 in the reform year, otherwise 0. $TREAT_i$ takes the value 1 for individuals born from 1 July to 31 December 1960 and 0 for those born from 1 July to 31 December 1955. Idiosyncratic errors are captured by $\varepsilon_{i,t}$. A range of observables are included in $X_{i,t-2}$ as controls, covering income, wealth, debt, real assets, capital gains, gender and a dummy for being married. Year-specific dummies are included in ω_t. Column 1 in Table 2 reports the parameter of interest, β_3, which quantifies an increase in liquid resources of DKK 12,800 that was plausibly caused by the 2012-reform. This withdrawal is intepreted as the average cashout by the treatment group minus the average cashout by the control group. The *ex ante* balances were identical for the two groups and the withdrawal option did not allow for cashing out only a part of the total balance. Either members opted out of the VERP scheme and withdrew the full balance or they remained within the scheme. The observed variation in withdrawals between the two groups implies that participation rates in the treatment group were reduced substantially more than for the control group. Appendix Figure 5 illustrates this exactly by plotting participation rates for the two groups over time. The estimate supports the graphical evidence that the 2012 policy was likely to generate a substantial increase in liquidity and this effect is estimated very precisely with a robust standard error DKK 261.

The increase in liquid resources was potentially spent immediately. To perform a statistical test of this hypothesis, we regress the change in spending on the change in VERP balances with a similar specification as in equation 1, where the interaction term, $POST_t \times TREAT_i$, is replaced by $\Delta VERP_{i,t}$. Estimating this model by OLS yields the parameter presented in column 3, Table 2, which points to an increase

Table 2: Marginal propensity to spend out of withdrawals

	Δ VERP		Δ Spending			
	(1)	(2)	(3)	(4)	(5)	(6)
POST=1 × TREAT=1	-12833***	-12764***				
	(261)	(261)				
Δ VERP			-0.350***	-0.361***	-0.459***	-0.433***
			(0.034)	(0.037)	(0.120)	(0.123)
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Individual FE		Yes		Yes		Yes
Model			OLS	OLS	IV	IV
Observations	216406	216406	216406	216406	216406	216406
F-statistic	2464.5	314.0	52.9	49.2	50.9	47.1

Notes: Robust standard errors in parentheses. * $p < .1$, ** $p < .05$, *** $p < .01$. Table 2 presents key estimates from equations 1 and 2. Columns (1) and (2) present the estimated difference in withdrawals between the treatment and control groups in 2012 relative to prior years, on average. Columns (3)–(6) present estimates of the propensity to spend out of VERP withdrawals for various model specifications. Control variables include income, wealth, debt, real assets, capital gains, gender and a dummy for being married.
Source: Own calculations based on Statistics Denmark's administrative registers.

in spending by DKK 35 for each DKK 100 withdrawal. Very similar spending propensity is estimated in a fixed effect regression model with identical specification (column 4). These estimates are, however, likely to be bias as withdrawals could be initiated by some demand shock, implying that the change in spending is driving the observed withdrawal behaviour. To account for possible reverse causality and confounds, we turn to estimating a two-stage-least-squares regression model given by

$$\Delta C_{i,t} = \alpha + \delta_1 POST_t + \delta_2 TREAT_i + \delta_3 \Delta \widehat{VERP}_{i,t} + \omega_t + \gamma X_{i,t-2} + r_{i,t}, \qquad (2)$$

where $\Delta C_{i,t}$ is the first difference in spending for each individual i in year t. Withdrawals from the VERP balance are instrumeted by the interaction term $POST_t \times TREAT_i$ described in equation (1). In doing so, the predicted values, $\Delta\widehat{VERP}_{i,t}$, should contain only variation that pertain to the 2012-reform which granted savers access to savings in the VERP scheme. In order for $POST_t \times TREAT_i$ to be a valid instrument, two assumptions must be met. First the instrument must be closely correlated with withdrawals. This is definately the case as the parameter in column 1 in Table 2 is highly significant. Second, the instrument should be uncorrelated to some common factor that potentially drives movements in both withdrawals and spending. This assumption is by definition untestable. However, the illustration of common pre-trends in both the left and right panels of Figure 3 indicates that the estimated shock is likely to be caused by the 2012-reform as developments between the compared groups of consumers were clearly identical in the pre-reform period. By arguing that this second assumption is not violated the estimated parameter δ_3 is unbiased. The error term $r_{i,t}$ is corrected for generated regressor bias as the two-step procedure is estimated simultaneously using STATA. The rest of the specification is identical to equation (1).

