Why Do People Hold Deposits?*

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Abstract

We provide the first comprehensive tests in the household finance literature of why individuals invest in deposits, for many their largest and sometimes only financial asset. Our findings are threefold. Using detailed register data covering every individual in Denmark we first show that people actively readjust their deposits following an exogenous increase, as captured by unexpected inheritances. Second, we find that people use deposits and voluntary unemployment insurance as substitutes. Finally, interest rates paid on deposits do not drive deposit demand. Collectively, these results support that people hold deposits primarily for precautionary reasons.

Keywords: household finance, bank deposits, liquidity management, precautionary savings

JEL classification: G11, G51

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1. Introduction

Many individuals have substantial savings in their deposit accounts. In the US, the median household allocated $5,300 to its deposit account in 2019, corresponding to app. 20% of total financial assets, including retirement accounts (Bhutta et al., 2020). In Denmark, for which we observe detailed deposit data, the median individual correspondingly allocated approximately DKK44,600 (corresponding to $6,400) to its deposit account in 2018. Furthermore, deposits is the only financial asset for 70% of the individuals in our sample. While research in household finance has advanced our understanding of many aspects of households' financial decisions,[1] there is scarce empirical evidence on why people hold deposits, despite their importance for many households. This paper fills this gap by offering a comprehensive empirical investigation into individuals' use and demand of deposits.

It is important to understand why people hold deposits if research is to speak to the question of potential overallocation. It is also important for understanding the credit debt puzzle, i.e. the finding that many people hold deposits in spite of substantial debts on their credit cards (Gross & Souleles, 2002; Telyukova and Wright, 2008). Finally, it is important for banks, as deposits constitute a main source of financing. We hypothesize that individuals hold deposits in excess of what is needed for transactions either due to i) inactiveness, ii) precautionary saving motives, or iii) that deposits serve as a risk-free savings device.[2] Our overall conclusion is that people primarily hold deposits for precautionary reasons.

We reach this conclusion by first examining potential inactiveness, which refers to the finding that households generally exhibit inertia with respect to their financial decisions (see e.g., Madrian and Shea, 2001; Andersen et al, 2020). Inaction might explain the substantial amount of savings we see on individuals' deposit accounts, as most people receive a salary and/or government transfers (unemployment benefits, child support, pensions, etc.) on their deposit account and may accordingly just keep it there, even if it would be beneficial not to. To test if inactiveness explains the high savings level in deposits we need to identify exogenous variation in individual deposits. Motivated by Andersen & Nielsen (2011) we use unexpected inheritances resulting from a sudden death of a close relative. From Danish register data we know – in addition to individual-level household economics and finance data – the medical classifications of diseases. We focus on individuals who pass away unexpectedly to ensure

[1] Such as their stock market participation (e.g., Haliassos & Bertaut, 1995, Odean, 1999, and Vissing-Jørgensen & Attanassio, 2003), mutual fund holdings (e.g., Florentsen et al., 2021), real estate purchases (e.g., Andersen & Nielsen, 2016), mortgage decisions (e.g., Andersen, Campbell, Nielsen, & Ramadorai, 2020), and credit card debt (e.g., Gross & Souleles, 2002).

[2] In our data the median (average) individual has more than two (seven) months of disposable income in their deposit account, arguable vastly exceeding what is needed for immediate day-to-day transactions. Hence, the pure transaction motive for holding deposits presented by Santomero (1974) cannot by itself explain why individuals hold deposits, as has long been known (see e.g., Plessner & Reid, 1980).

that the windfall gain to the beneficiaries is entirely random and exogenous, and that the beneficiaries thus do not act in anticipation of an inheritance. We find that treated individuals on average experience an increase in their deposit account of DKK 62,000 (app. $8,800) after an inheritance, corresponding to 3.3 months of disposable income, relative to a matched control sample of non-inheritors. We then test whether the average treated individual subsequently readjusts her deposit account. We find that the initial increase in deposits due to inheritance is subsequently reversed to a level of DKK 25,000 (app. $3,500), corresponding to one month of disposable income, after four years. The finding that people readjust their deposit levels following an exogenous windfall gain speaks against inactiveness in deposit behavior.

We examine if there is heterogeneity in the extent of the deposit adjustment among the treated individuals. Our hypothesis is that a target level of deposits implies that individuals with relatively low deposits levels prior to receiving an inheritance will keep the windfall gain in their deposit account, while those with an already liquid position will readjust downwards. This is confirmed in the data, where the effect of inheritance on deposits is persistent for individuals with low prior liquidity holdings, while it becomes less persistent the more liquid assets an individual has before receiving the inheritance. In other words, the most liquid individuals readjust their deposit holdings completely within four years, while the least liquid people use their inheritance to build up a persistent level of deposits.

We next investigate the two remaining hypotheses, namely if individuals target a certain level of deposits to insure against unexpected income shocks, or if deposits serve as a risk-free investment asset. To examine the pre-cautionary motive we study the effect of unemployment and unemployment insurance on individuals' use of their deposit accounts. The idea is that unemployment is the kind of negative income shock that the deposit account should insure against, if people hold deposits for precautionary reasons. Alternatively, however, many individuals in Denmark purchase an unemployment insurance that supplements the public unemployment benefits in case of unemployment. If people invest in deposits for precautionary reasons, we hypothesize that individuals with an unemployment insurance invest less in their deposit account, compared to a matched sample of uninsured individuals. Our results support this, demonstrating that deposits are used as a substitute for unemployment insurance. To further establish this, we test if individuals with an unemployment insurance reduce their deposit holdings less than those without an insurance when unemployment hits, i.e., whether the deposit account indeed functions as a cushion in the event of a realized negative income shock. We find that unemployment leads to a drop in deposits of 5% for individuals with an unemployment insurance, whereas those without insurance face a drop of 23%, thereby heavily drawing upon their deposit wealth during unemployment.

Collectively, these results indicate that deposits function as a precautionary buffer to adverse wealth shocks.

Finally, for completeness, we examine whether people hold deposits because they consider it an attractive risk-free investment. While there are other risk-free assets that return-dominate deposit holdings – such as government bonds or certificates of deposits – there are reasons why individuals might prefer investing in deposit accounts. Most notably, provided the return dominance is not too excessive, people may naturally prefer deposits as it is the predominant asset used in transactions (Campanale, Fugazza and Gomes, 2015). But any risk-free investment motive naturally implies that deposit levels should correlate positively with interest levels on deposits. To examine this, we rank people according to the interest rate they earn on their deposit. We then compare individuals who earn a high interest rate on their deposits to individuals who earn a low interest rate, making sure that the individuals in the two groups have the same level of deposits (we match people in the high-interest rate group with people in the low-interest group based on their level of deposits). We find that individuals who earn a high interest rate on their deposits do not increase their deposit holdings the following year, compared to their low-interest rate peers, controlling for the size of deposits and other background characteristics. This indicates that other motives than risk-free return, such as precautionary motives, drive deposit demand and the observed readjustments of deposits following cashflow shocks.

To sum up, this paper provides new and comprehensive evidence on individuals' decision to hold deposits using detailed high-quality micro-data on the entire Danish population. We find that people actively readjust and target their investments in deposits when exposed to windfall shocks. We also show that people use their deposits as a buffer against negative income shocks. Collectively, these results demonstrate that precautionary motives are key determinants of people's deposit demand.

Literature. Whereas the demand for deposits has not received a lot of attention in the household finance literature, Gross & Souleles (2002) document that almost all households in the US with credit card debt simultaneously have a positive amount of liquid assets. This is surprising because the interest charged on credit card debt is far greater than the interest earned on a deposit account. Bertaut, Haliassos, & Reiter (2009) found similar numbers using a larger sample over multiple years. This has been explained in a variety of ways, such as household self-control issues (e.g., Bertraut et al., 2009) and precautionary borrowing (e.g., Druedahl & Jørgensen, 2018), but relevant for this paper are the ones that focus on the benefits of having money in a deposit account. For example, Telyukova & Wright (2008) develop a model showing that it might be optimal for individuals to hold deposits despite having credit card debt. Telyukova (2013) develops a similar model. It predicts that any amount held in excess of what is needed for transactions is caused by the precautionary motive. She fits the model to US data and find that the precautionary motive can account for around 50% of household deposit demand. While this helps us understand deposit demand, structural models as provided by Telyukova (2013) restrict attention to the transactional and precautionary motives, while disregarding other potential motivations for deposit demand, such as inaction. We examine empirically such alternative potential motivations.

Mostly related to our paper is Deuflhard, Georgarakos, & Inderst (2019), which empirically study the drivers of deposit demand among households. Their focus is different from ours, though. They study deposit-return heterogeneity in a Dutch Household Survey and examine whether households switch between banks to obtain higher returns on their deposits. They find that some households switch banks while others do not, and that financial sophistication explains this heterogeneity. We, in contrast, investigate the demand for deposits more generally, not merely focusing on the risk-free savings motive. In doing so we apply high-quality register data and utilize exogenous changes in deposits and employment to identify drivers of deposit demand.

The rest of our paper is structured as follows. Section 2 describes the data we use, as well as presents summary statistics on deposits. Section 3 studies whether people are inactive or whether they adjust towards a deposit target level, while Section 4 studies the precautionary motive for holding deposits. Section 5 investigates whether interest rate levels affect the demand for deposits. Section 6 contains additional robustness tests and a final section concludes.

2. Data

To investigate the demand and use of deposits we apply annual high-quality register data from Statistics Denmark that covers all individuals in Denmark above the age of 18 who have a deposit account. Our data spans the period from 2003 to 2018. We are interested in how individuals use their most liquid accounts, and hence only consider standard bank accounts, including accounts into which salary is paid and savings accounts where there are no limits as to when the money can be withdrawn. Child savings accounts, real estate savings accounts, or private pension accounts are not considered. Our sample of accounts make up 98% of all accounts in 2018. Some individuals have accounts in one bank only, whereas others have accounts in several banks. We sum the amount individuals have in their different accounts at the end of each year. Our data contain a personal identification number for each individual, which we can use to merge the individual's deposit account data with other data registries provided by Statistics Denmark. This means that we can observe numerous characteristics and financial information for each individual, such as age, gender, labor income, employment status, employment insurance, educational history, marital status, number of kids, debt (both mortgage, bank and other debt), holdings of stocks and bonds, etc. Most important for our study we can also identify children-parent relations, which is needed when investigating the effects of inheritance on deposits.

2.1 Summary statistics for the full sample

Panel A of Table 1 gives an overview of our full sample. The full sample includes all individuals in Denmark above the age of 18 who have a deposit account. We omit self-employed individuals and those working for their spouse (this is standard in the literature, see e.g., Andersen et al., 2020). This is because their involvement with their own business can drive their demand for deposits, as opposed to the motives studied in this paper. We also omit individuals who are not in the labor market (as defined by the authorities in our data), while also not receiving government aid (including public pensions) nor attending any educational institution. These individuals generally have very little disposable income and it is unclear how they finance their consumption. In total this leaves us with 5,285,371 individuals and 55,039,371 person-year observations for the period from 2003 to 2018. All DKK-variables are winsorized at a 1% and 99% level within each year to make our results more robust to outliers.

[Insert Table 1]

It follows from Panel A in Table 1 that the average level of deposits equals DKK 116,743, but it is noted that the distribution is relatively dispersed and skewed with a median of deposits equal to DKK 34,955. For ease of interpretation, our study mostly uses the number of months of income deposits to measure an individual's deposit holdings, defined as 12 times the deposits over income. It follows that an average individual holds 7.70 months of income in her deposit account, with a median of 2.34 months of income. Here income is the disposable income of the individual defined as labor income, social welfare, unemployment benefits, child benefits, pension payouts, capital income, and inheritance, less interest payments and tax payments.

The average individual has DKK 723,456 in assets, where assets include deposits, stock and bond holdings, as well as the public property value of any held properties.[3] 55% of assets are held in the deposit account for the average individual. Deposits therefore make up a very large part of the individual's assets. On top of that, the fraction of deposits to assets is 100% for 43% of the individuals in the population, i.e., for almost half the population their bank deposits are their sole asset.

[3] The property value is divided by the number of owners.

