Empirical Monetary-Fiscal Equivalence*

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Abstract: What sequence of stimulus payments induces the same consumption path as a desired (but potentially infeasible) interest rate cut? This mapping depends exclusively on slopes of an aggregate consumption function, recoverable through microeconomic causal variation. Using granular full-population administrative data, we combine spending responses to monetary-policy transmission through mortgage contracts and windfall gains from unanticipated cash inheritances. Matching the consumption response from a 1 percentage point decrease in the monetary-policy rate requires stimulus payments totaling 1.3% of GDP over 5 years. We show that this estimate is robust to accounting for heterogeneity in the cross-sectional incidence of these macro-equivalent policies.

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1 Introduction

Both monetary and fiscal policy can be used for the purpose of business-cycle stabilization. Even though they may operate differently, a mapping between them could characterize to what extent one type of policy could substitute for the other. This becomes all the more relevant when the space for conducting one type of policy is constrained. For the most part of this century, monetary policy was constrained by the persistence of low nominal interest rates, and its ability to stimulate consumption was compromised as was the potency of well-established transmission channels of monetary policy, including the bank lending channel.

Irrespective of the reasons underlying constrained monetary policy—be they political, practical, or due to low rates eroding the space for maneuver—policymakers will seek alternative policy instruments. Early theoretical work focused on time-varying taxes and subsidies, labeled "unconventional fiscal policy" (Correia et al., 2013). Appealingly, the mapping from desired interest rate cut to equivalent unconventional fiscal policy is straightforward: the policymaker simply needs to increase subsidies by the same percentage amount as she would have lowered interest rates. In practice, however, policymakers rarely resort to such fiscal price manipulation. Instead, a much more popular fiscal-stabilization instrument are uniform transfers to households, typically in the form of stimulus checks. While such transfers can theoretically also replicate conventional monetary policy, assuming that they are accompanied by appropriate investment stimuli (Wolf, 2025), the key implementation challenge is the mapping from interest rate cut to equivalent transfer policy: instead of merely replicating post-tax intertemporal prices, the policymaker now needs to figure what dollar amount of checks stimulates private spending just as much as any given desired but infeasible interest rate cut would.

This paper quantifies how interest rate changes translate to equivalent stimulus transfers. Our point of departure is that this relationship is determined solely by the slopes of the aggregate consumption function: to replicate the effects of an interest rate policy, we must identify a stimulus payment policy that generates an identical increase in private-sector net excess demand. These slopes can be recovered through microeconomic causal variation by examining household-level responses. By studying the cross section of households, we can isolate how consumer spending responds to either interest rate changes or income gains independently, without considering any further general equilibrium effects. Such cross-sectional variation—often much maligned because it does not tend to give aggregate-level causal effects (e.g., Wolf, 2023)—is precisely what we need to quantify the mapping between monetary and fiscal policy instruments. This approach allows us to avoid the challenges of directly estimating the general equilibrium effects of transfer policies.

We operationalize our empirical strategy by using granular administrative data from Denmark. First, by considering heterogeneous households with different mortgage-rate reset dates, we can estimate the spending response to interest rate-related cash flow effects—one of the largest components of the direct monetary transmission mechanism to households (e.g., Wong, 2021). Second, by exploiting *unexpected* cash inheritances, we can in the same data estimate the spending responses to one-off lump-sum income gains. Combining the two estimated slopes allows us to construct the desired mapping between policy tools.

Our headline finding is that matching the aggregate consumption response stemming from a 1 percentage point decrease in the monetary-policy rate requires stimulus payments of around $1,013 per person. While macro-equivalent, these two stimulus policies differ materially in their cross-sectional incidence: interest rate policy stimulates spending through high-income mortgage owners, while the direct stimulus effects of checks are more concentrated among the bottom of the income distribution. Translating percentage changes in monetary policy to dollar amounts in fiscal transfers offers a novel method to quantify the practical relevance of MPC distributions. Despite the many heterogeneities, documented in the literature and in our study, across dimensions such as disposable income, liquid wealth, and other household characteristics, our total macro-equivalent stimulus payments are robust to incorporating distributional differences in household responses.

The first step of our empirical analysis uncovers the effect of interest rate changes on consumer spending, i.e., the slope of an aggregate consumption function with respect to interest rate changes. Specifically, we seek to estimate individual household consumption responses to changes in mortgage rates. Our identification leverages institutional details of the Danish mortgage market. In Denmark, more than half of all retail mortgage contracts are adjustable-rate mortgages (ARMs), featuring mortgage-rate resets at different pre-determined intervals. The yield paid by mortgagors on ARMs is closely linked to the policy rate set by the Danish central bank, which in turn follows the policy rate of the European Central Bank. Our empirical strategy looks across borrowers whose mortgage rates are reset at different points in time, but are otherwise plausibly identical. As such, borrowers are similarly exposed to all higher-round general equilibrium effects induced by the monetary intervention.

Relative to prior work, our identification strategy benefits from the granularity and the richness of our administrative data. Using data on households' balance sheets for the general Danish population, we can back out consumption—rather than expenditures for a certain category of goods—at the annual frequency. In addition, an important advantage of our setting is that we can estimate spending responses to a given mortgage-rate change upon reset, rather than considering the average spending response across resets (Di Maggio et al., 2017). This allows us to arrive at a mapping from interest rate to dollar demand space.

We find that a 1 percentage point reduction in the policy rate leads to a contemporaneous increase in net excess consumption demand by 3.6%. This consumption response becomes even larger one year after the mortgage-rate reset, and halves only by the fourth year. We arrive at these numbers by first looking across mortgagors whose mortgage rates reset every three years versus those whose rates reset every five years, and then aggregate across the population of mortgagors by assuming homogeneous responses irrespective of the reset frequency.[1] These aggregated micro consumption responses account for almost one-third of the total aggregate consumption response in general equilibrium estimated through a Structural Vector Autoregression (SVAR) model. This suggests that we capture a substantial portion of the direct effects of monetary policy.

The second step is to map this identified micro-level spending response to a monetary intervention back into stimulus-check space. That is, we need to know how individual households respond to lump-sum windfall income gains. For this purpose, we implement a second empirical design using inheritance shocks stemming from sudden deaths. Using our administrative data, we derive inheritance amounts from the deceased relative's asset portfolio. In contrast to the literature that considers large lottery prizes (Fagereng et al., 2021), or reverts to surveys (Andreolli and Surico, 2021; Colarieti et al., 2024), we vary inheritance amounts by focusing on deposit-only inheritances. Those inheritances are (roughly) similar in size to the stimulus checks required to replicate empirically relevant movements in monetary-policy rates, and unanticipated due to the sudden nature of the deaths.

While we assume, and present evidence, that alternative transmission channels of monetary policy to household consumption—some less direct than changes in mortgage payments, including general equilibrium effects—affect households identically, this is almost surely the case for the inheritance amounts under consideration. This absence of general equilibrium effects safeguards that we trace out the slope of a consumption function in income space. In this manner, we show that average individual-level marginal propensities to consume (MPC) are sizable: 41% in the year of the inheritance and then gradually decreasing over time.

It then remains to combine our two sets of empirical estimates to construct the desired

[1] We furthermore show the robustness of our conclusions with respect to alternative assumptions about across-mortgagor aggregation.

mapping between macro-equivalent policy stimulus tools. At our preferred point estimates, a 1 percentage point nominal interest rate cut could be equivalently synthesized through a total stimulus payment of approximately 6,582 DKK per person (or $1,013), paid out over five years. In the aggregate, such stimulus would correspond to 1.3% of Denmark's GDP in 2018, comparable to the first round of Covid stimulus checks in the U.S. in 2020 (equal to 1.9% of the U.S. GDP).

Our analysis extends beyond aggregate effects to examine heterogeneous consumption responses across households, yielding three key insights. First, monetary and fiscal policy have markedly different distributional impacts. Monetary policy exhibits a regressive pattern, disproportionately stimulating consumption among higher-income households. Although these households show somewhat weaker conditional responses to mortgage-rate resets, their substantially higher likelihood of holding mortgages in the first place dominates this effect. In contrast, fiscal policy demonstrates a progressive pattern, with uniform stimulus payments disproportionately stimulating consumption at the bottom of the income distribution.

Second, accounting for heterogeneous behavior across households bears little relevance for understanding the total partial-equilibrium effect of monetary policy, irrespective of whether we sort households by disposable income, liquid wealth, or alternative measures such as transfer income relative to total income, age, and geographic location. These results validate the premise that macro-equivalent stimulus policies can be reliably estimated without granular accounting of cross-sectional incidence, providing policymakers with a practical framework for calibrating fiscal stimulus as an alternative to monetary policy.

A potential reason why heterogeneity does not matter as much for aggregate inference is the modest variation in MPCs that we uncover across the income and even across the liquid-wealth distribution. Typically, heterogeneity matters most when households that are differently exposed to a policy also have different MPCs (e.g., Auclert, 2019; Bilbiie, 2024). However, we find that average MPCs range only from 38 to 58% across income or liquid-wealth quintiles—significantly smaller differences than calibrated HANK models typically predict (e.g., Kaplan and Violante, 2022). Further, again unlike in calibrated HANK models, we observe non-monotonic MPC patterns along both income and liquid-wealth distributions.

Third, these modest MPC variations substantially limit the potential efficiency gains from targeted stimulus policies. For instance, even the most cost-effective targeting strategy—concentrating payments on the lowest income quintile—would reduce fiscal costs by merely 7% compared to uniform transfers. Similarly, designing transfers to replicate monetary policy's regressive distributional effects would increase costs by only 7%. This pattern extends also to liquid-wealth heterogeneity. This suggests that income and liquid-wealth heterogeneity are of little importance for the effectiveness of targeted fiscal policies and, thus, possibly also for general equilibrium effects, giving credence to our macro-equivalence approach.

Our work relates to several strands of literature. First, we contribute novel empirical measurement to a largely theoretical literature on alternative policy stimulus when interest rates are constrained by a binding lower bound (Correia et al., 2008, 2013; Galí, 2020; Reis and Tenreyro, 2022). For classical unconventional fiscal policy (as defined in Correia et al., 2013), there is no measurement challenge, as both interest rate changes and consumption subsidies induce the exact same movement along an aggregate consumer demand curve, at least in the family of representative-agent macroeconomic models (but see Seidl and Seyrich, 2023, for equivalence conditions in a Heterogeneous Agent New Keynesian (HANK) framework). Measurement becomes a challenge only for the alternative—but arguably more practically relevant—case of mapping to equivalent fiscal transfers (Wolf, 2025).

Second, we add to the literature on heterogeneous effects of monetary and fiscal policy. We provide evidence that while both policies can be equivalent in the aggregate, they induce heterogeneous responses across the income distribution and can, thus, have different effects on inequality. As such, our study naturally relates to a deep literature on measuring household consumption responses to income shocks (e.g., Agarwal and Qian, 2014; Jappelli and Pistaferri, 2014; Commault, 2022), in particular fiscal policies that are targeted at stabilizing consumption in crisis times, such as fiscal stimulus payments (e.g., Johnson et al., 2006).

We also speak to the recent literature about what drives MPC heterogeneity. Our results that MPCs vary with certain observable characteristics, but quantitatively not too much, are in line with previous findings that find only weak or modest roles for observables such as income, liquid wealth, or age in explaining MPC differences (Parker et al., 2013; Broda and Parker, 2014; Jappelli and Pistaferri, 2014, 2020; Ganong et al., 2020; Andreolli and Surico, 2021; Fagereng et al., 2021; Fuster et al., 2021; Bilbiie et al., 2025).

Finally, our estimates shed further light on the forces underlying the transmission of monetary policy, in particular the relative importance of indirect (e.g., through the general equilibrium responses of prices and wages) and direct effects (Ampudia et al., 2018). To the extent that households' exposure to monetary policy through mortgage contracts appropriately represents the direct effects of monetary policy (i.e., the partial equilibrium consequences thereof), our empirical characterization of the demand-equivalence mapping between stimulus payments and the transmission of monetary policy potentially allows for inferring direct from indirect effects and vice versa.

2 Conceptual Background

When nominal interest rates first began to be constrained by a zero (or effective) lower bound, much academic interest centered on the ability of unconventional tax policy—notably consumption subsidies—to replicate conventional monetary-policy stimulus (e.g., Correia et al., 2013). In (U.S.) policy practice, on the other hand, a different stabilization tool has taken center stage: stimulus checks. Recent theoretical work has identified conditions under which such stimulus checks can perfectly replicate monetary-policy transmission through household consumption (Bilbiie et al., 2021; Wolf, 2025). Our objective is to *quantify* the mapping between these conceptually equivalent stimulus tools; that is, we wish to know what *amount* of stimulus checks is required to mimic a given desired (but infeasible) interest rate cut.

In this section, we begin by briefly reviewing relevant parts of the theoretical arguments in Wolf (2025). The key insight will be that to learn about the desired equivalence mapping between policy tools, cross-sectional evidence on spending behavior *between households*—rather than the more conventional aggregate data on consumer spending over time—is sufficient.

A SIMPLE MODEL ENVIRONMENT. For the purposes of our arguments in this paper, it will suffice to consider a simple partial-equilibrium model of household consumption-savings decisions. We consider a unit continuum $i \in [0, 1]$ of ex-ante identical households that consume (c_{it}), earn stochastic labor income (y_{it}) and receive government transfers (τ_t), save and borrow in a nominally risk-free bond (b_{it}) with nominal return i_t^n, and pay interest on their outstanding mortgage ($q_{it} \times \bar{a}$). The consumption-savings problem is as follows:

$$\max_{\{c_{it}, b_{it}\}_{t=0}^{\infty}} \mathbb{E}_0 \left[\sum_{t=0}^{\infty} \beta^t u(c_{it}) \right] \tag{1}$$

subject to

$$c_{it} + b_{it} = y_{it} + \tau_t + \frac{1 + i_{t-1}^n}{1 + \pi_t} b_{it-1} + q_{it}(\{i_{t-\ell}^n, \pi_{t-\ell}\}_{\ell=0}^{\infty})\bar{a}, \quad b_{it} \geq \underline{b}, \tag{2}$$

where π_t denotes inflation. The only non-standard part of this consumption-savings problem is the mortgage term $q_{it}(\{i_{t-\ell}, \pi_{t-\ell}\}_{\ell=0}^{\infty})\bar{a}$. The idea here is that households have a fixed outstanding mortgage $\bar{a}$ (that they cannot adjust), and that total payments on the mortgage at time t are some known function $q_{it}(\bullet)$ of current and past aggregate interest rates. This specification is chosen with an eye towards our empirical setting in Section 3.2. Finally, we let $y_{it} = e_{it} \times y_t$, where y_t is aggregate labor income and e_{it} is individual i's (stochastic) productivity, with $\int_0^1 e_{it} di = 1$.

We assume that households have perfect foresight about sequences of future macroeconomic aggregates, $\{y_t, \tau_t, i_t^n, \pi_t\}_{t=0}^{\infty}$. For each individual household i, the consumption-savings problem (1) maps sequences of these macroeconomic aggregates into a sequence of optimal consumption $\{c_{it}\}_{t=0}^{\infty}$. Aggregating across households i, and using boldface notation for sequences, we thus obtain an aggregate consumption function $\mathcal{C}(\bullet)$, as in Auclert et al. (2024) and Wolf (2025):

$$\boldsymbol{c} \;=\; \mathcal{C}(\boldsymbol{y}, \boldsymbol{\tau}, \boldsymbol{i}^n, \boldsymbol{\pi}). \tag{3}$$

For each input $x \in \{y, \tau, i^n, \pi\}$, we can then define the (infinite-dimensional) derivative map

$$\mathcal{C}_x \;\equiv\; \frac{\partial \mathcal{C}(\bullet)}{\partial \boldsymbol{x}}, \tag{4}$$

where the derivative is evaluated at the model's deterministic steady state, with $x_t = \bar{x}$ for all x and $t = 0, 1, \ldots$.

From interest rate space to transfer space. The basic idea of the policy-instrument equivalence argument in Wolf (2025) is that interest rate policy—the time path $\boldsymbol{i}^n$—and transfer policy—the time path $\boldsymbol{\tau}$—can manipulate aggregate consumption net excess demand "equally flexibly." Formally, given some desired (but perhaps infeasible) interest rate policy $\boldsymbol{i}^n$, the question is whether we can find a time path of transfers $\boldsymbol{\tau}$ that induces the same time path of net excess demand—that is, whether

$$\mathcal{C}_{i^n} \times \boldsymbol{i}^n \;=\; \mathcal{C}_{\tau} \times \boldsymbol{\tau}. \tag{5}$$

Wolf (2025) gives conditions under which we can indeed find such a time path of transfers. We provide direct empirical measurement of this mapping.