Column 5 in Table 2 presents the two-step estimator, indicating an average propensity to spend out of the liquidity shock of 46 per cent. By including individual fixed effects the model controls for time-invariant and person-specific factors such as preferences for spending which are fixed for each individual for the entire lifetime. This implies that the model produces a within-comparison, where each consumer is compared to themselves over time, such that unobservable characteristics between individuals are not biasing our results. This is our prefered specification, which points to an average increase in the propensity to spend out of a liquidity shock of 43 per cent. Column 2 presents the estimated parameter in the first stage, using this exact specification. In addition, all outcomes are estimated in reduced form and presented in Appendix Table 7.

According to data collected from all Danish unemployment insurance schemes by 1 October 2012 when the withdrawal window closed, more than 615,000 members had opted out of the VERP scheme and cashed out their balances, amounting to a total withdrawal of DKK 25.5 billion. By applying our estimated propensity to spend the implied increase in overall spending corresponds to 0.5 per cent of GDP—a non-neglegible growth contribution caused by a reform that was not specifically intended to boost demand.

The part of the withdrawals which is not spent immediately must be used to repay debt or increase the balance in other types of savings accounts. Table 3 presents the estimated substitution of savings from the VERP balance to taxable savings accounts in column 1, repayments of mortgage debt in column 2 and savings in pension accounts in column 3. The parameters in Table 3 are estimated using the prefered specification described above and all account types are measured after taxes.[7] Taxable accounts include deposits minus credit accounts in banks and end-

[7]Pension contributions are recorded in the administrative records before taxes are paid. To account for the value of the tax all pension contributions are adjusted by a fixed 40 per cent rate.

of-year values of stock and bonds. Mortgage debt is measured at the end of the year and pension accounts are measured as the sum of contributions in each calendar year. The model shows that 41 per cent of the withdrawals were saved in taxable accounts. No changes are detected in mortgage debt as this parameter is estimated with insufficient precision to be statistically significant. Pension contributions are estimated to increase by 7 per cent of the withdrawals.

Table 3: Substitution for savings accounts

	Δ Taxable savings	Δ Mortgage	Δ Pension
	(1)	(2)	(3)
Δ VERP	-0.414***	-0.041	-0.074***
	(0.118)	(0.053)	(0.012)
Controls	Yes	Yes	Yes
Year FE	Yes	Yes	Yes
Individual FE	Yes	Yes	Yes
Model	IV	IV	IV
Observations	216406	216406	216406
F-statistic	82.2	64.8	27.3

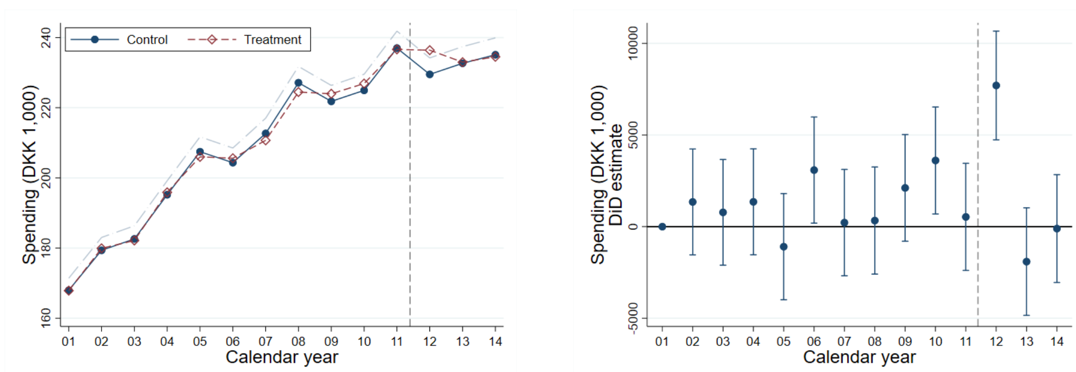
Notes: Robust standard errors in parentheses. * $p < .1$, ** $p < .05$, *** $p < .01$. Table 3 presents key estimates from equation 2. Columns (1) yields substitution to savings in deposits (net of credit), stocks or bonds for each DKK 1 of VERP withdrawal. Similarly, columns (2) and (3) show substitution for mortgage debt and retirement accounts, respectively. Other specifications follow those of Table 2. Source: Own calculations based on Statistics Denmark's administrative registers.

