DOES LIQUIDITY SUBSTITUTE FOR UNEMPLOYMENT INSURANCE?

EVIDENCE FROM THE INTRODUCTION OF HOME EQUITY LOANS IN DENMARK

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JOB MARKET PAPER

November 29, 2014

FOR LATEST VERSION: http://scholar.harvard.edu/sandor/jmp

Would the value of unemployment insurance fall if more people had a buffer stock of liquid savings? Using quasi-experimental evidence from the unexpected introduction of home equity loans in Denmark, where public unemployment insurance is voluntary, we find that liquidity and insurance are substitutes. A Danish reform provided less levered homeowners with more liquidity. Using a ten-year-long panel dataset drawn from administrative registries, we find that people who obtained access to extra liquidity were less likely to sign up for unemployment insurance. The effect is concentrated among those for whom insurance has negative expected value. In this group, extra liquidity from housing equity worth one year's income decreases insurance up-take by as much as a 0.3 percentage point fall in the risk of unemployment. Placebo tests for earlier years show no differential trends by leverage before the natural experiment. This implies that the liquidity of financial assets influences unemployment insurance uptake in the absence of public provision of insurance.

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We are grateful to Raj Chetty, Mette Ejrnæs, Larry Katz, David Laibson, and Søren Leth-Petersen for careful guidance and encouragement throughout the project. We benefited greatly from comments of Rob Alessie, Joseph Altonji, Paul Bingley, Martin Browning, David Cutler, John Friedman, Ed Glaeser, Thais Lærkholm Jensen, Daniel Le Maire, Daniel Prinz, and numerous seminar participants at Harvard, SFI, DGPE, and CAM. We are grateful to Torben Heien Nielsen and Tore Olsen for graciously sharing their code and tax calculations with us. Funding was provided by the Danish Strategic Research Council (grants DSF-09-065167 and DSF-09-070295). All remaining errors and omissions are our own.

I. Introduction

To what extent is liquidity a substitute for insurance? The answer to this question has important implications for the design of optimal social insurance policies. If relaxing liquidity constraints enables people to better smooth marginal utility and address their specific needs, credit could be a partial substitute for government insurance, attenuating the typical distortions and fiscal externalities of traditional tax-and-benefit schemes.

Unemployment insurance is a classic example of social insurance, so much so that in most countries public unemployment insurance schemes are standardized and mandatory. While Denmark has a public unemployment insurance scheme, individual participation is voluntary. Because the supply of unemployment insurance is fixed, as this scheme is standardized and publicly regulated, we are able to study the demand for insurance by looking at changes in subscription rates. In this paper, we bring quasi-experimental evidence on the insurance choices of 113,000 homeowners in their late twenties and thirties, and test whether those who were allowed to borrow against equity in their homes bought differentially less unemployment insurance afterwards.

Prior to an unanticipated 1992 mortgage reform, borrowing against home equity from mortgage banks for consumption purposes was illegal in Denmark. By introducing home equity loans, the reform unexpectedly provided some homeowners with extra liquidity, without any differential change in their wealth. This motivates what is essentially a difference-in-differences research design. Combining this approach with a ten-year panel dataset drawn from administrative registers, we find that liquidity does substitute for unemployment insurance. More specifically, homeowners who had a substantial amount of home equity at the time the reform was enacted subscribed relatively less to unemployment insurance funds after the reform, compared to homeowners with little or no accessible home equity. We find that an increase in accessible liquidity worth one year of income caused about one Dane in two hundred to forgo unemployment insurance. The effects are concentrated among the group whose insurance is not actuarially fair; a year's income's worth of extra liquidity reduces their insurance up-take by 0.94 percentage points. This is equivalent to the effect of a 0.3 percentage point, or 15%, decrease in their estimated risk of unemployment. We show that groups with higher unemployment risk show little or no response. Our placebo tests validate our design, as home equity is not correlated with differential trends in insurance prior to the reform.

The Danish institutional features put the magnitude of the effect into context. First, we document substantial persistence in unemployment insurance membership and high baseline insurance up-take, a finding confirmed by Parsons et al. (2003). Only 13% of the individuals in our sample change insurance status over the ten years covered by our data. This type of persistence can stem from the psychological costs of changing insurance status, from the relative generosity of the scheme, or from social norms of solidarity. Second, home equity loans in Denmark carried large transaction costs compared to a HELOC (home equity line of credit) in countries such as the United States: The process required interviews at the issuing bank, and even after a line of credit had been established, the borrower could not freely draw upon home equity with a credit card, but instead had to apply for each additional loan. Hence, the 1992 reform likely had a smaller impact than it would have had under the more permissive loan policies in place in other countries.

We contribute to the literature on social insurance by estimating the extent of the substitution between formal insurance and a buffer stock of savings, a crucial quantity for the design of optimal unemployment insurance schemes.[1] From empirically verified models of lifetime consumption, we know that agents should and do accumulate liquidity as a means to smooth consumption. Precautionary savings respond to income risk (Carroll, 1997, 2009), liquidity constraints (Alessie et al., 1997; Deaton, 1991), and commitment constraints (Chetty and Szeidl, 2007). Engen and Gruber (2001) show that more generous public insurance schemes crowd out private savings; reducing the unemployment insurance replacement rate by 50% in the U.S. would increase gross financial asset holdings by 14%. This is true even though savings do not help transfer wealth from the more fortunate to those with more or longer unemployment spells. Yet by cushioning the blow in the bad state, buffer stocks can limit the need for state contingent claims, and are thus often called self-insurance.[2] If people are unable to

[1]Davidoff (2010) shows that home equity does limit the demand for long-term care insurance. In his framework, the house is sold before moving to a nursing home, which does yield more resources to pay for care, yet does not show that a liquid buffer and anticipated intertemporal smoothing would limit the need for insurance.

[2]If all unemployment risk were within person, i.e. only the spells' timing were unpredictable but not

smooth consumption and are forced to cut back spending in unemployment (Gruber, 1997) then insurance schemes with high replacement rates are optimal (Hansen and Imrohoroğlu, 1992; Crossley and Low, 2011; Lentz, 2009). With unconstrained borrowing however, much smaller benefits are optimal as residual insurance against the duration of the unemployment spell (Shimer and Werning, 2008).

Empirical work on distinguishing between the liquidity and the moral hazard effects of unemployment benefits also document that the former overwhelms the latter (Chetty, 2008). Our result suggests that the accumulation of liquidity is a real alternative for some households; even preferable to paying the premium on an unemployment insurance contract.[3] While mandatory savings accounts have been speculated to be a more efficient alternative to conventional insurance (Feldstein and Altman, 2007), and in place for instance in Chile or in Singapore, we have little quasi-experimental evidence on their effects (Chetty and Finkelstein, 2013).

Evidence from the standardized and subsidized Danish market, however, is not without limitations: In the absence of endogenous pricing, let alone contract design as discussed by Hendren (2013), the Danish experience does not prove that an unsubsidized unemployment insurance market is viable with tight liquidity constraints, nor how much such a market would unravel if the constraints were relaxed. This paper documents one channel for partial unraveling, which is likely to exacerbate adverse selection in an open market.[4]

We do not observe home equity after 1992 and thus cannot perform an analysis of ex post responses to shocks to verify that home equity serves as a liquid asset. However, there is evidence that people, given the opportunity, draw upon their home equity to

the overall exposure over a lifetime, free intertemporal smoothing would eliminate the need for any formal insurance. Even if there is a cross-sectional component of risk across individuals, if they can more freely reallocate their own resources over time, insuring an unlucky career against luckier ones becomes less important.

[3]This does not contradict the fact that these people did not accumulate liquid buffer stocks in the old regime, when only specific instruments were liquid.

[4]Unemployment insurance markets could unravel if only inherently risky groups insure themselves (adverse selection), or once insured, people are less careful to keep their jobs and get back to work quickly (moral hazard). This paper suggests that people can also select out of insurance if they foresee their ability to take precautions, which would exacerbate adverse selection. Einav et al. (2013) document similar selection on an anticipated behavioral response, that more price sensitive patients select into plans with lower copays, which they call selection on moral hazard in health insurance. Our data limits us from gathering direct evidence on whether people with overpriced insurance invested more in home equity after the reform to grow a buffer.

finance consumption. Hurst and Stafford (2004) find that households borrow against their home equity in periods of unemployment. Moreover, using the same reform, a liquidity shock has been found to increase consumption over time (Leth-Petersen, 2010), and even encourage entrepreneurship (Jensen et al., 2014). Finally, Chetty and Szeidl (2010) show that higher property values, holding wealth equal, lead to increased tolerance for risk; an increase in home equity increases the probability of investing in the stock market. These empirical findings all support our interpretation of home equity as an imperfectly liquid asset, once home equity loans are allowed.

The paper proceeds as follows. Section II introduces the 1992 credit market reform and the Danish unemployment insurance system in more detail and outlines our empirical strategy. Section III describes our data, motivates our sample selection, and provides summary statistics. Section IV presents our results and discusses their robustness. Section V concludes.

II. Institutional Details and Empirical Strategy

This paper identifies the effect of liquidity on the demand for public unemployment insurance by exploiting a large, sudden, and unexpected policy variation and the features of the Danish unemployment insurance system. The mortgage reform, which was approved by the Danish parliament in May 1992, unexpectedly endowed some homeowners with extra liquidity. The voluntary nature of Danish unemployment insurance enables us to study its demand. This section of the paper describes how we exploit these two features of the Danish system to identify the effect of liquidity on the demand for unemployment insurance.

A. The Danish Unemployment Insurance System

The Danish unemployment insurance system builds upon several unemployment insurance funds (42 in 1992), which are private associations of workers with the purpose of providing economic support to their members during unemployment. However, as funds are complemented by the state—the system is self-supporting only if the unemployment rate is around 3%—strict regulation at the national level demands that each fund offers a uniform insurance product, independent of the occupation and industry of its members. While unemployment benefits are thus identical nationwide, Danish workers are free to choose whether to subscribe to an unemployment insurance fund or bear the risk of unemployment themselves.

These funds are the main, but not the only, source of income contingent on losing a job. While a publicly funded welfare program exists, supplemental security income eligibility requirements are very strict. In principle, applicants cannot own any assets, or be able to sustain themselves in any other way (for example through another earner in the household), in order to be considered for welfare benefits, which are also lower than those received from unemployment insurance funds.[5] Because no major changes in the supplemental security income system occur during the period of interest, we ignore welfare benefits in our analysis. Benefit amounts are detailed in Table D1 of Appendix D.

Wage earners and the self-employed have access to different unemployment insurance schemes in terms of eligibility rules and requirements once unemployed. For wage earners (about 90% of the Danish workforce), to whom we restrict our analysis, eligibility for receiving unemployment insurance benefits requires uninterrupted membership in an unemployment insurance fund in the 12 months preceding unemployment and at least 26 weeks of paid work over the last three years. The funds do not screen applicants for membership. Special rules apply to recent graduates, who are immediately eligible for unemployment insurance benefits if they sign up within one month from graduation.[6]

To retain benefits, the unemployed must comply with a set of rules, specified in ministerial guidelines on active labor market policies. These guidelines require recipients to make their resumes publicly available, apply for at least a given number of jobs per month, and participate in courses and other activities assigned by their caseworker on the basis of individual abilities and potential. Under these criteria, the daily benefits can amount to up to 90% of their daily gross income averaged over the preceding 12 weeks. However, the benefits are capped at a relatively low level. In 1992, the cap on benefits

[5] Probably the largest variation in the value of the unemployment insurance contract comes from changes in marital status, which changes supplemental security income eligibility. We do not model this explicitly, but, as a robustness check, we investigate separately households with constant marital status around the reform in the appendix.