By (5), this means learning about the entries of the linear maps $\mathcal{C}_{i^n}$ and $\mathcal{C}_{\tau}$. Note that the entries of these linear maps collect household spending responses to changes in interest rates or transfers *alone*, with no other inputs to the consumption-savings problem changing. As such, they are in principle estimable through *cross-sectional* variation: while the absence of general equilibrium effects in typical cross-regional regressions is typically a problem for the estimation of macroeconomic counterfactuals (see Wolf, 2023), it is actually desirable in this case because our objects of interest—entries of $\mathcal{C}_{i^n}$ and $\mathcal{C}_{\tau}$—are essentially slopes of a demand function.

Our empirical analysis in Section 4 will be directly informative about entries of $\mathcal{C}_\tau$. For $\mathcal{C}_{i^n}$, note that interest rates enter the consumption-savings problem (1) in two places: returns on short-term bonds and payments on outstanding mortgages. In light of the latter's greater empirical relevance, for which we will provide further evidence, we focus on the second component, defined as

$$\mathcal{C}^*_{i^n} \equiv \underbrace{\mathcal{C}_q}_{\text{consumption response}} \times \underbrace{\boldsymbol{q}(\boldsymbol{i_b};\bullet)}_{\text{cash flow effects}} . \tag{6}$$

We will then study the mapping between policies in (5) based on direct empirical evidence on $\mathcal{C}_\tau$ and $\mathcal{C}^*_{i^n}$.

3 Empirical Setup and Data

3.1 Empirical Implementation

In this section, we discuss conceptually how $\mathcal{C}_\tau$ and $\mathcal{C}_{i^n}$ can be estimated empirically, starting with the consumption response to changes in interest rates. In the following, we focus our discussion on the contemporaneous consumption response for ease of exposition (i.e., the first entry in $\mathcal{C}_x$, defined as $\mathcal{C}_x[0]$). The models can easily be amended to estimate dynamic effects. We discuss this in more detail in our identification strategy.

Estimating the *direct* (i.e., partial equilibrium) consumption responses to interest rate changes requires that any factors other than interest rate effects can be controlled for in the empirical design. This is typically not feasible in aggregate data. Instead, cross-sectional variation, i.e., a setting in which only a (random) subset of "treated" individuals are exposed to an interest rate change, is required.

In a first step, the *average* direct consumption response of individuals to interest rate changes is estimated using a reduced-form regression setup akin to:

$$\Delta c_{it}/c_{it-1} = \beta \Delta i^n_{it} + \alpha_t + \varepsilon_{it}, \tag{7}$$

where $\Delta c_{it}/c_{it-1}$ is the consumption growth of individual i from period $t-1$ to t, $\Delta i^n_{it} \equiv d^{i^n}_{it} \times \Delta {i^n}_t$, and α_t denotes time fixed effects. $d^{i^n}_{it}$ is a indicator function equal to one if individual i is exposed to a change in the interest rate in the economy in period t (e.g., because she refinances her outstanding debt at the now prevailing rate while others do not), and zero otherwise. If variation in $d_{it}^{i^n}$ is (as good as) random, β gives the causal average direct consumption response induced by a change in the economy-wide interest rate $\Delta {i^n}_t$.

Next, to obtain an empirical estimate for $\mathcal{C}_{i^n}[0]$, the average effect needs to be aggregated up to yield the whole population's direct consumption response to a variation in the economy-wide interest rate:

$$\mathcal{C}_{i^n}[0] = \beta \sum_{i=1}^{N} \left(d_{it} \times c_{it-1} \right), \tag{8}$$

where N is the total number of individuals in the population. Note that β is scale dependent. If Δi_{it}^n in (7) is measured in percentage points (p.p.), then β gives the average consumption growth response to a 1 p.p. change in the economy-wide interest rate.

Equipped with these estimates, we can ask what fiscal response in the form of (untargeted) stimulus payments is required to induce the same aggregate direct consumption effect as a 1 p.p. change in the interest rate:

$$\mathcal{C}_{i^n}[0] \times 1 \equiv \mathcal{C}_{\tau}[0] \times \tau = \phi \tau \sum_{i=1}^{N} d_{it}^{\tau}, \tag{9}$$

where ϕ is the average marginal propensity to consume (MPC) with respect to τ, and d_{it}^{τ} is an indicator function equal to one if individual i receives a stimulus payment τ in period t, and zero otherwise.

We estimate ϕ as follows, using random variation in d_{it}^{τ}:

$$\Delta c_{it} = \phi \tau_{it} + \alpha_t + \varepsilon_{it}, \tag{10}$$

where $\tau_{it} \equiv d_{it}^{\tau} \times \tau$ and Δc_{it} is the change in consumption from period $t-1$ to t.

Note that d_{it}^{τ} is generally under the control of the fiscal authority, i.e., stimulus payments can be targeted. In our baseline demand-equivalence estimation, however, we assume that τ is not targeted. That is, while we require exogenous variation in τ_{it} when estimating ϕ, we assume that *all* people in the population receive the *same* stimulus payment τ. Given ϕ and assuming $d_{it}^{\tau} = 1$ for all i, we can impute our main object of interest τ such that (9) holds. In additional analyses, we will relax the assumptions that β, ϕ, and d_{it}^{τ} are the same for all i, and explore the effects of heterogeneous MPCs and targeted stimulus payments to certain groups in the economy (e.g., low-income, high-MPC individuals).

This discussion highlights the key challenges associated with estimating the equivalence of fiscal-policy and monetary-policy stimuli. The researcher requires settings with (as good as random) cross-sectional variation in $d_{it}^{i^n}$ and d_{it}^{τ} to estimate β and ϕ. In the next section, we discuss how we utilize rich, full-population administrative microdata to speak to economy-wide effects.

3.2 Identification Strategy

We identify how individual consumption responds to (i) changes in the monetary-policy rate and (ii) windfall gains in order to estimate the counterparts of $\mathcal{C}_{i^n}^*$ and $\mathcal{C}_{\tau}$. While the exact implementation for yielding the average direct consumption responses varies for the two types of shocks, the data sources employed are the same, and so is our main identification strategy.

Monetary-policy transmission through adjustable-rate mortgages. We start out by estimating the effect of monetary-policy transmission to individual consumption by using the rate pass-through of monetary policy to mortgages (similar to Di Maggio et al., 2017; Berger et al., 2021). We exploit mortgage-rate resets for mortgagors at different pre-determined intervals, a prominent feature of so-called adjustable-rate mortgages (ARMs) offered in Denmark.[2] This allows us to compare borrowers who face a mortgage-rate reset in a given year with borrowers who do not, holding constant the mortgage type and any regional economic effects.

An important feature of our identification strategy is that mortgage-rate changes occur at regular fixed intervals as a result of the design of the mortgage contract, and not because borrowers refinance or take other decisions that influence their mortgage rate. The yield mortgage borrowers pay on ARMs is closely linked to the monetary-policy rate of the Danish central bank. However, the policy goal of the Danish central bank is to stabilize the exchange rate towards the euro. This implies that the Danish monetary-policy rate follows the policy rate of the European Central Bank (Andersen et al., 2023), which in turn allows us to use it as a source of exogenous variation in the interest rate paid by Danish mortgagors.

Furthermore, all mortgages are backed by bonds of the same maturity at the respective reset frequency. The price of the bond is the sole determinant of the interest rate on the mortgage loan. This implies that Danish mortgage banks cannot price-discriminate between

[2]We discuss the Danish mortgage market in greater detail in Section B of the Online Appendix.

borrowers and interest payments from borrowers are directly passed on to bondholders.[3] Therefore, our variation in interest rates is plausibly exogenous to mortgage banks' supply considerations.

This enables us to estimate the average direct consumption response of individuals to interest rate changes, as in (7), which in turn allows us to compute $\mathcal{C}^*_{in}$. Using administrative data that we describe in Section 3.3, we estimate the following regression specification:

$$\Delta \ln(c_{it+h}) = \beta \Delta R_{it} + \theta \mathbf{X}_{it} + \delta_{jt} + \varepsilon_{it}, \tag{11}$$

where $\Delta \ln(c_{it+h})$ denotes the log change (i.e., percent change) in consumption of individual/borrower i (in region j) from year $t-1$ to year $t+h$, where $h \in \{0,1,2,3,4\}$, R_{it} is mortgage interest payment in year t over outstanding debt in year $t-1$, so that ΔR_{it} is the change in mortgage payments, scaled by the previous year's outstanding debt, for borrower i from year $t-1$ to year t (in %), $\mathbf{X}_{it}$ is a vector of contemporaneous borrower-level controls, and δ_{jt} denotes region by year fixed effects. We cluster standard errors at the level of the individual.

The coefficient of interest, β, captures the effect of a change in the monetary-policy rate on consumption. Besides controlling for individual borrower characteristics, we include region by year fixed effects, which control for time-varying unobserved heterogeneity at the level of the region where borrower i resides, such as local economic conditions that could affect consumption decisions.

We yield variation in ΔR_{it} by virtue of the design of adjustable-rate mortgages: some borrowers will see their mortgage rate change in a given year t because it is scheduled to be reset that year and the monetary-policy rate has changed compared to the previous mortgage-rate reset, while other borrowers will not see their mortgage rate change in year t. Given the annual frequency of our data, we focus on borrowers with adjustable-rate mortgages who face a mortgage-rate reset at a frequency lower than one year. This leaves us with so-called F3 and F5 mortgage loans, the rates on which are reset every three and five years, respectively. In addition to the fact that ARMs make for more than half of all residential mortgages in Denmark, F3 and F5 mortgage loans make for roughly half of the aggregate loan amount in our sample and can, thus, be considered fairly representative.

[3] Danish mortgage bonds are primarily held by banks, pension funds, life insurers, and foreign entities. Danish households hold only 1.7% of outstanding mortgage bonds (in 2017). When rates fall, households thus save on their debt payments, and only very few households see a drop in their returns on investments in mortgage bonds.

Our empirical identification strategy is similar to that in Di Maggio et al. (2017) in that it relies on the fact that the timing of mortgage-resets depends on the rate-reset schedule and issue year of the mortgage as well as on fluctuations in the monetary-policy rate. This allows us to identify within-borrower variation in consumption responses. Our setting has a number of advantages over that in Di Maggio et al. (2017), mainly due to the richness of the Danish register data that we use, which we describe in detail in Section 3.3. In particular, we observe continuous mortgage-rate changes, which is essential for quantifying the consumption response, whereas Di Maggio et al. (2017) employ a dummy variable for reset years, thereby effectively treating all mortgage-rate changes equally irrespective of their magnitude. This empirical approach generally differs from that in Flodén et al. (2021), who rely on variation in households' leverage choices.[4] Furthermore, given that we have access to the full set of income, wealth, and related information for each individual, we can back out total consumption of the individual, whereas Di Maggio et al. (2017) focus on car purchases.

Windfall gains from inheritance shocks. Based on our results for individual consumption responses to monetary-policy pass-through, we match the estimated monetary-policy stimulus with a path of stimulus payments to households such that the two policies induce the same path of aggregate consumption. To characterize the properties of this equivalent fiscal policy, we use estimates of consumption responses to one-time windfall gains. Given the different frequency compared to our monetary-policy shocks, we need to adjust our empirical strategy accordingly.

Our empirical goal is to estimate the average MPC with respect to a stimulus payment τ in order to ultimately back out $\mathcal{C}_\tau$. As a close substitute for stimulus payments, we use windfall gains stemming from unexpected inheritance events following the sudden death of a close relative (e.g., cardiac arrests or accidents, as in Andersen and Nielsen, 2010). We discuss in detail how sudden inheritance events are identified from family relationships and death records in Section C of the Online Appendix. We zoom in on deposit-only inheritances, which have the advantage of matching the size of typical stimulus payments. Unlike lottery prizes (as in Fagereng et al., 2021), inheritance events affect individuals across the entire population, whereas entering a lottery is more likely to be endogenous to individual characteristics.

Based on the same administrative data for the subset of individuals with at least one in-

[4] Our setting also allows for a sharper identification than the one in Flodén et al. (2021), who assume that all mortgage borrowers are affected by changes in the monetary-policy rate, regardless of whether households have fixed-rate or adjustable-rate mortgages.

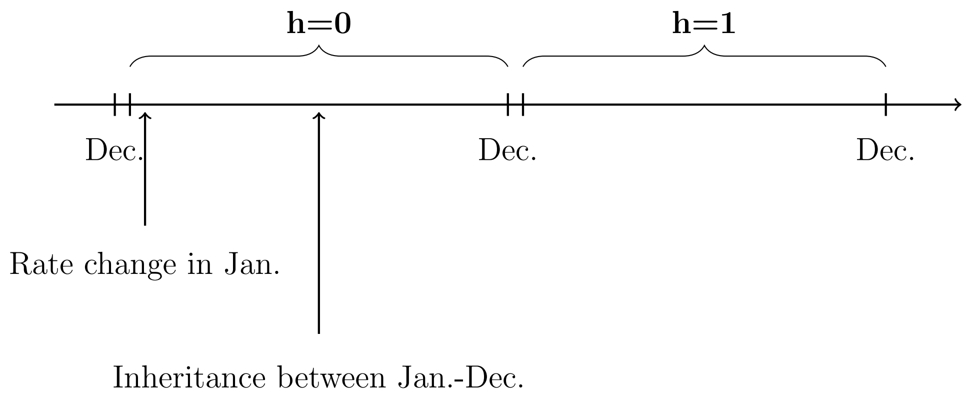

Figure 1: Timing of Mortgage-Rate Changes and Inheritance Events
This graph visualizes the timing of mortgage-rate changes and inheritance events in our data.

heritance event, we estimate MPCs following a similar approach as Fagereng et al. (2021). In particular, we estimate a variant of (10), using as dependent variable individual consumption while including individual fixed effects on the right-hand side:

$$c_{it+h} = \beta Inheritance_{it} + \theta \mathbf{X}_{it} + \mu_i + \delta_{jt} + \varepsilon_{it}, \tag{12}$$

where c_{it+h} denotes consumption of individual i (in region j) in year $t+h$, where $h \in \{0, 1, 2, 3, 4\}$, $Inheritance_{it}$ is the size of the inheritance for a given inheritance event in year t, $\mathbf{X}_{it}$ is a vector of contemporaneous borrower-level controls, and μ_i and δ_{jt} denote, respectively, individual and region by year fixed effects. We cluster standard errors at the level of the individual.

Importantly, we keep person-year observations running up to an inheritance event but—as in Fagereng et al. (2021)—keep only one post-inheritance observation, either that in the same year t or in one of the following years $t+h$, where $h \in \{1, 2, 3, 4\}$. This is to prevent lagged responses from affecting our estimate of β.

Timing. Figure 1 summarizes the timing of mortgage-rate changes and inheritance events in our data. Rates in ARM contracts are typically reset at the end of a calendar year (November or December) and apply from the beginning of the next calendar year. We define the first year after the reset (i.e., the first year in which the new rate is applied) as $h = 0$. Similarly, for inheritance events $h = 0$ refers to the first year in which the heir can plausibly receive an inheritance following the death of a close relative. Given that we impute inheritances from family relationships and death records, $h = 0$ is defined as the year in which the death is recorded (events can be spread out over the entire calendar year). To account for potential concerns about differences in the number of months passing since January of a given year $h = 0$ for inheritance events, we include in our analysis a robustness check in which we keep only inheritances that occur in the first half of a year (Section 6).

We next turn to an in-depth description of our data and their features. In doing so, we will also discuss further details regarding the empirical implementation.

3.3 Data Description

As our data foundation, we use individual-level data, at the annual frequency from 2003 to 2020, from Statistics Denmark, which cover the entire adult (above the age of 18) Danish population. These data comprise a full breakdown of households' balance sheets. As such, we have access to a host of individual variables, such as age, gender, the level of education, disposable income, wealth, the value of property, etc. In particular, we use some of these variables to impute consumption following the procedure originally developed by Browning and Leth-Petersen (2003) and Leth-Petersen (2010), and subsequently adopted in a number of papers on the role of mortgages for households (Andersen et al., 2016; Larsen et al., 2020; Flodén et al., 2021).[5]

The imputation starts from the premise that consumption of an individual equals the difference between income and net savings. We empirically proxy for net savings by the change in assets minus the change in debt, i.e.,

$$c_{it} = y_{it} - (\Delta a_{it} - \Delta d_{it}), \tag{13}$$

where c_{it} is consumption, y_{it} disposable income, Δa_{it} the change in assets, and Δd_{it} the change in debt of individual i in year t. We disregard years when a housing transaction takes place as this causes large jumps in imputed consumption. We also disregard stock and bond holdings from households' liquid assets as asset-market fluctuations may lead to excessively volatile imputed consumption. Furthermore, we exclude person-year observations for which imputed consumption is negative or the absolute year-on-year consumption growth is larger than 50% (following Flodén et al., 2021), as well as households with self-employed

[5]In contrast to survey data that are available for a small subsample of the population only, imputed consumption can be calculated for the total population. Browning and Leth-Petersen (2003) find that imputed consumption represents households' self-reported total expenditures well. Comparing Swedish consumption survey data with imputed consumption data, Koijen et al. (2015) argue that imputed consumption is superior to consumption surveys as surveys are often plagued by reporting errors.