4.2 Heterogeneous spending response

The final step in the empirical analysis is to examine if the spending response differs based on individuals' *(ex ante)* access to liquid wealth. To proxy for access to liquidity, we use the cash-to-income ratio, which is found to correlate with the marginal interest rate at the individual level (Kreiner et al., 2019). Specifically, the marginal propensity to spend is estimated using the prefered model specification for subgroups in the sample which are possibly affected by a lack of access to liquid assets. We use the median in the cash-to-income ratio to divide the sample into a potentially liquidity constrained group (below-median) and an unconstrained group (above-median).

The left panel in Figure 4 shows the unconditional average spending levels for the treatment and control groups across calendar years. The blue line, representing the control group, has been shifted downwards from their actual position (grey line) based on the pre-reform years such than pre-reform years align for the treatment and control groups. This allows for easier comparison of pre-trends and reform effect in 2012, while imposing no parametric assumptions on spending behaviour. The spending developments are very alike over time but seem to deviate in 2012, which is consistent with our main results. The fact that the spending response can possibly be spotted by only observing plots from the raw data is reassuring. The right panel in Figure 4 plots the estimated coefficients from a regression model similar in spirit to equation 1, where $Post_t$ has been replaced by year dummies. Each year-treatment parameter represents the difference in spending between the treated and the counterfactual for each particular year and vertical bars are 5%-confidence levels. This illustration is a formalised test of what was observed in the left panel in Figure 4, namely that spending developments were almost identical between the two groups prior to the reform, while there was a statistically significant increase in spending for the treatment group in 2012.

Figure 4: Spending for savers with below-median cash-to-income ratio



(a) Nonparametric nominal terms

(b) Estimated treatment effect

Notes: The left panel show spending developments in nominal terms, unconditionally, for the treatment and control groups. The grey line is the actual spending by the control group, whereas the blue line shows the exact same developments but shifted downwards such that pre-reform years align with those of the treatment group, on average. The right panel shows difference-in-differences estimates of a regression almost identical to equation 1, where $Post_t$ is replaced by year dummies. The specification yields $\Delta c_{i,t} = \alpha + \beta_1^t \omega_t + \beta_2 TREAT_i + \beta_3^t \omega_t \times TREAT_i + \gamma X_{i,t-2} + \varepsilon_{i,t}$.
Source: Own calculations based on Statistics Denmark's administrative registers.

Table 4 provides estimates corresponding to the illustration in the right panel in Figure 4 using the prefered specification for equations 1 and 2. Column 1 shows that the average spending propensity increases by 68 per cent for the liquidity constrained group. The parameter is statistically significant at the 1% level. Column 2 shows an estimated spending propensity of 5 per cent for the group which, according to our cash-on-hand indicator, is not constrained by a lack of access to liquidity. This estimate is not significantly different from zero. This points to the fact that those with limited access to liquidity are driving the estimated spending response, which is consistent with the notion of hand-to-mouth behaviour. Savers with ample liquidity did not change their spending behaviour. The high degree of detail of the data and the sample size allows us to split the sample both by cash-on-hand but also based on the savers' total wealth. Columns 3 and 4 in Table 4 present the estimated spending propensities of poor and wealthy savers, respectively, who both hold less than two months' worth of income. The results show that spending increases by 67 and 62 per cent, respectively, and both measures are statistically highly significant. This indicates that liquidity constraints exist across the whole wealth distribution, which is consistent with the existence of wealthy hand-to-mouth households.