[6] Students receive reduced benefits the first year, which corresponds to approximately 80% of the standard benefit level.

corresponded to a gross monthly salary of approximately $2,000, and thus affected 95% of full-time insured workers.[7]

The unemployment insurance contract is a bargain for most Danes, unless they have very low (subjective) unemployment risk or high hassle costs of the contract. The yearly statutory membership fee was the equivalent of eight times the maximum daily benefits over this period, e.g. eight times 417 DKK in 1992 for full-time workers. Hence, in absence of additional administrative fees and taxes, the insurance would be actuarially fair for workers facing duration-weighted unemployment risk of about 2.6% per year.[8] Benefits count towards taxable income and every person i in year t can calculate the expected value of future benefits using their future retention rate, $(1 - MTR_{i,t+1})$.[9] Membership fees are tax deductible, but only from a special notion of taxable income, where the top two tax brackets do not apply. Thus the relevant retention rate for the fees is $\left(1 - MTR^{bottom}_{i,t+1}\right)$. The expected net benefit of membership after taxes is a multiple of daily benefits (DB):

$$NB_{i,t} = \left[(1 - MTR_{i,t+1}) \cdot YED_{t+1} \cdot UR_{i,t+1} - \left(1 - MTR^{bottom}_{i,t+1}\right) \cdot 8\right] \cdot DB_{t+1}, \tag{1}$$

where YED is the full-time, full-year (FTFY) equivalent number of days (312 in 1992) and UR is unemployment risk as a fraction of the year spent unemployed. We plot our estimates about expected net benefits for the following year by levels of (estimated) unemployment risk in our estimation sample, using 1987 as an example, in Figure 1. Note that the subsidized insurance scheme is a lottery with positive net expected value for many, though our calculation probably overestimates net benefits as we can calculate risk (FTFY benefit take-up) only on the insured, who are subject to adverse selection, moral hazard, and selection on moral hazard.

[Figure 1 about here.]

[7] Apart from the documentation of rules, we report all monetary amounts in US dollars, using the 1991 exchange rate of 5.91, while also correcting for domestic inflation, using 2005 prices for more familiar magnitudes.

[8] E.g. a 2% Bernoulli risk of spending half a year on benefits corresponds to 1% risk for the calculation of expected benefits.

[9] This calculation ignores the fact that spells long enough for people close to thresholds of tax brackets could knock them down into a lower tax bracket.

Table D2 of Appendix D collects the relevant parameters of unemployment insurance over our time period. The fairly high benefit level combined with low after-tax insurance fees makes unemployment insurance attractive for many. Meanwhile social norms and inertia together with the historically tight bond between unemployment insurance funds and labor unions imply high insurance up-take in Denmark. The characteristics of the unemployment insurance market in Denmark allow us to study the unemployment fund membership of Danes as a proxy for their effective risk tolerance. As the supply of insurance is fixed and publicly regulated, the market outcome is determined only by the demand for insurance.

Unemployment insurance fund membership is, however, not entirely driven by demand for insurance against job loss, but also by eligibility for an early retirement scheme (*efterløn*), which allows members to retire at age 60 rather than at age 67 (the official retirement age during the period of interest). Many Danes take advantage of this possibility to retire early. Approximately 50% of the population received *efterløn* at the age of 64 between 2007 and 2011, which is halfway between the earliest eligible age of 60 and 67, when public pensions become available.

Until 1992, eligibility for early retirement benefits required membership of an unemployment insurance fund for the last ten years before retirement; then this requirement increased to twenty years. People between the ages of 40 and 50, who were not already unemployment insurance fund members, were given the option to join no later than March 1992 to acquire eligibility at age 60. Many people in their forties committed to the scheme in 1992, and hardly made a choice about insurance ever after.[10] Those who did not join that year constitute a self-selected group, some of whom still joined later to enjoy early retirement at an age older than 60. We restrict our analysis to younger cohorts, unaffected by this change. A detailed account of how older cohorts are affected by early retirement reform is deferred to Appendix B.

[10]The reform might have changed ex post behavior for those who found themselves insured for this unrelated reason. Ejrnæs and Hochguertel (2011) attribute different self-employment patterns at different ages to this shift of ten cohorts into the funds in 1992, also insuring some business risk of sole proprietors.

B. The Danish Mortgage System and the 1992 Reform

Most real estate purchases in Denmark are financed via mortgage credit institutions, which offer loans with the property as collateral.[11] The legal cap on loan to value (LTV) is 80%, the homeowner must provide a 20% downpayment. Mortgage credit institutions issue callable bonds to fund pools of loans, and the securitized loans are thus low-risk and highly liquid. Real estate loans are cheaper than personal loans established with commercial banks after a credit review, especially after losing a job and without collateral. Denmark has no national credit bureau, and few workers could expect to have a line of credit open, mainly in the form of a credit card, after being laid off.

In 1992, Folketinget, the Danish parliament, voted in favor of a mortgage reform, shortly after a brief discussion in the spring. Before May 21, 1992, Danes could get a securitized mortgage only for real estate investments (purchase or remodeling). Thus home equity used to be a highly illiquid asset, which could be turned into cash only through a sale or perhaps a costly and uncertain loan. The reform changed mortgage regulation in three ways: maximum maturity, remortgaging, and the use of the loan. The last is the crucial element for the purpose of this paper; allowing mortgage loans to finance purposes other than real estate investments effectively let Danes to use up to 80% of their real estate wealth as collateral for consumption loans established through mortgage credit institutions.[12]

The reform was unanticipated; Leth-Petersen (2010) has documented that not even the major finance and economics newspaper in Denmark covered the reform until the month it was enacted. This unanticipated access to credit of particular homeowners allows us to isolate the causal effect of an increase in liquidity on the demand for formal insurance, holding wealth fixed: Households did not hold more or less home equity at the time of the reform because they anticipated its use as a liquid buffer stock.[13]

[11] On general features of the Danish mortgage market, including their implementation of the European covered bond system, see Campbell (2013).

[12] The limit was initially set at 60% but was quickly raised to 80% by December 1992.

[13] For the thought experiment behind our causal reasoning, wealth should not change along with liquidity. In our quasi-experiment, most of the sample became wealthier in the years following the reform, but not differentially for those with more home equity, thus a higher dose of treatment. As our summary statistics in Table 1 show, home equity is not correlated with total housing wealth in our sample, so house price rises after 1991, be they secular or an effect of the mortgage reform, made everyone equally wealthier.

After 1992, turning home equity cheaply into cash on hand still required a new mortgage contract with non-trivial transaction costs, yet homeowners were no longer forced to sell a house and move just to tap into this asset. However, the liquidity of homeowners with a large established mortgage did not change, because they could not mortgage up any more than they already had. They can thus serve as a control group, making our identification strategy straightforward. We compare homeowners endowed with home equity just before the reform to homeowners who mortgaged to the limit, and estimate how their insurance choices evolved differently over time. Our specification is therefore similar to a difference-in-differences design.

The 1992 reform has two key elements that make the Danish case uniquely valuable to identifying liquidity effects. First, the reform was unanticipated. Because people could not know that the reform was to be implemented, they could not have adjusted their housing, insurance and liquidity accordingly, and no voluntary selection into treatment could have taken place. As we have access to data on homeowners from 1987, five years before the reform took place, we can show that the trends in insurance up-take were identical for the treatment and the control groups (high and low equity owners, respectively) up to when the reform was implemented.

Second, the reform only changed the costs of tapping into home equity, but did not affect individual wealth differently for those with more or less home equity. Therefore, we are able to identify the liquidity effect on insurance demand, independently of wealth. This unique feature of our identification strategy distinguishes this paper from those studying behavioral effects of changes in wealth (Shapiro and Slemrod, 2003; Chetty and Szeidl, 2010; Andersen and Nielsen, 2011), and is more directly comparable to studies of direct liquidity shocks (Gross and Souleles, 2002).

However, the reform also changed mortgage regulations in two other ways, namely by introducing the right to cash-out refinancing and by expanding the maximum maturity of real estate loans from 20 to 30 years. Remortgaging gives the debtor the possibility to lower the cost of his debt when market interest rates fall. A borrower is entitled to redeem a mortgage bond at par at any time prior to maturity by prepayment, and thus to exploit interest rate changes to reduce the costs of funding. Because interest rates were falling on average during our sample period—shown in the right pane of Figure 2—this opportunity was particularly valuable for holders of large mortgages. Though the option value to remortgage constitutes a wealth transfer to our control group, it is annuitized, as remortgaging changes monthly installments, and thus is hard to turn into cash on hand. With no equity in their homes, even the more flexible refinancing options after 1992 do not allow our control group to get cash out from refinancing.

[Figure 2 about here.]

Figure 2 shows that between 1987 and 1993 Denmark suffered a period of economic stagnation, with rising unemployment rates. Moreover, real estate prices changed considerably in our period of interests, both for apartments and houses, and generally increased after 1993. Because of this economic turmoil, we exploit not only the long panel structure of our dataset to control for year-specific fixed effects, but also the richness of Danish administrative registers to control for several demographic and financial characteristics.

C. Econometric Methods

Our general specification takes the form of the linear probability model

$$I_{i,t} = \alpha_{\mathrm{HE}} \cdot \mathbf{HE}_{i,1991} + \tau \cdot \mathbf{HE}_{i,1991} \cdot \mathbf{1}\left[t \geq 1992\right] + \mathbf{X}_{i,t}^{d}\boldsymbol{\beta}_{d} + \mathbf{X}_{i,t}^{f}\boldsymbol{\beta}_{f} + \nu_t + u_{i,t} \tag{2}$$

where $I_{i,t}$ indicates insurance status in year t for individual i. The linear probability model in equation (2) is similar to a difference-in-differences design, where the coefficient τ identifies the change in average enrollment in an insurance fund for any given mortgageable home equity HE, as measured in December 1991, relative to secular time fixed effects ν_t and how (1991) home equity correlates with insurance demand in the cross section, α_{HE}. This model includes financial and demographic controls, $\mathbf{X}_{i,t}^{f}$ and $\mathbf{X}_{i,t}^{d}$, and allows for arbitrary correlation within individuals in the residuals $u_{i,t}$. We normalize home equity and financial controls by permanent income.

We also estimate standard difference-in-differences models with no scaling by treatment dose, with a treatment and a control group based on the amount of home equity held in December 1991 relative to our permanent income measure. We assign to the control group those holding no mortgageable equity, and to the treatment group those who hold more than a month's income in home equity. We argue that those who owned a home at the beginning of 1992 but had too little equity to take advantage of the new rules and those who held more home equity before 1992 were experiencing common underlying trends when it came to unemployment insurance. Under this assumption, the difference in changing insurance behavior from 1992 onwards is caused only by the availability of home equity loans from mortgage credit institutions.

While this definition of the control group reflects important individual choices before 1992, the reform was unexpected, thus those choices could not be motivated by the need of extra liquidity. Our identifying assumption is that the underlying differences between the treatment and the control group do not drive different trends in insurance purchase. We show that the trends in unemployment insurance fund memberships were identical in the two groups before the reform.