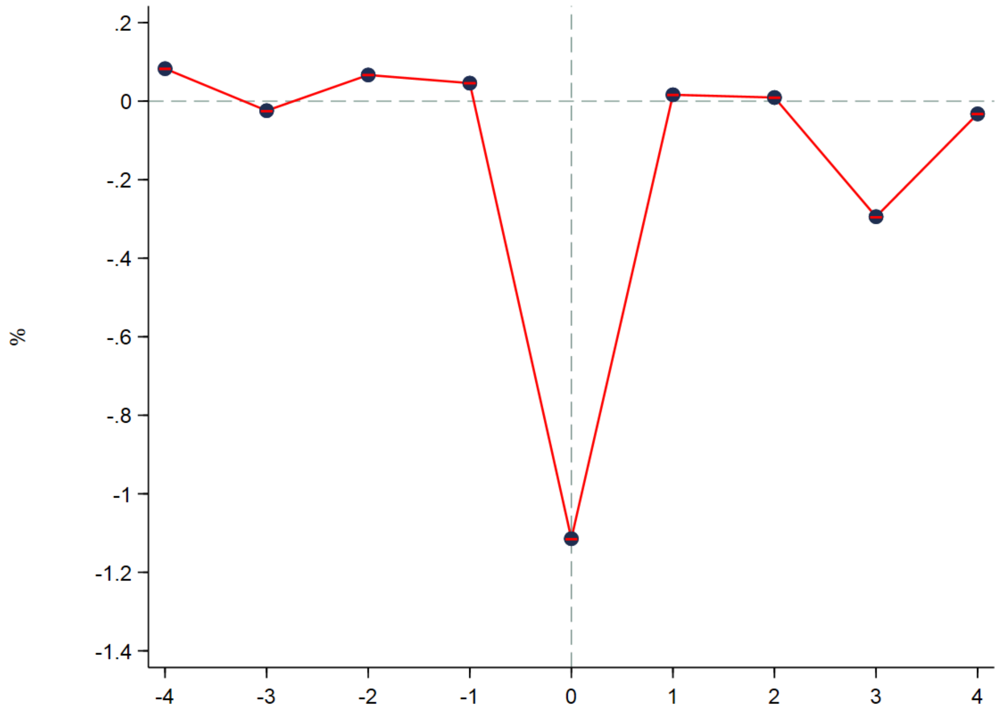

Figure 2: Interest Rate Changes around Mortgage-Reset Events
The figure plots the coefficient estimates for β over time from the following regression:

$$\Delta y_{it} = \alpha_t + \beta_1 D_{it}^{t-4} + \beta_2 D_{it}^{t-2} + ... + \beta_7 D_{it}^{t+4} + \epsilon_{it},$$

where D_{it}^{t+h} is an indicator variable equal to one in year $t+h$ before/after a reset event for individual i ($t-1$ is the omitted category), Δy_{it} is the change in mortgage payments, scaled by the previous year's outstanding debt, for borrower i from year $t-1$ to year t, and α_t denotes year fixed effects. As a person may experience more than one reset, we only use the largest reset for each individual in this analysis.

individuals as their income is unstable and subject to notable uncertainty (see Browning and Leth-Petersen, 2003). Finally, we winsorize consumption and all individual-level control variables (such as house value, income, wealth, etc.) at the top and bottom 0.5% of their respective distributions.

We present summary statistics (at the individual-year level) for the entire Danish adult population from 2003 to 2016 in Panel A of Table 1, which reflects the sample period of our inheritance data (we additionally use four more years of individual-level data, e.g., to measure consumption responses). In the last few rows, we consider the proportion of mortgagors for our first set of tests using the pass-through of monetary policy through adjustable-rate mortgages. For this purpose, we bring in mortgage-type data, which are available from Statistics Denmark for all mortgagors from 2009 to 2018. For each individual, we know the number of mortgages and the mortgage characteristics, such as the original principal, the outstanding principal, maturity of the loan, loan-to-value ratio, etc. If some individuals have a joint loan (e.g., a couple sharing their loan), the outstanding amount and interest payment are equally divided among the borrowers. Most importantly for our setting, we also know the mortgage type, i.e., whether the mortgage is a fixed-rate mortgage or an adjustable-rate mortgage (along with the frequency of the mortgage-rate resets), as well as the annual interest payments on the mortgage. We calculate for each individual i with an ARM mortgage the percentage interest payment as:[6]

$$R_{it} = \text{Interest payment on variable loans}_{it}/\text{Outstanding variable debt}_{it-1}. \qquad (14)$$

Using these data, Figure 2 confirms that we correctly measure the timing of the mortgage-rate reset, which on average has an accommodative effect for the mortgagors and amounts to a bit more than 1 percentage point (of the previous year's outstanding debt).

Panel B of Table 1 shows summary statistics for our samples of F3 and F5 borrowers that see their mortgage rates reset every three and five years, respectively, exemplarily for the last year of the respective dataset (2018).[7] We have 187,332 F3 borrowers and 421,092 F5 borrowers in 2018.[8] F3 and F5 borrowers are to a large extent similar, i.e., they are around 52 years old on average, 37-38% have a higher education, 52-54% are male, 70-71% are married, and 24% have kids living at home. Disposable income, bank deposits, and the value of real estate are all comparable, and range from 200,000 to 400,000 DKK ($\approx \$31,000$ to $\$62,000$) per individual. So is liquid wealth, which we define as bank deposits minus bank liabilities plus investments in stocks and bonds.

The average loan amount is around 1,000,000 DKK ($\approx \$154,000$) per individual. The loan amounts are somewhat skewed, as the mean is noticeably larger than the median. Note that using data from Statistics Denmark confines us to reporting the pseudo median, i.e., the arithmetic mean of the five observations around the actual median. The average interest payment, measured as a fraction of outstanding debt, is 1.5% for F3 loans and 3.9% for F5 loans, reflecting both an upward-sloping yield curve, with the three-year yield being lower than the five-year yield, and the fact that yields have fallen during our sample period.

[6]We drop observations where the change in outstanding debt is more than 10% over a year, as this cannot be explained by regular annual payments but should, instead, be interpreted as an extraordinary instance of paying down debt, or adding debt.

[7]These two groups make for more than half of all ARM borrowers in 2018. For a detailed overview of how the proportion of different types of borrowers has evolved during our sample period, see Table A.1 in the Online Appendix.

[8]Some individuals have both types of mortgages.

Furthermore, the fact that F3 and F5 borrowers are very similar along many relevant demographic and economic dimensions—including their income composition—also suggests that they are unlikely to be differentially affected by variations in the monetary-policy rate through channels other than their different reset dates. For instance, both groups have a similar proportion of liquid to total wealth (14% vs. 19%), which has been shown to matter for the transmission of monetary policy to household consumption across the distribution (e.g., Holm et al., 2021).

Due to our data restrictions, we end up with consumption data for at most 152,220 (336,690) F3 (F5) borrowers. Compared to Flodén et al. (2021), which is the paper with data closest to ours, using register-based individual-level data from a Nordic country (Sweden), we retain a large fraction of individuals in our final sample. Flodén et al. (2021) end up using only 67,425 observations out of their full sample of 2,434,359 observations, i.e., only approximately 3% of their full sample. In contrast, we use around 80% of the sample in our merged dataset.

Consumption on average equals around 270,000 DKK ($\approx \$42,000$). Aggregate private consumption from the national accounts amounted to 1,027bn DKK in 2018. In 2018, there were 4.58m Danes above the age of 18, implying an average consumption of 224,236 DKK. This difference reflects that owners of real estate are typically wealthier and, thus, have higher consumption as well. Overall, our imputed consumption levels seem to correspond well to those from the aggregate national accounts.

For our inheritance shock, we can cover a longer time period, from 2003 to 2016. Out of the entire adult population, about 41,000 have at least one sudden inheritance event during our sample period, of which 28,000 constitute inheritance events not involving any housing assets and 16,000 are deposit-only inheritances. The latter are inheritance events where a person only inherits liquid bank deposits and stock investments from the deceased (i.e., no house or other assets). Following Andersen and Nielsen (2010), we infer inheritance amounts from observed deaths of relatives and the latter's asset portfolio.[9]

In Panel C of Table 1, we present summary statistics for the inheritance events, separately for the type of inheritance, and the characteristics of the respective heirs. Note that we report values from the last year prior to an inheritance event.[10] Deposit-only inheritances

[9] Section C of the Online Appendix expands on the treatment of the inheritance data.

[10] As a consequence, the number of observations in Panel C of Table 1 is somewhat smaller than the total number of inheritance events as no pre-inheritance information is available for events in the first year of our sample period.

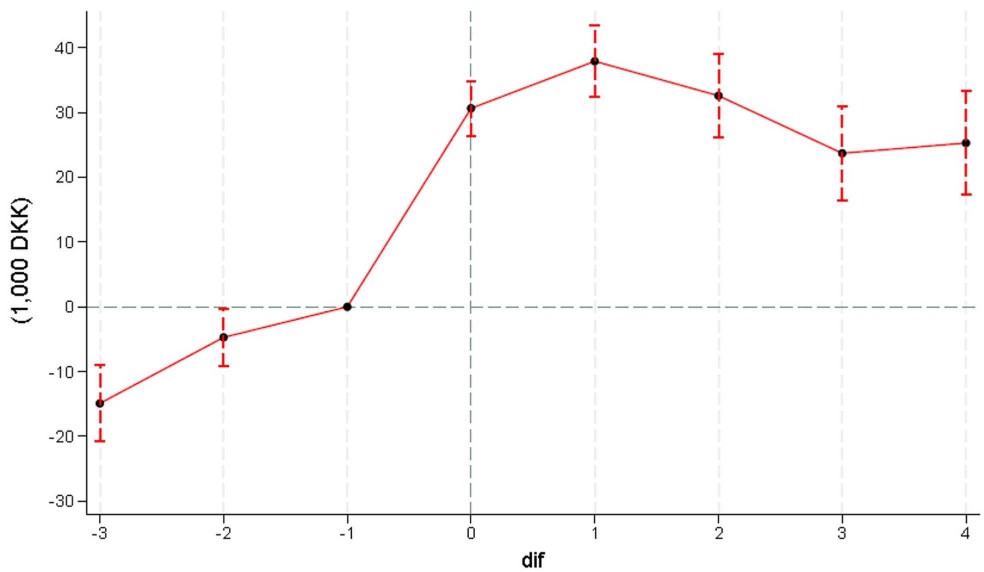

Figure 3: Total Wealth Changes around Deposit-Only Inheritance Events
The figure plots the coefficient estimates for β, alongside 99% confidence intervals, over time from the following regression:

$$Wealth_{it} = \alpha_i + \alpha_t + \beta_1 D_{it}^{t-3} + \beta_2 D_{it}^{t-2} + ... + \beta_7 D_{it}^{t+4} + \epsilon_{it},$$

where D_{it}^{t+h} is an indicator variable equal to one in year $t+h$ before/after an inheritance event for individual i ($t-1$ is the omitted category), $Wealth_{it}$ is the wealth of individual i (in thousands of DKK) in year t, and α_i and α_t denote individual and year fixed effects, respectively. The sample comprises all individuals with deposit-only inheritances.

(in the right panel) are smaller compared to the overall average, with a median of 31,000 DKK (about \$4,800) and a mean of 122,000 DKK (about \$18,800). The summary statistics for the characteristics of heirs by inheritance type are measured in the last pre-inheritance year, and indicate that wealth is somewhat lower for the deposit-only group. However, the heirs are fairly similar across groups in terms of many other characteristics, such as their disposable income or their consumption.

Regarding the measurement of the latter, one faces additional challenges. Using the approximation (13) leads to a mechanical underestimation of consumption in years with inheritances, as inheritances indirectly show up in the change in wealth (the part that is not consumed immediately) but not as income. This is compounded by the possibility of delayed recognition of some inheritances. To address these issues, we add back the inheritance value to (13) in the year of the inheritance. By focusing on deposit-only inheritances (i.e., involving no transfer of a house or other financial assets), which are most likely to be recognized immediately, we make sure that this adjustment is necessary only for the year of the inheritance. Finally, the fact that deposit-only heirs are broadly similar in terms of observables lends support to the idea that deposit-only inheritances are representative while enabling us to match the size of feasible stimulus payments.

Figure 3 confirms that deposit-only inheritances are realized instantaneously and, on average, hover around 30,000 DKK. This for one reflects a lower wealth increase than the actual deposit amount (on average 85,000 DKK, as mentioned above) and, more importantly, attests to the idea that there is substantial consumption out of this inheritance shock.

4 Monetary and Fiscal Results

4.1 Estimating $\mathcal{C}_\tau$ and $\mathcal{C}_{i^n}$

Monetary-policy transmission through adjustable-rate mortgages. In column 1 of Table 2, Panel A, we estimate regression specification (11) without borrower-level controls. Adding them in column 2 leaves our estimate for the contemporaneous response of consumption to a change in the mortgage rate virtually unaltered: a 1 p.p. increase (reduction) in the mortgage rate decreases (increases) consumption by 3.6%. In the remaining columns, we estimate dynamic effects on consumption one to four years after the mortgage-rate reset, and document a hump-shaped evolution of our estimate.[11] While the consumption response increases further one year later (column 3), it drops well below the magnitude of the contemporaneous response after year 2, eventually reaching 1.7% in year 4 (last column).[12]

For our identification, we assume that among F3 and F5 borrowers, those without a reset in a given year emulate the counterfactual consumption response of borrowers experiencing a mortgage-rate reset. One way to give credence to this assumption is to show the lack of pre-trends in consumption prior to such mortgage-rate resets. As F3 borrowers face such a reset every three years and because we have seen that the consumption response is relatively long lived, we restrict our sample to individuals that have at least one F5 loan outstanding as of 2009, and consider their consumption response in the two years prior to a reset. Furthermore, to make sure that we capture the effect of an accommodative mortgage-rate reset, we focus on the subset of F5 borrowers whose debt was issued between 2005 and 2009, i.e., during a

[11]Di Maggio et al. (2017) uncover similar dynamics for U.S. borrowers' new-car spending.

[12]In Table A.3, Panel A, of the Online Appendix, we document furthermore that our estimated consumption responses would be even stronger if we used all available observations for imputed consumption, instead of dropping those that are negative or that imply absolute year-on-year consumption growth in excess of 50% (following Flodén et al., 2021).

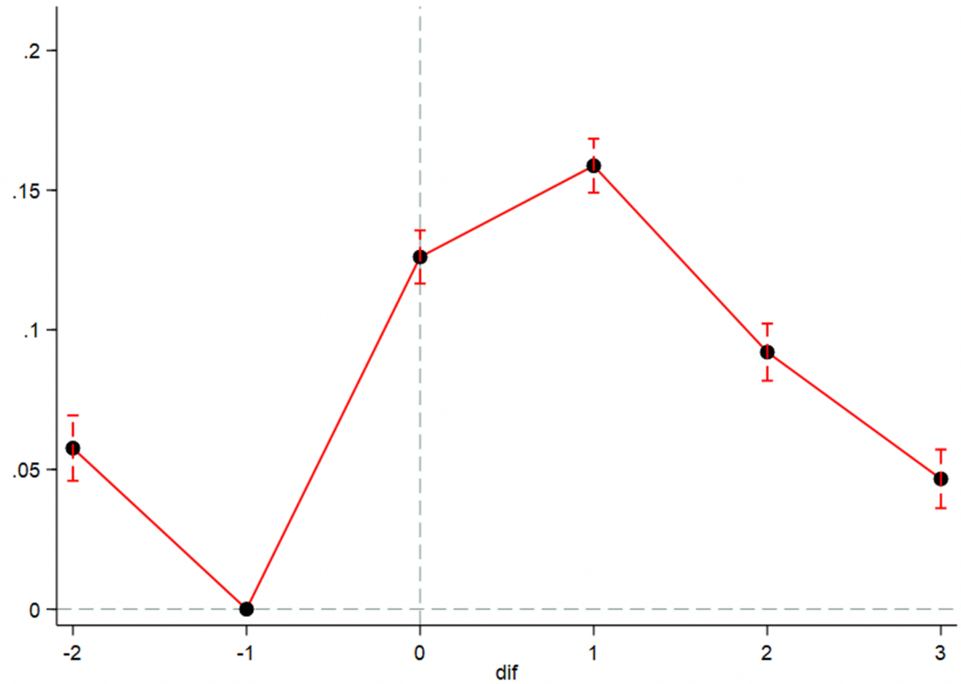

Figure 4: Consumption Response to Changes in Mortgage Payments—Pre-Trends
This figure plots the coefficient estimates for β, alongside 99% confidence intervals, from the following regression:

$$\ln(c_{it}) = \alpha_i + \alpha_t + \beta_1 D_{it}^{t-2} + \beta_2 D_{it}^{t-0} + \beta_3 D_{it}^{t+1} + \beta_4 D_{it}^{t+2} + \beta_5 D_{it}^{t+3} + \epsilon_{it},$$

where D_{it}^{t+h} is an indicator variable equal to one in year $t+h$ before/after the first reset event for F5 borrower i ($t-1$ is the omitted category), c_{it} is consumption (winsorized at the 0.5% and 99.5% levels) of individual i in year t, and α_i and α_t denote individual and year fixed effects, respectively. The sample comprises all individuals that have at least one F5 loan outstanding as of 2009 that was issued between $2005-2009$, i.e., during a high interest-rate period.

high interest-rate period. The associated rate resets are large and amount to 2 to 3% of the previous year's outstanding debt.