Table 4: Marginal propensity to spend across hand-to-mouth indicators

	Cash-to-income ratio				Housing-to-total wealth ratio	
	(1)	(2)	(3)	(4)	(5)	(6)
	$<$ median	$>$ median	$<$ median/low wealth	$<$ median/high wealth	$<$ median	$>$ median
Δ VERP	-0.677***	-0.054	-0.671***	-0.624***	-0.450	-0.599***
	(0.115)	(0.305)	(0.135)	(0.204)	(0.303)	(0.179)
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Individual FE	Yes	Yes	Yes	Yes	Yes	Yes
Model	IV	IV	IV	IV	IV	IV
Observations	108197	108209	65333	42864	76261	72540
F-statistic	35.0	31.9	16.1	19.6	28.2	37.0

Notes: Robust standard errors in parentheses. * $p < .1$, ** $p < .05$, *** $p < .01$. The cash-to-income ratio (cash-on-hand) and housing-to-total wealth ratio distributions are illustrated in Figure 1. Estimates correspond to the prefered specification used in Table 2 column (6) for below and above median subsamples. Low and high wealth correspond to the below and above median, respectively, of total wealth for the full sample.
Source: Own calculations based on Statistics Denmark's administrative registers.

Another way to identify the driving mechanisms behind our results is to split the sample based on how much of the individuals' wealth can be considered illiquid. We use the indicator illustrated in the right panel in Figure 1, showing the housing-to-total asset ratio by the end of 2011. If the observed behaviour in our data supports of wealthy hand-to-mouth behaviour, we should see a strong spending response for homeowners for whom a large part of their wealth is held as real assets rather than relatively more liquid financial assets. Columns 5 and 6 in Table 4 present estimates when splitting the housing-to-total asset ratio at the median. The group of savers with fewer assets bound in real estate shows no statistically significant sign of a spending response. However, savers with a high housing-to-total asset ratio increase spending by 60 per cent. The estimated heterogeneity presented in Table 4 indicates that indicators of liquidity constraints are good predictors of increased spending out of unanticipated liquidity shocks—even for savers with substantial wealth.

4.3 Robustness

To address possible caveats in the research design, Table 5 presents estimates associated with a set of robustness tests.

First, savings and retirement decisions are often decided at the household level rather than at the individual level. To account for the fact that the 2012-reform could impact the partners' incentives to opt out of the VERP scheme and withdraw the balance, spouse-cohort fixed effects are added to the specification. This addition removes variations in the spending response for individual i, which are potentially caused by a change in the behaviour of this individual's spouse. Column 1 shows that the average propensity to spend remains unchanged. Moreover, the spending response at the individual level could potentially underestimate the actual effect of the reform as one member of the household could increase expenditure as a direct response to the partner's VERP withdrawal. To test the importance of this, a similar regression model is estimated using spending and VERP withdrawals which are measured at the household level. Column 2 shows that the propensity to spend increase to 53 per cent, a larger spending effect than in our baseline model albeit not statistically different.

Second, we include municipality-year fixed effects into the specification. These covariates flexibly allows each of the 98 municipalities in Denmark to have their own time trend in the regression. Individuals living in certain geographical areas, with for example large house price increases, are likely to increase spending more than individuals in other areas. This specification provides an estimate of δ_3 which is not likely to be bias by for example local labour or housing market developments. The estimated average propensity to spend, reported in column 3, does not change by doing this.

Third, the consumption imputation is potentially affected by developments in market returns that are not associated with any active savings decisions by individuals in the sample. The wealth data do not contain separate values for quantities

Table 5: Marginal propensity to spend out of withdrawals

	Δ Spending					
	(1)	(2)	(3)	(4)	(5)	(6)
Δ VERP	-0.431***	-0.526***	-0.417***	-0.294**	-0.446***	-0.494*
	(0.123)	(0.156)	(0.122)	(0.130)	(0.122)	(0.261)
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Spouse-cohort FE	Yes	Yes	Yes	No	No	No
Municipality-year FE	No	No	Yes	No	No	No
Individual FE	Yes	Yes	Yes	Yes	Yes	Yes
Model	IV	IV	IV	IV	IV	IV
Observations	216406	207259	216406	153869	216402	266334
F-statistic	43.5	47.3	6.8	24.2	50.5	35.1