In our subsequent regression analysis we control for age, education, net wealth, stock market participation, homeownership status, marital status, household type, number of children, work experience, time spent unemployed, region of residence, and status on the labor market. Some of these variables need explaining. Statistics Denmark reports for each individual an education level from 1 to 4 (primary school, secondary school, higher education, and PhD) and it follows from Table 1, Panel A, that for example 26% have a higher education, i.e., level 3 or above. Homeownership status is a binary dummy indicating that approximately 44% of people owns their home. Household type can be classified as "living alone", "live as a couple", or "live with several families" (household's adults belong to different families), where 30% of our sample live by themselves. Number of children equals the number of children an individual has, that are below the age of 25 and still live at home. Work experience is the total amount of time an individual has spent employed, measured in years. Time spent unemployed is the total amount of time an individual has spent unemployed during his/her lifetime, measured in years. The region of residence is represented by which of the five administrative regions of Denmark (Copenhagen, Zealand, Southern Denmark, Central Jutland, Northern Jutland) the individual has her home-address. Finally, Statistics Denmark defines each individual's status on the labor market, where the relevant categories for this study are salaried workers (54.43%) and those who have been unemployed at least half the corresponding year (1.72%), with the remainder being those who receive sick benefits, are students, have ailments making them permanently unable to work, are retired, etc.

2.2 Inheritance data

As a first step, we want to know if inactiveness explains the substantial amount of savings people have in their deposit accounts. As motivated in the Introduction we want to test this, by examining potential adjustments to deposits of beneficiaries who receive a sudden and unexpected inheritance paid into their deposit account. We identify those individuals who receive inheritance due to the sudden death of a close relative, where a sudden death is defined as an unexpected death occurring less than 24 hours from the onset of symptoms. A sudden death is, by nature, a random event, and hence the inheritance that results from the sudden death is a natural experiment that induces exogeneous variation in the deposit account of the beneficiaries. It is unlikely that the beneficiaries anticipate the timing of the inheritance and act accordingly (such as borrowing in anticipation of future wealth). To mitigate the possibility that some individuals might nevertheless anticipate an inheritance, even if a death is sudden, we follow Andersen & Nielsen (2011) by using various control variables and narrowly defining sudden death, as described below.

In the Danish Inheritance Act passed in 1964, relatives are divided into three subgroups: the spouse, children, and grandchildren of the deceased (Group 1), the legal parents and siblings of the deceased (Group 2), and grandparents and their children (Group 3). The default rule is that Group 1 inherits, but if there are no living relatives in Group 1 (or 2), then Group 2 (or 3) relatives inherit. Within Group 1, the default sharing rule is that the spouse and children divide the deceased estate evenly unless the spouse delays the children's inheritance until their death.[4] We focus on deaths where Group 1 relatives exist and the deceased has no spouse (due to being widowed, divorced/separated, or never married). This simplifies the analysis, as the default sharing rule for children is an even split.[5] The default sharing rule can be changed with a will, but not to less than 25% of what they would have inherited according to the default rule (50% before 2008). Since less than 10% of the Danish population has a will (Fagbladet FOA, 2017), we will for simplicity assume that the default rule applies. Group 1 is subject to an estate tax of 15% if the net wealth of the estate exceeds DKK 301,900 (app. $50,000). This tax applies to all assets and as unrealized capital gains are furthermore not taxed directly, there is no tax motive to keep or liquidate specific assets (Andersen & Nielsen, 2011). The Probate Court will soon after an individual's death take control of the deceased's assets to meet the liabilities and will then transfer the remaining wealth to the beneficiaries according to the default rule or a will. This process must be completed at the latest one year after the date of death.

Between 2003 and 2018, we identify 288,667 parents who die without a spouse; however, we are only interested in those that face a sudden death. Following Andersen & Nielsen (2011) we distinguish between natural deaths and non-natural deaths and consider the following natural causes of death to be sudden (medical ICD-10 diagnosis code in parenthesis): acute myocardial infarction (I22-I23), cardiac arrest (I46), congestive heart failure (I50), stroke (I60-I69), and sudden death where the cause is unknown (R95-R97). Most non-natural causes of death are defined as sudden (V00-V99: vehicular accidents; X00-X59: exposure to harmful substances and forces of nature; X86-X90: death related to drugs or chemical substances), but exclude suicide and violent assault, as those could potentially be anticipated.

Table A.1 in the Appendix gives an overview of the number of deaths by cause of death over our sample period from 2003 to 2018. It follows that over our sample we have 32,038 cases where a person without a spouse faces a sudden death. Of these, we keep only those who die with a positive net-wealth leaving us with a sample of 26,929 death cases. From Table A.1 it follows that in total we have 58,515

[4] Before 2008 it was one-third to the spouse and two-thirds to the children.

[5] Most widows choose to delay the inheritance to their children, so when the widow dies, the children inherit the entire shared estate of both of their parents (Andersen & Nielsen, 2011). Additionally, some people might inherit twice, due to their unmarried parents dying at different times. These beneficiaries are excluded to simplify the analysis.

beneficiaries above 18. However, we only consider those who inherit a positive amount and is included in our deposit sample the year they inherit, and hence, the total number of beneficiaries reduces to 36,409. Additionally, we require that all beneficiaries are in the sample consecutively (i.e., do not disappear from the data one year and then reappear), and that data exist at least two years before and after the inheritance. Finally, we remove all observations more than 4 years after inheritance, as we find this to be an appropriate number of years to investigate the dynamics of how inheritance affects deposits. This leaves us with 18,934 individuals and 228,491 person-year observations.

Panel B of Table 1 gives an overview of our inheritance-sample. The summary statistics are calculated in the year prior to the inheritance. Comparing with the summary statistics of the full sample stated in Panel A of Table 1, we see that the inheritance-sample is similar with respect to the deposits and months of income in deposits but differs in several of the other variables. The reason for the difference in for example work-experience, fraction of homeowners and fraction of salaried workers is due to the fact, that our full sample include all individuals above 18, i.e., there is a much higher fraction of students and retired individuals in the full sample compared to the inheritance-sample.[6] This is reflected in the much bigger standard deviation in the age-variable for the full sample compared to the inheritance-sample. There is no reason to believe that our inheritance-sample is not representative for the general population (e.g., months of income in deposits, fraction of females, fraction of people living in the different regions of Denmark are quite similar), but to control for the above-mentioned differences we apply matching in our analysis that follows.

3. Do people have a target for their deposits?

In this section, we explore whether people mean-revert their deposit holdings after large deposit fluctuations, i.e., whether people target a certain level of deposits. In the first test, we investigate what happens after individuals receive a large, unexpected windfall gain on their deposit account. Do people simply leave the money there or do people readjust, bringing deposits back to their level before the event? In a second supplementary test, we investigate how people adjust their deposit account when they buy a house and use some of their deposits to make the down payment.

3.1. Deposits and windfall gains

The windfall gain we examine is inheritance resulting from the unexpected death of a parent. This is an exogenous change in the amount of money on peoples' deposit account.

[6] 8% (23%) of the individuals in the full sample are students (retired), whereas less than 1% (3%) of the individuals in our inheritance-sample are students (retired).

When a person inherits from his/her deceased parent, the wealth of deceased is transferred to the beneficiaries' deposit account. If financial inertia is a main reason for holding deposits, then one would expect that people would simply keep their inherited money in their deposit account, as reallocating it requires decisive action. On the other hand, if individuals have a target for their deposit accounts, one would expect that they subsequently readjust their deposit level as a response to observing it increase following inheritance.

Note that a target level is consistent with both the precautionary and risk-free motive, as both suggest that there is an optimal level of savings in the deposit account. According to the precautionary motive, deposits are used to insure against income fluctuations and sudden expenditures, but at some point, people have enough insurance and would prefer to seek higher returns with other investments. Similarly, there is some optimal level of risk-free investment, so if an individual gets a large sum of money, some part of that will be divested to other assets. Therefore, testing whether there is a target level is a test of whether the inaction motive is a main determinant of deposit demand. If inaction is empirically rejected due to readjustment of deposit levels, the next task is to test whether it is the precautionary motive or the risk-free rate motive that determines the target level.

In Section 2.2, we described how we identify inheritance resulting from the sudden death of a parent. To further identify the causal effect of inheritance on deposits, we construct a matched control sample. We find a match for each beneficiary in the year before they inherit from a sample of Danish individuals who have not received an unexpected inheritance and fulfill the sample selection criteria applied to the beneficiaries as described in Section 2.2. We match based on the linear propensity score estimated with a logit regression and 1:1 nearest neighbor matching with replacement, as suggested by Stuart (2010) to reduce bias. We use the control variables such as age, gender, immigration status, and unemployment in a given year as the covariates for the matching. Further details on the matching methodology are provided in Appendix B. A comparison of the summary statistics of the treatment and control groups can be found in Table A.2 in the Appendix. The summary statistics are calculated in the year prior to the inheritance. Comparing the treatment and control groups, i.e. Panel A and Panel B of Table A.2, it follows that the covariates are well balanced, as all the means, medians and standard deviation of the various control variables are very similar in the two groups.

Figure 1 shows how deposits of the beneficiaries (the treatment group) as well as the control group evolve around the time of receiving an inheritance. We show developments in deposits four years prior to receiving an inheritance until four years after. Recall that our data are end-of-year observations. Year 0 is the year the parent passes away, so when we have end-of-the-year observations, some beneficiaries receive the funds already in year "0" while others receive it in year "1". Recall also, as mentioned in Section 2.2, that the Probate Court has one year to complete the transfer of wealth from the deceased to the beneficiaries, and, in addition, it might take time to sell those inherited assets the beneficiary does not wish to keep. For these reasons, deposits increase during the year the parent passes away (year 0) as well as the following year (year 1).

[Insert Figure 1]

In this section, we are particularly interested in what happens after the inheritance, i.e., whether people subsequently relocate or simply keep their inheritances on their deposit account. Figure 1 indicates that people reallocate. After Year "1" in Figure 1, deposits of the treatment groups start falling relative to those of the control group, i.e., the treated individuals readjust their deposits holdings. This suggests that the inheritance brings people above their target level, leading them to subsequently decrease their deposit holdings, i.e., people are not inactive.

The evidence in Figure 1 is suggestive. To test our hypotheses more rigorously, we run the following two-way fixed-effects difference-in-differences regression to identify the causal effect of inheritance on deposits:

$$y_{it} = \gamma AfterInheritance_{it} + \boldsymbol{x_{it}\beta} + \theta_t + c_i + u_{it} \tag{3.1}$$

where $AfterInheritance_{i,t}$ is a dummy variable equal to "1" during years after an individual has inherited and 0 before the event, $\boldsymbol{x_{it}}$ is a row vector of control variables, $\boldsymbol{\theta_t}$ is a time-fixed effect, and $\boldsymbol{c_i}$ is an individual fixed effect. As control variables we include income, education, net wealth (excluding deposits), stock market participation, homeownership status, civil status, household type, number of children, work experience, the cumulative time spent unemployed, connection to the labor market (salaried, student, etc.), and region of residence.[7] Our coefficient of interest in (3.1) is $\boldsymbol{\gamma}$. It measures the differences-in-difference in deposits between those who have inherited and those who have not, i.e. $\boldsymbol{\gamma}$ reveals how an average change in deposits following inheritance affects the outcome variable, compared to an average change in deposits of those who have not inherited in the same period.[8]

[Insert Table 2]

[7] When months of income in deposits is the dependent variable, income is not among our control variables.
[8] As people can inherit in different years, this is not the typical difference-in-difference estimate. Rather the estimated coefficient is equal to the variance-weighted average of all possible simple 2x2 differences-in-difference estimates that compare one group that changes inheritance status to another group that does not (Goodman-Bacon, 2021).

We look at three outcome variables: deposits in levels, 1 + deposits in log, and months of income in deposits. The results of the regressions can be seen in Panel A of Table 2. The results for deposits in levels can be seen in columns 1 and 4, deposits in logs can be seen in columns 2 and 5, and months of income in deposits can be seen in columns 3 and 6. Furthermore, in columns 1-3, we show unconditional average effects, while in columns 4-6, we show conditional averages, i.e., results from regressions where we include individual control variables, year fixed effects, and individual fixed effects. Focusing on the most interpretable and rigorous results in columns 5-6, we see as expected that the effect of inheritance, controlling for other variables, is generally positive and rather large. Column 5 shows that deposits increase on average and across all beneficiaries by around 23% as a result of an inheritance, corresponding to 2.3 months of disposable income (column 6). This validates our identification, but to capture whether people readjust their deposits, we need to study the dynamics and to separate between people who have potentially already reached their targeted level of deposits before inheriting and people who were not at their target before the inheritance.