As we do not observe home equity after 1992, our definition of treatment and control groups allows for some in the control group gaining some treatment over time, as house prices rise and mortgages are paid down. As the initial difference in liquidity does not diminish, our preferred interpretation is that more liquidity causes less demand for insurance, with little to say about the nonlinear effects of having any or little liquidity. The parallel trends of uniformly improving liquidity is important for the interpretation of either specification. If the home equity gains after 1992 are more valuable for our control group than our treatment group, this attenuates our estimates of the long-run effect of liquidity.

Using initial home equity as an instrument for equity in later years would essentially correct our estimates for measurement error and imperfect compliance in slack liquidity constraints later (maybe partly in response to the reform). We interpret the reduced-form estimates as a lower bound for the substitution between private precautionary savings and formal unemployment insurance.

Unemployment insurance choices are characterized by strong inertia. Only about 13% of the individuals in our sample change insurance status at least once during the ten years of our analysis. This persistence in insurance choice comes not only from the eligibility criteria for unemployment insurance benefits, which encourage continued enrollment, but also from the historically strong connection between unemployment insurance funds and labor union memberships, social norms of solidarity, and psychological costs of changing insurance status.

We model this inertia in two alternative ways. First, we include individual fixed effects in some specifications, thereby identifying the parameters using only variation among those who change their insurance status. Second, we estimate a lagged dependent variable model, which has a particularly meaningful interpretation in a random utility framework. In this setup, which in its simplest form corresponds to a standard logit model, an agent subscribes to an unemployment insurance fund if the utility of being insured is larger than the utility of being uninsured. Essentially, we assume a random utility model

$$u_{i,I,t} = \nu_{i,I,t} + \varepsilon_{i,I,t}, \quad I \in \{0,1\}, \tag{3}$$

where we model the predictable utility of insurance status I for individual i at time t, $\nu_{i,I,t}$, as a linear combination of observables, while $\varepsilon_{i,I,t}$ is unobserved and follows a logistic distribution. The individual chooses to be a member of an insurance fund if $u_{i,1,t} > u_{i,0,t}$. Thus,

$$\mathbf{Pr}_i\left(I_t\right) = \frac{1}{1+\exp\left(\nu_{i,1-I,t} - \nu_{i,I,t}\right)}. \tag{4}$$

To model inertia, we assume that the agent pays a one-time utility cost c_1 for subscribing to an unemployment insurance fund and a parallel cost c_2 to unsubscribe from the fund. These costs can reflect administrative fees, opportunity costs, or simply the psychological effort of gathering information on how to change one's fund membership and submit the necessary paperwork. The non-random part of individual utility will then be state dependent and (without loss of generality absorbing the relative effect of observables $\boldsymbol{X}_{i,t}$ in the status of insuring) equal to

$$\nu_{i,0,t} = -c_2 I_{t-1}, \quad \nu_{i,1,t} = \alpha + \boldsymbol{X}_{i,t}\boldsymbol{\beta} - c_1\left(1 - I_{t-1}\right) \tag{5}$$

and the probability of $I = 1$ is

$$\mathbf{Pr}_i\left(I_t = 1\right) = \frac{1}{1+\exp\left[-\left(\boldsymbol{X}_{i,t}\boldsymbol{\beta} + \alpha - c_1 + I_{t-1}\left(c_1 + c_2\right)\right)\right]}. \tag{6}$$

In this model, α, c_1, and c_2 are not separately identified. To see this, suppose (α^*, c_1^*, c_2^*) maximize the likelihood. Then the set $(\alpha', c_1', c_2') = (\alpha^* + k, c_1^* + k, c_2^* - k)$ yields the same likelihood for any $k \in \mathbb{R}$. In a standard lagged dependent variable logit model, the coefficient of I_{t-1} will then identify the sum of the two costs $c_0 = c_1 + c_2$. As follows from equation (6), the larger the switching costs, the larger the difference between previous members' and non-members' insurance up-take, as more people renew a membership or still do not join. Therefore, given the amount of inertia in the data, we expect the coefficient of I_{t-1} to be positive and significant.

III. Data and Summary Statistics

We draw data from Danish administrative records, which are linked at the individual level. They hold detailed information on individual background characteristics, family composition, labor market attachment, insurance status, income, and wealth. The registers all provide longitudinal information on the entire Danish population, mainly at an annual frequency. The tax authority records provide detailed data on total taxable income and transfers as well as taxable wealth from 1987 to 1996 because of a wealth tax that was in effect over this period. The wealth tax implied third-party reporting of both income and wealth holdings by banks and other financial intermediaries to the tax authorities. Thus, the data we use for our empirical analysis span those ten years.

The mortgage reform in 1992 allowed homeowners to finance non-housing consumption up to 80% of the property value from mortgage credit institutions. Therefore, we use the last observation before the reform to calculate the unexpected liquidity shock by taking 80% of housing wealth and subtracting mortgage debt. The tax registers report the publicly assessed housing value by December 31 each year, which takes into account only objective and easily observable characteristics. However, a home equity loan is granted on the basis of the market price of the property. To better reflect the market fluctuations in real estate prices, we follow Leth-Petersen (2010) and use aggregate data on market transactions to adjust the observed property values by the ratio between market prices and public evaluations for each year and municipality. Each mortgage is recorded in our data as a snapshot of the market value of its callable mortgage bonds, taken on December

31.[14] This is the value that counts towards LTV limits on new loans.

We normalize the liquidity shock and financial controls by a proxy for permanent income, the 22-year average of real earnings from 1987 to 2008. We use as many earnings observations as possible for this calculation to reduce the risk of comparing individuals on different parts of their life-cycle earnings trajectory.[15] The normalized liquidity shock we use throughout the paper is thus given by

$$L_{1991} = \frac{0.8 \times H_{1991} - M_{1991}}{Y^P} \tag{7}$$

where H_{1991} and M_{1991} denote housing and mortgage values as of December 31, 1991, respectively, and Y^P is annual permanent income.

We measure insurance against adverse labor market outcomes by membership in an unemployment insurance fund. The administrative records provide annual information on individual membership status by December 31 reported directly by the unemployment insurance funds.

The mortgage reform coincided with the sudden increase in the incentive to join an unemployment insurance fund for early-retirement purposes, which invalidates our identification strategy for the affected birth cohorts. Therefore, we restrict the estimation sample to individuals, who were between ages 25 and 39 throughout the period of interest, old enough to exhibit non-trivial housing and insurance choices and for whom early retirement motives did not affect the demand for unemployment insurance membership. Thus, our initial sample consists of homeowners in 1992 from the cohorts born between 1957 and 1962. As those over 35 might have joined early to count towards the retirement criterion in case they missed some (at most five) years before 60, in Appendix C we present that our results are robust to using only the youngest of cohorts.

The housing and mortgage information used to calculate the liquidity shock reflects the values by December 31, 1991, five months before the mortgage reform in late May,

[14]Mortgage debt is reported separately only until 1992. This limits the scope for supplementary investigations of post-reform behavior, e.g. whether they take out a home equity loan in case of unemployment.

[15]We assume away moral hazard in the earnings process. Post-reform wage earnings potentially are affected by reoptimization due to the changed portfolio composition caused by the reform. However, we do not regard this as a substantial issue compared to the improvement in the approximation to permanent income that these extra years provide. Restricting the measure to pre-reform earnings does not change our results qualitatively.

1992. To ensure that our estimates are not confounded by variation from homeowners who choose to move, and thus refinance, before the reform took effect, we exclude individuals who moved within the first five months of 1992, according to residence records.

Our financial variables directly reflect individual tax forms from third-party reports. Irregularities may or may not have been corrected; as most Danes have too little wealth to be taxed, neither has the tax agency any incentive to correct underestimates, nor the taxpayer to correct overestimates. Because the wealth data comes from snapshots as of December 31, imprecisions in the timing of the reports can affect what the researcher observes in the data. Such transitory irregularities are unavoidable in public administrative records and introduce noise in the financial variables. However, we exclude persistent outliers, for example, the very rich. An individual is excluded from the sample if, for at least one of the financial variables (housing wealth, mortgage debt, assets, liabilities, disposable or permanent income), his average value over the entire sample period is in the top 1% of the distribution. We further condition on participation in the labor force and being a wage earner (the self-employed have a different unemployment insurance scheme). We also exclude records with incomplete information on labor market attachment such as industry code or experience.

Buying and selling real estate involves several transactions that are potentially executed and registered at different points in time. Because housing and mortgage values are snapshots on December 31, 1991, a real estate transaction close to that date potentially implies that these values refer to different pieces of property. While such patterns are obvious for some observations, we are unable to systematically identify these errors because both values fluctuate from year to year, and people may buy either a new home or another home. We exclude individuals for whom we calculate a liquidity shock in the top or bottom 1% of the distribution.

Finally, we restrict our analysis to individuals who are observed in all years between 1987 and 1996. We keep a fully balanced sample to avoid changes in sample composition due to attrition by migration or death. As we do not model how Danes plan their insurance membership when they go on parental leave, back to school, abroad, or into self-employment, the fully balanced sample also implies that we document the substitution between liquidity and insurance in the self-selected subgroup who remain employed (or unemployed) from 1987 to 1996. This might affect the external validity of our findings, e.g. the population treatment effect including post-1992 entrepreneurs' need of insurance might be higher. The internal validity of our difference-in-differences design is not under threat in the fully balanced sample.

Our final sample consists of 113,344 individuals, detailed in Table A1 of Appendix A. We compare summary statistics of this sample to the entire population of the same cohorts in Table 1. Columns 2-5 divide the selected sample of homeowners in 1991 into quartiles of the liquidity shock induced by the reform, while the last column reports values for the entirety of the six cohorts.

The table reports the liquidity shock and its subcomponents (housing wealth, mortgage debt, and permanent income) as well as the evolution of the insurance up-take, move-in date, and socioeconomic variables. In addition, labor market attachment is characterized by disposable income (total current-year income net of taxes), accumulated labor market experience over the past five years, and individual unemployment risk. The unemployment risk is given by the following year's industry- and occupation-specific unemployment rate.[16] Financial variables include liquid assets net of stock holdings, which are very noisily recorded in the registers, and total debt net of mortgage debt.

[Table 1 about here.]

The table shows that the reform changed the liquidity of less than half the homeowners in the sample. The average amount of extra liquidity gained by homeowners in the top quartile is two thirds of annual permanent income, whereas homeowners in the bottom quartile were far from being able to use their real estate as collateral for personal loans from mortgage credit institutions. The time trends in insurance up-take show that those who are affected more by the reform generally bought more unemployment insurance in the first place.

The median housing values do not vary much across the quartiles of the liquidity shock; they are only slightly higher in the top and bottom quartiles than in the middle two. This implies that the variation in the liquidity shock comes from differences in the mortgage values, which indeed differ substantially: The median mortgage value decreases

[16] As we do not observe occupational level after 1995, we cannot compute this measure for 1996, and we therefore exclude 1996 from our conditional analysis.

by more than $10,000 from each quartile to the next; the largest decrease being $25,000 from the bottom quartile to the next. Many borrowers are under water in 1992 after recent declines in interest rates.

Much of the variation in mortgages is a result of people settling down at different points in time. This is consistent with what we observe about the time spent in the house they live in in 1991: The longer one had already lived there, the smaller was the still outstanding debt. All other variables are fairly stable across quartiles of the liquidity shock. This supports the intuition that the variation in home equity holdings is primarily caused by timing of purchase rather than selection on observed characteristics.