Our year-by-year estimates in Figure 4 reveal a strong consumption response only starting in the year of the mortgage-rate reset, but not in the previous year (the reference year). There is a relatively weak (positive) response in year $t-2$ in spite of the contractionary monetary policy in place, which is similar in magnitude to the consumption response in year $t+4$ after the reset and statistically significant due to the high statistical power of our tests. Finally, after taking into account the size of the rate reset ($\Delta R_{it} \approx 2-3\%$) the consumption response in the reset year t is similar to our baseline estimates in Table 2.

These estimates imply that there is a strong and persistent transmission of monetary-policy rates to individual consumption through adjustable-rate mortgages in Denmark. The consumption response is persistent because so is the effect on disposable income from a one-time change in the interest rate: mortgagors benefit from a lower interest rate at least during the subsequent three years for F3 borrowers, and accordingly for five years for F5 borrowers.

Windfall gains from inheritance shocks. We next turn to our results for consumption responses to one-time windfall gains. In columns 1 and 2 of Table 2, Panel B, we estimate regression specification (12) without and with borrower-level controls, respectively, for the sample of individuals with deposit-only inheritances. Our estimate of β is robust to doing so, and implies an MPC of 41.1% to 44.9% in the year of the inheritance. In column 3, we estimate this MPC to drop to 29.6% one year after the inheritance. Thereafter (columns 4 to 6), the response shrinks further until it reaches 6.5% in $t + 4$.[13]

The MPC estimates following deposit-only inheritances are very similar to the evidence reported by Fagereng et al. (2021) for lottery winnings in Norway. They find an average MPC of around 50% in the year of the lottery winning, which decays quickly over the next 3 years.

4.2 Aggregated Consumption Response to Monetary Policy

In this section, we discuss our main baseline result. Specifically, we are interested in the sequence of stimulus payments that induces the same aggregate consumption response as a 1 p.p. decrease in the monetary-policy rate. The first step is to compute the aggregate change in consumption stemming from the transmission of monetary policy.

Aggregated consumption response to a 1 p.p. monetary-policy rate decrease. To compute the aggregate change in consumption stemming from the transmission of monetary policy, we assume, first, that only the consumption of F-loan borrowers is affected and, second, that the consumption response to a change in mortgage payments is the same for all F-loan borrowers. At least when comparing F3 and F5 borrowers in Panel B of Table 1, we fail to find any notable differences between the two groups.

We then proceed as follows. We calculate the level of consumption for each F-loan borrower during our $2009 - 2018$ sample period (or set it equal to the median value of the respective year if missing). From this we calculate the increase in consumption in absolute

[13]In Table A.5, Panel A, of the Online Appendix, we show that our estimates are robust to including all observations for individual consumption.

terms (in DKK) at $t, t+1, t+2, t+3$, and $t+4$ for each F-loan borrower who experiences a change in their mortgage rate. While F1 borrowers face mortgage-rate changes every year, F3 and F5 borrowers face mortgage-rate changes every third and fifth year, respectively. Put differently, on average, one-third of F3 borrowers and one-fifth of F5 borrowers face a change in their mortgage rate the year the monetary-policy rate changes. We apply the same logic to all remaining mortgagors.

We use our estimates (accounting for control variables) in Table 2, Panel A, and compute each F-loan borrower's increase in consumption in year t by multiplying the respective estimate with each individual's consumption in the previous year $t-1$. That is, if a borrower in a given F-loan category has a level of individual consumption of DKK 200,000 in year $t-1$, we assume that she increases her consumption by DKK 7,200 ($= 3.6\% \times 200,000$) in response to a 1 p.p. drop in the monetary-policy rate in year t, and by another DKK 8,000 ($= 4.0\% \times 200,000$) in year $t+1$, etc.). Then, we aggregate these consumption responses in DKK across individuals for each F-loan category.

We summarize our results in Table 3. In doing so, we fix $t = 2018$, the end of our sample period.[14] The first column shows the different types of F-loans available in Denmark, and the second and third column the absolute number and overall fraction of people holding them in 2018. The five columns to the right show the aggregate change in consumption following a 1 p.p. drop in the monetary-policy rate for each group at time t, $t+1$, $t+2$, $t+3$, and $t+4$. For instance, F0.25 borrowers increase their consumption by 0.41bn DKK during the year following a change in the monetary-policy rate, whereas F3 borrowers increase their consumption by 0.57bn DKK.[15] Summarizing these consumption responses across all F-loan borrowers, the bottom panel of Table 3 presents the time series for changes in aggregate consumption, which amount to 5.60bn DKK in year t, 6.22bn DKK in year $t+1$, etc. We will target these aggregate consumption responses when computing equivalent stimulus payments in the respective years.

The total level of consumption across all individuals (above the age of 18) in Denmark in 2017, i.e., not only across F-loan borrowers, amounts to 952bn DKK. Based on this, we calculate the percent change in aggregate consumption following a monetary-policy shock of 1 p.p. in the last row. Aggregate consumption increases by 0.6% in period t. Aggregate

[14]Our results are close to invariant to considering any year from $2015 - 2018$.

[15]Note that F0.5 borrowers are affected each year. In fact, they are affected twice a year (and F0.25 borrowers four times a year). In Table 3, we assume a monetary-policy shock of 1 p.p. materializes at most once a year.

consumption increases by 0.7% at $t+1$, by 0.4% at $t+2$, and by 0.4% at $t+3$. At $t+4$, consumption is still 0.3% higher than before the change in the monetary-policy rate, reflecting the persistence of the effects.

COMPARISON TO AGGREGATE CONSUMPTION RESPONSE TO MONETARY-POLICY SHOCKS IN DENMARK. Our identification strategy is geared towards allowing us to estimate the slope of an aggregate consumption function in income space. In the following, we estimate aggregation consumption responses to monetary-policy shocks in Denmark to gauge the relative importance of alternative transmission channels of monetary policy to household consumption, including indirect general equilibrium effects, which are differenced out in our identification strategy.

For this purpose, we collect data from FRED between 1995 and 2020 directly at the quarterly frequency for i) 3-Month Rates and Yields: Interbank Rates, not seasonally adjusted; ii) Private Final Consumption Expenditure, seasonally adjusted; iii) Real Gross Domestic Product, seasonally adjusted; iv) Consumer Price Index, not seasonally adjusted. All variables, except for interest rates, are log-transformed. For our monetary-policy shock, we use the long-standing currency peg as a source of exogenous variation in monetary policy in Denmark to the euro (as in Di Giovanni et al., 2009; Jordà et al., 2020; Andersen et al., 2023; Gabriel, 2023). In particular, we follow the "Trilemma IV" approach of Jordà et al. (2020) in an "internal-instrument" recursive SVAR setting (Plagborg-Møller and Wolf, 2021).

We use a specification with CPI and GDP (both for Denmark and the euro area), real consumption, and the short-term interest rate. We also include real stock prices for Denmark as a robustness exercise to have a forward-looking variable, yielding similar results. The SVAR is estimated in levels with four lags.[16] Figure 5 shows the impulse responses of the interest rate, real consumption, and GDP to a 1 p.p. increase in the monetary-policy rate using the Trilemma IV as our instrument. Consumption and GDP both decrease significantly in response to a monetary-policy shock with a lag and reach their strongest response between four and five quarters after the shock, with the biggest drop up to 3% for both GDP and consumption, converging afterwards toward zero in the long run (consistent with the estimates in Jordà et al., 2020). In Section D of the Online Appendix, we show that the impulse responses are robust to different identification strategies in Denmark, when using the classic zero short-run restriction or a panel local projections instrumental variable (Panel LP-IV) approach (Jordà, 2005; Stock and Watson, 2018).

[16] We provide further details in Section D of the Online Appendix.

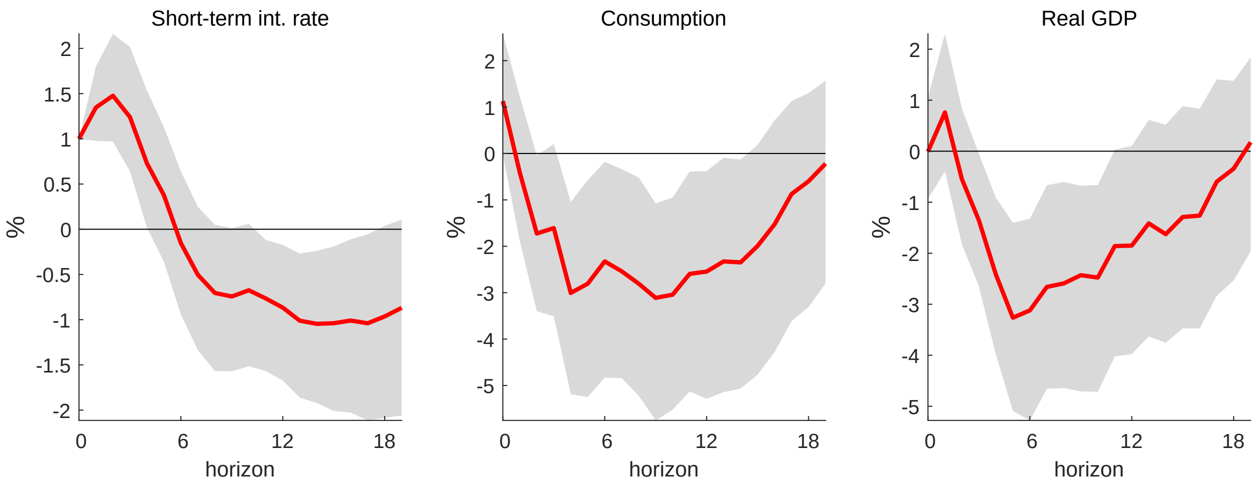

Figure 5: Response to a Monetary-Policy Shock—Trilemma IV in SVAR
This figure plots the impulse responses of real consumption, real GDP, and CPI to a 1 p.p. increase in interest rates. The responses are estimated according to equation (D.1), using the internal-instrument SVAR. The 90% bootstrapped confidence bands are obtained through 1,000 replications using the "wild bootstrap" of Gonçalves and Kilian (2004).

Focusing on the SVAR estimation results, the consumption response on impact and over the course of the first year is stable and around $2-3\%$.[17] This allows for a comparison with the change in aggregate consumption based on identified micro-level spending responses to a 1 p.p. decrease in the monetary-policy rate due to mortgage-rate resets. The cumulative response is 0.7% in $t+1$ (cf. Table 3), implying that the direct effect of nominal interest rate changes on consumer spending accounts for up to one-third of the total aggregate consumption response. This lends support to the idea that we capture a significant portion of the direct transmission of monetary policy to Danish households' consumption, and that heterogeneity in households' exposure through adjustable-rate vs. fixed-rate mortgages is a key determinant of the relative strength of direct effects (Ampudia et al., 2018; Pica, 2023).

4.3 Demand Equivalence

Equipped with the estimates from Table 3, we can now back out the sequence of stimulus payments that would induce the same aggregate consumption response as a 1 p.p. decrease in the monetary-policy rate. We summarize our results in Table 4. We start with the (per-person) average consumption response. At time t, this equals 1,223 DKK (total consumption

[17] Our focus on the response on or shortly after impact can also be justified by the typically growing noise and confidence bands for longer horizons.

response of 5.60bn DKK divided by the adult population size in 2018 of around 4.6 million. The estimates for $t+1$ to $t+4$ are calculated accordingly.

Next, we can ask what sequence of stimulus payments is required to induce the same (per-person) consumption response. To this end, we require the dynamic MPC estimates in response to lump-sum payments as estimated in Table 2, Panel B. Given an MPC of 41.1% (column 2), a payment of $(1,223/0.411 =)$ 2,975 DKK is required in t to induce a consumption response of 1,223 DKK. The stimulus payments for $t+1$ to $t+4$ are calculated accordingly. Note that in the calculation of the dynamic payments, a delayed consumption response for stimulus payments from prior years is taken into account. For example, of the payment of 2,975 DKK in t, 41.1%, 29.6%, 6.7%, etc. is left after consumption over the subsequent 0, 1, 2, etc. years.

Overall, abstracting from discounting, a total stimulus payment of 6,582 DKK ($\approx \$1,013$) paid out over five years per person is required to induce the same aggregate consumption path as a 1 p.p. reduction in the monetary-policy rate.[18] To put this estimate in perspective, if this amount was paid out to the Danish population, the aggregate stimulus payment would add up to $(4,580,547 \times 6,582$ DKK=) 30.15bn DKK or 1.3% of Denmark's GDP in 2018. The total payment in t would already amount to 0.6%. For comparison, the first round of coronavirus stimulus checks in the U.S. in 2020 amounted to $\approx$ \$388bn, corresponding to 1.9% of the U.S. GDP that year.[19]

5 Distributional Non-Equivalence and Heterogeneity (Irrelevance)

Building on our main demand-equivalence result, we next show that while households react heterogeneously to monetary and fiscal policy, accounting for these differences seems to be of little importance to understand the aggregate impact of both policies. We present the distributional impact and the heterogeneity-irrelevance result along the income and the liquid-wealth distributions as these are key dimensions of heterogeneity in the Heterogeneous Agent New Keynesian (HANK) literature (e.g., Kaplan et al., 2018a; Auclert et al., 2024).

[18] As over three-fifths of the total stimulus payment is paid out in the first two years, this also suggests a limited role for discounting.

[19] See https://www.cbs58.com/news/6-trillion-stimulus-heres-who-got-relief-money-so-far.

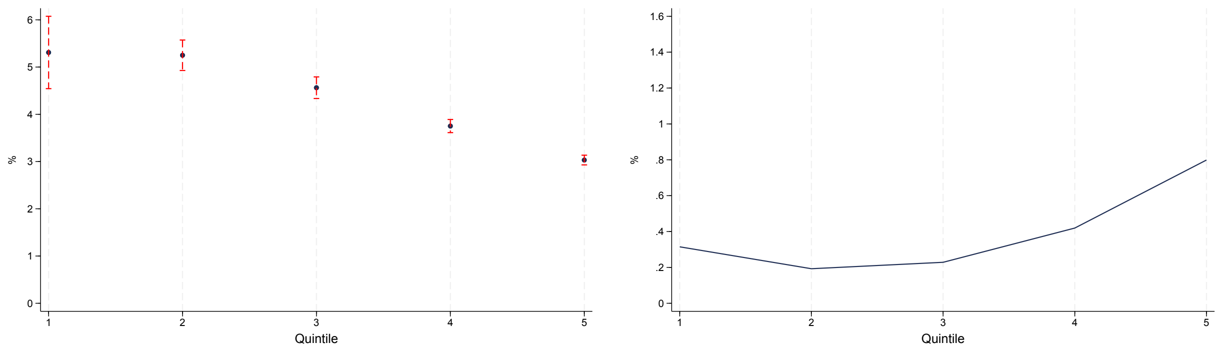

Figure 6: Monetary-Policy Heterogeneity—Disposable Income
The left panel plots the estimates from the contemporaneous consumption-response regression in column 1 of Table 2, Panel A, separately for each disposable-income quintile, alongside 99% confidence intervals. The right panel plots the unconditional partial-equilibrium response to a 1 p.p. decrease in the interest rate for the disposable-income quintiles, taking into consideration their behavior as well as their exposure to monetary policy.