To explore the average effects of inheritance on deposits over time, we run the following regression:

$$y_{it} = \sum_{k=0}^{4} \gamma_k Inheritance_{i,t-k} + \boldsymbol{x_{it}\beta} + \theta_t + c_i + u_{it} \tag{3.2}$$

where $Inheritance_{it}$ is a dummy taking the value 1 in the year of the inheritance, and 0 otherwise. We include 4 lagged values of this inheritance dummy, such that γ_k reveals the cumulative effect of inheritance *k* years after inheritance. Essentially, γ_k is splitting the effect identified in regression (3.1) into 5 parts, to reveal the effect of inheritance in each individual year after inheritance.[9]

The results of these regressions are in Table 2, Panel B. In column 6, controlling for everything else, we see that one year after inheritance, deposits have increased by 3.3 months of income, but 4 years after inheritance, that effect has decreased to 1.3 months of income. This shows that while there is a large mechanical initial increase in deposits due to an inheritance, this effect vanishes over time, meaning that people do not keep all the money they inherit in their deposit account. The difference between what people have on their deposit account four year after inheriting and the year after the inheritance is statistically significant (tests are in the "4 years – 1 year" row), i.e., the readjustment of deposits is

[9] With staggered introduction of treatment, estimation of the average treatment effect on treated in a given year relative to the treatment year, requires the assumption that the average treatment effects are homogeneous across treatment cohorts (Sun & Abraham, 2021). In our case this assumption seems reasonable, and we thus interpret our estimates causally.

statistically significant. This confirms that the readjustments we observed in Figure 1 are significant after controlling for individual characteristics and fixed effects.

The results in Panel B of Table 2 indicates that people readjust their deposits after a windfall gain. But the results also indicates that people after four years on average have a higher level of deposits. There is heterogeneity around this average effect, however. There might be people who were not at their target level of deposits before they inherited, leading them to increase their deposit holdings following the event, while others might return fully to their target. The hypothesis we want to test, thus, is that those individuals who already had a relatively high level of liquid assets prior to the treatment should, after the treatment, reallocate most of their inheritance away from their deposit account, while those with relatively less should keep more of their inheritance in their deposit account. To test this hypothesis, each year we divide the sample into quintiles based on people's liquid position in the year prior to inheritance.[10] As we split people into groups, we redo our matching procedure, such that we match within liquidity quintiles. Essentially, instead of looking for similar individuals to those that inherit in our full sample one year before they inherit, we split our sample into five based on liquidity in that year, and then find matches from within that quintile. We then re-estimate the regression in Equation (3.2) for each quintile (liquidity group). As the outcome variable, we for brevity focus on months of income in deposits. Looking at deposits in levels or logs yield similar results (available upon request). The results are in Table 3 and shown graphically in Figure 2. Group 1 contains the individuals with the lowest level of liquidity, while group 5 contains individuals with the highest level of liquidity.

[Insert Table 3]

The results confirm our hypothesis. There are vast differences in the dynamic behavior of people who have a very liquid position prior to inheriting and those with low liquidity. The results in Table 3 show that people who held relatively few liquid assets prior to inheriting increase their deposits corresponding to 2.1 months of income one year after the event. Four years after the event, their deposits are still 1.5 months of income higher than prior to the event. On the other hand, those individuals who held large positions in liquid assets prior to the event (those in liquidity group 5) experienced an increase in deposits corresponding to 3.5 months of income one year after the event. Four years after the event, the deposits of these people are 1.9 months of income *lower* than before the event. This suggests that those with only

[10] Liquid position is defined as the value of deposits, stocks, and bonds divided by income, following Kreiner, Dreyer Lassen, Leth-Petersen (2019).

small positions in liquid assets prior to receiving a windfall gain keep most of what they inherit in their deposit account, while those with a large liquid position do not.[11]

Comparing the dynamic behavior of the groups in Figure 2, we see that the decrease after the initial increase, $(\gamma_1 - \gamma_4)$, is stronger the more liquidity individuals held before inheritance, as the downward trend after inheritance is larger for individuals with more liquid holdings.[12] Our interpretation of this result is that those with low liquidity are presumably further away from their target level prior to receiving the windfall gain, so they keep most of what they inherit on their deposit account in order to get closer to that level. On the other hand, those with a very liquid position prior to the windfall gain already are at a sufficient level of liquidity, meaning they readjust their deposits more after inheritance.

[Insert Figure 2]

One potential concern with this interpretation could be that people with little liquidity might be less financially literate than those with a more liquid position. If people are less financially literate, and therefore do not adjust their financial situation following shocks, for the pattern we observe. This would go against our interpretation that people generally have a target level of deposits. To test this, we run regression (3.2) taking into account different levels of education of the treated individuals. We thus use education as a proxy for financial literacy, in line with existing literature (see e.g., van Rooij, Lusardi, & Alessie, 2011). The results are in Table 4, columns 1-3. We find that the decrease in deposits, following the initial increase, is similar across education groups. This indicates that differences in financial literacy are not driving different reactions to inheritance depending on prior liquidity, lending support to the target level conclusion.

[Insert Table 4]

A concern with the inheritance data is that although a death may seem unexpected and sudden to the coroner, the death might be less surprising to close family members. To address this concern, we rerun our regressions using only those who inherited after their parents died of non-natural causes (e.g., traffic accidents), as this can hardly be anticipated. In Table 4, column 4, we show results from regression (3.2) but including only the beneficiaries of non-natural deaths. One year after inheritance, deposits are

[11] These individuals even decrease their deposit holdings compared to the pre-inheritance level, which may relate to the fact that they are split into quintiles based on their liquidity one year prior to inheritance. Due to this pre-determined split they might have been considerably over their target level in that one year, in which case they would not only go back to the level before inheritance, but rather decrease even further to their target level.

[12] Comparing $(\gamma_1 - \gamma_4)$, as well as, $(\gamma_1 - \gamma_4)/\gamma_1$, between liquidity groups 1 and 5, we find that the difference between them is significant at a 0.1% level.

increased by 3.9 months of income, controlling for everything else. This decreases to 1.3 months of income after four years. We saw the same pattern for the full sample of beneficiaries in Table 2. This supports our previous results.

A last concern could be that the size of the inheritance affects our results. While the control variables account for a lot of heterogeneity in inheritance (for example high-income people usually have high-income parents, which leads to larger inheritance), we can alter regression (3.2) to explicitly check the effect of an extra inherited DKK:

$$y_{it} = \sum_{k=0}^{4} \gamma_k Inheritance_{i,t-k} + \sum_{k=0}^{4} \omega_k Inhertiance_{i,t-k} Wealth_i + \boldsymbol{x_{it}\beta} + \theta_t + c_i + u_{it} \tag{3.3}$$

where $Wealth_i$ is the net wealth of the deceased divided by the number of children, adjusted for taxes and measured in DKK 100,000 (year 2018 level). We include four lags and we use the interaction with $Inheritance_{i,t-k}$ to estimate the coefficients ω_k. These coefficients show how much higher the effect of inheritance is on deposits in each year after inheritance, for each DKK 100,000 inherited. The results are shown in Table 4, column 5, and we see the same pattern as we have previously reported, i.e., an initial increase in deposits followed by a decrease. The effect is 0.26 months of income for each additional DKK 100,000 inherited one year after the inheritance (significant at a 5% level), while it drops to 0.18 months after 4 years (significant at a 5% level). Thus, our overall results hold when controlling for heterogeneity in inheritance size.

3.2 Property purchases and deposits

One potentially important aspect of deposit demand is that people tend to use their deposit accounts when saving for large purchases such as first-time house purchases. In the sample period we study, those who wish to buy a house need by Danish regulation to make a down payment of at least 5% of the value of the house, while the rest typically is financed by a combination of a mortgage and a bank loan. We examine if the deposit account is used to save up for such large purchases, as well as what happens afterwards, as this is a second way to test the hypothesis that individuals have a target level of deposits. If deposits increase before buying a house, drops when buying the house (when people withdraw funds from the deposit account to make the down payment), but deposits do not drop below their level before the individuals started to save for the down payment, we interpret this as indicating a target level for deposits.

We note that the decision to buy a house is endogenous with the level of deposits. In this sense, the results of the previous section that studies inheritances resulting from sudden deaths are cleaner when it comes to tightly identifying the effects we are after. The results of this section are thus supplementary, but potentially less strong, tests of our hypothesis.

We identify those who buy a house for the first time following the procedure of Ejarque & Leth-Petersen (2009), where further details of the property data and the sample selection process can be found.[13] We look at first-time buyers as other buyers normally sell their current house before buying a new one, and thus have a lower need to save up for the down payment. We observe 407,108 individual first-time buyers.

To investigate how deposit demand changes around the first housing purchase, we run the following regression:

$$y_{it} = \sum_{k=-3}^{4} \zeta_k HousePurchase_{i,t-k} + \boldsymbol{x_{it}\beta} + \theta_t + c_i + u_{it} \qquad (3.4)$$

where $HousePurchase_{i,t}$ is a dummy indicating that an individual purchased a house for the first time in a given year and taking on the value 0 in all other years. We include three leads and four lags of this variable, where ζ_k is the corresponding coefficient. The interpretation of ζ_k is the cumulative effect of the housing purchase on deposits, so the leads indicate how households act in anticipation of the housing purchase, and the lags indicate the effect of the purchase itself, compared to years before the third year prior to the purchase. If we find $\zeta_{-1} > 0$, this would show that deposits are on average higher one year before the housing purchase, compared to the average more than three years before the purchase. Similarly, $\zeta_4 < 0$ would show that deposits are four years after the housing transaction on average lower than they were in the time period occurring more than three years prior the purchase. The results of the regression are in Table 4, column 6. We find that there is a large accumulation of deposits prior to the housing transaction. The year before the transaction, deposits are on average 2.4 months of income higher than they were more than three years before the housing purchase, controlling for everything else. Deposits then drop after the housing transaction, stabilizing at around 1 months of income. This shows (i) that deposits are used to save up for large expenses, such as the down payment required to buy a house, but it also shows that (ii) individuals have a target level of deposits. This can be seen by noting that $\zeta_k \geq 0 \; \forall \; k > 0$, thus, even after purchasing their house, deposits do not on average drop below their level prior to the period people started to save up for the down payment.

[13] In addition to the sample selection criteria in Ejarque & Leth-Petersen (2009), we require that all individuals are in the sample consecutively (i.e., does not disappear from the data one year), and that there are data on them at least two years before and after the housing purchase. We also remove observations more than four years after the housing purchase.

Our overall conclusion from this section investigating deposit developments after large sudden windfall gains (inheritances) as well as large purchases (housing transactions) is that we reject the inaction motive as the main driver of deposit demand. Instead, we conclude that individuals have a target level for their deposit accounts. In the next section, we test if the target level is due to a precautionary motive.

4. Precautionary motive for holding deposits

According to the precautionary motive, deposits are held primarily as a buffer to large, unexpected expenses or income shocks. Unemployment is arguably such a shock. The question is therefore whether people hold deposits to insure against unemployment shocks. Those unemployed in Denmark are eligible for unemployment insurance benefits if they have been a member of an unemployment insurance fund for at least a year, have been employed at least 52 weeks in the last three years, are actively searching for a new job, and have been earning more than a pre-determined minimum amount. To be a member one pays a monthly fee. The unemployment insurance paid by the fund covers at most 90% of the wage of an individual, but there is a limit to the size of the unemployment benefit. On average, unemployment insurance ends up covering around 60% of wages (Svarer, 2011).

In Denmark, around 80% of the labor force are members of an unemployment insurance fund. The unemployment insurance benefits are generally significantly larger than government-provided unemployment benefits. For this reason, one would expect that individuals without an unemployment insurance have larger deposit holdings to insure against unemployment, if the precautionary motive is the main motive for deposit target levels. We conduct two tests to evaluate this hypothesis. First, we test if those with unemployment insurance generally have less in the deposit accounts, compared to those without insurance. Second, we test if those with unemployment insurance withdraw less from their deposit account when they become unemployed. The first test evaluates if deposits and insurance are substitutes, and thus whether deposits are held for precautionary motives, while the second part tests if the deposit account is in fact used as insurance when people become unemployed.