Notes: Robust standard errors in parentheses. * $p < .1$, ** $p < .05$, *** $p < .01$. Each column in Table 5 represents a test of robustness by reproducing the propensity to spend estimator from column (6) in Table 2 with a variation of model specification or sample selection. Column (1) includes spouse-cohort fixed effects to account for variations in spending caused by the partner being affected by the policy change. Column (2) uses spending and VERP withdrawals which are measured at the household level. Column (3) includes municipality-year specific dummies to account for differential developments over time between the 98 municipalities in Denmark. Column (4) omits savers with non-zero savings in stocks or bonds. The data do not allow us to separate passive capital returns from active buying and selling of financial assets. By estimating the spending propensity on a subsample without financial assets *(ex ante)* the parameter value remains statistically significant. This implies that the estimated spending response is not driven by noise caused by financial markets. Column (5) uses spending and withdrawal measures that have been divided by lagged income. In doing so, the liquidity shock caused by the 2012 reform is scaled to the size of each individuals' economy and accounts for the possibility that low wage earners would respond differently to the shock than high wage earners. Column (6) reproduces the estimated propensity to spend out of VERP withdrawals by censoring outliers at the 1st and 99th percentiles.
Source: Own calculations based on Statistics Denmark's administrative registers.

and prices. An isolated increase in stock prices would therefore look like an increase stock savings for savers who hold this particular stock. The majority, about 3/4, of individuals in the sample does not hold stock or bonds. By estimating our regression models on this group of savers, the average propensity to spend remains statistically significant—albeit lower at 30 per cent (column 4). This implies that the imputed spending measure is not driven solely by noise from financial market developments. The point estimate of this very selected subgroup does not differ significantly from that of our prefered model.

Fourth, the size of withdrawls in nominal terms could potentially affect low income earners more than high income earners. To account for the size of each individuals' economy we scale both the change in spending and the liquidity shock by lagged income. This does, however, not change our results in any important way as shown in column 5.

Finally, we re-estimate the two-step model using variables that have been censored at the 1st and 99th percentiles. In doing so, we allow for more noise in the data, with the effect that standard errors increase substantially. However, the estimated propensity to spend, reported in column 6, does not change significantly.

5 Conclusion

This paper has evaluated a Danish 2012 retirement reform that tightened eligibility for early retirement benefits differentially across birth cohorts. One side effect of this policy was that savers could opt out of the scheme and automatically have prior contributions repaid. These were illiquid savings that until the reform were locked in the early retirement scheme and paid out only upon opting out and paying a 30 per cent tax penalty. In 2012, the whole balance, about DKK 60,000 (USD 9,500), could be cashed out on a tax-free basis.

By exploiting a particular feature of the reform that generated plausible exogenous variation in the incentive to opt out of the voluntary early retirment pension scheme, this paper aims to identify the propensity to spend out of a liquidity shock. Identification hinges on the idea that cohorts most severely affected by the policy were more likely to opt out of the scheme and withdraw their balance compared to less affected cohorts. Access to detailed administrative data enables an empirical investigation where savings and spending behaviours are tracked across time. Using a difference-in-differences setup, we estimate an average increase in the marginal propensity to consume of 43 per cent.

The level of detail of the administrative registers makes it possible to stratify the estimated spending response across important characteristics of the consumers. Indicators of liquidity constraints are found to predict the increase in spending. Moreover, we are able to demonstrate that liquidity constraints also play a role for high-wealth consumers. These findings support the hypothesis that spending out of unanticipated liquidity shocks can be explained both by a lack of access to liquidity but also by the notion of wealthy hand-to-mouth households.

The take away from these empirical results is that government policies which have a long term aim, for example to increase labour supply, could also affect the business cycle in the very short run. A back of the envelope calculation indicates that the spending increase in the context of the reform studied in this paper corresponds to 0.5 per cent of GDP. Any policy that provides households with a substantial shock to liquidity could potentially increase spending in the short term. Policymakers should keep this in mind, so that policies are not implemented at times when they could work procyclically.