5.1 Cross-Sectional Consumption Response to Monetary Policy

In our baseline estimates, we pool across all adjustable-rate mortgage holders to maximize the statistical power when estimating the average consumption response. This, however, implies that when computing the *aggregate* consumption response to monetary policy, we need to assume that conditional on experiencing a mortgage-rate adjustment, all households increase their consumption by the same percentage points. This approach might, however, mask substantial heterogeneity in behavior across households. Uncovering potential heterogeneous responses helps to understand the partial-equilibrium redistribution effect of monetary policy, and how accounting for them could also affect inference about the aggregate response.

Consumption response to monetary policy along the income distribution. We revisit our estimation of consumption responses to monetary policy and the computation of demand-equivalent stimulus payments across the disposable-income distribution. We start out by re-estimating our baseline consumption responses to changes in mortgage payments (Table 2, Panel A) *separately* for each disposable-income quintile.[20] The left panel of Figure 6 plots the respective results for the contemporaneous consumption responses. It uncovers significant heterogeneity in the behavior conditional on experiencing mortgage-rate adjustments. In particular, the consumption response is monotonously downward sloping in

[20]We use the disposable-income distribution across the entire population, not only within the subset of mortgagors.

income: while households in the lowest disposable-income quintile increase their consumption by more than 5% on average, the highest-income households increase their consumption only by about 3% on average.

To understand the unconditional heterogeneous response to a change in monetary policy along the income distribution, we also need to account for the unequal exposure to mortgage-rate changes. Table 5 shows the share of households with an adjustable-rate mortgage as well as the average reset times along the income distribution. Conditional on having a mortgage, disposable income is uncorrelated with the use of ARMs (column 2), which further validates our empirical identification of the respective consumption response. This, in turn, leads to similar average reset frequencies across groups (column 3).

In contrast, the shares of mortgage holders are highly unevenly distributed across the income distribution. Higher-income households are much more likely to hold a mortgage and are, thus, more exposed to monetary policy. Only between 6% and 9% of households in the lowest three income quintiles have adjustable-rate mortgages, compared to 21% in the fourth quintile and 47% in the fifth quintile. As a result, the consumption responses to monetary policy are highly regressive. The right panel of Figure 6 depicts the on-impact average consumption response to a 1 p.p. decrease in the monetary-policy rate across the income distribution, highlighting that consumption increases disproportionately among richer households.[21]

Consumption response to monetary policy along the liquid-wealth distribution. In standard HANK models, liquid wealth is a key predictor of households' consumption behavior and their MPCs (see, for example, Kaplan and Violante, 2022). Against this background, we repeat our analysis by sorting households according to their liquid wealth rather than disposable income. The left panel of Figure 7 displays the consumption response, conditional on experiencing a mortgage-rate reset, for each liquid-wealth quintile. There is considerable heterogeneity in behavior across the liquid-wealth distribution. Notably, the relationship is hump-shaped: households in the bottom and top quintiles of disposable income exhibit the weakest response, while those in the third quintile display the strongest average response in terms of percentage changes.

Table 6 shows the exposure to mortgage-rate adjustments across these quintiles, with the

[21] In Section E.1 of the Online Appendix, we show that aggregate inference is also not affected when we group households in terms of transfer to total income instead of disposable income to break the strong correlation between income and mortgage exposure.

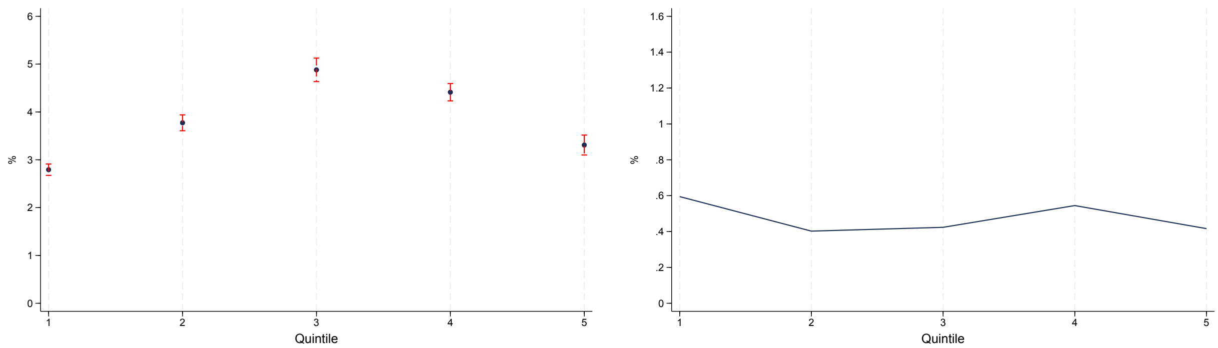

Figure 7: Monetary-Policy Heterogeneity—Liquid Wealth
The left panel plots the estimates from the contemporaneous consumption-response regression in column 1 of Table 2, Panel A, separately for each liquid-wealth quintile, alongside 99% confidence intervals. The right panel plots the unconditional partial-equilibrium impact response to a 1 p.p. decrease in the interest rate for the liquid-wealth quintiles, taking into consideration their behavior as well as their exposure to monetary policy.

share of adjustable-rate mortgage holders ranging from 11% to 33%. The lowest liquid-wealth quintile has the highest share, indicating a strong prevalence of households with little liquid wealth but nevertheless a mortgage—wealthy hand-to-mouth households in the parlance of Kaplan and Violante (2022). The right panel of Figure 7 reports the unconditional average consumption response to monetary policy along the liquid-wealth distribution, accounting for heterogeneous exposure. It shows a remarkably uniform consumption response in terms of percentage changes across quintiles, suggesting that differences in behavior conditional on experiencing mortgage-rate resets and different exposure to these mortgage-rate resets cancel each other out.

Still, our results in Table 6 lend support to the theoretical finding in Kaplan et al. (2018b) that wealthy hand-to-mouth households play an important role in the transmission of monetary policy. Households in the lowest liquid-wealth quintile exhibit the strongest response in terms of level changes. This is because, on average, they have the highest consumption levels to begin with, explaining their small percentage changes displayed in the left panel of Figure 7.

Heterogeneity irrelevance. So far, we have established that there might be notable heterogeneity in households' behavior after changes in the interest rate. Yet, accounting for these differences does not significantly alter aggregate inference.

To illustrate this point, we aggregate average consumption across disposable-income and

Figure 8: Distribution of Aggregate Consumption Responses
The figure plots the estimates for the first-year aggregate consumption response when accounting for heterogeneous behavior across different dimensions of heterogeneity.

liquid-wealth groups,[22] and find that the aggregate effect remains virtually unchanged from our baseline estimate. Specifically, when accounting for disposable-income heterogeneity, aggregate consumption increases in the first year by 5.35bn DKK rather than 5.60bn DKK, and by 5.37bn DKK when considering liquid-wealth heterogeneity, as shown in Figure 8.

Qualitatively, these estimates are lower because households with greater exposure, on average, respond less conditional on being exposed. Yet, the small quantitative differences suggest that pooling all households in our baseline estimation introduces negligible bias in the measured average response to mortgage-rate adjustments. This remains true when we group households along other heterogeneity dimensions such as transfer to total income, age, and geographic location (Section E in the Online Appendix). From this we conclude that heterogeneity appears to be close to irrelevant for inferring the partial-equilibrium effect of monetary policy.

[22] Besides the five disposable-income and liquid-wealth quintiles, we generate a separate category for individuals without data coverage of their disposable income or liquid wealth ("NA"). This ensures that we can compare the resulting demand-equivalent stimulus payments with our estimate for the entire adult population in Table 4.

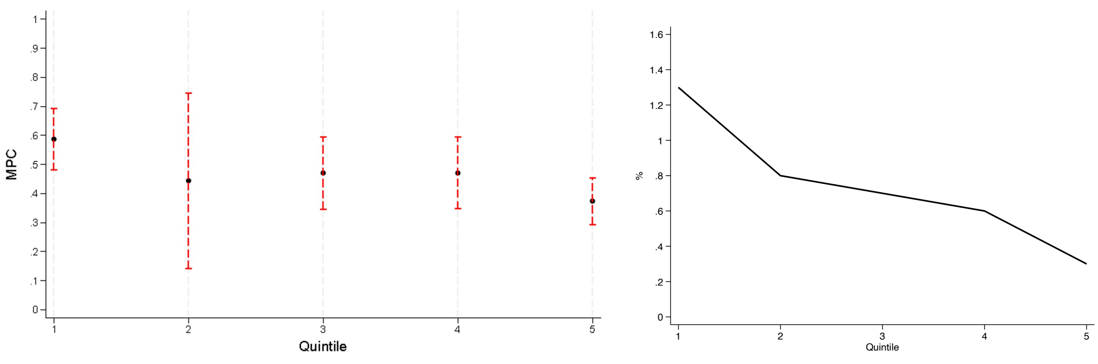

Figure 9: Fiscal-Policy Heterogeneity—Disposable Income
The left panel plots the estimates from the contemporaneous consumption-response regression in column 1 of Table 2, Panel B, separately for each disposable-income quintile, alongside 99% confidence intervals. The right panel plots the unconditional partial-equilibrium impact consumption response to uniform stimulus policies that mimic a 1 p.p. decrease in the interest rate for the disposable-income quintiles.

5.2 Cross-Sectional Consumption Response to Fiscal Policy

The average MPC is a sufficient statistic for uniform stimulus checks as these are by construction exposure-neutral. In this section, we nevertheless analyze the heterogeneous response to stimulus checks to understand (i) the cross-sectional impact of uniform stimulus policies and (ii) the potency of targeted ones.

Consumption response to fiscal policy along the income distribution. We re-estimate the average MPCs out of (deposit-only) inheritances within disposable-income quintiles.[23] The left panel of Figure 9 plots these MPCs along the income distribution. MPCs tend to decrease in income as they are highest for households in the lowest quintile and lowest for households in the highest quintile. Notably, the average MPCs vary modestly across quintiles, ranging between 38% and 58%.

Two key policy implications arise. First, these estimates allow us to assess the distributional impact of a uniform transfer stimulus designed to achieve aggregate equivalence with monetary policy. Accordingly, the right panel of Figure 9 converts the MPCs into consumption changes in percentage points by accounting for the average consumption within each quintile. This allows for a direct comparison with the distributional effects of a macro-equivalent change in the monetary-policy rate (see right panel of Figure 6). In contrast to

[23]We assign households to quintiles for each calendar year. Treated households are then grouped according to their quintile in the last pre-inheritance year ($t-1$).

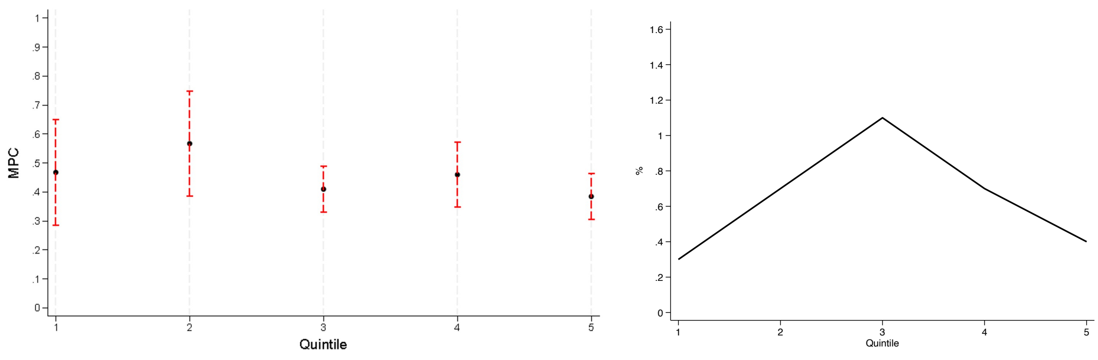

Figure 10: Fiscal-Policy Heterogeneity—Liquid Wealth
The left panel plots the estimates from the contemporaneous consumption-response regression in column 1 of Table 2, Panel B, separately for each liquid-wealth quintile, alongside 99% confidence intervals. The right panel plots the unconditional partial-equilibrium impact consumption response to uniform stimulus policies that mimic a 1 p.p. decrease in the interest rate for the liquid-wealth quintiles.

monetary policy, which tends to be regressive, a uniform stimulus payment proves to be progressive by eliciting a larger consumption increase among lower-income households. This observation is consistent with the theoretical predictions in Wolf (2025): while flat stimulus payments achieve macro equivalence with monetary policy, they stimulate consumption at different parts of the distribution. Nonetheless, our findings suggest that the progressivity of uniform transfers is considerably lower than the levels typically implied by standard calibrated HANK models.[24] This result is particularly important for policymakers that care about stabilizing cross-sectional consumption shares (McKay and Wolf, 2023).

Second, the relatively modest variation in average MPCs across income quintiles suggests that budget-neutral redistribution policies are unlikely to have a meaningful impact on aggregate consumption. In addition, there is limited scope for increasing the aggregate effectiveness of stimulus payments by targeting them conditional on income.[25]

We illustrate this point with two examples. For this purpose, we focus on the first three years $(t, t+1, t+2)$ so as to preclude the loss of statistical power associated with individual survivorship bias in disposable-income and liquid-wealth quintiles.[26] First, the most cost-effective method to achieve the same partial-equilibrium stimulus as a uniform transfer of

[24]Standard HANK models oftentimes predict MPCs to decrease sharply in income (Alves et al., 2020; Pfäuti et al., 2025), implying uniform transfers to be highly progressive in income.

[25]See also Pfäuti et al. (2025) who construct a theoretical model with a rather flat MPC-income distribution and analyze its implications for the effectiveness of targeted transfer payments in general equilibrium.

[26]In Table 4, this would amount to a total stimulus payment of 5,030 DKK per person.

4,539 DKK (uniform payment with quintile-specific MPCs, cf. Table 5) is to concentrate all stimulus payments on Q1 households. However, the fiscal cost of this approach would be 4,224 DKK per capita, only 7% lower than the baseline transfer. Second, we consider how to target stimulus payments to replicate the distributional effects of monetary policy along the income distribution on top of macro equivalence. This requires the stimulus package to primarily target Q4 and Q5 households. Since high-income households exhibit an average MPC similar to that of the overall population, the cost of this targeted package would only be marginally higher than that of a uniform transfer: approximately 4,866 DKK instead of 4,539 DKK overall. Therefore, income heterogeneity seems to be of little importance for the effectiveness of targeted fiscal policies.

Consumption response to fiscal policy along the liquid-wealth distribution. Next, we re-estimate the average MPCs out of (deposit-only) inheritances within each liquid-wealth quintile. The left panel of Figure 10 displays these MPCs, revealing a non-monotonic pattern, with the second quintile exhibiting the highest average MPC and the highest liquid-wealth quintile exhibiting the lowest average MPC. Moreover, the average MPCs also do not vary too much along liquid-wealth quintiles. These findings contrast with the predictions of standard HANK models (Kaplan and Violante, 2022) in which MPCs decrease sharply with liquid wealth. As a result, our estimated MPC patterns imply that even if it were feasible to target households by their liquid wealth, such an approach would not greatly enhance the effectiveness of transfers. Thus, liquid-wealth heterogeneity also appears to be of little relevance for targeted fiscal policies.

The right panel of Figure 10 converts the MPCs into consumption changes (in p.p.) following a uniform stimulus, allowing for a direct comparison with the distributional effects of a macro-equivalent change in the monetary-policy rate along the liquid-wealth distribution (see right panel of Figure 7). Interestingly, a uniform stimulus payment yields a hump-shaped effect along the liquid-wealth distribution. The reason is that average consumption varies strongly along the liquid-wealth distribution and it is lowest for the third quintile. Contrasting these findings to the rather flat response to monetary policy (right panel in Figure 7) highlights again the different distributional impact between monetary policy and macro-equivalent fiscal policy. And yet, given that liquid-wealth heterogeneity plays only a negligible role for the effectiveness of targeted fiscal policies, designing transfers to replicate the distributional effects of monetary policy along the liquid-wealth distribution on top of macro equivalence would increase costs by only $(4,687/4,498-1=)$ 4% (cf. Table 6).

6 Robustness

In this section, we present additional evidence and a battery of robustness checks for both empirical settings.

Monetary-policy transmission through adjustable-rate mortgages. We first consider robustness of our estimates for the transmission of monetary policy through ARMs. Our empirical strategy exploits plausibly exogenous variation in the pass-through of monetary policy to mortgage payments by different types of F-loan borrowers (F3 and F5). This keeps constant the selection of individuals into adjustable-rate mortgage contracts, which offer the advantage of mortgage resets not being a choice variable (unlike for fixed-rate mortgages, where mortgagors might take suboptimal refinancing decisions, as documented by Andersen et al., 2020).