4.1 Are deposits and insurance substitutes?

To investigate the substitutability of unemployment insurance and deposit accounts, we construct a sample of individuals who have a salaried job. We are interested in seeing whether those without unemployment insurance, when they have a stable salaried job, use deposits as a precautionary savings vehicle to hedge against an income loss if suddenly losing their job. Hence, from our full sample we remove observations of individuals who are retired, studying, unemployed, or receiving government aid. Furthermore, we remove observations after an individual has been unemployed for the first time. The summary statistics of the sample can be seen in Panel A of Table A.3 in the Appendix, and reveal that those with an unemployment insurance hold slightly more in deposits than those with an insurance. However, those without an unemployment insurance have lower income, less wealth, etc. To control for differences in observables when evaluating whether uninsured hold more in deposits, we run the following regression:

$$y_{it} = \xi Unempl.Insurance_{it} + \boldsymbol{x_{it}\beta} + \theta_t + c_i + u_{it} \qquad (4.1)$$

where $Unempl.Insurance_{it}$ is a dummy taking the value 1 if an individual has unemployment insurance in year *t*, and zero otherwise, and ξ is the corresponding coefficient. Essentially, ξ is the effect of having unemployment insurance on an individual's deposit holdings, controlling for everything else. We run this regression on the abovementioned sample, and the results from the regression are in Table 5.

Table 5 reveals a negative relation for all outcome variables. The coefficient is statistically and economically significant, showing that, controlling for everything else, those with unemployment insurance have over a month of income less in their deposit account than those without insurance. This indicates that uninsured people prefer to hold a larger buffer against an adverse income shock such as unemployment.

[Insert Table 5]

4.2 Deposits as insurance

In a second analysis, we test if individuals with an unemployment insurance in fact reduce their deposit holdings by less than those without an insurance when unemployment hits. Hence, we now remove everyone who does not become unemployed in our sample period.[14] After an individual has regained employment, they are removed from the sample. We also remove individuals getting an education, retiring, or receiving government aid at any time during the sample period, since these people might be recorded as unemployed without have experienced an actual income shock (e.g., those finishing an education might be recorded as being unemployed before getting a job, or only having a part-time job before getting unemployed). We also remove those who are not part of the sample in consecutive years, and those who were not part of the sample two years or more before becoming unemployed. Panel B in Table A.3 in in the Appendix shows the summary statistics, where we again have split the sample in two,

[14] An individual is classified as unemployed if she is either receiving unemployment insurance benefits or governmental unemployment benefits and is deemed to be part of the labor force.

by those who are a member of an unemployment insurance fund, and those who are not. The summary statistics are calculated the year prior to unemployment.

We investigate what happens to the deposits of individuals if an adverse income shock occurs, i.e., if an individual becomes unemployed, and if this depends on whether the individual has an unemployment insurance. We run the following regression:

$$y_{it} = \xi Unempl.Ins._{it} + \lambda Unemployed_{it} + \delta Unempl.Ins._{it} * Unemployed_{it} + \boldsymbol{x_{it}\beta} + \theta_t + c_i + u_{it} \quad (4.2)$$

where $Unemployed_{it}$ is a dummy indicating whether a person is unemployed in a given year and λ the corresponding coefficient, showing the effect of being unemployed on deposits, while δ is the coefficient to the interaction between $Unempl.Ins._{it}$ and $Unemployed_{it}$. δ is thus our main coefficient of interest, showing the difference between the effect of unemployment on deposits for an individual who has an unemployment insurance compared to the effect for an uninsured individual. We run this regression on the sample of individuals who become unemployed for the first time. The results of this regression are in Table 6.

[Insert Table 6]

The results in columns 1-2 show that the effect of unemployment on deposits is relatively small. With control variables, depending on the specification, the effect of unemployment is not significant, or it leads to a 4.5% decrease in deposits. However, including the interaction between unemployment and unemployment insurance in columns 3-4, we see in the log regression (column 4) that unemployment leads to a 23% decrease in deposits. At the same time, in the log regression, δ is 0.175. This shows that those with an unemployment insurance do not reduce their deposits as much as those without. Or, in other words, individuals without unemployment insurance, but become unemployed, reduce their deposits by 17.5%-points more than those who have an unemployment insurance (and become unemployed). This shows that deposits are used more actively by those who face a more severe negative income shock, lending credence to the precautionary motive.

We perform a number of robustness checks on these results. In the analysis above, we define someone as being unemployed in a given year if they have been unemployed for any period within that year. Here we revisit the analysis of the effect of unemployment on deposits, but we define someone as becoming unemployed when they have been unemployed for 10%, 25%, or 75% of the year. These results are reported in Table 7. The results are largely similar those appearing from Tables 6, i.e., unemployment leads to a decrease in deposits, but relatively less for those unemployed who have an unemployment insurance. Furthermore, the effect of unemployment on deposits is increasing in the length of unemployment, as one would intuitively expect. If individuals have been unemployed 10% of the year, the drop in deposits for those without insurance amounts to 13,000 DKK, while it is almost DKK 21,000 for individuals who are unemployed for 75% of the year.

[Insert Table 7]

From Table A.3, Panel B, in the appendix, it is easy to see that members of an unemployment insurance fund are quite different in terms of observables such as age, income, education, etc., compared to those who are not members. One might then be worried that controlling for these variables linearly might not be sufficient in a regression setting. To alleviate those worries, we match individuals who are not members of an unemployment insurance fund with those who are, in the year prior to their unemployment, using the same approach and matching covariates as in the inheritance analysis. Running regression (4.2) as in our main analysis, but on the matched sample, we find very similar results, i.e., that those who are not members withdraw more from their deposits when they become unemployed (see Table 8).

[Insert Table 8]

Overall, the results of this section support the hypothesis that individuals target a certain level of deposits mainly for precautionary motives. We see this because people without an unemployment insurance generally hold more in their deposit account and they withdraw more from their deposits account when they become unemployed, compared to individuals with an unemployment insurance.

5. Effect of the interest rate

Theories of buffer stock saving predict that precautionary saving should be nearly independent of interest rates (Carroll, 1997). But to test whether interest rates affect deposit levels, we cannot simply run a regression of deposits on interest rates earned on deposits, as in our sample period banks rewarded individuals with higher levels of deposits with higher percentage rates of interest. This simultaneity leads to biased estimators. Instead, we compare individuals who receive high interest rates on their deposits in a given year to individuals with similar deposit levels but receiving a lower interest rate. Controlling for deposit size, we hypothesize that individuals who receive a higher interest rate on their deposits are relatively more sophisticated and active investors who search for the best deals. If interest rates drive deposit demand, these people would be more inclined to keep investing in their deposits accounts over time, compared to individuals in a control group (the relatively less sophisticated that earn a lower rate on their deposits).

We impute the interest rate an individual earns as in Iyer, Jensen, Johannesen & Sheridan (2019), removing observations where the deposit account changes by more than 20%, as the imputation becomes less precise with larger deposit changes. We then rank all depositors (for whom we have a valid interest rate imputation and who are observed the following year as well) in a given year according to the interest rate they have earned on their deposit accounts. We then identify the top and bottom quartiles. These quartiles are our high- and low-interest rate groups. We match each individual in the high-interest rate group with a corresponding individual in the low-interest rate group, i.e. match on the individual with the most similar amount in deposits (with replacement).[15] We collect the observations for the year where they were matched and the year after for each individual.[16] In addition, in the year after an individual was matched, we subtract the interest payments in that year from the value of their deposits that year to ensure that our results are not driven by the fact that the high-interest group naturally has higher deposits over time simply due to interest. We hypothesize that if interest rates drive deposit demand, individuals in the high-interest group should be more inclined to increase their deposit holdings. To test this, we run the following regression:

$$y_{it} = \eta \mathrm{HighInterest}_{\mathrm{it}} + \mathrm{X}_{\mathrm{it}}\beta + c_i + \theta_t + u_{it} \qquad (5.1)$$

where $HighInterest_{it}$ is a dummy equal to 1 the year after matching and the individual was in the high-interest group in the matching year. The coefficient η thus gives us the difference between the changes in deposits over the next year between the high- and the low-interest groups. In Table 9, columns 1-3, we see that the estimates of η are negative in all specifications. The results in column 3, e.g., indicate that individuals in the high-interest group decrease their deposit holdings by 0.15 months of income the following year, compared to individuals in the low-interest group. As mentioned, if deposit demand was driven by interest rates, we would expect that those receiving high interest rates would increase their savings in deposits. We do not, however, find evidence of such behavior. Our interpretation is that the interest rate is not a major driver of deposit demand.

One might nevertheless still expect that financially sophisticated individuals are more attuned to the interest rate they receive and might deposit more to capitalize on this. In columns 4-6 in Table 9, we show results from regressions where we interact the $HighInterest_{it}$ variable with two dummies

[15] Figure C.1 shows that the median interest rates earned in the two groups are substantially different every year. The high-interest group received a median interest rate ranging from 1 to 4% while the low-interest rate group received an interest rate around 0 in all years.

[16] An individual can be used again in a different year, providing they have a valid interest rate imputation and are observed the year after, but are treated as a separate individual.

respectively indicating different lengths of higher education. We find that the effect of interest rates on deposits is increasing in the level of education. However, after controlling explicitly for education, we still find that individuals in the high-interest group reduce their deposits the year after sorting, supporting that interest is not a primary driver of deposit demand.

Lastly, we conduct an additional, admittedly simple, test of the effect of the interest rate on deposit demand. We recognize that the general level of interest in the economy has fallen since the financial crisis of 2008-09. We thus rerun the inheritance analysis of Section 3.1, but for two different sample periods: before 2009 and after. We find that the behavior following an inheritance is not statistically different after 2009, even though the interest rate level was substantially lower (results not shown but available upon request). This adds to our evidence that interest rates are not a major driver of deposits, as one would expect that people would keep less of their inheritance on their deposit account after 2009 if the interest earn was of primary concern.

6. Further analysis

In addition to the robustness tests already mentioned, Appendix C includes several other auxiliary tests. First, we test if those over the age of 65 have a target level of deposits (Table C.1 in Appendix C), as they generally have the highest levels of liquidity, and might have a deposit demand that is different from the rest of the population. Finally, we also rerun the analysis of the effect of unemployment on deposits, but where we include those who have also previously been unemployed (Table C.2 in Appendix C), and second, we do not exclude those who do not have salaried jobs before and after their unemployment (Table C.3 in Appendix C). In short, all tests show that our results hold in different subsamples and with different sample selection criteria. Therefore, we leave the details of those tests to appendix C.

7. Conclusion

We provide the first comprehensive tests in the household finance literature on why individuals invest in deposits, for many individuals their largest and sometimes only financial asset. We provide a number of findings. First, we find that when an individual receives a large sudden windfall gain, in our case an inheritance resulting from a sudden death of a parent, the affected individuals do not just keep their windfall gain in their deposit account. Instead, they reallocate. We also find that the fraction of the windfall gain beneficiaries keep on their deposit account depends on their liquid position prior to inheriting. This indicates that people are not only active, but have a target for how much to hold in deposits. Second, we investigate whether precautionary demand determines this target. We find that individuals with an unemployment insurance hold less money in their deposit accounts and withdraw less during unemployment, compared to the uninsured. This indicates that deposit accounts are used as insurance substitutes. Finally, we evaluate whether people invest more in their deposit accounts if they benefit from higher interest rates. We find that they do not. All together, these results make us conclude that the precautionary motive is the main reason people hold deposits.

Lastly, we notice that there is significant heterogeneity in our sample and that some individuals make very large investments in their deposit accounts (the median investment in deposits correspond to 2.3 months of disposable income, while the average corresponds 7.5 months, meaning there are some households who make very large investments in deposits). There are several possible explanations why the precautionary motive may be this strong for some individuals. One reason can be severe liquidity constraints (Carroll & Kimball, 2001). Another possibility is provided by theories of decision making under risk that propose that prudence is stronger than one would expect under expected utility theory, such as with habit formation utility (Guariglia & Rossi, 2002), rank dependent utility (Bleichrodt & Eeckhoudt, 2005), prospect theory (Aizenman, 1998), or multiplier utility (Baillon, 2017). Investigating if some of these theories can explain heterogeneity in deposit demand across individuals, and in particular why some individuals have very large precautionary demand, provides interesting avenues for further research.

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Figure 1: Deposits Before/After Inheritance

This figure shows the mean value of deposits in the years before and after the year in which an individual in the treatment group inherits, and for the control group the years before and after their "counterfactual" inheritance year. The treatment group is everyone who inherits unexpectedly, and the control group is the matched individuals. On the x-axis is the number of years from the year of inheritance. Panel A displays the mean value of deposits measured in DKK, and panel B displays deposits measured in months of income.