The sample of homeowners differs from the general population in their unemployment insurance and employment rates. Both of these differences could, however, potentially be attributed to those out of the labor force. Students, whom we exclude, have no incentive to buy unemployment insurance before graduation, while they are included in the calculation of the employment rate. Danes out of the labor force or renters imply the differences in other variables such as income, assets, and debt. Furthermore, as people who stay longer in the educational system tend to settle down at later ages, students may also contribute to the lower propensity to live with a spouse, even though the number of kids is not that different between our sample and the population in general.

These differences do not affect the internal validity of our results, or the mechanism we describe. On the external validity of the magnitudes of some responses, we underestimate the general population's substitution between liquidity and insurance if renters have even stronger consumption commitments or tighter liquidity constraints (if they did not qualify for mortgages).

IV. Results

In this section we present evidence that after 1992 homeowners with much home equity reduced their demand for unemployment insurance compared to owners with large mortgages on their homes, which left them unaffected by the mortgage reform. Because our empirical strategy relies on the common trend assumption across various levels of leverage, we study the correlation between year-on-year changes over time in the proportion of insured and home equity by December 31, 1991, and we present evidence that insurance trends did not differ significantly in the pre-reform period across groups. In subsection B, we perform placebo tests on the years before the reform and show that, without the mortgage reform, home equity at time t has no impact on the demand for unemployment insurance at time $t + 1$.

A. Main Results

We perform our analysis of the effect of the liquidity shock on unemployment insurance membership rates after 1992 with both a discrete and a continuous formulation for the liquidity treatment. We define the discrete treatment group as those having more than a month of permanent income worth of credit in 1991 home equity. The control group for the discrete formulation are those homeowners whose liquidity shock measure is less than or equal to zero.[17] We compare the trends over time in insurance demand of the two groups in Figure 3. The figure plots in both panes the insured share over time in solid black for the treated group and in solid gray for the control group. For easy comparison, the dashed gray line represents the parallel shift of the control group up towards the treated group, such that 1991 percentages coincide for both groups. The figure marks the years in which the difference between the control group average and the projection of the treatment group average is statistically significant at the 1% confidence level according to a proportion test.

[Figure 3 about here.]

Figure 3 is divided into two panes. Pane 3a plots the unconditional yearly share of insurance over time for the two groups. Pane 3b plots the same trends conditioned on a set of controls, which includes marital status, gender, the number of children below the age of 18 in the household, disposable income in the year, unemployment risk, and the 1991 values of liquid assets and debts. Additionally, we control for year, cohort, industry and education fixed effects, and for our measure of permanent income.

Both the unconditional and the conditional analysis show that the trend for the treated group and the projected trend for the control group closely follow each other

[17] To mitigate eventual misclassification bias between the treatment and control groups, we exclude from this analysis individuals with positive home equity worth less than a month of permanent income.

before the reform, but they significantly diverge from 1992 onwards. That the pre-trends line up closely before 1992 supports the common trends assumption, on which our identification strategy rests. The figure shows that treated individuals reduce their demand for unemployment insurance after the reform relative to the control group. Unconditionally, the effect seems to unfold over time after 1992, trends controlling for unobservables diverge sharply in 1992, and that their difference remains constant afterwards.

This finding does not depend on the chosen cutoff between the treated and the control groups. Similar results hold across treatment intensities. To study the trends across treatment intensities, we focus on the partial correlation between accessible home equity in 1991 and the yearly (net) changes in insurance purchase, i.e. subscriptions minus unsubscriptions. Figure 4 plots yearly conditional changes in insurance purchase, by vigintiles of conditional home equity in 1991.[18] This procedure returns a set of twenty groups with different treatment intensity (or dosage), and a more credible graphical analysis of the partial correlation between treatment and outcome over the years.[19]

[Figure 4 about here.]

Pane 4d shows that only in 1992 is the correlation between the changes in the net unemployment insurance membership and the treatment strong and negative: The more home equity individuals hold by the end of 1991, the less insurance they buy in 1992 after the reform. After 1992, the correlation disappears, confirming the post-reform stabilization of the effect shown in Figure 3.

Figure 4 shows that the negative effect of a shock to liquidity on demand for insurance depends on treatment intensity (or dosage). We report partial treatment effect estimates

[18] For each year in our sample, we regress both the first difference in insurance purchase and accessible home equity in 1991 on our set of controls. We divide the residuals from the regression on accessible home equity in 1991 into twenty vigintiles, and for each vigintile we plot in Figure 4 the average residuals from the two regressions, adding back the overall mean.

[19] Figure 4 shades the top and bottom 5% by treatment intensity. These percentiles include extreme values of the treatment variable, potentially caused by the time difference between property transactions and mortgage contracts being recorded in the registers even after our sample restrictions. As we cannot test this hypothesis, we keep those observations in our analysis for the sake of robustness, but we present two different linear regression lines in Figure 4 to show the magnitude of the correlation with and without these vigintiles: The dashed gray lines show regression lines for the full sample; the solid black lines for the sample excluding the top and bottom vigintiles. As expected, errors in the extreme values of our measure of the liquidity shock dilute the effect of the 1992 reform, consistent with the hypothesis that these are for the most part due to measurement error.

of the liquidity effect for the continuous treatment definition in Table 2, with the same set of controls as in Figures 3b and 4.

[Table 2 about here.]

The first column of Table 2 collects the estimated coefficients for a linear probability model of insurance purchase, where the first row shows the estimated coefficients for the liquidity shock measure interacted with the post-reform period. These are the partial effects on the probability of buying insurance in percentage points. Standard errors are clustered at the individual level, allowing for autocorrelation of errors within individuals across the pre- and post-reform periods (Bertrand et al., 2004).

According to the OLS estimates, potential access to credit equivalent to one year of estimated permanent income decreases the probability of buying unemployment insurance by approximately 0.5 percentage points. The estimate is highly significant, and robust to individual fixed effects, as reported in the second column of the table. Because unemployment insurance in Denmark is subsidized and convenient for all those who face non-trivial unemployment risk, and because we do not observe the other adjustment channels that increased access to liquidity crowds out (e.g. durable consumption as in Browning and Crossley, 2009), we interpret this estimate as a lower bound on the effect such a reform would have in an environment with fewer rigidities, more salience of the insurance decision, and fewer people facing better than actuarially fair prices. This finding suggests that liquidity affects demand for insurance significantly, inducing people on the margin of insurance choice to change their behavior.

As insurance choice is affected by many unobserved idiosyncrasies (e.g. risks, circumstances, preferences), it is natural to extend the model with individual fixed effects.[20] Column 2 shows that while the explanatory power of the model rises considerably across specifications, the coefficient of interest changes slightly. This result allows us to apply the bounding exercise introduced by Oster (2013) to calculate what the true treatment effect could be in the linear model under an assumption of proportional selection on unobservables (relative to observables, including the fixed effects). We compare the specification including individual fixed effects with an unconditional difference-in-differences

[20]Fixed effects cannot confound our estimates identified from differential changes after 1992, yet they improve the precision of the treatment effect estimates and controls.

model that does not control for observables, and we assume that the confounding effects of observable variables are proportional to those of unobservable confounders. The identification of the treatment effect in the unconditional difference-in-differences design requires the inclusion of only the group identifier (or in this continuous specification, end-of-91 home equity) and year dummies as controls. This (unreported) model has a point estimate of -0.544 for the treatment effect, with an R^2 of only 0.0046. In the conditional model that includes fixed effects (column 2), the point estimate is -0.460, while the R^2 of the model is 0.7416. This finding suggests that the true treatment effect is larger in magnitude than $-0.460 - [-0.544 - (-0.460)]\,\frac{1-0.7416}{0.7416-0.0046} = -0.43$ percentage points of insurance purchase.[21]

The subsidies to the insurance system in Denmark and the social norms associated with unemployment fund membership (see section II) imply that the majority of the population always insures, and even others rarely change membership status. As described in Section II, we model inertia in two alternative random utility models. In the fourth column, we show the estimates from a fixed-effect logit model, which estimates the parameters of the model on the 13% subsample that changes insurance membership status during the period of interest. While we estimate a negative coefficient associated with the liquidity shock provided by the reform in this model, this estimate is just below a 5% significance threshold.

In column 5, we show the predicted partial effect of liquidity (more home equity after the reform) from the random utility model with fixed switching costs for changing membership status described in equation (6). This model explicitly incorporates inertia in the estimation. As a comparison, we show the coefficient associated with its linear probability model counterpart in column 3. The results from the model in column 5 show a significant negative effect associated with the liquidity shock after 1991, and are thus in line with those from the linear probability models. However, this model also highlights the importance of inertia and state dependence in our setup. The sum of the costs for changing unemployment fund membership accounts for over six times as much of the

[21]This calculation assumes that the unobservable confounders are not only proportional, but also equally powerful explanatory factors, i.e. $\delta = 1$ in the notation of Oster (2013). Specification errors or invalid identification clearly remain a threat to the estimation of this effect. The nonlinear specifications and placebo tests that follow are thus complementary to this argument.

variation in insurance decisions as the unobservable variation in the latent model.

Across the columns of Table 2, we can compare how the predicted partial effect of liquidity (more home equity after the reform) changes across the specifications.[22] The linear model predicts that 1 in 200 Danes choose not to buy formal unemployment insurance because of the extra liquidity. This estimate is robust to linear fixed effects but, as many of the observations have high baseline probability to insure, is smaller when computed according to the nonlinear models. The effects seem half as strong in the linear model controlling for previous insurance status, and a fourth as large in a logit model with the same control for persistence. The few observations that allow a fixed effects logit estimation have a larger point estimate, but 5-10 times the standard errors. Covariates have effects with the expected signs, e.g. the risk of unemployment significantly raises the chance of buying unemployment insurance (roughly 1-to-1 in percentage points). The strong persistence of the insurance decision is also evident, indicating large inertia in insurance choices.

We repeat our baseline estimation and robustness checks for the discrete treatment definition in Table 3. While a discrete treatment allows for a more straightforward implementation of the difference-in-differences estimator, we lose information relative to the continuous treatment definition. We still find a highly significant effect with the OLS and fixed effects estimators.[23] Homeowners who gained access to extra liquidity in 1992 decreased their likelihood of purchasing unemployment insurance by 0.7 percentage points compared to homeowners who already mortgaged to the limit. Our results using the random utility models are similar: Partial effects in the linear specification suggest a roughly half a percentage point drop in insurance up-take because of the liquidity buffer after the reform. However, the predicted drop in insurance probability is half as much once we control for the persistence of the insurance decision, and only -0.2 if we do so in a logit model. This finding suggests that the insurance choice is closely related to the

[22]This prediction calculates the marginal effects at the observed levels of all covariates in the post-1991 period. This measure is closer to the ATET (average treatment effect on the treated) than that obtained using the full sample. Because overall insurance up-take increased after 1992, we expect smaller marginal effects using the post-1991 period than using the full sample.

[23]As individual fixed effects again raise the R^2 to 0.742, the upper bound on the treatment effect is -0.48 percentage points under the necessary assumptions for applying Oster (2013), which we state earlier in the text. We conclude that omitted variable bias does not seem to threaten our finding of a small but significant effect.

amount of accessible credit, rather than access to credit itself.

[Table 3 about here.]