While the summary statistics for F3 and F5 borrowers in Panel B of Table 1 suggest that the two groups are similar along observables, it should still be noted that we cannot—by definition—estimate the whole range of dynamic effects in both groups, as F3 borrowers see a reset of their mortgage rate in $t+3$. Therefore, the effects in $t+3$ and $t+4$ are estimated using only F5 borrowers. In Table A.2 of the Online Appendix, we estimate all specifications separately for F3 (up until $t+2$) and F5 borrowers, respectively. The immediate consumption response is similar, while it is somewhat more emphasized for F5 borrowers in $t+1$ and $t+2$. This difference can, however, be explained by the different horizons of the mortgagors, as their consumption responses are similar in the last year prior to the next potential reset year (i.e., in $t+2$ for F3 borrowers and in $t+4$ for F5 borrowers).

To assess other dimensions of sample selection, we consider the subsample of F5 borrowers whose consumption needs to be observed until $t+4$ and the subsample of all F3 and F5 borrowers who experience the whole range of monetary-policy changes during our sample period. Panels B and C of Table A.3 in the Online Appendix show that our results are also robust to these dimensions of selection, especially in the years $t+1$ and beyond.

In Table 7, Panel A, we consider the curvature of the individual consumption response, in particular any potential nonlinearities. In particular, we re-define our explanatory variable to reflect only large mortgage-rate resets, i.e., it is zero for resets smaller than $|1\%|$. Doing so leaves our estimates virtually unaltered. This implies that the consumption response does not exhibit any notable nonlinearities, and also justifies our choice of a 1 p.p. drop in the monetary-policy rate for the purpose of computing aggregation consumption responses in

Table 3. More than that, our results are also similar, or somewhat stronger, for accommodative mortgage resets than for interest-rate increases, as can be seen in Table A.4, Panel A, of the Online Appendix where we define the explanatory variable to be non-zero only for positive values. Note, however, that the vast majority of mortgage-rate resets in our sample are accommodative due to the ECB's monetary-policy stance during that time period.

We also consider alternative specifications. In Table 7, Panel B, we use the natural logarithm of consumption as dependent variable while controlling for borrower fixed effects. After controlling for time-invariant unobserved heterogeneity at the individual level, the consumption responses tend to be stronger from year $t+1$ on. Last, in Table A.4, Panel B, of the Online Appendix, we move to our baseline regression specification (11), but re-define the dependent variable to reflect the change in consumption from $t-1$ to $t+h$ while keeping only non-reset observations prior to the respective year. In this manner, the specification is closer in spirit to (12), which we use to estimate the individual consumption response to inheritance shocks, but our estimates remain similar to our baseline.

Windfall gains from inheritance shocks. With regard to our estimated consumption response to lump-sum payments, to make sure that the total payout under the inheritance shock accrues shortly after the death of the relative, we focus on deposit-only inheritances. Furthermore, the latter also have the advantage of reflecting the typical size and volume of stimulus payments that we use to match the consumption response. Nevertheless, we re-run our analysis using all inheritances including real estate, the transfer of which may be well spread out over time. As can be seen in Table A.5, Panel B, of the Online Appendix, our estimates would naturally be somewhat smaller given that non-deposit inheritances tend to be larger, but not drastically so. In conjunction with the fact that deposit-only heirs are similar along observables, other than the size of the inheritance (see Panel C of Table 1), this suggests that using deposit-only inheritances, which have the advantage of most likely materializing in the year of the inheritance, does not markedly bias our estimates.

We also revisit the timing of inheritance events in our empirical setup. We assume that a deposit-only inheritance is paid out in the same year as the respective death of a relative. Due to the potentially delayed recognition of inheritances, this assumption is more likely to hold true in the first half of the year. For this purpose, in Table A.5, Panel B, of the Online Appendix, we limit the sample to inheritance events resulting from deaths in the first half of each year. Our results are robust.

Finally, in Table 8 we replace our level-level regression specification (12) with a log-log specification. The estimates in later years gain statistical significance, but the interpretation of the economic significance is subject to the relatively bad approximation of growth rates by the natural logarithm when moving from zero to any positive value of $Inheritance_{it}$.

Demand equivalence. We also consider the robustness of our demand-equivalence result—i.e., the required stimulus payments to induce the same aggregate consumption response as a 1 p.p. decrease in the monetary-policy rate—in Table 4 against the backdrop of these estimates. To this end, we can vary two inputs. First, we consider robustness to sample selection (in particular, the requirement for borrowers' consumption to be observed until $t + 4$ as in Table A.3, Panel B) in our estimation of individual consumption responses to changes in mortgage payments, which in turn feeds into our calculation of the aggregate consumption response (in Table 3). The resulting series of equivalent stimulus payments is in the second row of Table 9, and—in comparison to our baseline in the first row—frontloads payments that are otherwise similar in aggregate size. In the last row, we consider robustness to accounting for the potential nonlinearity of consumption responses to changes in mortgage payments (Table 7, Panel A). Given the lack of nonlinearity, the corresponding series of equivalent stimulus payments also resembles that implied by our baseline estimates.

7 Conclusion

We quantify the size of stimulus checks needed to replicate the aggregate consumption response of a desired but potentially infeasible interest rate cut. We sidestep the difficulty of analyzing stimulus policies in general equilibrium by exploiting that equivalence between monetary and fiscal policy can be achieved by equivalence in partial-equilibrium consumption stimulus (Wolf, 2025). To operationalize this strategy, we use Danish administrative data, allowing us to analyze the partial-equilibrium effects of both monetary and fiscal policy within the same population. We focus on the transmission of monetary policy through variable mortgage costs, while exploiting unexpected cash inheritances to analyze the effects of stimulus checks. In this manner, we find that matching the aggregate consumption response stemming from a 1 percentage point decrease in the monetary-policy rate requires stimulus payments of around $1,013 per person over five years. This translation from percentage changes in monetary policy to dollar amounts in fiscal transfers offers a novel empirical framework for evaluating the practical relevance of MPC distributions.

Our findings have direct implications for the interaction of monetary and fiscal policy in business-cycle stabilization. First, the transfer amounts required to replicate a typical monetary-policy rate cut are moderate, suggesting stimulus checks are a practical substitute whenever monetary policy is constrained. Second, while macro-equivalent, stimulus checks exhibit progressive distributional effects, in contrast to the regressive nature of monetary policy. From this follows, third, that even when monetary policy is unconstrained, combining monetary and fiscal policy stimulus may be advantageous if policymakers wish to neutralize the (partial-equilibrium) distributional impact of business-cycle interventions.

We show that our total macro-equivalent stimulus payments are robust to accounting for heterogeneous responses to both monetary and fiscal policy. A key explanation for this robustness is the modest variation in household MPCs that we document across income, liquid-wealth, and other distributions. This limited heterogeneity substantially reduces the importance of income and wealth inequality for aggregate policy effects, as different exposure patterns along these distributions have minimal impact on policy outcomes.

Our findings suggest a fruitful direction for advancing HANK models. Part of their popularity stems from their ability to replicate the high average MPCs typically found empirically, as our study confirms. However, these models typically generate this result by producing a steeply negative relationship between MPCs and household income or liquid wealth—a pattern our empirical evidence contradicts. Our results therefore warrant the incorporation of mechanisms in HANK models that weaken the link between household MPCs and their position in the income or liquid-wealth distribution (see Pfäuti et al., 2025, for one step in this direction).

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Tables

Table 1: Summary Statistics

Panel A. Total Population

	Mean	Pseudo Median	Std.	*N*
Male (0/1)	0.49	0	0.49	60,525,558
Age	48.3	47.0	18.2	60,525,558
Married (0/1)	0.49	0	0.49	60,525,558
Kids (0/1)	0.17	0	0.38	60,525,558
Higher educ (0/1)	0.21	0	0.40	60,525,558
House	319.6	0.0	812.2	60,493,889
Bank account	116.0	29.8	234.2	60,493,889
Disposable income	213.8	196.9	122.9	60,493,889
Total wealth	395.3	36.0	1,098.7	60,493,889
Liquid wealth	18.4	4.5	397.4	60,525,558
Consumption (base)	208.9	173.5	170.2	47,885,851
Consumption	196.8	171.1	141.9	37,979,554
Any mortgage (0/1)	0.39	0	0.49	44,429,284
F3 (0/1)	0.05	0	0.21	44,429,284
F5 (0/1)	0.06	0	0.23	44,429,284
Inheritance (0/1)*100	0.06	0	2.6	60,525,558

Panel B. Mortgagors

	F3				F5			
	Mean	Pseudo Median	Std.	*N*	Mean	Pseudo Median	Std.	*N*
Male (0/1)	0.54	1	0.50	187,332	0.52	1	0.50	421,092
Age	52.4	51.0	13.4	187,332	51.8	51.0	14.2	421,092
Married (0/1)	0.70	1	0.46	187,332	0.71	1	0.45	421,092
Kids (0/1)	0.24	0	0.43	187,332	0.24	0	0.43	421,092
Higher educ (0/1)	0.37	0	0.48	187,332	0.38	0	0.49	421,092
House	442.1	187.6	946.2	187,332	343.7	130.6	874.7	421,092
Bank account	182.8	70.4	317.8	187,332	173.2	69.2	298.5	421,092
Disposable income	350.9	317.7	187.7	187,330	344.9	312.3	182.8	421,090
Total wealth	564.0	198.9	1,441.9	187,330	457.5	148.2	1,317.2	421,090
Liquid wealth	79.5	18.9	670.3	187,330	85.9	21.2	626.5	421,090
Consumption	268.2	222.4	201.5	152,220	276.2	224.2	214.8	336,690
Interest	1.50	0.52	4.40	187,332	3.90	2.62	4.90	421,092
Outstanding debt	1,026.2	790.7	1,042.8	187,332	1,001.9	797.2	933.9	421,092

(continued on next page)

Panel C. Heirs

	All inheritances				Deposit only			
	Mean	Pseudo Median	Std.	*N*	Mean	Pseudo Median	Std.	*N*
Male (0/1)	0.53	1	0.49	38,408	0.53	1	0.49	22,263
Age	48.5	49.0	8.6	38,408	48.9	50.0	8.7	22,263
Married (0/1)	0.57	1	0.49	38,408	0.57	1	0.49	22,263
Kids (0/1)	0.24	0	0.42	38,408	0.23	0	0.42	22,263
Higher educ (0/1)	0.26	0	0.44	38,408	0.22	0	0.41	22,263
House	411.1	34.3	914.8	38,406	331.5	0.00	788.1	22,263
Bank account	130.9	37.5	252.0	38,406	119.0	32.5	234.8	22,263
Disposable income	258.0	239.7	129.1	38,406	248.2	232.0	121.0	22,263
Total wealth	467.0	107.8	1,180.2	38,406	349.4	52.8	1,020.0	22,263
Liquid wealth	-4.15	-3.35	431.2	38,406	-18.82	-12.9	406.8	22,263
Consumption	222.8	197.0	140.3	23,766	216.9	195.9	132.4	14,166
Inheritance amount	422.7	121.7	1,282.8	38,408	122.1	30.8	709.46	22,263

Panel A presents summary statistics for the entire Danish population in our data from 2003 to 2016 (mortgage-related variables are available over the 2009 to 2018 period). Panel B zooms in on mortgagors (with F3 and/or F5 adjustable-rate mortgages in the left and right panel, respectively) in 2018, while Panel C presents summary statistics in the last pre-inheritance year for all individuals with any inheritance event (left panel) or deposit-only inheritances (right panel) anytime from 2003 to 2016. All monetary values are in thousands of DKK. Note that for confidentiality reasons, reported (pseudo) median values are calculated as averages over the five observations around the median.

Table 2: Estimating $\mathcal{C}_\tau$ and $\mathcal{C}_{i^n}$

Panel A. Consumption Response to Changes in Mortgage Payments

	$\Delta\ln(c_{it+h})$					
	$h=0$	$h=0$	$h=1$	$h=2$	$h=3$	$h=4$
Variable	(1)	(2)	(3)	(4)	(5)	(6)
Δ Interest payment/Out. debt	-0.036***	-0.036***	-0.040***	-0.027***	-0.022***	-0.017***
	(-94.00)	(-95.83)	(-102.84)	(-67.67)	(-35.73)	(-25.67)
Controls	N	Y	Y	Y	Y	Y
Region-year FE	Y	Y	Y	Y	Y	Y
Adj. R^2	0.015	0.020	0.021	0.017	0.019	0.018
No. of individuals	566,977	566,977	531,777	523,697	265,380	230,222
N	1,623,879	1,623,879	1,462,757	1,389,964	614,939	495,262

Panel B. Consumption Response to Lump-sum Payments

	c_{it+h}					
	$h=0$	$h=0$	$h=1$	$h=2$	$h=3$	$h=4$
	(1)	(2)	(3)	(4)	(5)	(6)
Inheritance	0.449***	0.411***	0.296***	0.067***	0.055***	0.065***
	(13.06)	(10.48)	(12.11)	(2.90)	(3.99)	(3.82)
Controls	N	Y	Y	Y	Y	Y
Individual FE	Y	Y	Y	Y	Y	Y
Region-year FE	Y	Y	Y	Y	Y	Y
Adj. R^2	0.56	0.60	0.55	0.53	0.53	0.54
No. of individuals	17,558	10,252	10,222	10,200	10,139	10,107
N	59,906	33,043	32,985	32,726	32,050	31,437

In Panel A, the level of observation is the individual-year level it, based on the sample of all individuals with F3 and/or F5 adjustable-rate mortgages from 2009 to 2018. The dependent variable is the log change in consumption (winsorized at the 0.5% and 99.5% levels) of individual/borrower i (in region j) from year $t-1$ to $t+h$, where h varies from 0 (in columns 1 and 2) to 4 (in the last column). $\Delta Interest\ payment_{it}/Outstanding\ debt_{it-1}$ denotes the change in mortgage payments, scaled by the previous year's outstanding debt, for borrower i from year $t-1$ to year t (in %). The set of control variables, when included, comprises indicator variables for female individuals, higher education, individuals with kids and whether an individual is married, individual i's age, as well as individual i's disposable income, total wealth, the value of the house, and the value of the securities portfolio (all winsorized at the 0.5% and 99.5% levels) in year t. t-statistics in parentheses are based on robust standard errors clustered at the individual level. In Panel B, the level of observation is the individual-year level it, based on the sample of all individuals with deposit-only inheritances anytime between 2003 and 2016. For each inheritance event, we keep all individual-year observations running up to the event, but only one post-inheritance observation, either that in the same year t or in one of the following four years. Accordingly, the dependent variable is consumption (winsorized at the 0.5% and 99.5% levels) of individual/heir i (in region j) in year $t+h$, where h varies from 0 (in columns 1 and 2) to 4 (in the last column). $Inheritance_{it}$ is the size of the inheritance (in 1,000 DKK) for an inheritance event of individual i in year t. The set of control variables is the same as in Panel A. t-statistics in parentheses are based on robust standard errors clustered at the individual level.

Table 3: Aggregate Consumption Response to Changes in Mortgage Payments

	#people	Fraction	Cumulative increase in consumption (bn DKK)				
			t	$t+1$	$t+2$	$t+3$	$t+4$
F0.25	41,678	3.8%	0.41	0.46	0.31	0.25	0.20
F0.5	259,347	23.4%	2.32	2.57	1.73	1.40	1.11
F1	150,962	13.6%	1.40	1.56	1.04	0.85	0.67
F2	9,165	0.8%	0.04	0.04	0.03	0.02	0.02
F3	187,332	16.9%	0.57	0.64	0.43	0.35	0.28
F4	16,823	1.5%	0.04	0.04	0.03	0.02	0.02
F5	421,092	38.1%	0.79	0.88	0.59	0.48	0.38
F6	6,436	0.6%	0.01	0.01	0.01	0.01	0.00
F7	1,844	0.2%	0.00	0.00	0.00	0.00	0.00
F8	921	0.1%	0.00	0.00	0.00	0.00	0.00
F9	543	0.0%	0.00	0.00	0.00	0.00	0.00
F10	10,032	0.9%	0.01	0.01	0.01	0.00	0.00
Sum	1,106,175	100%					
Agg. increase in bn DKK:			5.60	6.22	4.17	3.38	2.69
Agg. consumption 2017 in bn DKK:							952
% increase in agg. consumption:			0.6%	0.7%	0.4%	0.4%	0.3%

This table computes aggregate-consumption responses using information on all individuals with F-loan contracts in 2018, in particular the distribution of F-loan contracts, the implied reset frequencies, and individuals' lagged consumption.