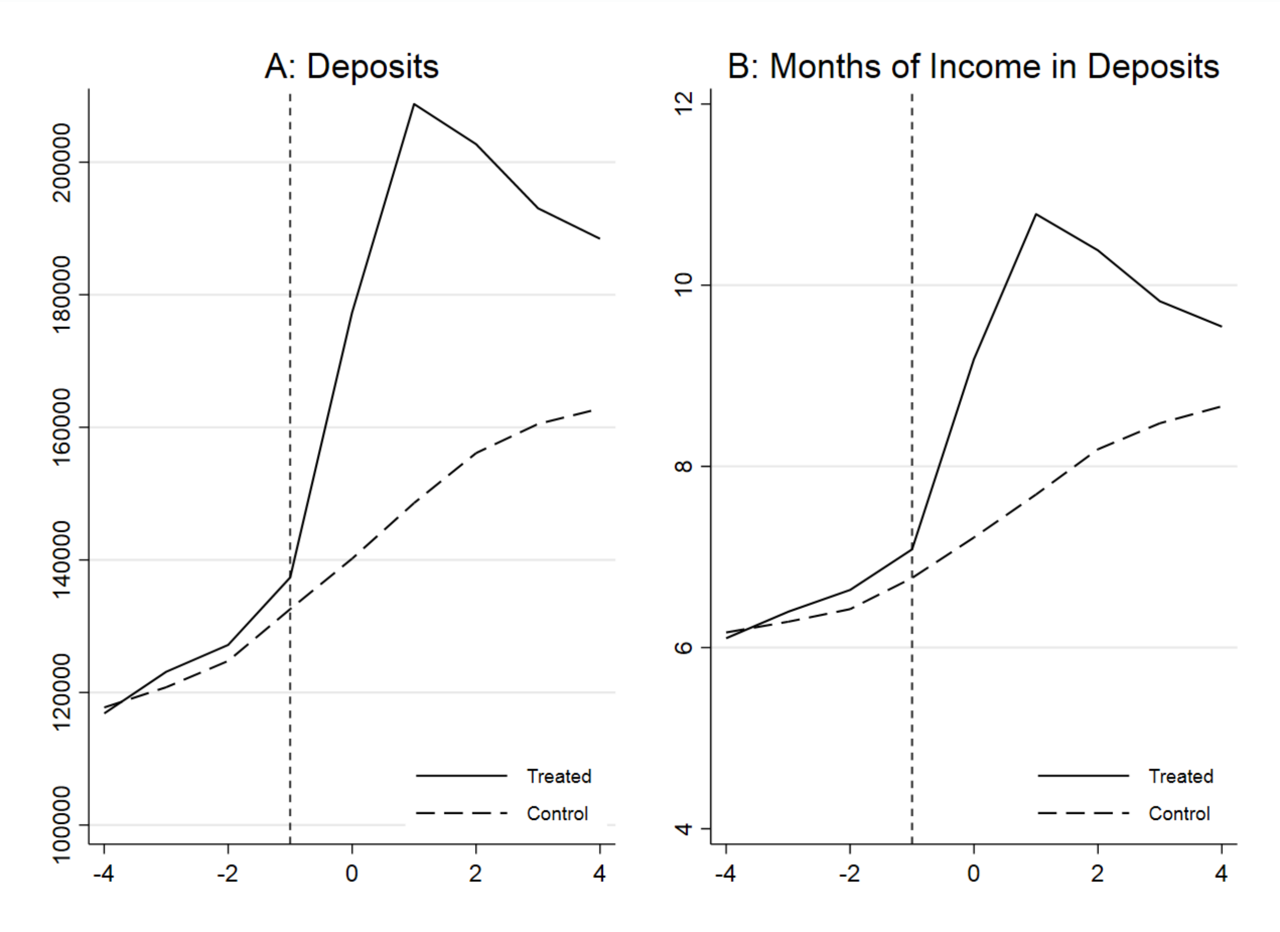

Figure 2: Dynamic Effects of Inheritance on Deposits Depending on Prior Liquidity

This figure displays the coefficient γ_l of Table 2 panel B column 6, as well as the results from Table 3, where *l* is years since inheritance. Essentially, this can be interpreted as the average increase in months of income in the deposit account, controlling for other factors, *l* years after inheritance for different groups. Panel A shows the coefficients of Table 2 panel B column 6, and panels B-F shows the coefficients of columns 1-5 in Table 2. 95% confidence intervals are displayed around the point values of the coefficients. The x-axis is years after inheritance.

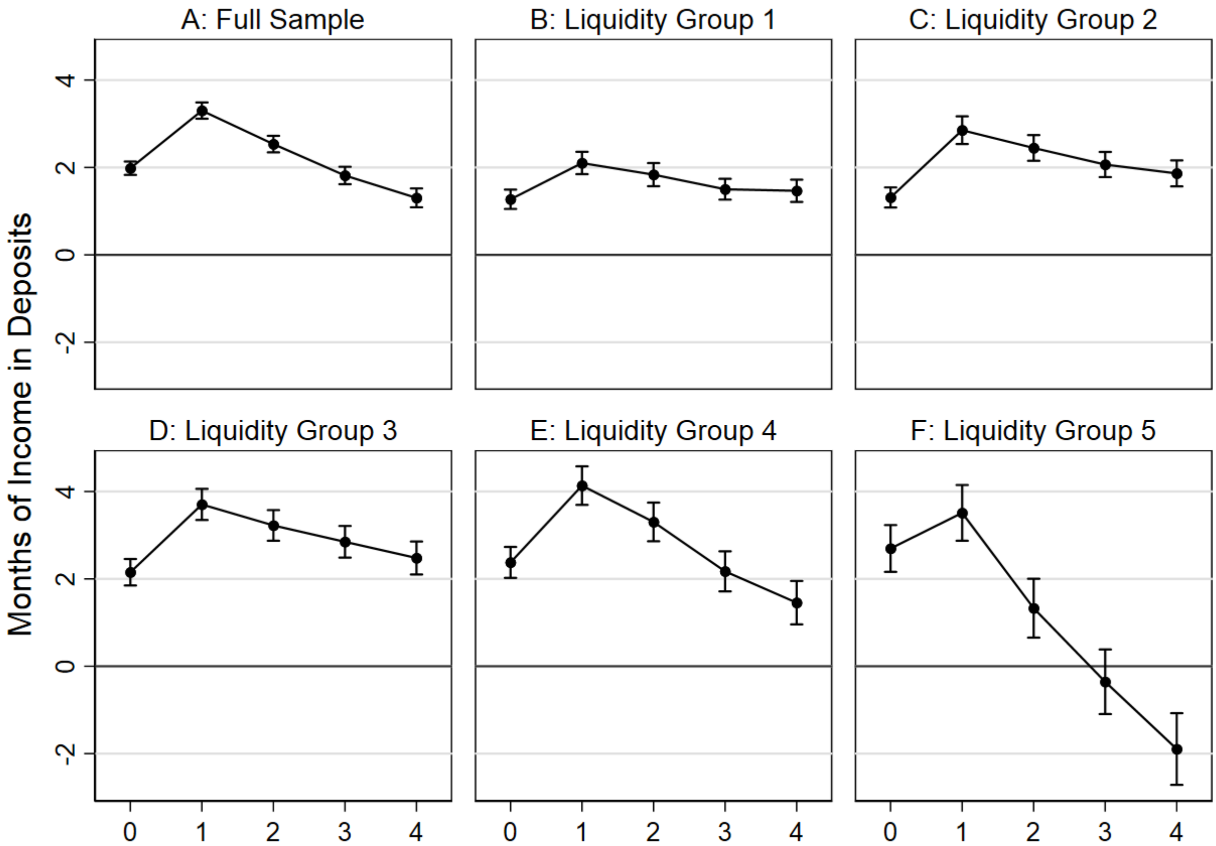

Table 1: Summary Statistics

The table shows the mean, median, and standard deviation for relevant variables, as well as an overview of the distribution of our sample. The numbers are based on the full sample period, i.e., from 2003-2018. Panel A gives the summary statistics for the full sample. Panel B gives the summary statistics for our inheritance sample one year prior to inheritance. All DKK variables are winsorized within each year at the 1st and 99th percentiles, and are inflation adjusted to December 2018 DKK. All medians reported are the means of the 5 observations closest to the median. See Section 2 for a more detailed description of variables.

	Panel A: Full sample			Panel B: Inheritance		
Continuous Variables	Mean	Median	Std	Mean	Median	Std
Deposits (DKK)	116,743.48	34,954.89	209,683.48	137,357.50	51,595.89	219,529.27
Log(Deposits)	10.44	10.49	1.89	10.76	10.85	1.75
Months of Income in Deposits	7.70	2.34	13.63	7.08	2.81	11.18
Deposits/Assets	0.55	0.59	0.44	0.43	0.18	0.43
Income (DKK)	192,158.99	181,407.41	89,468.02	230,442.57	220,953.30	80,717.60
Age	48.74	48.00	18.89	49.19	50.00	8.34
Assets (DKK)	723,456.00	303,313.72	975,895.13	913,147.60	703,316.06	976,199.69
Debt (DKK)	394,645.27	113,424.32	545,780.75	507,842.06	355,640.31	554,081.81
Number of children	0.52	0.00	0.92	0.73	0.00	0.97
Work experience (Years)	12.83	11.47	10.18	20.44	22.32	9.11
Time spent unemployed (Years)	1.28	0.19	2.27	1.97	0.74	2.79
Categorical Variables		Fraction			Fraction	
Female		53.07%			51.48%	
Only has deposits		42.93%			30.69%	
Unemployment insurance		53.30%			77.55%	
Stock market participation		26.11%			28.63%	
Homeowner		44.42%			59.35%	
Primary school		33.14%			24.57%	
Secondary school		41.27%			44.26%	
Higher education		25.58%			31.17%	
Married		46.28%			54.96%	
Live alone		30.00%			28.16%	
Salaried worker		54.43%			79.63%	
Unemployed		1.72%			1.98%	
Copenhagen		31.51%			29.03%	
Central Jutland		22.03%			22.72%	
Northern Jutland		10.32%			10.36%	
Zealand		14.78%			16.11%	
Southern Denmark		21.36%			21.78%	
Observations		55,039,371			228,491	
Individuals		5,285,371			18,934	

Table 2: Effect of inheritance on deposits

Panel A shows the regression results from the OLS regression (3.1) (estimating the average effect of inheritance on various deposit variables). Panel B shows the regression results from the OLS regression (3.2) (estimating the effect of inheritance on various deposit variables in each year following the inheritance). The sample consists of a treated group consisting of those who inherit, and a control group, a matched group of similar individuals, but who does not inherit. In column 1 the dependent variable is deposit account size in DKK, in column 2 the dependent variable is the natural logarithm of 1 + deposits, and in column 3 the dependent variable is months of income in the deposit account. Columns 4-6 shows the same regressions, just controlling for individual fixed effects and other control variables. The coefficients in Panel B should be interpreted cumulatively as "*k* years after inheritance" is the effect of inheritance *k* years after inheritance. The standard errors are robust to heteroskedasticity and serial correlation and are clustered on the individual level. t-statistics are reported in parenthesis. * statistically significant at 1% level.