B. Placebo Tests and Robustness Checks

Our results indicate that those who gained access to home equity due to the 1992 reform decreased their demand for unemployment insurance compared to those who had no access. In this section, we address two potential mechanisms that might confound our results. First, our treatment selects people with much home equity by December 31, 1991, and we cannot a priori distinguish the effect of the reform from the effect of having much home equity in one given year.

Second, home purchases and mortgaging decisions are strongly dependent on household formation choices, and household composition itself affects the attractiveness of the unemployment insurance scheme. In particular, the alternative of supplemental security income changes with marriage, as the means testing for supplemental security income is more severe for couples. We tackle these two concerns separately.

To identify the specific effect of the 1992 reform, and rule out that the effect in Table 2 is in fact caused by mechanical correlates of treatment, we repeat our analysis for a series of placebo reforms, taking place in all years in our sample before 1992. Figure 5 shows the equivalent of Figure 4 for placebo reforms from 1988 to 1991. We plot partial correlations between the net insurance sign-up in a given year and the amount of home equity in the year before, using the same regressors and specifications used for the analysis of the 1992 reform.[24] That is, in the first panel we plot the change in the percentage of insured between 1987 and 1988 on the vigintiles of unexplained 1987 home equity. The sample selection for the placebo analyses carries over from the 1992 reform, except that for each placebo year, we only keep homeowners in the year preceding the placebo reform. Therefore, the number of observations changes year by year.

[Figure 5 about here.]

All placebo tests exhibit unemployment insurance–home equity correlations that

[24]We hold wealth controls constant at their pre-reform levels. That is, for each of the (placebo) reform years, we control for wealth held by the end of the previous year.

scatter around zero, with no systematic pattern that Figure 4d would fit into. This finding not only supports the validity of our controls, but it also rules out that home equity has a mechanical effect on demand for insurance, independently of liquidity, and therefore supports the causal interpretation of our estimates.

As a second earner can also cushion shocks and even affects whether one qualifies for the fallback benefits, our results can be confounded if individuals with more home equity also form more households. To rule out this confounding channel, we estimate our models using a specific subsample of stable households. Instead of controlling for household size (single or couple), as we did in our baseline specification, we estimate the models in Tables 2 and 3 after excluding observations for which marital status is different from that of 1991. That is, if we observe an individual getting married in 1989, we keep only observations from 1989 onwards. This way we obtain a subsample of observations that, though unbalanced, contains only households with stable marital status throughout the estimation period.

[Table 4 about here.]

Table 4 shows that results are robust to restricting the sample to stable households. All controls and model specifications are the same as those in Tables 2 and 3. Compared to our baseline estimates, these results are similar, if not stronger. We therefore argue that the results in Table 2 and 3 are not driven by differential patterns in household formation across levels of home equity.

C. Heterogenous Effects

Figure 1 showed that we predict insurance to have positive expected value for a large fraction of the population in 1987, and this is true for all years. If these people are making a fully-informed rational decision about insurance, liquidity should be irrelevant for them; they should buy insurance regardless. The average treatment effect in the population is supposed to come from the left tail of the risk distribution. In Table 5, we show our main specification over five equal-sized cuts of the 1992 risk distribution, and indeed, for the lowest quintile with average risk of 1.85% in 1992, the estimated effect is double the population average from the corresponding column 1 of Table 2. Relaxing liquidity constraints by one year's worth of permanent income decreases insurance purchase by 0.94 percentage points. To put this magnitude into context, this amounts to more than 5% of this low-risk subpopulation who was without insurance in 1991, though some of the effect also comes from insured Danes not renewing their membership. For higher risk quintiles, where insurance is a bargain, the estimated effect of more liquidity is not only lower, but not significantly different from zero.

[Table 5 about here.]

We also argued that the insurance system faces some rigidities, and it might be puzzling why not all high-risk Danes join a fund, or why low-risk occupations are ready to cross-subsidize others. Yet for those making an active choice about insurance, we can compare two competing incentives: How does the effect of liquidity compare to that of a 1 percentage point reduction in unemployment risk? From the point estimates for the low-risk quintile in Table 5, we can conclude that one year's income in liquidity has similar effects as a 0.3 percentage point drop in unemployment risk, or 15% of their baseline risk in 1992, among those for whom insurance is priced most unfairly.[25]

V. Conclusion

If liquidity is a pressing concern during unemployment, people will be partially protected by a buffer stock of savings. This paper documents how increased access to liquidity through the exogenous introduction of home equity loans lowered the demand for unemployment insurance, implying that private self-insurance substitutes for formal public insurance. The demand for unemployment insurance increased in Denmark throughout our period of interest. However, after the 1992 reform, demand increased relatively less for those who held equity in their homes compared to those who did not. By exploiting the unique policy-induced variation provided by a mortgage reform, we show that access to liquidity affects insurance choices on the margin, even when wealth does not change with it. Simply relaxing liquidity constraints shields people from misfortune to such an extent that some prefer to avoid paying an unemployment insurance premium.

[25] For the correct interpretation of this rescaling, we do not claim to have identified the causal impact of unemployment risk, nor that our predicted risk measure is an unbiased and properly scaled estimate of subjective risk perceptions each year.

We show that an additional increase in accessible liquidity worth one year of income caused only about 0.5 percentage of Danes to forgo public unemployment insurance. Among individuals for whom insurance is much more expensive than actuarially fair, a year's income's worth of extra liquidity reduces insurance up-take by 0.94 percentage points. This effect is equivalent to that of a 0.3 percentage point, or 15%, decrease in the risk of unemployment, while higher-risk groups show no effect.

Our findings relate to the discussion about the scope of social insurance programs and whether unemployment insurance should be mandatory: The mere option to use one's own resources more flexibly alleviates the welfare costs from job loss. Some workers in our sample were able to perceive this opportunity in a forward-looking manner and to make a conscious insurance choice accordingly. While the modest crowd-out reminds us of other important drivers of insurance up-take, this finding suggests that increased access to liquidity for the general population substitutes partially for a publicly funded unemployment insurance scheme.

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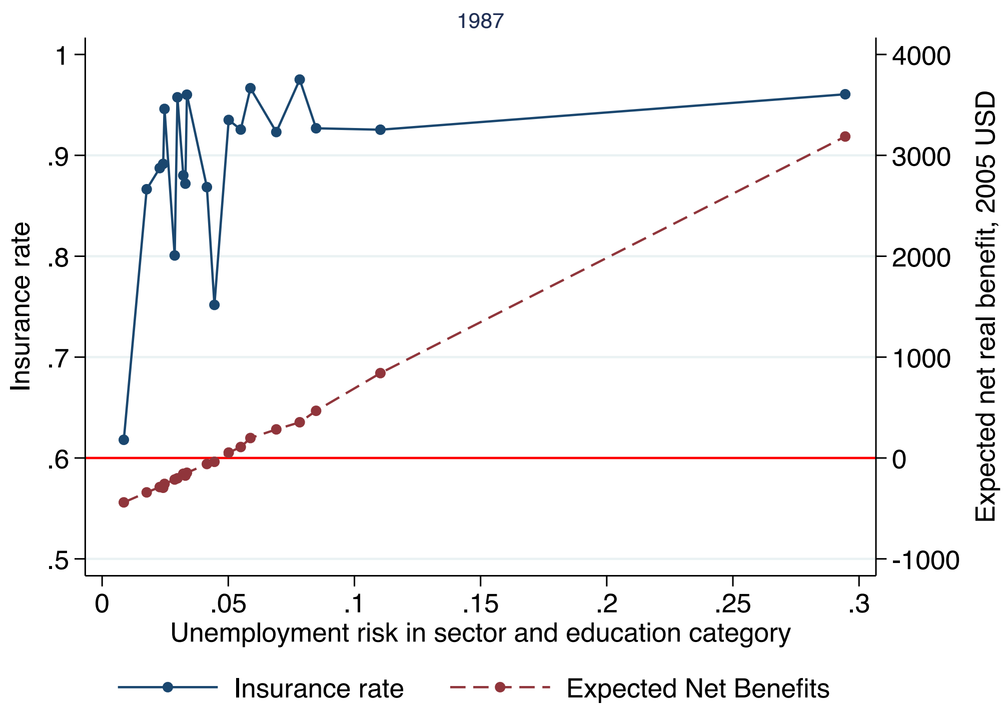
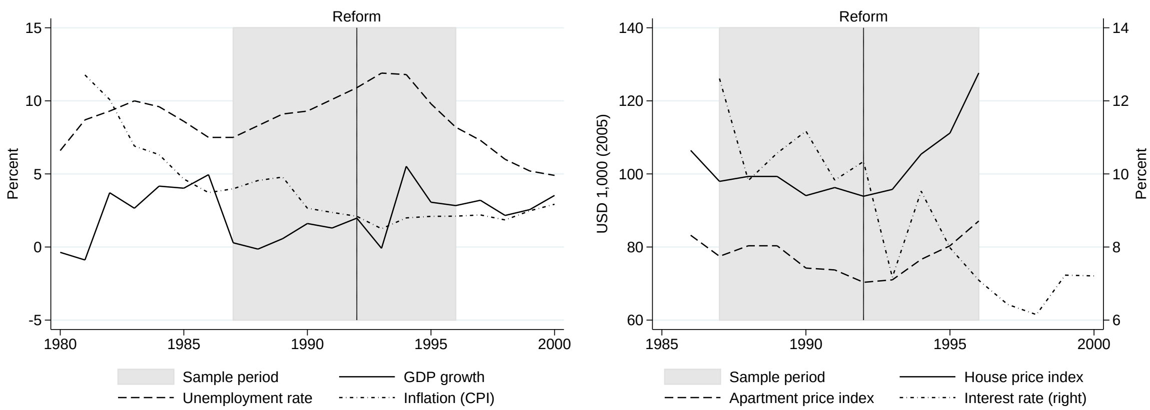
List of Figures

Figure 1.—Expected Net Benefits of Unemployment Insurance in the Estimation Sample



Note.— The figure presents average insurance purchase (membership in November) and our calculated expected net benefits (dashed line) against average full-time full-year equivalent unemployment risk in twenty equal-sized bins, for the 1987 insurance decision. Both series are plotted as means in 20 equal sized bins by risk, connected for illustration. The net benefit is expressed in 2005 US dollars (2005 DKK values using the domestic CPI, converted to USD using the 1991 exchange rate of 5.91). Unemployment fund membership is measured in November 1987 but coverage applies to 1988. The marginal (bottom) tax rate used for net benefits come from each taxpayer's actual MTR in 1988, according to our calculation based on observed incomes and determinants of the tax schedule. Unemployment risk here is the average FTFY equivalent time spent on benefits in 1988 for others in the estimation sample who are full-time insured in the same industry and broad education category in November 1987. This leave-out mean unemployment risk predicts realized unemployment with an R^2 of 0.59 over the 1987-1995 period. In 1988, FTFY unemployment corresponded to 312 days of the daily maximum benefits, and membership fees to 8 days worth of benefits. This calculation does not use the 90% replacement rate for those who do not hit the benefit cap. See equation (1) for the specific formula used.