Table 4: Interest Rate Cuts and Stimulus Payments—Demand Equivalence

		t	$t+1$	$t+2$	$t+3$	$t+4$
Consumption response to interest rate changes						
Δ Aggregate consumption (bn DKK):		5.60	6.22	4.17	3.38	2.69
Population size:	4,580,547					
Δ Cons. per person (Δc_{t+h}) (DKK):		1,223	1,358	910	738	587
Equivalent stimulus payments						
Stimulus MPC:		41.1%	29.6%	6.7%	5.5%	6.5%
Demand-equiv. stimulus (τ_{t+h}):		2,975	1,162	893	963	590
Total stimulus payments:	6,582					

The calculation of demand-equivalent stimulus payments takes into account the dynamic MPCs—i.e., each stimulus payment is consumed over the next three years as given by the dynamic MPCs—as follows:

$$\tau_{t+h} = \begin{cases} \Delta c_{t+h}/MPC_t & \text{for } h = 0 \\ \left(\Delta c_{t+h} - \sum_{j=0}^{h-1} \tau_{t+j} \times MPC_{t+h-j}\right)/MPC_t & \text{for } h > 0, \end{cases}$$

where $h \in \{0, 1, 2, 3, 4\}$ and Δc_{t+h}, τ_{t+h}, and MPC_{t+h} denote, respectively, the average consumption increase per person, the demand-equivalent stimulus payment, and the stimulus MPC in year $t + h$.

Table 5: Heterogeneity—Disposable Income

		% w/ mortgage	% w/ mortgage reset	Avg. reset (yrs)	Avg. agg. cons.	Cumulative Δ cons. (bn DKK) t	$t+1$	$t+2$
Low income	Q1	7%	6%	2.54	54	0.17	0.21	0.13
	Q2	12%	6%	2.74	109	0.21	0.22	0.16
	Q3	21%	9%	2.73	162	0.37	0.41	0.27
	Q4	43%	21%	2.72	217	0.91	1.03	0.72
High income	Q5	67%	47%	2.80	303	2.42	2.65	1.77
NA		76%	62%	3.34	107	1.27	1.41	0.95
Total (sum):					952	5.35	5.93	3.98
Macro-equivalent stimulus (heterogeneous MPCs)								
Total:					**4,539**			
Macro-and-distributional-equivalent stimulus								
Total:					**4,866**			

Table 6: **Heterogeneity—Liquid Wealth**

		% w/ mortgage	% w/ mortgage reset	Avg. reset (yrs)	Avg. agg. cons.	Cumulative Δ cons. (bn DKK) t	$t+1$	$t+2$
Low liq. wealth	Q1	49%	33%	2.67	232	1.38	1.65	1.15
	Q2	30%	15%	2.77	174	0.70	0.82	0.55
	Q3	20%	11%	2.81	85	0.36	0.39	0.27
	Q4	33%	17%	2.82	145	0.79	0.83	0.53
High liq. wealth	Q5	33%	20%	2.88	209	0.87	0.82	0.53
NA		76%	62%	3.34	107	1.27	1.41	0.95
Total (sum):					952	5.37	5.92	3.99
Macro-equivalent stimulus (heterogeneous MPCs)								
Total:					**4,498**			
Macro-and-distributional-equivalent stimulus								
Total:					**4,687**			

Table 7: Consumption Response to Changes in Mortgage Payments—Robustness

Panel A. Nonlinearity of the Effect

	$\Delta\ln(c_{it+h})$					
	$h=0$	$h=0$	$h=1$	$h=2$	$h=3$	$h=4$
Variable	(1)	(2)	(3)	(4)	(5)	(6)
Δ Interest payment/Out. debt	-0.033***	-0.034***	-0.038***	-0.026***	-0.022***	-0.018***
	(-84.33)	(-85.75)	(-94.60)	(-62.96)	(-34.43)	(-25.61)
Controls	N	Y	Y	Y	Y	Y
Region-year FE	Y	Y	Y	Y	Y	Y
Adj. R^2	0.014	0.018	0.020	0.017	0.019	0.018
No. of individuals	566,977	566,977	531,777	523,697	265,380	230,222
N	1,623,879	1,623,879	1,462,757	1,389,964	614,939	495,262

Panel B. Alternative Specification: Logarithm

	$\ln(c_{it+h})$					
	$h=0$	$h=0$	$h=1$	$h=2$	$h=3$	$h=4$
Variable	(1)	(2)	(3)	(4)	(5)	(6)
Δ Interest payment/Out. debt	-0.013***	-0.013***	-0.099***	-0.076***	-0.044***	-0.031***
	(-18.77)	(-19.80)	(-97.85)	(-71.67)	(-23.76)	(-13.12)
Controls	N	Y	Y	Y	Y	Y
Individual FE	Y	Y	Y	Y	Y	Y
Region-year FE	Y	Y	Y	Y	Y	Y
Adj. R^2	0.008	0.105	0.080	0.048	0.038	0.033
No. of individuals	713,665	713,665	581,277	516,431	232,743	191,883
N	2,623,070	2,623,070	1,860,035	1,493,086	584,780	422,065

The level of observation is the individual-year level it, based on the sample of all individuals with F3 and/or F5 adjustable-rate mortgages from 2009 to 2018. In Panel A, the dependent variable is the log change in consumption (winsorized at the 0.5% and 99.5% levels) of individual/borrower i (in region j) from year $t-1$ to $t+h$, where h varies from 0 (in columns 1 and 2) to 4 (in the last column). In Panel B, the dependent variable is the natural logarithm of consumption (winsorized at the 0.5% and 99.5% levels) of individual/borrower i (in region j) in year $t+h$, where h varies from 0 (in columns 1 and 2) to 4 (in the last column). $\Delta Interest\ payment_{it}/Outstanding\ debt_{it-1}$ denotes the change in mortgage payments, scaled by the previous year's outstanding debt, for borrower i from year $t-1$ to year t (in %). The variable is set to non-zero only for resets in excess of $|1\%|$. The set of control variables, when included, comprises indicator variables for female individuals, higher education, individuals with kids and whether an individual is married, individual i's age, as well as individual i's disposable income, total wealth, the value of the house, and the value of the securities portfolio (all winsorized at the 0.5% and 99.5% levels) in year t. t-statistics in parentheses are based on robust standard errors clustered at the individual level.

Table 8: Consumption Response to Lump-Sum Payments—Robustness

Alternative Specification: Logarithm

	$\ln(c_{it+h})$					
	$h = 0$	$h = 0$	$h = 1$	$h = 2$	$h = 3$	$h = 4$
	(1)	(2)	(3)	(4)	(5)	(6)
$\ln(1 + \text{Inheritance})$	0.052***	0.058***	0.042***	0.024***	0.020***	0.020***
	(29.48)	(24.35)	(18.72)	(10.92)	(8.63)	(8.05)
Controls	N	Y	Y	Y	Y	Y
Individual FE	Y	Y	Y	Y	Y	Y
Region-year FE	Y	Y	Y	Y	Y	Y
Adj. R^2	0.61	0.67	0.63	0.63	0.63	0.62
No. of individuals	17,558	10,252	10,222	10,200	10,139	10,107
N	59,906	33,043	32,985	32,726	32,050	31,437

The level of observation is the individual-year level it, based on the sample of all individuals with deposit-only inheritances following a sudden death anytime between 2003 and 2016. For each inheritance event, we keep all individual-year observations running up to the event, but only one post-inheritance observation, either that in the same year t or in one of the following four years. The dependent variable is log of consumption (winsorized at the 0.5% and 99.5% levels) of individual/heir i (in region j) in year $t + h$, where h varies from 0 (in columns 1 and 2) to 4 (in the last column). The set of control variables, when included, comprises indicator variables for female individuals, higher education, individuals with kids and whether an individual is married, individual i's age, as well as individual i's disposable income, total wealth, the value of the house, and the value of the securities portfolio (all winsorized at the 0.5% and 99.5% levels) measured in the same year as the dependent variable. t-statistics in parentheses are based on robust standard errors clustered at the individual level.

Table 9: Demand Equivalence—Robustness

	t	$t+1$	$t+2$	$t+3$	$t+4$	Total
Equivalent stimulus payments						
Baseline:	2,975	1,162	893	963	590	6,582
$t+4$ consumption available:	2,635	1,895	745	924	642	6,840
Nonlinearity:	2,757	1,252	352	924	658	5,943

This table re-estimates the series of demand-equivalent stimulus payments (in the last two rows of Table 4, reproduced in the first row), varying inputs based on Table A.3, Panel B, and Table 7, Panel A (in the second and third row, for the consumption response to changes in mortgage payments).

Internet Appendix to "Empirical Monetary-Fiscal Equivalence" (not intended for publication)

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A Supplementary Tables

Table A.1: Distribution of F-Loan Categories Over Time

	2009	2010	2011	2012	2013	2014	2015	2016	2017	2018
F0.25	0%	1%	1%	1%	1%	2%	2%	3%	4%	4%
F0.5	31%	27%	26%	23%	22%	22%	22%	24%	23%	23%
F1	38%	39%	38%	35%	31%	27%	21%	17%	15%	14%
F2	3%	2%	3%	2%	2%	2%	2%	1%	1%	1%
F3	15%	17%	17%	17%	18%	20%	22%	21%	19%	17%
F4	2%	1%	2%	2%	3%	4%	4%	5%	5%	2%
F5	10%	11%	12%	18%	20%	22%	25%	27%	32%	38%
F6	0%	0%	0%	1%	1%	1%	1%	1%	1%	1%
F7	0%	0%	0%	0%	0%	0%	0%	0%	0%	0%
F8	0%	0%	0%	0%	0%	0%	0%	0%	0%	0%
F9	0%	0%	0%	0%	0%	0%	0%	0%	0%	0%
F10	1%	1%	1%	1%	1%	1%	1%	1%	1%	1%

This table summarizes the relative frequencies of different F-loan contracts over time from 2009 to 2018.

Table A.2: Consumption Response to Changes in Mortgage Payments—F3 vs. F5 Borrowers

Panel A. F3 Borrowers Only

	$\Delta\ln(c_{it+h})$			
	$h=0$	$h=0$	$h=1$	$h=2$
Variable	(1)	(2)	(3)	(4)
Δ Interest payment/Out. debt	-0.033***	-0.033***	-0.029***	-0.017***
	(-60.83)	(-61.79)	(-52.76)	(-30.22)
Controls	N	Y	Y	Y
Region-year FE	Y	Y	Y	Y
Adj. R^2	0.015	0.019	0.023	0.019
No. of individuals	300,015	300,015	278,837	276,508
N	789,285	789,285	708,126	677,931

Panel B. F5 Borrowers Only

	$\Delta\ln(c_{it+h})$					
	$h=0$	$h=0$	$h=1$	$h=2$	$h=3$	$h=4$
Variable	(1)	(2)	(3)	(4)	(5)	(6)
Δ Interest payment/Out. debt	-0.035***	-0.036***	-0.046***	-0.031***	-0.022***	-0.017***
	(-65.05)	(-67.16)	(-80.65)	(-53.69)	(-35.73)	(-25.67)
Controls	N	Y	Y	Y	Y	Y
Region-year FE	Y	Y	Y	Y	Y	Y
Adj. R^2	0.017	0.022	0.022	0.017	0.019	0.018
No. of individuals	332,476	332,476	313,497	307,005	265,380	230,222
N	886,205	886,205	802,477	757,727	614,939	495,262

The level of observation is the individual-year level it, based on the sample of all individuals with F3 adjustable-rate mortgages (Panel A) or F5 adjustable-rate mortgages (Panel B) from 2009 to 2018. The dependent variable is the log change in consumption (winsorized at the 0.5% and 99.5% levels) of individual/borrower i (in region j) from year $t-1$ to $t+h$, where h varies from 0 (in columns 1 and 2) to 4 (in the last column). $\Delta Interest\ payment_{it}/Outstanding\ debt_{it-1}$ denotes the change in mortgage payments, scaled by the previous year's outstanding debt, for borrower i from year $t-1$ to year t (in %). The set of control variables, when included, comprises indicator variables for female individuals, higher education, individuals with kids and whether an individual is married, individual i's age, as well as individual i's disposable income, total wealth, the value of the house, and the value of the securities portfolio (all winsorized at the 0.5% and 99.5% levels) in year t. t-statistics in parentheses are based on robust standard errors clustered at the individual level.

Table A.3: Consumption Response to Changes in Mortgage Payments—Robustness II

Panel A. Robustness to Outliers

	$\Delta\ln(c_{it+h})$					
	$h=0$	$h=0$	$h=1$	$h=2$	$h=3$	$h=4$
Variable	(1)	(2)	(3)	(4)	(5)	(6)
Δ Interest payment/Out. debt	-0.094***	-0.095***	-0.112***	-0.095***	-0.080***	-0.072***
	(-104.26)	(-105.93)	(-120.78)	(-97.21)	(-47.56)	(-33.10)
Controls	N	Y	Y	Y	Y	Y
Region-year FE	Y	Y	Y	Y	Y	Y
Adj. R^2	0.014	0.022	0.034	0.032	0.028	0.029
No. of individuals	650,745	650,745	581,277	516,431	232,743	191,883
N	2,293,475	2,293,475	1,860,035	1,493,086	584,780	422,065

Panel B. Sample Selection I

	$\Delta\ln(c_{it+h})$					
	$h=0$	$h=0$	$h=1$	$h=2$	$h=3$	$h=4$
Variable	(1)	(2)	(3)	(4)	(5)	(6)
Δ Interest payment/Out. debt	-0.031***	-0.032***	-0.046***	-0.031***	-0.022***	-0.017***
	(-49.26)	(-50.41)	(-69.18)	(-45.67)	(-31.29)	(-25.67)
Controls	N	Y	Y	Y	Y	Y
Region-year FE	Y	Y	Y	Y	Y	Y
Adj. R^2	0.021	0.026	0.026	0.021	0.022	0.018
No. of individuals	229,738	229,738	217,196	212,688	212,614	230,222
N	517,730	517,730	474,176	454,880	453,081	495,262

Panel C. Sample Selection II

	$\Delta\ln(c_{it+h})$					
	$h=0$	$h=0$	$h=1$	$h=2$	$h=3$	$h=4$
Variable	(1)	(2)	(3)	(4)	(5)	(6)
Δ Interest payment/Out. debt	-0.035***	-0.035***	-0.042***	-0.029***	-0.024***	-0.019***
	(-71.21)	(-71.89)	(-86.98)	(-59.87)	(-32.42)	(-25.70)
Controls	N	Y	Y	Y	Y	Y
Region-year FE	Y	Y	Y	Y	Y	Y
Adj. R^2	0.018	0.024	0.027	0.024	0.026	0.023
No. of individuals	309,718	309,718	307,283	305,938	163,517	162,181
N	768,216	768,216	743,926	732,245	374,935	371,717

The level of observation is the individual-year level it, based on the sample of all individuals with F3 and/or F5 adjustable-rate mortgages from 2009 to 2018. The dependent variable is the log change in consumption (winsorized at the 0.5% and 99.5% levels) of individual/borrower i (in region j) from year $t-1$ to $t+h$, where h varies from 0 (in columns 1 and 2) to 4 (in the last column). The sample includes all observed values for consumption. $\Delta Interest\ payment_{it}/Outstanding\ debt_{it-1}$ denotes the change in mortgage payments, scaled by the previous year's outstanding debt, for borrower i from year $t-1$ to year t (in %). The panels differ from the main analyses reported in Table 2 in the following way: The sample in Panel A includes all observed values for consumption, including person-year observations for which imputed consumption is negative or the absolute year-on-year consumption growth is larger than

50%. The sample in Panel B is restricted to individuals with F5 adjustable-rate mortgages for whom the dependent variable is also available in $t + 4$. The sample in Panel C includes all individuals with F3 and/or F5 adjustable-rate mortgages but is limited to individuals for whom the whole range of monetary-policy changes ($\Delta Interest\ payment_{it}/Outstanding\ debt_{it-1}$) is available throughout the sample period.