	Deposits	Ln(Deposits)	Months of Income in Deposits	Deposits	Ln(Deposits)	Months of Income in Deposits
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Average Effect						
After Inheritance	71,987.6*	0.516*	3.541*	43,571.6*	0.234*	2.311*
	(39.13)	(43.91)	(39.78)	(26.44)	(23.62)	(28.30)
Control Variables	No	No	No	Yes	Yes	Yes
Year Fixed Effects	No	No	No	Yes	Yes	Yes
Individual Fixed Effects	No	No	No	Yes	Yes	Yes
Nr. of observations	457,353	457,353	457,353	457,353	457,353	457,353
Nr. of individuals	37,868	37,868	37,868	37,868	37,868	37,868
Within R^2	0.0166	0.0130	0.0149	0.0930	0.0929	0.0821
R^2	0.0166	0.0130	0.0149	0.603	0.580	0.575
Panel B: Dynamic Effects						
Year of Inheritance	54,991.2*	0.424*	2.762*	36,825.2*	0.239*	1.983*
	(30.64)	(34.14)	(29.40)	(25.27)	(23.74)	(25.25)
1 Year After Inheritance	86,412.6*	0.590*	4.360*	61,929.6*	0.347*	3.303*
	(42.40)	(46.86)	(40.78)	(34.80)	(31.01)	(34.68)
2 Years After Inheritance	80,353.5*	0.556*	3.960*	47,987.4*	0.241*	2.535*
	(40.02)	(44.12)	(37.83)	(26.29)	(20.68)	(26.17)
3 Years After Inheritance	70,679.7*	0.512*	3.398*	34,800.6*	0.155*	1.817*
	(33.57)	(38.21)	(31.33)	(18.04)	(12.32)	(17.91)
4 Years After Inheritance	66,082.5*	0.491*	3.118*	25,552.4*	0.0873*	1.304*
	(29.25)	(34.41)	(26.95)	(12.20)	(6.34)	(11.84)
4 Years - 1 Year	-20,330.1*	-0.099*	-1.24*	-36,377.2*	-0.260*	-2.000*
	(108.96)	(66.57)	(144.75)	(389.09)	(474.44)	(408.68)
Control Variables	No	No	No	Yes	Yes	Yes
Year Fixed Effects	No	No	No	Yes	Yes	Yes
Individual Fixed Effects	No	No	No	Yes	Yes	Yes
Nr. of observations	457,353	457,353	457,353	457,353	457,353	457,353
Nr. of individuals	37,868	37,868	37,868	37,868	37,868	37,868
Within R^2	0.0171	0.0132	0.0154	0.0944	0.0939	0.0836
R^2	0.0171	0.0132	0.0154	0.603	0.580	0.571

Table 3: Dynamic Effects of Inheritance on Deposits Depending on Prior Liquidity

This table shows the regression results from the OLS regression (3.2) (estimating the effect of inheritance months of income in the deposit account in each year following the inheritance) estimated for the five liquidity groups. The dependent variable is months of income in the deposit account, and column 1 show the results for the first quintile of liquidity (lowest liquidity), column 2 the second quintile, etc. The sample consists of a treated group consisting of those who inherit, and a control group, a matched group of similar individuals, but who does not inherit, and each group are split into liquidity quintiles separately. The reason there are not the same number of individuals in each group, is that the quintile split is performed each year, so small differences in group size in one year accumulates when added over 11 years. The coefficients should be interpreted cumulatively, as *k* Years After Inheritance is the effect of inheritance on a deposit variable *k* years after inheritance. The standard errors are robust to heteroskedasticity and serial correlation and are clustered on the individual level. t-statistics are reported in parenthesis. * statistically significant at 1% level.

	Months of Income in Deposits				
	Liquidity Group 1	Liquidity Group 2	Liquidity Group 3	Liquidity Group 4	Liquidity Group 5
	(1)	(2)	(3)	(4)	(5)
Year of Inheritance	1.272*	1.314*	2.152*	2.377*	2.697*
	(11.24)	(11.20)	(13.92)	(13.17)	(9.87)
1 Year After Inheritance	2.103*	2.854*	3.705*	4.135*	3.511*
	(16.13)	(17.67)	(20.37)	(18.39)	(10.78)
2 Years After Inheritance	1.837*	2.447*	3.224*	3.303*	1.329*
	(13.62)	(16.28)	(17.98)	(14.61)	(3.87)
3 Years After Inheritance	1.502*	2.068*	2.849*	2.173*	-0.355
	(12.37)	(14.09)	(15.41)	(9.29)	(-0.94)
4 Years After Inheritance	1.467*	1.865*	2.478*	1.455*	-1.894*
	(11.27)	(12.28)	(12.88)	(5.75)	(-4.53)
4 Years - 1 Year	-0.636*	-0.990*	-1.226*	-2.680*	-5.405*
	(20.43)	(33.54)	(44.59)	(125.14)	(260.010)
Control Variables	Yes	Yes	Yes	Yes	Yes
Year Fixed Effects	Yes	Yes	Yes	Yes	Yes
Individual Fixed Effects	Yes	Yes	Yes	Yes	Yes
Nr. of observations	92,872	95,882	96,109	92,590	79,679
Nr. of individuals	7,764	7,900	7,908	7,566	6,730
Within R^2	0.0241	0.0415	0.0691	0.135	0.196
R^2	0.204	0.237	0.275	0.359	0.559

Table 4: Non-natural deaths, size of inheritance, financial literacy, and house purchase

The dependent variable in the regressions is months of income in deposits. In columns 1-3 the analysis of the dynamic effects of inheritance on deposits is repeated for 3 different education groups. In column 4 the analysis of the dynamic effects of inheritance on deposits is repeated, but with only those who died of non-natural causes. In column 5 the analysis of the dynamic effects of inheritance on deposits is repeated, but where the regressor is the inherited wealth instead of a dummy indicating inheritance. Column 6 shows the effects of a housing purchase on deposits *k* years before/after the housing purchase (*k* years before/after shows the cumulative effect on deposits in the *k*-th year before/after the purchase compared to the average level of deposits before the 4-th year prior to the housing purchase). In column 6, homeownership is not among the control variables. The standard errors are robust to heteroskedasticity and serial correlation and are clustered on the individual level. t-statistics are reported in parenthesis. * statistically significant at 1% level.

	Education Group = 1 (1)	Education Group = 2 (2)	Education Group = 3 (3)	Non-natural (4)	Wealth (5)	House Purchase (6)
Year of Inheritance	1.251*	2.082*	2.309*	2.190*	1.530*	
	(8.96)	(17.12)	(15.83)	(8.77)	(8.18)	
1 Year After Inheritance	2.075*	3.401*	4.017*	3.913*	2.315*	
	(12.37)	(23.02)	(22.68)	(12.29)	(5.81)	
2 Years After Inheritance	1.353*	2.697*	3.126*	2.985*	1.658*	
	(7.88)	(17.90)	(17.33)	(9.16)	(4.52)	
3 Years After Inheritance	0.890*	1.852*	2.376*	1.898*	1.116*	
	(4.76)	(11.84)	(12.51)	(6.12)	(3.51)	
4 Years After Inheritance	0.400	1.447*	1.715*	1.317*	0.657	
	(1.98)	(8.43)	(8.32)	(4.05)	(2.04)	
Inherited Wealth x Year of Inheritance					0.121	
					(2.49)	
Inherited Wealth x 1 Year After Inheritance					0.263	
					(2.44)	
Inherited Wealth x 2 Years After Inheritance					0.235	
					(2.36)	
Inherited Wealth x 3 Years After Inheritance					0.190	
					(2.22)	
Inherited Wealth x 4 Years After Inheritance					0.175	
					(2.05)	
3 Years Before House Purchase						0.356*
						(20.88)
2 Years Before House Purchase						0.825*
						(35.90)
1 Year Before House Purchase						2.355*
						(78.58)
Year of House Purchase						1.467*
						(41.66)
1 Year After House Purchase						1.286*
						(32.29)
2 Years After House Purchase						1.103*
						(24.75)
3 Years After House Purchase						1.045*
						(21.00)
4 Years After House Purchase						0.996*
						(18.06)
Control Variables	Yes	Yes	Yes	Yes	Yes	Yes
Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Individual Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Nr. of observations	111,259	204,334	141,760	44,984	457,353	4,351,268
Nr. of individuals	10,212	18,085	12,127	3,694	37,868	407,108
Within R^2	0.0661	0.0885	0.0951	0.0774	0.0930	0.0540
R^2	0.617	0.573	0.541	0.534	0.575	0.548

Table 5: Relationship Between Unemployment Insurance and Deposits

This table shows the regression results from the OLS regression (4.1)(showing the effect of having an unemployment insurance on various deposit variables), and the effect of unemployment insurance on deposits. In column 1 the dependent variable is deposit account size in DKK, and in column 2 the dependent variable is the log of 1 + deposit account, and in column 3 the dependent variable is size months of income in the deposit account. The coefficient on unemployment insurance, is the ξ from the regression equation, and is thus showing how deposits substitute for unemployment insurance. Naturally, cumulative time spent unemployed and connection to the labor market are not among the control variables in these regressions. The standard errors are robust to heteroscedasticity and serial correlation and are clustered on the individual level. t-statistics are reported in parenthesis. * statistically significant at 1% level.

	Deposits	Ln(Deposits)	Months of Income in Deposits
	(1)	(2)	(3)
Unemployment Insurance	-29,642.5*	-0.0124*	-1.203*
	(-97.11)	(-4.83)	(-91.05)
Control Variables	Yes	Yes	Yes
Year Fixed Effects	Yes	Yes	Yes
Individual Fixed Effects	Yes	Yes	Yes
Nr. of observations	8,920,456	8,920,456	8,920,456
Nr. of individuals	1,530,121	1,530,121	1,530,121
Within R^2	0.0811	0.0681	0.0409
R^2	0.585	0.549	0.534

Table 6: Effect of Unemployment on Deposits

This table shows the effect of becoming unemployed on deposits. In column 1 the dependent variable is deposit account size in DKK, and in column 2 the dependent variable is the natural logarithm of 1 + the deposit account size. Columns 3 and 4 shows the estimates of the OLS regression (4.2) (showing the effect of becoming unemployed on deposits), thus including Unemployed x Unemployment Insurance, the interaction effect between the dummy indicating that an individual is unemployed and the dummy indicating the individual has unemployment insurance. In column 3 the dependent variable is deposit account size in DKK, and in column 4 the dependent variable is the natural logarithm of 1 + the deposit account size. In column 3 and 4, the total effect of becoming unemployed is found by taking the sum of the coefficients on Unemployed and the interaction with Unemployment Insurance. The sample consists of only those who at some point become unemployed, and are otherwise employed (i.e. not on sick leave, retired, or getting educated) in our full sample period. Naturally, cumulative time spent unemployed and connection to the labor market are not among the control variables in these regressions. The standard errors are robust to heteroscedasticity and serial correlation and are clustered on the individual level. t-statistics are reported in parenthesis. * statistically significant at 1% level.

	Deposits	Ln(Deposits)	Deposits	Ln(Deposits)
	(1)	(2)	(3)	(4)
Unemployed	313.9	-0.0462*	-12,253.4*	-0.225*
	(0.53)	(-6.70)	(-6.95)	(-5.68)
Unemployment Insurance	-	-	-8,118.2*	0.0833*
	-	-	(-11.92)	(9.29)
Unemployed x Unemployment Insurance	-	-	13,565.8*	0.175*
	-	-	(7.76)	(4.44)
Control Variables	Yes	Yes	Yes	Yes
Year Fixed Effects	Yes	Yes	Yes	Yes
Individual Fixed Effects	Yes	Yes	Yes	Yes
Nr. of observations	582,448	582,448	582,448	582,448
Nr. of individuals	85,375	85,375	85,375	85,375
Within R^2	0.0574	0.0428	0.0578	0.0432
R^2	0.573	0.507	0.573	0.507

Table 7: Unemployment Spells

This table shows the effects of unemployment on deposits, but with a different cut-off for when people are considered unemployed. In column 1, you are considered unemployed if you are unemployed more than 10% of the year, in column 2 more than 25%, and in column 3 more than 75%. Naturally, cumulative time spent unemployed and connection to the labor market are not among the control variables in these regressions. The standard errors are robust to heteroscedasticity and serial correlation and are clustered on the individual level. t-statistics are reported in parenthesis. * statistically significant at 1% level.

	Deposits		
	>10%	>25%	>75%
	(1)	(2)	(3)
Unemployed	-12,544.0*	-15,347.1*	-21,191.4*
	(-5.30)	(-4.67)	(-2.65)
Unemployment Insurance	-7,689.3*	-8,260.6*	-10,135.1
	(-9.97)	(-7.78)	(-2.39)
Unemployed x Unemployment Insurance	14,113.4*	16,823.7*	17,103.9*
	(6.11)	(5.45)	(3.03)
Control Variables	Yes	Yes	Yes
Year Fixed Effects	Yes	Yes	Yes
Individual Fixed Effects	Yes	Yes	Yes
Nr. of observations	452,195	278,219	38,620
Nr. of individuals	64,290	38,529	4,836
Within R^2	0.0580	0.0591	0.0747
R^2	0.584	0.598	0.630

Table 8: Effect of Unemployment on Deposits – Matched Sample

This table shows the effect of becoming unemployed on deposits on a matched sample. In column 1 the dependent variable is deposit account size in DKK, and in column 2 the dependent variable is the natural logarithm of 1 + the deposit account size. Columns 3 and 4 shows the estimates of the OLS regression (4.2) (showing the effect of becoming unemployed on deposits), thus including Unemployed x Unemployment Insurance, the interaction effect between the dummy indicating that an individual is unemployed and the dummy indicating the individual has unemployment insurance. In column 3 the dependent variable is deposit account size in DKK, and in column 4 the dependent variable is the natural logarithm of 1 + the deposit account size. In column 3 and 4, the total effect of becoming unemployed is found by taking the sum of the coefficients on Unemployed and the interaction with Unemployment Insurance. The sample consists of only those who at some point become unemployed, and are otherwise employed (i.e. not on sick leave, retired, or getting educated) in our full sample period. From this sample we match those without unemployment insurance the year prior to unemployment to similar individuals with insurance. Naturally, cumulative time spent unemployed and connection to the labor market are not among the control variables in these regressions. The standard errors are robust to heteroscedasticity and serial correlation and are clustered on the individual level. t-statistics are reported in parenthesis. * statistically significant at 1% level.