Figure 2.—ECONOMIC ENVIRONMENT



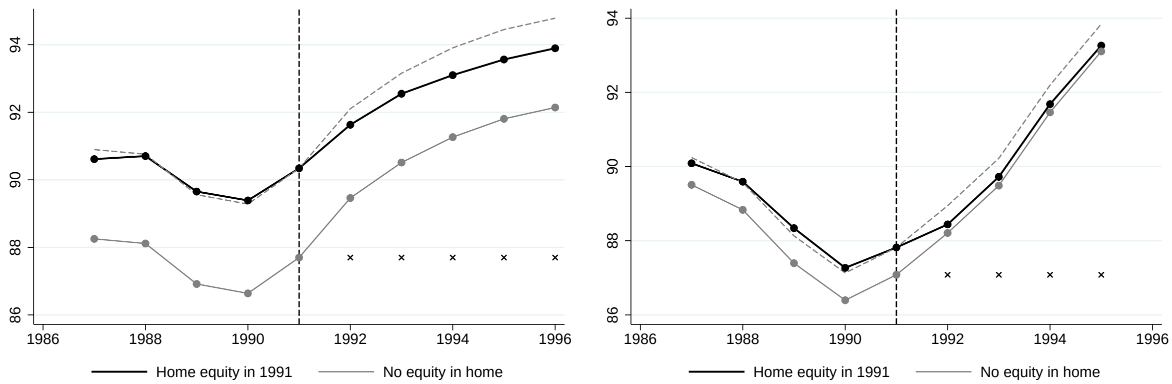
(a) Business Cycle

(b) Real Estate Markets

NOTE.— Real estate prices reflect market transactions. Interest rates refer to annual average yields of 20-year maturity mortgage-credit bonds. 2005 US dollar values are 2005 DKK values using the domestic CPI, converted to USD using the 1991 exchange rate of 5.91.

Sources: For one-family home and apartment prices, Statistical Yearbook (*Statistisk Årbog*), 1988-1998; For other variables, Statistics Denmark (www.statistikbanken.dk, NAT02 B1.*g, AULAAR, PRIS12 and DNRENTA series, accessed on December 5, 2012).

Figure 3.—INSURANCE UP-TAKE BY TREATMENT GROUP AROUND THE REFORM

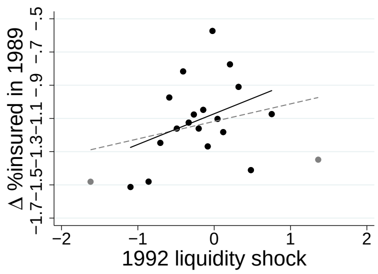


(a) Unconditional

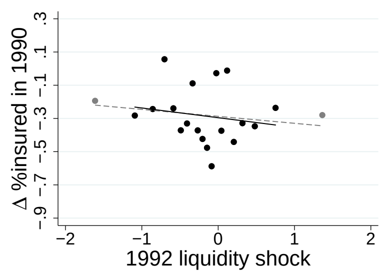
(b) Conditional on controls

NOTE.— The black solid line shows the average insurance up-take over time for those homeowners who experienced a liquidity shock larger than a month's worth of their permanent income in 1992; the gray solid line indicates the average insurance up-take over time for those homeowners who experienced no liquidity shock, that is they held no equity they could borrow against in December 1991. We define our measure of liquidity shock in equation (7). The dashed gray line shifts the average insurance up-take of those who did not experience a liquidity shock such that the average insurance rates of the two groups coincide in 1991. The estimation sample comprises homeowners between ages 29 and 34 in 1991 – for more detailed information about sample selection, see Section III.

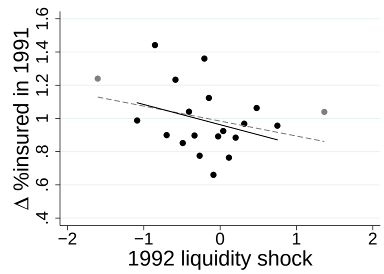
Figure 4.—Impact of 1991 Home Equity on Unemployment Insurance Sign-Up (with controls)



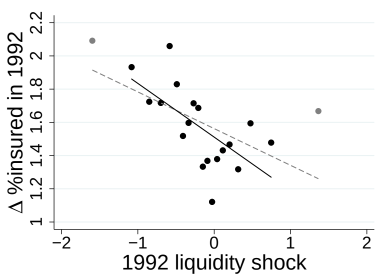
(a) 1989



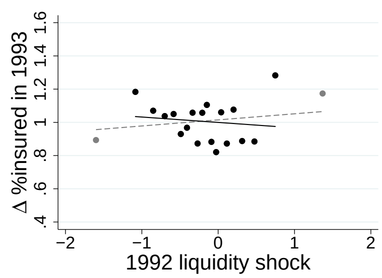
(b) 1990



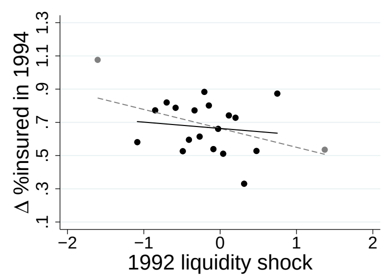
(c) 1991



(d) 1992



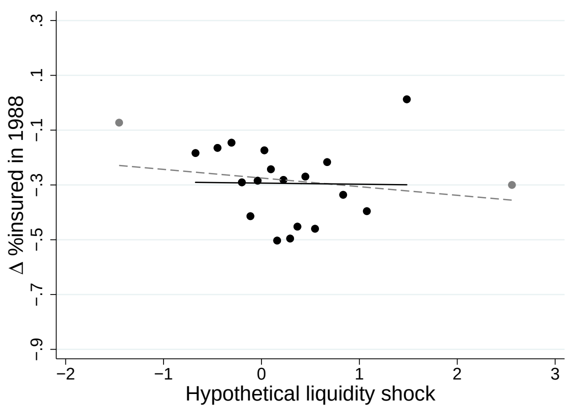
(e) 1993



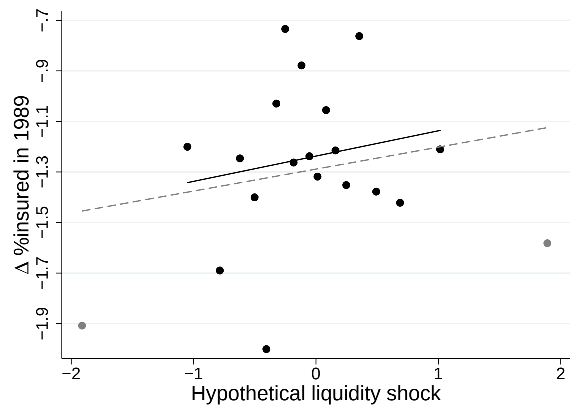
(f) 1994

Note.— This figure plots yearly percentage point changes in insurance up-take, conditional on controls, by vigintiles of home equity in 1991. See footnote 18 for details and Section III for details of our sample selection. For easy comparison, the scale of the axes is constant throughout the panes. Intercepts differ because of different mean net subscription rates. The dashed gray lines show the regression lines for the full sample; the solid black lines for the sample excluding the top and bottom vigintiles.

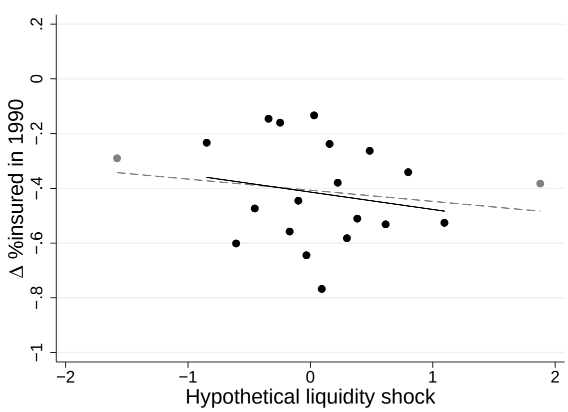
Figure 5.—PLACEBO TESTS: IMPACT OF PRE-1991 HOME EQUITY ON UNEMPLOYMENT INSURANCE SIGN-UP (WITH CONTROLS)



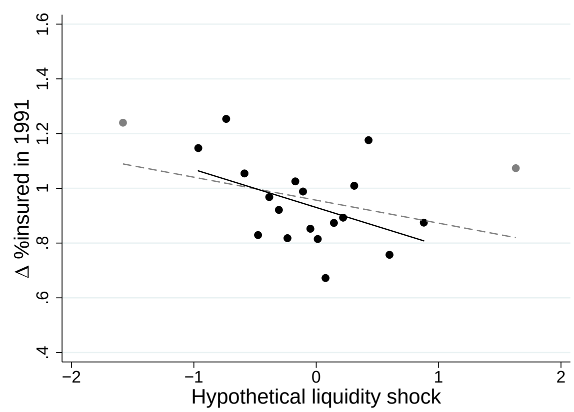
(a) Placebo Reform in 1988



(b) Placebo Reform in 1989



(c) Placebo Reform in 1990



(d) Placebo Reform in 1991

NOTE.—The figure shows the analogue of Figure 4 Panel (d) for placebo reforms from 1988 to 1991. Thus it plots yearly percentage point changes in insurance purchase, conditional on controls, by vigintiles of home equity in the year of the placebo shock. See footnote 18 for details and Section III for details of our sample selection.

List of Tables

Table 1

SUMMARY STATISTICS FOR THE ESTIMATION SAMPLE IN 1991 BY LIQUIDITY SHOCK QUARTILES, COMPARED TO THE DANISH POPULATION OF THE SAME BIRTH COHORTS

	Q1	Q2	Q3	Q4	Population
Liquidity shock (see text)	-1.07	-.37	-.02	.69	
Insurance rate 1989 (%)	85.7	87.4	89.8	89.7	74.5
Insurance rate 1991 (%)	86.5	88.2	90.4	90.3	75.5
Insurance rate 1993 (%)	89.8	91	92.6	92.7	79.8
Moved to 1991 housing (year)	1987.3	1987.1	1986.5	1984.9	
Financial variables (2005 USD)					
Housing wealth	$63,688	$58,444	$58,217	$64,265	
Mortgage debt	$84,479	$59,544	$47,441	$33,654	
Permanent income	$34,360	$34,738	$34,097	$32,831	$28,259
Disposable income	$33,883	$32,868	$31,943	$30,936	$27,786
Liquid assets	$2,850	$2,915	$2,971	$3,275	$1,847
Debts	$14,819	$15,893	$16,041	$16,252	$9,206
Labor market measures					
Employment rate (%)	97.7	97.8	97.3	97.2	75.4
Experience, 1987-91 (years)	4.6	4.6	4.6	4.5	3.4
Unemployment risk (%)	8.4	8.5	8.8	9.1	8.9
Industry, fewest	Fi	Mi	Fi	Fi	Mi
Industry, most	Me	Me	Me	Me	So
Demographic information					
Age	31.6	31.5	31.6	31.9	31.5
College graduates (%)	22.7	22.1	20.5	16.5	24
Married or cohabiting (%)	64	59.8	59.8	60.5	47.6
Number of kids	1.3	1.2	1.3	1.3	1.1
Female (%)	44	37.4	35.6	42	49.2
Observations	28,336	28,336	28,336	28,336	439,016

NOTE.—Industry codes (NACE rev. 1): Fi: Fishing (B); Mi: Mining (C); Me: Metal industry (DJ); So: Other community, social and personal service activities (O). The financial variables are reported as medians in 2005 US dollars (2005 DKK values using the domestic CPI, converted to USD using the 1991 exchange rate of 5.91). Because renters and people living with parents are included in the population (column 5), we do not report housing and mortgage values for this group. The estimation sample comprises homeowners between 25 and 35 in 1991 – for more detailed information about sample selection, see Section III.