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A Appendix

Table 6: Postponement and reduction in years eligible for early retirement benefits across cohorts

		Years of postponement						
		0	**.5**	**1**	**1.5**	**2**	**2.5**	**3**
	0	1953	1954 H1	1954 H2	1955H1	1955H2		
Years of reduction	**.5**						1956H1	
	1						1956H2	1957/1958
	1.5							1959H1
	2					1960H2	1960H1	1959 H2

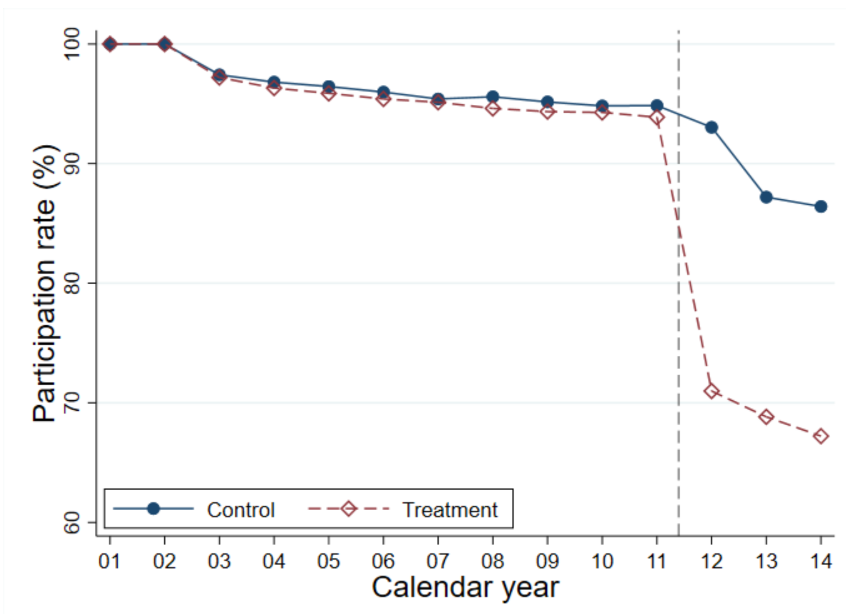
Notes: The table presents the 2012 reform impact across cohorts. The vertical axis is divided in cells that count the number of years by which eligiblity for Voluntary Early Retirement Pension (VERP) benefits was reduced. The horizontal axis divides savers into cells that count the years of postponement of the VERP retirement age. H1 and H2 categorise individuals based on birth dates, where H1 covers people born from 1 January to 31 June and H2 covers people born from 1 July to 31 December. Coloured cells indicate which cohorts became subject to asset testing when the 2012 reform was implemented.
Source: Own calculations.

Table 7: Difference-in-differences estimates

	Δ VERP		Δ C		Δ Taxable savings		Δ Mortgage		Δ Pension	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
POST=1	-3044***	-3033***	-15096***	-10042***	10485***	15071***	-11477***	-11693***	-2573***	-1603***
	(329)	(332)	(1790)	(1905)	(1680)	(1768)	(935)	(1060)	(158)	(176)
TREAT=1	8	0	442	0	-752**	0	-347	0	-67	0
	(5)	(.)	(415)	(.)	(373)	(.)	(316)	(.)	(43)	(.)
POST=1 × TREAT=1	-12833***	-12764***	5887***	5521***	4963***	5288***	427	522	975***	943***
	(261)	(261)	(1544)	(1566)	(1488)	(1506)	(676)	(681)	(153)	(157)
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Individual FE		Yes		Yes		Yes		Yes		Yes
Observations	216406	216406	216406	216406	216406	216406	216406	216406	216406	216406

Notes: Robust standard errors in parentheses. * $p < .1$, ** $p < .05$, *** $p < .01$. The table presents reduced form estimates of all output variables used in the analysis using equation 1.
Source: Own calculations based on Statistics Denmark's administrative registers.

Figure 5: Membership rate for the VERP scheme



Notes: The lines represents the probability of being a member of the Voluntary Early Retirement Pension (VERP) scheme for the treatment and control groups across calendar years. Membership is defined as having contributed to the scheme since its launch in 1999 until contributions are reported as zero by the unemployment insurance scheme.
Source: Own calculations.

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