	Deposits	Ln(Deposits)	Deposits	Ln(Deposits)
	(1)	(2)	(3)	(4)
Unemployed	-1555.9	-0.0959*	-7900.0*	-0.233*
	(-1.93)	(-5.58)	(-5.56)	(-4.60)
Unemployment Insurance	-	-	-628.6	-0.0305
	-	-	(-0.73)	(-1.61)
Unemployed x Unemployment Insurance	-	-	7358.9*	0.167*
	-	-	(5.10)	(3.20)
Control Variables	Yes	Yes	Yes	Yes
Year Fixed Effects	Yes	Yes	Yes	Yes
Individual Fixed Effects	Yes	Yes	Yes	Yes
Nr. of observations	105,389	105,389	105,389	105,389
Nr. of individuals	20,838	20,838	20,838	20,838
Within R^2	0.0360	0.0372	0.0363	0.0374
R^2	0.544	0.446	0.544	0.446

Table 9: Effect of Interest Rate on Deposits

This table shows the effect of earning a high interest on deposits in the following year. In column 1 and 4 the dependent variable is deposit account size in DKK, and in column 2 and 5 the dependent variable is the log of 1 + deposit account, and in column 3 and 6 the dependent variable is size months of income in the deposit account. The sample consists of only those who have a valid interest rate imputation in a given year and who are observed the following year. From this sample we match those receiving a high interest rate with an individual receiving a low interest rate in the same year, solely based on their deposit level. The coefficient of interest is thus a dummy indicating whether an individual was in the high interest group the year after they were matched with someone else. This can be interpreted as the yearly change in deposits for the high interest individuals, compared to the low interest individuals. We also interact this with two dummies indicating if the highest education level is secondary school or higher education, showing differences in effects. The standard errors are robust to heteroscedasticity and serial correlation and are clustered on the individual level. t-statistics are reported in parenthesis. * statistically significant at 1% level.

	Deposits	Ln(Deposits)	Months of Income in Deposits	Deposits	Ln(Deposits)	Months of Income in Deposits
	(1)	(2)	(3)	(4)	(5)	(6)
High Interest Year After Match	-3,681.6*	-0.00827*	-0.150*	-5,704.0*	-0.0131*	-0.425*
	(-32.36)	(-13.98)	(-20.50)	(-34.07)	(-13.77)	(-35.13)
High Interest Year After Match x Secondary School	-	-	-	2,589.0*	0.00196	0.268*
	-	-	-	(13.98)	(1.78)	(20.07)
High Interest Year After Match x Higher Education	-	-	-	2,804.0*	0.0127*	0.501*
	-	-	-	(12.92)	(10.46)	(35.64)
Control Variables	Yes	Yes	Yes	Yes	Yes	Yes
Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Individual Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Nr. of observations	13,883,840	13,883,840	13,883,840	13,883,840	13,883,840	13,883,840
Nr. of individuals	6,941,920	6,941,920	6,941,920	6,941,920	6,941,920	6,941,920
Within R^2	0.0606	0.0325	0.0624	0.0606	0.0326	0.0626
R^2	0.909	0.847	0.906	0.909	0.847	0.906

Appendices

Appendix A – Summary Statistics

Table A.1: Causes of Death

The table presents the sudden causes of death of those that died without a spouse from 2003-2018. The ICD-10 codes are the International Classification of Diseases administered by the World Health Organization. I22-I23 is the code for acute myocardial infarction, I46 is cardiac arret, I50 is congestive heart failure, I60-I69 is stroke, and R95-R99 is sudden death by unknown cause. The last two columns show the total number of deaths over the years and the total number of beneficiaries, i.e., the number of adult children of the deceased.

	Natural Deaths			Non-Natural Deaths			Number of
ICD-10	I22-I23, I46, R95-R99	I50	I60-I69	V00-V89	V90-V99, X00-X59, X86-X90	Total	Beneficiaries
2003	134	358	1,175	58	169	1,894	3,175
2004	108	363	1,187	44	164	1,866	3,124
2005	217	279	1,213	54	210	1,973	3,372
2006	191	354	1,266	45	272	2,128	3,632
2007	99	364	1,216	53	229	1,961	3,475
2008	24	360	1,164	60	229	1,837	3,216
2009	20	349	1,299	50	187	1,905	3,428
2010	26	359	1,327	51	133	1,896	3,422
2011	26	317	1,281	38	193	1,855	3,356
2012	36	345	1,284	37	167	1,869	3,560
2013	42	354	1,335	37	198	1,966	3,700
2014	56	363	1,386	37	188	2,030	3,890
2015	73	396	1,547	46	184	2,246	4,257
2016	82	422	1,444	43	205	2,196	4,243
2017	58	412	1,439	34	188	2,131	4,200
2018	105	446	1,499	43	192	2,285	4,465
	1,297	5,841	21,062	730	3,108	32,038	58,515

Table A.2: Comparison of the treatment and control group

The table shows the mean, median, and standard deviation for relevant variables, as well as an overview of the distribution for the treatment (inheritance) and control group. The numbers are based on the full sample period, i.e., from 2003-2018. Panel A gives the summary statistics for the treatment group, i.e., the inheritances. Panel B gives the summary statistics for the control group based on matching. All DKK variables are winsorized within each year at the 1st and 99th percentiles, and are inflation adjusted to December 2018 DKK. All medians reported are the means of the 5 observations closest to the median. See Section 2 for a more detailed description of variables.

	Panel A: Treatment			Panel B: Control		
Continuous Variables	Mean	Median	Std	Mean	Median	Std
Deposits (DKK)	137,357.50	51,595.89	219,529.27	132,588.28	47,708.45	214,013.80
Log(Deposits)	10.76	10.85	1.75	10.72	10.77	1.74
Months of Income in Deposits	7.08	2.81	11.18	6.77	2.60	10.68
Deposits/Assets	0.43	0.18	0.43	0.42	0.18	0.43
Income (DKK)	230,442.57	220,953.30	80,717.60	231,683.22	222,657.48	80,755.40
Age	49.19	50.00	8.34	49.16	50.00	10.42
Assets (DKK)	913,147.60	703,316.06	976,199.69	915,429.98	716,943.81	970,879.50
Debt (DKK)	507,842.06	355,640.31	554,081.81	516,929.49	374,056.53	553,855.31
Number of children	0.73	0.00	0.97	0.75	0.00	1.02
Work experience (Years)	20.44	22.32	9.11	20.61	22.31	8.83
Time spent unemployed (Years)	1.97	0.74	2.79	1.89	0.63	2.77
Categorical Variables		Fraction			Fraction	
Female		51.48%			50.96%	
Only has deposits		30.69%			30.16%	
Unemployment insurance		77.55%			77.62%	
Stock market participation		28.63%			29.01%	
Homeowner		59.35%			59.84%	
Primary school		24.57%			24.14%	
Secondary school		44.26%			45.18%	
Higher education		31.17%			30.69%	
Married		54.96%			56.06%	
Live alone		28.16%			27.55%	
Salaried worker		79.63%			80.18%	
Unemployed		1.98%			1.89%	
Copenhagen		29.03%			29.07%	
Central Jutland		22.72%			22.56%	
Northern Jutland		10.36%			10.24%	
Zealand		16.11%			16.76%	
Southern Denmark		21.78%			21.37%	
Observations		228,491			228,862	
Individuals		18,934			18,934	

Table A.3: Unemployment

The table shows the mean, median, and standard deviation for relevant variables, as well as an overview of the distribution. The sample is split by whether individuals are member of an unemployment insurance fund or not. The summery statistics are based on the full sample period, i.e., from 2003-2018. Panel A shows the sample of salaried individuals who have never become unemployed and used in regression (4.1). Panel B shows the sample of salaried individuals who become unemployed for the first time and used in regression (4.2). In Panel B the summary statistics are calculated the year prior to unemployment. All DKK variables are winsorized within each year at the 1st and 99th percentiles, and are inflation adjusted to December 2018 DKK. All medians reported are the means of the 5 observations closest to the median.

	Panel A						Panel B					
	Member of unemployment insurance fund			Not member of unemployment insurance fund			Member of unemployment insurance fund			Not member of unemployment insurance fund		
Continuous Variables	Mean	Median	Std	Mean	Median	Std	Mean	Median	Std	Mean	Median	Std
Deposits (DKK)	124,994.55	46,953.15	204,770.89	126,071.88	35,631.29	233,033.59	83,242.09	26,885.74	161,877.47	33,661.08	12,395.40	73,001.05
Log(Deposits)	10.66	10.76	1.83	10.31	10.48	2.23	10.18	10.20	1.82	9.23	9.43	2.00
Months of Income in Deposits	5.68	2.36	8.87	6.06	2.20	10.02	4.25	1.60	7.49	2.68	1.03	5.19
Deposits/Assets	0.42	0.16	0.43	0.61	0.98	0.44	0.57	0.90	0.45	0.88	1.00	0.29
Income (DKK)	253,540.01	239,521.42	87,329.04	225,686.83	200,637.08	119,589.45	217,715.93	204,823.56	77,762.02	148,732.19	135,095.00	49,040.58
Age	40.64	40.00	13.29	36.30	30.00	16.93	32.24	29.00	11.58	22.59	20.00	6.65
Assets (DKK)	942,305.29	740,401.25	1,009,123.94	826,857.99	111,078.26	1,244,384.13	592,565.85	149,177.03	830,770.75	114,363.27	14,521.31	349,429.97
Debt (DKK)	610,201.78	481,823.72	624,142.19	454,134.18	54,041.25	690,729.50	461,347.14	161,942.44	572,200.50	123,804.25	17,297.48	299,570.28
Number of children	0.64	0.00	0.94	0.35	0.00	0.78	0.56	0.00	0.87	0.12	0.00	0.47
Work experience (Years)	16.37	15.42	10.53	10.57	6.00	10.92	10.88	7.01	9.37	4.10	3.35	3.75
Categorical Variables		Fraction			Fraction			Fraction			Fraction	
Female		45.58%			37.23%			36.17%			23.10%	
Only has deposits		29.75%			48.39%			46.51%			77.53%	
Stock market participation		31.60%			27.20%			21.47%			13.19%	
Homeowner		59.76%			37.62%			43.34%			8.02%	
Primary school		16.22%			24.40%			31.42%			75.77%	
Secondary school		49.95%			43.65%			50.23%			17.78%	
Higher Education		33.84%			31.95%			18.35%			6.46%	
Married		52.55%			36.08%			35.29%			6.96%	
Live Alone		19.27%			22.02%			21.05%			26.20%	
Copenhagen		37.04%			43.55%			30.91%			30.63%	
Central Jutland		20.54%			19.41%			21.92%			21.88%	
Northern Jutland		8.12%			7.06%			9.95%			10.15%	
Zealand		14.95%			12.89%			15.35%			15.34%	
Southern Denmark		19.36%			17.09%			21.87%			22.00%	
Observations		5,951,113			2,969,343			531,145			51,303	
Individuals		1,004,965			966,979			74,956			10,419	

Appendix B - Matching

We want to find a matched control for each individual inheriting. As we require that each beneficiary is present at least two years before and after the inheritance year, and are present in the sample consecutively, we require the same for the sample in which we search for matches. We use the year of inheritance for the beneficiaries we are looking for a match for, as the "inheritance year" for the matched control group when performing this sample selection. I.e., for someone inheriting in 2005, we look for matches amongst adult Danes which are present in the data in 2003 and 2007 and have no gaps in their presence in the sample. We match in the year before inheritance, and we use the control variables, as well as age, gender, immigration status as the covariates for the matching.

We measure the distance between individuals *i* and *j*, D_{ij} using the linear propensity score:

$$D_{ij} = \left|\Lambda^{-1}(e_i) - \Lambda^{-1}(e_j)\right|$$

Where e_i is the propensity score estimated using a logistic regression and the variables mentioned earlier, and Λ is the logistic function. This is found to be particularly effective in terms of reducing bias in the estimates (Rubin & Thomas, 1996; Rubin, 2001; Stuart; 2010). We use 1:1 nearest neighbor matching (Rubin, 1973) with replacement (reduces bias, Stuart, 2010).