Table 2

IMPACT OF 1992 MORTGAGE REFORM ON UNEMPLOYMENT INSURANCE PARTICIPATION (TREATMENT WITH DOSAGE)

	(1) OLS	(2) FE	(3) LDV	(4) FE Logit	(5) LDV Logit
1991 home equity, after 1991	-0.508**	-0.460**	-0.281**	-0.775	-0.130**
	(0.0835)	(0.0826)	(0.0410)	(0.401)	(0.0327)
1991 home equity	0.825**		0.239**		0.119**
	(0.114)		(0.0331)		(0.0249)
1991 liquid assets	0.459*		0.148**		0.120**
	(0.226)		(0.0426)		(0.0301)
Permanent income	-0.615**		-0.104**		-
					0.0424**
	(0.0299)		(0.00769)		(0.00415)
1991 debt	-2.100**		-0.424**		-0.274**
	(0.186)		(0.0367)		(0.0336)
1991 housing wealth	-1.109**		-0.179**		-0.165**
	(0.0920)		(0.0206)		(0.0188)
Disposable income	-10.30**	0.867	-1.768**	5.058*	-0.125
	(0.986)	(0.636)	(0.267)	(2.216)	(0.162)
Unemployment risk (pp.)	0.817**	0.319**	0.0725**	1.386**	0.0510**
	(0.0251)	(0.0187)	(0.00648)	(0.0930)	(0.00555)
Experience (year)	-1.802**	-1.129**	-0.229**	-9.541**	-0.281**
	(0.0811)	(0.0656)	(0.0242)	(0.504)	(0.0285)
Number of kids	-0.377**	0.117	-0.141**	1.745**	-0.129**
	(0.0746)	(0.0611)	(0.0176)	(0.332)	(0.0166)
Female	2.949**		0.614**		0.451**
	(0.200)		(0.0427)		(0.0354)
Married or cohabiting	1.124**	0.638**	0.247**	5.299**	0.269**
	(0.133)	(0.106)	(0.0335)	(0.587)	(0.0312)
Lagged insurance			84.44**		10.84**
			(0.125)		(0.0399)
Year	Yes	Yes	Yes	Yes	Yes
Cohort	Yes	No	Yes	No	Yes
Industry	Yes	Yes	Yes	Yes	Yes
Education	Yes	No	Yes	No	Yes
Municipality	Yes	Yes	Yes	Yes	Yes
Income vigintiles	Yes	Yes	Yes	Yes	Yes
Observations	1,020,096	1,020,096	906,752	130,986	906,752
Individuals	113,344	113,344	113,344	14,554	113,344

NOTE.—Standard errors clustered by individual in parentheses; * p<0.05, ** p<0.01. The table collects Average Partial Effects (APE) on insurance up-take computed for the post-1991 subsample, in percentage points, from models of 0-1 unemployment insurance status estimated on the entire 1987-1995 panel. These models include the continuous measure of home equity (relative to permanent income) in 1991 as the dosage of the extra liquidity treatment afterwards. The first column shows the estimates from an OLS model. The second column includes individual fixed effects. The model in the third column adds the lagged insurance status to control for inertia. The fourth column shows the APE from a fixed effect logit model, estimated on only those who ever change their insurance status. Finally, the model in the fifth column is a discrete choice model with fixed costs of switching, which translates into a lagged dependent variable logit. Financial variables are scaled by annual permanent income. The estimation sample is the fully balanced 1987-1995 panel of homeowners between ages 29 and 34 in 1991; for more detailed information about sample selection, see Section III.

Table 3

IMPACT OF 1992 MORTGAGE REFORM ON UNEMPLOYMENT INSURANCE PARTICIPATION (DISCRETE TREATMENT)

	(1) OLS	(2) FE	(3) LDV	(4) FE Logit	(5) LDV Logit
Treatment	-0.613**	-0.523**	-0.364**	-0.306	-0.196**
	(0.124)	(0.123)	(0.0605)	(0.682)	(0.0533)
Treated group	0.615**		0.261**		0.122**
	(0.169)		(0.0491)		(0.0405)
1991 liquid assets	0.477*		0.156**		0.124**
	(0.227)		(0.0427)		(0.0305)
Permanent income	-0.612**		-0.103**		-0.0415**
	(0.0309)		(0.00784)		(0.00423)
1991 debt	-2.034**		-0.413**		-0.272**
	(0.187)		(0.0369)		(0.0339)
1991 housing wealth	-1.091**		-0.172**		-0.160**
	(0.0949)		(0.0212)		(0.0195)
Disposable income	-10.29**	0.685	-1.794**	4.374	-0.128
	(1.016)	(0.646)	(0.270)	(2.310)	(0.164)
Unemployment risk (pp.)	0.831**	0.319**	0.0734**	1.383**	0.0522**
	(0.0260)	(0.0192)	(0.00669)	(0.0954)	(0.00573)
Experience (year)	-1.780**	-1.144**	-0.229**	-9.661**	-0.283**
	(0.0840)	(0.0680)	(0.0250)	(0.488)	(0.0295)
Number of kids	-0.351**	0.131*	-0.138**	1.868**	-0.127**
	(0.0771)	(0.0633)	(0.0182)	(0.344)	(0.0172)
Female	2.923**		0.616**		0.454**
	(0.207)		(0.0441)		(0.0366)
Married or cohabiting	1.078**	0.641**	0.234**	5.341**	0.254**
	(0.137)	(0.109)	(0.0346)	(0.604)	(0.0323)
Lagged insurance			84.47**		10.91**
			(0.128)		(0.0412)
Year	Yes	Yes	Yes	Yes	Yes
Cohort	Yes	No	Yes	No	Yes
Industry	Yes	Yes	Yes	Yes	Yes
Education	Yes	No	Yes	No	Yes
Municipality	Yes	Yes	Yes	Yes	Yes
Income vigintiles	Yes	Yes	Yes	Yes	Yes
Observations	1,020,096	1,020,096	906,752	130,986	906,752
Individuals	113,344	113,344	113,344	14,554	113,344

NOTE.—Standard errors clustered by individual in parentheses; * p<0.05, ** p<0.01. The table collects Average Partial Effects (APE) on insurance up-take computed for the post-1991 subsample, in percentage points, from models of 0-1 insurance status estimated on the entire 1987-1995 panel. These models include a discrete measure of any home equity vs none in 1991 as a binary treatment of any extra liquidity afterwards. Any home equity here is more than a month's income below the LTV limit, while none corresponds to being over the LTV limit. The first column shows the estimates from an OLS model. The second column includes individual fixed effects. The model in the third column adds the lagged insurance status to control for inertia. The fourth column shows the APE from a fixed effect logit model, estimated on only those who ever change their insurance status. Finally, the model in the fifth column is a discrete choice model with fixed costs of switching, which translates into a lagged dependent variable logit. Financial variables are scaled by annual permanent income. The estimation sample is the fully balanced 1987-1995 panel of homeowners between ages 29 and 34 in 1991 — for more detailed information about sample selection, see Section III.

Table 4

IMPACT OF 1992 MORTGAGE REFORM ON UNEMPLOYMENT INSURANCE PARTICIPATION AMONG HOUSEHOLDS STABLE AROUND 1991

	(1) OLS	(2) FE	(3) LDV	(4) FE Logit	(5) LDV Logit
Continuous	-0.611**	-0.516**	-0.315**	-1.045**	-0.170**
	(0.103)	(0.101)	(0.0504)	(0.405)	(0.0454)
Observations (C)	622,521	622,521	553,352	77,598	552,915
Individuals (C)	69,169	69,169	69,169	8,622	69,114
Discrete	-0.804**	-0.628**	-0.373**	-1.178	-0.215**
	(0.158)	(0.156)	(0.0771)	(0.846)	(0.0773)
Observations (D)	587,970	587,970	522,640	73,818	522,234
Individuals (D)	65,330	65,330	65,330	8,202	65,279

NOTE.—Standard errors clustered by individual in parentheses; * p<0.05, ** p<0.01. The table collects Average Partial Effects (APE) on insurance up-take computed for the post-1991 subsample, in percentage points, from models of 0-1 unemployment insurance status estimated on the entire 1987-1995 panel. The continuous and discrete specifications correspond to Tables 2 and 3, respectively, restricted to those whose marital status does not change over the period. See Section III for details of our sample selection otherwise. The first column shows the estimates from an OLS model. The second column includes individual fixed effects. The model in the third column adds the lagged insurance status to control for inertia. The fourth column shows the the coefficient in a fixed effect logit model, estimated on only those who ever change their insurance status. Finally, the model in the fifth column is a discrete choice model with fixed costs of switching, which translates into a lagged dependent variable logit. The estimation sample is the 1987-1995 panel of homeowners between ages 29 and 34 in 1991, when they live in the same household as in 1991 — for more detailed information about sample selection, see Section III. All models include the same controls used for the estimates shown in Tables 2 and 3.

Table 5

IMPACT OF 1992 MORTGAGE REFORM ON UNEMPLOYMENT INSURANCE PARTICIPATION BY UNEMPLOYMENT RISK QUINTILES

	Risk Q1	Risk Q2	Risk Q3	Risk Q4	Risk Q5
1991 home equity, after 1991	-0.944**	-0.440*	-0.128	-0.0542	-0.275
	(0.245)	(0.184)	(0.200)	(0.136)	(0.151)
1991 home equity	1.146**	0.508	2.059**	0.200	0.112
	(0.300)	(0.272)	(0.309)	(0.186)	(0.218)
1991 liquid assets	0.974	0.517	0.307	0.212	-0.620
	(0.588)	(0.572)	(0.494)	(0.392)	(0.515)
Permanent income	-0.864**	-0.627**	-0.751**	-0.363**	-0.360**
	(0.0580)	(0.0591)	(0.0725)	(0.0613)	(0.0738)
1991 debt	-3.424**	-2.583**	-1.857**	-1.288**	-1.120**
	(0.517)	(0.441)	(0.403)	(0.354)	(0.341)
1991 housing wealth	-1.398**	-1.081**	-1.808**	-0.670**	-0.830**
	(0.244)	(0.221)	(0.271)	(0.150)	(0.163)
Disposable income	-14.37**	-8.406**	-5.854*	-4.405*	-3.822
	(1.988)	(2.043)	(2.471)	(1.846)	(2.018)
Unemployment risk (pp.)	3.298**	0.193	0.0539	-0.287**	-0.0861
	(0.171)	(0.121)	(0.114)	(0.0873)	(0.0471)
Experience	-2.526**	-0.807**	-3.663**	-0.290	-0.968**
	(0.189)	(0.243)	(0.234)	(0.158)	(0.141)
Number of kids	-0.328	-0.182	-0.626**	-0.200	-0.268*
	(0.189)	(0.173)	(0.215)	(0.120)	(0.126)
Female	5.161**	0.122	3.052**	1.155**	-0.248
	(0.499)	(0.475)	(0.472)	(0.333)	(0.396)
Married or cohabiting	1.374**	1.690**	0.126	1.066**	1.075**
	(0.326)	(0.315)	(0.377)	(0.212)	(0.227)
Year	Yes	Yes	Yes	Yes	Yes
Cohort	Yes	Yes	Yes	Yes	Yes
Industry	Yes	Yes	Yes	Yes	Yes
Education	Yes	Yes	Yes	Yes	Yes
Municipality	Yes	Yes	Yes	Yes	Yes
Income vigintiles	Yes	Yes	Yes	Yes	Yes
Observations	219,319	188,580	189,880	20,1792	196,009
Individuals	24,401	20,967	21,131	22,462	21,822

NOTE.—Standard errors clustered by individual in parentheses; * p<0.05, ** p<0.01. The table reports the estimated coefficient associated with our treatment variable in the continuous specification, by five quintiles of 1992 unemployment risk. These estimates correspond to the OLS estimates in column 1 in Table 2. The table collects coefficients from OLS models of 0-1 insurance status. Financial variables are scaled by annual permanent income. The estimation sample comprises homeowners between 29 and 34 in 1991 — for more detailed information about sample selection, see Section III. Unemployment risk here is the average FTFY equivalent time spent on benefits in 1993 for others in the estimation sample who are full-time insured in the same industry and broad education category in November 1992. This leave-out mean unemployment risk predicts realized unemployment with an R^2 of 0.59 over the 1987-1995 period.

Appendix

A. Sample Definition

Table A1
Sample Selection

Selection criteria	Number of Individuals
Cohorts 1957-1962	452,583
Homeowner in 1992	202,561
Drop if moved in 1992 before reform	195,418
Trim financial outliers	189,021
Balance sample	183,251
Drop if out of labor force	162,139
Drop if self-employed	144,619
Drop if in education	133,785
Drop if insufficient obs. to calc. unemp. risk	133,779
Drop if missing industry code	119,952
Drop if missing labor market experience	118,018
Trim top & bottom 1% of liquidity shock	115,656
Trim top & bottom 1% of permanent income	113,344

Note.—The table describes the number of observations retained in each step of our sample restrictions, as described in the main text.

B. Cohorts Affected by Early Retirement Reforms of the Unemployment Insurance System

Table B1
COHORTS AFFECTED BY EARLY RETIREMENT REFORMS OF THE UNEMPLOYMENT INSURANCE SYSTEM

Cohort	1987	1988	1989	1990	1991	1992	1993	1994	1995	1996	Note
1920	67	68	69	70	71	72	73	74	75	76	They do not need insurance anyway – they can just retire at will if they lose their job
1921	66	67	68	69	70	71	72	73	74	75	
1922	65	66	67	68	69	70	71	72	73	74	
1923	64	65	66	67	68	69	70	71	72	73	
1924	63	64	65	66	67	68	69	70	71	72	
1925	62	63	64	65	66	67	68	69	70	71	
1926	61	62	63	64	65	66	67	68	69	70	
1927	60	61	62	63	64	65	66	67	68	69	
1928	59	60	61	62	63	64	65	66	67	68	
1929	58	59	60	61	62	63	64	65	66	67	
1930	57	58	59	60	61	62	63	64	65	66	
1931	56	57	58	59	60	61	62	63	64	65	
1932	55	56	57	58	59	60	61	62	63	64	
1933	54	55	56	57	58	59	60	61	62	63	Transitional allowance into ER
1934	53	54	55	56	57	58	59	60	61	62	
1935	52	53	54	55	56	57	58	59	60	61	
1936	51	52	53	54	55	56	57	58	59	60	These cohorts need to take up insurance in 1992 if they did not before because they planned to retire at, say 62
1937	50	51	52	53	54	55	56	57	58	59	
1938	49	50	51	52	53	54	55	56	57	58	
1939	48	49	50	51	52	53	54	55	56	57	
1940	47	48	49	50	51	52	53	54	55	56	
1941	46	47	48	49	50	51	52	53	54	55	
1942	45	46	47	48	49	50	51	52	53	54	These cohorts need to take up insurance in 1992 (and keep it until they retire) if they want early retirement at all
1943	44	45	46	47	48	49	50	51	52	53	
1944	43	44	45	46	47	48	49	50	51	52	
1945	42	43	44	45	46	47	48	49	50	51	
1946	41	42	43	44	45	46	47	48	49	50	
1947	40	41	42	43	44	45	46	47	48	49	
1948	39	40	41	42	43	44	45	46	47	48	
1949	38	39	40	41	42	43	44	45	46	47	
1950	37	38	39	40	41	42	43	44	45	46	
1951	36	37	38	39	40	41	42	43	44	45	
1952	35	36	37	38	39	40	41	42	43	44	
1953	34	35	36	37	38	39	40	41	42	43	These cohorts take up insurance as they turn 40 in order to get early retirement
1954	33	34	35	36	37	38	39	40	41	42	
1955	32	33	34	35	36	37	38	39	40	41	
1956	31	32	33	34	35	36	37	38	39	40	
1957	30	31	32	33	34	35	36	37	38	39	No early retirement motive
1958	29	30	31	32	33	34	35	36	37	38	
1959	28	29	30	31	32	33	34	35	36	37	
1960	27	28	29	30	31	32	33	34	35	36	
1961	26	27	28	29	30	31	32	33	34	35	
1962	25	26	27	28	29	30	31	32	33	34	
1963	24	25	26	27	28	29	30	31	32	33	

NOTE.— Ages of different cohorts over the years, and the corresponding early retirement regulations.

C. Robustness to Youngest Cohorts Only

Table C1
IMPACT OF 1992 MORTGAGE REFORM ON UNEMPLOYMENT INSURANCE PARTICIPATION AMONG BIRTH COHORTS 1960-1962 FOR YEARS 1987-1994

	(1) OLS	(2) FE	(3) LDV	(4) FE Logit	(5) LDV Logit
Continuous	-0.663**	-0.585**	-0.274**	-1.550**	-0.122
	(0.127)	(0.126)	(0.0707)	(0.489)	(0.0637)
Observations (C)	421,096	421,096	368,459	52,688	368,336
Individuals (C)	52,637	52,637	52,637	6,586	52,618
Discrete	-0.751**	-0.650**	-0.362**	-1.608*	-0.212*
	(0.183)	(0.181)	(0.100)	(0.825)	(0.103)
Observations (D)	396,840	396,840	347,235	49,952	347,118
Individuals (D)	49,605	49,605	49,605	6,244	49,587

NOTE.—Standard errors clustered by individual in parentheses; * $p<0.05$, ** $p<0.01$. The table collects Average Partial Effects (APE) on insurance up-take computed for the post-1991 subsample, in percentage points, from models of 0-1 unemployment insurance status estimated on the entire 1987-1995 panel. The continuous and discrete specifications correspond to Tables 2 and 3, respectively, restricted to those who had no speculative incentive to join an unemployment insurance fund in order to start collecting early retirement eligibility for age 60. See Section III for details of our sample selection otherwise. The model in the third column adds the lagged insurance status to control for inertia, and the second column shows coefficients from a fixed effect model. The fourth column shows results from a fixed effect logit model, estimated on switchers only. Finally, the model in the fifth column is a discrete choice model with fixed costs of switching, which translates into a lagged dependent variable logit. All models include the same controls used for the estimates shown in Tables 2 and 3.

D. Program Details

Table D1
Supplemental Security Income

	1987	1988	1989	1990	1991	1992	1993	1994[4]	1995	1996
				After tax					Before tax	
Basic amount (per month)										
Single[1]	2579	2649	2728	2728	2796	2852	2909			
Couple	5158	5298	5456	5456	5592	5704	5818			
Breadwinner[2]								8852	8862	9057
Not breadwinner[2]								5546	6652	6803
Below age 23, non-breadwinner, living at home	1325	1361	1833.3	1857.9	1881	1826	1890	2080	2088	
Below age 23, non-breadwinner, not living at home	1847	1897	3000	3040.2	3135	3198	3310	4251	4268	
Below age 25, non-breadwinner, living at home										2138
Below age 25, non-breadwinner, not living at home										4370
Child supplement for kids below age 18 (per year)[3]										
Proportion of normal benefits, oldest/first child	2	2	1.67	1.67	1.67	1.67	1.67			
Proportion of normal benefits, other kids	1.67	1.67	1.67	1.67	1.67	1.67	1.67			
Normal contribution per child	7176	7176	7392	7572	7764	7920	8076			
Amount, first child	14352	14352	12320	12620	12940	13200	13460			
Amount, other children	11960	11960	12320	12620	12940	13200	13460			
Housing supplement	Given on individual basis to cover housing expenses not already subsidized. Default was to subsidize all these expenses									

NOTE.— * Benefit levels are per January 1, every year. Until 1993 benefit levels changed per July each year. After this, they changed January 1. * The basic amounts of benefits are generally lower for young people under the age of 23 (25) until (after) 1995. Yet they received the full "adult" benefit level if they met certain working requirements: Until 1995 individuals under age 23 (21) would receive full benefits if they had worked consecutively for the 3 (12) months preceding unemployment at a standard wage rate (as opposed to the usually lower wage rate of young workers). After 1995 this rule applied to individuals under 25 who worked consecutively for the 12 months preceding unemployment (by April 1, 1995, the requirement became 18 months).

1 After a consecutive period of 9 months the amount decreased by DKK 300-400 (approx. DKK 300 in 1987 and approx. DKK 400 in 1996)

2 Married couples received the sum of their benefits.

3 If the applicant paid child support then the supplement would equal the amount of child support but could not exceed standard child-support contribution. Any child support received would be subtracted in the supplement.

4 From 1994 supplemental security income became taxable and the rules/rates changed (were simplified). Standard rate became 80% of unemployment insurance benefits for breadwinners and 50% for non-breadwinners (changes to 60% in 1995). Young people under age 23 with no breadwinner duties received less.

Sources: The series "Sociale Ydelser", Statistisk Årbog (DSt) samt "Arbejdsmarkedspolitisk Årbog"/"Arbejdsmarkedet og Arbejdsmarkedspolitik"

Table D2

UNEMPLOYMENT BENEFITS AND BASIC MEMBERSHIP FEES

	1987	1988	1989	1990	1991	1992	1993	1994[4]	1995	1996
					Before tax					
Maximum benefits (per day)[1]										
Full time insured	342	354	389	399	409	417	425	509	511	523
Part time insured[2]	228	236	259	266	273	278	283	339	341	349
Recent graduate, full-time insured[3]	271	283	311	319	327	334	340	417	419	429
Recent graduate, part-time insured	181	189	207	213	218	222	226	277	280	286
Conversion										
Day to week	6	6	6	6	6	6	6	5	5	5
Day to year	312	312	312	312	312	312	312	261	261	261
Cost (per year, full-time insurance)	2736	2832	3112	3192	3272	3336	3456	3552	3600	
Bottom bracket MTR	51.0%	51.6%	51.6%	51.5%	51.8%	52.1%	52.2%	47.5%	47.0%	47.1%

NOTE.— * Benefit levels are per January 1, every year. Until 1993, benefit levels changed per July each year. After this, they changed January 1.
1 Benefit levels cannot exceed 90% of the average daily income calculated over the period of 12 weeks preceding the unemployment.
2 Maximum benefits for part time insured amounts to 2/3 of the full time insured benefit level.
3 The special benefit level for new graduates corresponds to 80% of the maximum standard benefits. (From 1994, 82%).
4 In 1994, the benefit period changed — effectively from infinity to 7 years.

Sources: The series “Sociale Ydelser”, Statistisk Årbog (DSt) samt “Arbejdsmarkedspolitisk Årbog”/”Arbejdsmarkedet og Arbejdsmarkedspolitik”