A necessary identifying assumption for the matching estimator to be consistent, is that of overlap or common support, i.e., that $0 < P(Inherited|X) < 1$ (Wooldridge, 2010). We test whether we have common support by investigating if any of the propensity scores in the treatment group are larger or smaller than the maximum or minimum propensity of the control group respectively (Stuart, 2010) and find that there are no instances of this happening, thus indicating that there is common support.

Finally, we assess the covariate balance in the matched groups. By this it is meant that we test whether the distributions of covariates are similar in the treatment and control group, or $f(X|Inherited) = f(X|Not\ Inherited)$ where *f* is the density function. We assess this by calculating the absolute standardized mean difference (ASMD) in covariates between the treatment and control group as (Imbens & Rubin, 2015):

$$SMD = \left|\frac{\bar{X}_T - \bar{X}_C}{\sqrt{\frac{s_T^2 + s_C^2}{2}}}\right|$$

Where $\bar{X}$ is a sample mean of a covariate, and s is a sample standard deviation of a covariate. We calculate this for every covariate in every year (i.e., compare beneficiaries in the year before they inherited with their matched control group in that year), and find that the ASMD is quite small for all covariates. In addition, we find the variance ratio between the two groups to be very close to 1 for all variables, and therefore conclude that our matching has resulted in balanced covariates, at least if we look at the univariate distributions.

Finally, Imbens & Rubin (2015) point out that it is theoretically sufficient to compare the means of the propensity scores between the treated and control group if one wants to test for balance in the multivariate distribution of covariates. Therefore, we calculate the ASMD of the linearized propensity scores between the treatment and control groups and following Rubin (2001) we also calculate the ratio of the variances of the propensity scores between the two groups. Rubin (2001) suggests that the ASMD should be less than 0.25 and the variance ratio should be between 0.5 and 2. Our ASMD is close to 0 and the variance ratio is close to 1, so our covariates should be well balanced.

Appendix C – Further analysis

Figure C.1: Median Interest Rates

This figure shows the median interest rates for the high and low interest rate group detailed in section 6, over the time period 2004 to 2017. The high interest group are those with a high interest rate in the year we match them with a low interest individual, while the low interest group is the corresponding low interest rate matches.

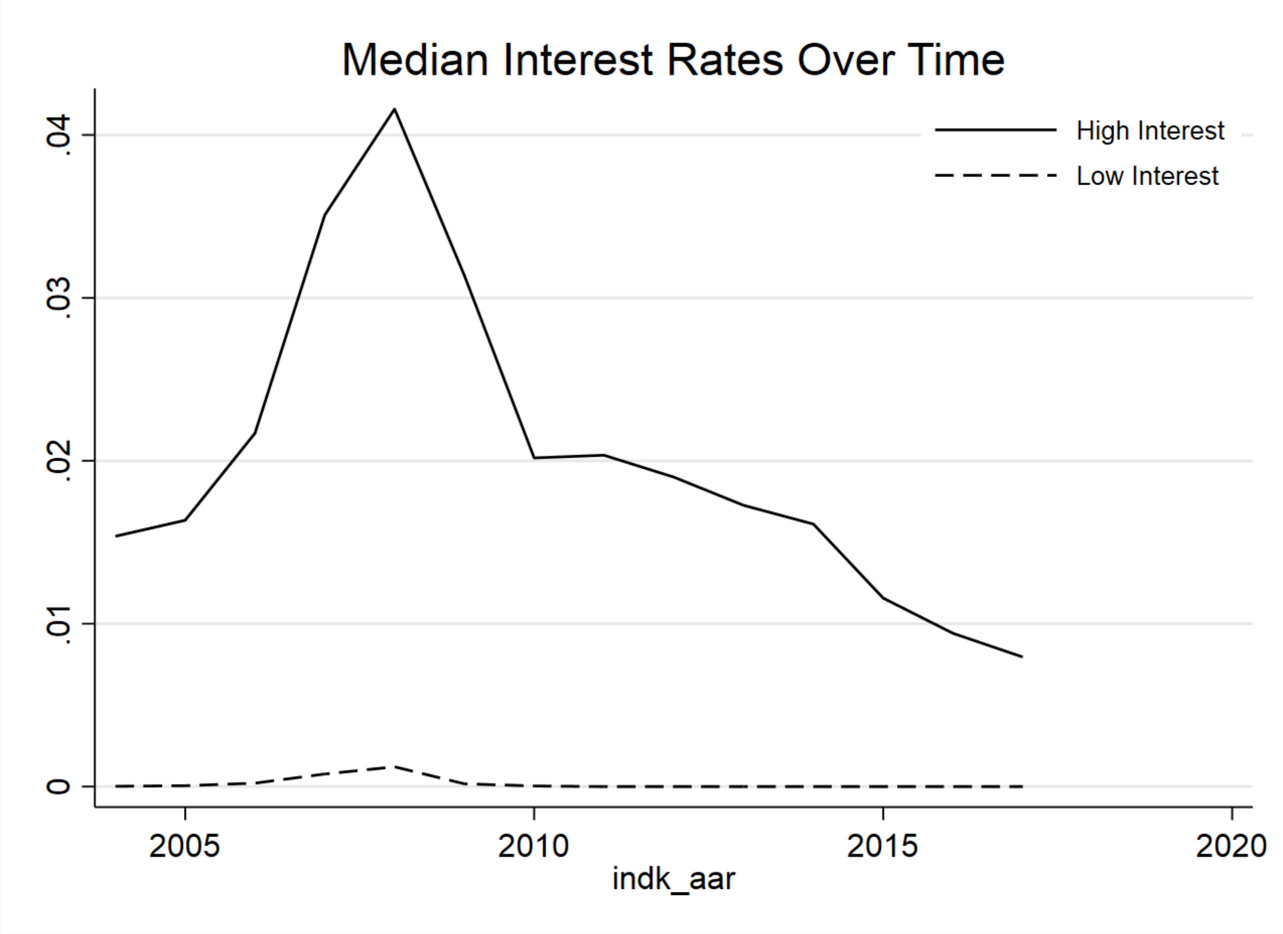

Table C.1: Effects of Inheritance for old people

This table shows the regression results from the OLS regression 3.2 (estimating the effect of inheritance on various deposit variables in each year following the inheritance). The sample consists of a treated group consisting of those who inherit, and a control group, a matched group of similar individuals, but who does not inherit, containing only those above the age of 65. In column 1 the dependent variable is deposit account size in DKK, in column 2 the dependent variable is the natural logarithm of 1 + deposits, and in column 3 the dependent variable is months of income in the deposit account. Columns 4-6 shows the same regressions, just controlling for individual fixed effects and other control variables. The coefficients should be interpreted cumulatively, as in *l* Years After Inheritance is the effect of inheritance *l* years after inheritance. The standard errors are robust to heteroskedasticity and serial correlation and are clustered on the individual level. t-statistics are reported in parenthesis. * statistically significant at 1% level

	Deposits	Ln(Deposits)	Months of Income in Deposits	Deposits	Ln(Deposits)	Months of Income in Deposits
	(1)	(2)	(3)	(4)	(5)	(6)
Year of Inheritance	44,365.8*	0.196*	5.635*	38,632.4*	0.192*	2.682*
	(3.48)	(2.84)	(6.64)	(4.32)	(3.70)	(4.21)
1 Year After Inheritance	53,39.5*	0.196*	6.180*	46,029.9*	0.205*	3.164*
	(4.06)	(2.80)	(6.94)	(4.48)	(3.64)	(4.34)
2 Years After Inheritance	23,653.8	0.0870	4.303*	15,187.3	0.0906	1.037
	(1.95)	(1.21)	(5.19)	(1.47)	(1.46)	(1.42)
3 Years After Inheritance	11,634.9	0.0542	2.985*	5,964.3	0.0437	0.0262
	(0.85)	(0.70)	(3.28)	(0.48)	(0.67)	(0.03)
4 Years After Inheritance	-3,333.8	0.0403	2.044	-16,184.6	-0.0242	-1.458
	(-0.22)	(0.50)	(2.02)	(-1.15)	(-0.34)	(-1.56)
Control Variables	No	No	No	Yes	Yes	Yes
Year Fixed Effects	No	No	No	Yes	Yes	Yes
Individual Fixed Effects	No	No	No	Yes	Yes	Yes
Nr. of observations	20,518	20,518	20,518	20,518	20,518	20,518
Nr. of individuals	1,574	1,574	1,574	1,574	1,574	1,574
Within R^2	0.00151	0.000505	0.00786	0.0993	0.0807	0.108
R^2	0.00151	0.000505	0.00786	0.679	0.639	0.621

Table C.2: Effect of Unemployment with Prior Unemployment

This table shows the effect of becoming unemployed on deposits. In column 1 the dependent variable is deposit account size in DKK, and in column 2 the dependent variable is the natural logarithm of 1+ the deposit account size. Columns 3 and 4 shows the estimates of the OLS regression 4.2 (showing the effect of becoming unemployed on deposits), thus including Unemployed x Unemployment Insurance, the interaction effect between the dummy indicating that an individual has become unemployed and the dummy indicating the individual has unemployment insurance. In column 3 the dependent variable is deposit account size in DKK, and in column 4 the dependent variable is the natural logarithm of 1 + the deposit account size. In column 3 and 4, the total effect of becoming unemployed is found by taking the sum of the coefficients on Unemployed and the interaction with Unemployment Insurance. The sample consists of only those who at some point become unemployed, and are otherwise employed (i.e. not on sick leave, retired, or getting educated) in our full sample period, but with no limitations to unemployment prior to the beginning of our sample period. The standard errors are robust to heteroskedasticity and serial correlation and are clustered on the individual level. t-statistics are reported in parenthesis. * statistically significant at 1% level

	Deposits	Ln(Deposits)	Deposits	Ln(Deposits)
	(1)	(2)	(3)	(4)
Unemployed	1,074.7*	-0.0338*	-10,325.2*	-0.100*
	(2.84)	(-8.40)	(-13.39)	(-6.45)
Unemployment Insurance	-	-	-6,386.1*	0.0670*
	-	-	(-10.82)	(8.75)
Unemployed x Unemployment Insurance	-	-	12,313.5*	0.0672*
	-	-	(15.97)	(4.29)
Control Variables	Yes	Yes	Yes	Yes
Year Fixed Effects	Yes	Yes	Yes	Yes
Individual Fixed Effects	Yes	Yes	Yes	Yes
Nr. of observations	1,649,568	1,649,568	1,649,568	1,649,568
Nr. of individuals	218,992	218,992	218,992	218,992
Within R^2	0.0571	0.0424	0.0573	0.0425
R^2	0.561	0.514	0.561	0.514

Table C.3: Effect of Unemployment with Non-Salaried

This table shows the effect of becoming unemployed on deposits. In column 1 the dependent variable is deposit account size in DKK, and in column 2 the dependent variable is the natural logarithm of 1 + the deposit account size. Columns 3 and 4 shows the estimates of the OLS regression 4.2 (showing the effect of becoming unemployed on deposits), thus including Unemployed x Unemployment Insurance, the interaction effect between the dummy indicating that an individual has become unemployed and the dummy indicating the individual has unemployment insurance. In column 3 the dependent variable is deposit account size in DKK, and in column 4 the dependent variable is the natural logarithm of 1 + the deposit account size. In column 3 and 4, the total effect of becoming unemployed is found by taking the sum of the coefficients on Unemployed and the interaction with Unemployment Insurance. The sample consists of only those who at some point become unemployed, but including those that are not otherwise employed (i.e. not on sick leave, retired, or getting educated) in our full sample period. The standard errors are robust to heteroskedasticity and serial correlation and are clustered on the individual level. t-statistics are reported in parenthesis. * statistically significant at 1% level

	Deposits	Ln(Deposits)	Deposits	Ln(Deposits)
	(1)	(2)	(3)	(4)
Unemployed	1,419.0*	0.00791	-5,481.4*	-0.0839*
	(6.40)	(2.38)	(-23.43)	(-15.37)
Unemployment Insurance	-	-	-8,497.9*	0.00601
	-	-	(-29.63)	(1.48)
Unemployed x Unemployment Insurance	-	-	11,792.4*	0.125*
	-	-	(36.11)	(21.03)
Control Variables	Yes	Yes	Yes	Yes
Year Fixed Effects	Yes	Yes	Yes	Yes
Individual Fixed Effects	Yes	Yes	Yes	Yes
Nr. of observations	2,622,856	2,622,856	2,622,856	2,622,856
Nr. of individuals	373,507	373,507	373,507	373,507
Within R^2	0.0493	0.0492	0.0504	0.0495
R^2	0.582	0.488	0.582	0.488