Table A.4: Consumption Response to Changes in Mortgage Payments—Robustness III

Panel A. Symmetry of Effect

	$\Delta\ln(c_{it+h})$					
	$h=0$	$h=0$	$h=1$	$h=2$	$h=3$	$h=4$
Variable	(1)	(2)	(3)	(4)	(5)	(6)
Δ Interest payment/Out. debt	-0.020***	-0.022***	-0.024***	-0.018***	-0.010***	-0.010***
	(-17.74)	(-19.66)	(-20.60)	(-14.87)	(-6.15)	(-5.25)
Controls	N	Y	Y	Y	Y	Y
Region-year FE	Y	Y	Y	Y	Y	Y
Adj. R^2	0.009	0.014	0.014	0.014	0.017	0.017
No. of individuals	566,977	566,977	531,777	523,697	265,380	230,222
N	1,623,879	1,623,879	1,462,757	1,389,964	614,939	495,262

Panel B. Alternative Specification II

	$\Delta\ln(c_{it+h})$					
	$h=0$	$h=0$	$h=1$	$h=2$	$h=3$	$h=4$
Variable	(1)	(2)	(3)	(4)	(5)	(6)
Δ Interest payment/Out. debt	-0.038***	-0.039***	-0.044***	-0.030***	-0.023***	-0.019***
	(-81.09)	(-82.49)	(-88.86)	(-58.67)	(-30.21)	(-23.13)
Controls	N	Y	Y	Y	Y	Y
Region-year FE	Y	Y	Y	Y	Y	Y
Adj. R^2	0.018	0.023	0.028	0.025	0.024	0.022
No. of individuals	345,505	345,505	324,347	318,161	164,089	148,510
N	690,054	690,054	623,355	594,687	327,094	286,207

The level of observation is the individual-year level it, based on the sample of all individuals with F3 and/or F5 adjustable-rate mortgages from 2009 to 2018. The dependent variable is the log change in consumption (winsorized at the 0.5% and 99.5% levels) of individual/borrower i (in region j) from year $t-1$ to $t+h$, where h varies from 0 (in columns 1 and 2) to 4 (in the last column). $\Delta Interest\ payment_{it}/Outstanding\ debt_{it-1}$ denotes the change in mortgage payments, scaled by the previous year's outstanding debt, for borrower i from year $t-1$ to year t (in %). The panels differ from the main analyses reported in Table 2 in the following way: In Panel A, $\Delta Interest\ payment_{it}/Outstanding\ debt_{it-1}$ is set to non-zero only for positive values. In Panel B, the sample is limited to the first reset event per individual and all non-reset years prior to the respective year. The set of control variables, when included, comprises indicator variables for female individuals, higher education, individuals with kids and whether an individual is married, individual i's age, as well as individual i's disposable income, total wealth, the value of the house, and the value of the securities portfolio (all winsorized at the 0.5% and 99.5% levels) in year t. t-statistics in parentheses are based on robust standard errors clustered at the individual level.

Table A.5: Consumption Response to Lump-Sum Payments—Robustness II

Panel A. Robustness to Outliers

	c_{it+h}					
	$h=0$	$h=0$	$h=1$	$h=2$	$h=3$	$h=4$
Inheritance	0.557***	0.538***	0.040***	0.068***	0.073***	0.064***
	(22.05)	(18.92)	(3.06)	(4.34)	(5.48)	(4.43)
Controls	N	Y	Y	Y	Y	Y
Individual FE	Y	Y	Y	Y	Y	Y
Region-year FE	Y	Y	Y	Y	Y	Y
Adj. R^2	0.41	0.49	0.37	0.37	0.36	0.38
No. of individuals	19.424	11,936	11,806	11,749	11,680	11,644
N	81,728	46,438	44,754	43,690	42,558	41,700

Panel B. All Inheritances

	c_{it+h}					
	$h=0$	$h=0$	$h=1$	$h=2$	$h=3$	$h=4$
Inheritance	0.329***	0.309***	0.163***	0.073***	0.051***	0.037***
	(22.72)	(19.44)	(14.46)	(14.95)	(13.06)	(7.89)
Controls	N	Y	Y	Y	Y	Y
Individual FE	Y	Y	Y	Y	Y	Y
Region-year FE	Y	Y	Y	Y	Y	Y
Adj. R^2	0.60	0.63	0.55	0.51	0.52	0.53
No. of individuals	28,613	17,588	17,624	18,162	18,156	18,058
N	93,480	54,476	54,071	57,340	56,781	55,692

Panel C. Deaths in First Half of Year

	c_{it+h}					
	$h=0$	$h=0$	$h=1$	$h=2$	$h=3$	$h=4$
Inheritance	0.393***	0.356***	0.288***	0.038**	0.055***	0.084***
	(11.04)	(9.17)	(9.26)	(1.21)	(3.16)	(3.15)
Controls	N	Y	Y	Y	Y	Y
Individual FE	Y	Y	Y	Y	Y	Y
Region-year FE	Y	Y	Y	Y	Y	Y
Adj. R^2	0.56	0.60	0.54	0.53	0.53	0.52
No. of individuals	9,063	5,279	5,333	5,304	5,248	5,230
N	31,032	17,078	17,247	17,039	16,576	16,268

The level of observation is the individual-year level it, based on the sample of all individuals with (deposit-only) inheritances anytime between 2003 and 2016. For each inheritance event, we keep all individual-year observations running up to the event, but only one post-inheritance observation, either that in the same year t or in one of the following four years. Accordingly, the dependent variable is consumption (winsorized at the 0.5% and 99.5% levels) of individual/heir i (in region j) in year $t+h$, where h varies from 0 (in columns 1 and 2) to 4 (in the last column). The panels differ from the main analyses reported in Table 2 in the following way: The sample in Panel A includes all observed values for consumption, including person-year observations for which imputed consumption is negative or the absolute year-on-year consumption growth is larger than 50%. The sample in Panel B includes all inheritance events, i.e., it is not limited to deposit-only inheritances. The sample in Panel C uses only (deposit-only) inheritance events resulting from deaths in the first half of each year. The set of control variables, when included, comprises indicator variables for female individuals, higher education, individuals with kids and whether an individual is married, individual i's age, as well as individual i's disposable income, total wealth, the value of the house, and the value of the securities portfolio (all winsorized at the 0.5% and 99.5% levels) measured in the same year as the dependent variable. t-statistics in parentheses are based on robust standard errors clustered at the individual level.

B The Danish Mortgage System

In this section, we provide a brief description of the Danish mortgage system and the specific features of Danish adjustable-rate mortgages (ARMs) that we use to identify the consumption effect of a change in the monetary-policy rate.

Established in 1797, the Danish mortgage system relied on covered bond financing since its foundation: mortgage banks issue bonds based on large pools of mortgage borrowers. There are three main characteristics of the Danish mortgage system:

a) **Loans are granted against mortgages on real property:**
Mortgage banks issue mortgage bonds, based on pools of borrowers who pledge their buildings as collateral, subject to strict loan-to-value (LTV) limits.[1]

b) **Mortgage banks operate subject to a balance principle:**
The so-called balance principle of Danish mortgage banks implies that loans are financed by the issuance of mortgage bonds. This implies that the value of outstanding bonds matches the value of the loans (at origination), and interest payments on loans match payments to bondholders. In this way, the balance principle essentially removes financial risks from the balance sheet of mortgage banks. Danish mortgage banks, thus, do not engage in speculative activity, as, for instance, U.S. mortgage banks did prior to the financial crisis of 2008.

c) **Interest rates are entirely market driven:**
Investors buy the mortgage bonds, and the price investors pay for the bonds solely determines the interest rate on the mortgage loan. This implies that all investors pay the same interest rate, given the same type of loan. For instance, whether the borrower has a strong credit history or not, the interest rate paid on the loan is the same and not negotiable. Similarly, the borrower can always observe the market value of her loan by observing the bond price on the market. Because no Danish mortgage bank has failed since the foundation of the system in 1797, the interest rate (on the same type of mortgage) is for all practical purposes identical across mortgage banks. Mortgage banks thus cannot price-discriminate between borrowers, and interest payments from borrowers are directly passed on to bondholders. It is in particular the latter feature of the Danish mortgage market that we rely upon in our examination of the effect of changes in mortgage rates on household consumption. That is to say, mortgage rates cannot change because a mortgage bank for some discretionary reason wants to change the interest rate; the interest rate only changes when bond-market conditions do, e.g., when the monetary-policy rate changes.

The size of the Danish mortgage bond market corresponds to approx. 150% of the size of Danish GDP. This makes it one of Europe's largest bond markets (not only in relation to

[1]Limits on loan-to-values depend on the type of loan: 80% of the value of a residential house/apartment can be financed by mortgage loans, 75% for non-residential summer houses, 70% for agriculture/forestry buildings, and 60% for commercial property. We focus on loans to residential housing in this paper.

GDP but also in absolute terms, i.e., in terms of the nominal value of outstanding bonds) and the largest covered-bond market in Europe (in terms of the value of outstanding bonds). Danish mortgage bonds are listed.

Danish mortgage banks are not allowed to take deposits.[2] By law, mortgage banks are required to finance all loans by issuing bonds (balance principle). The main role of mortgage banks is to issue mortgage bonds and transfer flows of funds from investors to borrowers (when loans are granted) and from borrowers to investors (when loans are paid bank and interest payments are made). In the end, the balance principle, strict regulation, and strict loan-to-value limits enabled the Danish mortgage system to fare well and retain its liquidity even during periods of severe stress in the financial system. For instance, Danish mortgage bonds remained as liquid as triple-AAA rated Danish government bonds during the financial crisis of 2008 (Dick-Nielsen and Gyntelberg, 2020). Not least for these reasons, analysts and academics have argued for a reformation of the U.S. mortgage system along the lines of the Danish mortgage system (Soros, 2008; Campbell, 2013).

B.1 Adjustable-Rate Mortgages

Adjustable-rate mortgages (ARMs) were introduced in Denmark in 1996, initially available for non-residential loans only but subsequently also made available for residential financing.[3] ARMs became particularly popular among households during the period leading up to the financial crisis of 2008-09. Figure B.1 shows the outstanding volume of households' fixed-rate and adjustable-rate mortgages since 2003 and the fraction of ARMs out of total outstanding household mortgages (right-hand scale). The aggregate data in Figure B.1 are from the macro statistics of Statistics Denmark (our microdata on households' mortgage choice start in 2009).

Prior to the run-up to the financial crisis, in 2003 ARMs accounted for approximately 28% of households' mortgage financing, increasing to almost 70% in 2012. The growth in total outstanding mortgage debt from 2003 through 2015 is solely due to growth in ARMs. The value of outstanding fixed-rate mortgage has remained more or less constant. Since 2012, the fraction of ARMs has fallen, to reach approximately 50% of all residential mortgages in 2020. During our period of analysis, $2009 - 2018$, the use of ARMs increased from $2009 - 2012$, from app. $\approx 44\%$ to $\approx 70\%$ of all household mortgages. Since 2012, the use of ARMs has declined whereas the use of fixed-rate mortgages has increased. The total outstanding amount of household mortgages corresponds roughly to the size of Danish GDP in 2020 (2,000bn DKK).

Different types of ARMs are available, differentiated by their interest-reset frequency. The interest rate is reset every X years, where $X = 0.25, 0.5, 1, 2, ..., 10$, i.e., the shortest reset interval is three months and the longest ten years. The ARMs are typically called

[2] Deposits are made at non-mortgage banks. There are thus important legal differences between Danish mortgage banks and Danish non-mortgage banks.

[3] Before 1996, only fixed-rate mortgages were allowed. Like in the US, Danish fixed-rate mortgages are typically 30-year loans with a prepayment option.

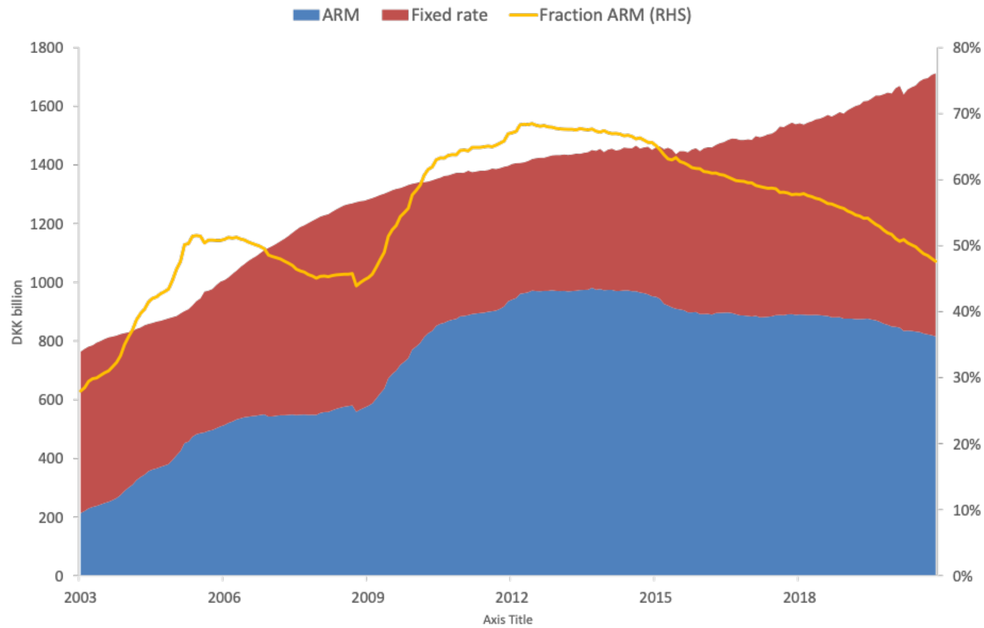

Figure B.1: Outstanding Fixed-rate and Adjustable-rate Mortgages in DK, 2003-2020 The figure shows the outstanding amounts of fixed-rate and adjustable rate mortgages in Denmark (left-hand scale) and the fraction of ARMs (right-hand scale).

"Flex-loans" or F-loans. The F0.5, F1, F3, and F5 groups are by far the largest in terms of use and outstanding volume. The maturity of the loan is typically 30 years, i.e., the investor buys a 30-year mortgage bond with an interest rate reset every X years.

The reset interest rates are determined at yearly auctions, historically taking place in late November/early December.[4] The interest rates determined at the auctions apply as of January 1 the following year. Thus, for an F1 loan, the underlying bonds are renewed every year, i.e., every year, the underlying one-year mortgage bonds expire at December 31 and are replaced by new one-year mortgage bonds as of January 1. The borrower thus pays one one-year interest rate until December 31, and another one-year interest rate as of January 1 the following year, where the new interest rate is determined at the auction in late November/early December. The same principle applies for the F3 loans where the underlying bonds are three-year bonds. At the end of a three-year period, three-year mortgage bonds are sold at auctions in late November/early December and the old three-year bond expires on December 31 and is replaced by a new three-year bond on January 1 the following year. The three-year interest rate the borrower is paying is thus renewed every three year in an F3 loan. The same principle applies to F0.5, F5, F10, etc., loans.

Given that we identify the consumption effect of mortgage-rate changes by comparing consumption responses of borrowers who face a mortgage-rate reset in one year to borrowers

[4]In recent years, auctions have been spread out during the year, to reduce refinancing risk. Still, the largest fraction of bonds are sold at the November/December auctions.

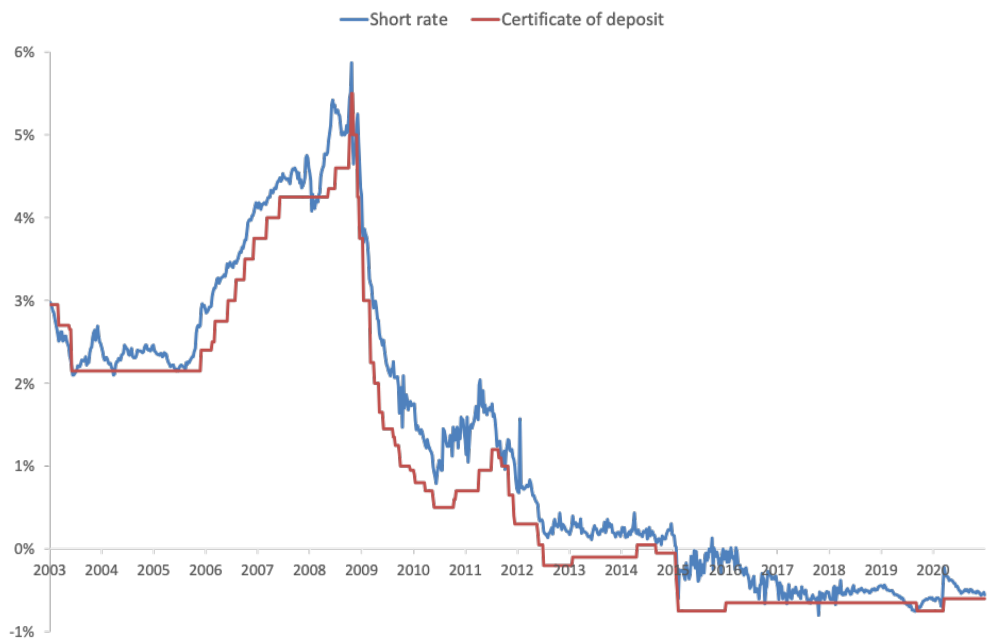

Figure B.2: ARM Mortgage Rate and Monetary-Policy Rate, 2003-2020
The figure shows the yield on short-term mortgage debt and the monetary-policy rate of the Danish central bank (Certificate of Deposit).

who do not face a reset that same year, we cannot use F0.25, F0.5, and F1 loans, as these are reset at least once within a year, implying there is no control group (in our annual data). Hence, in our analysis we focus on F3 and F5 loans. F3 and F5 loans correspond to about 45% of the total amount of ARMs in our sample.

A crucial feature of our setting is that the yield mortgage borrowers pay on ARMs is closely linked to the monetary-policy rate of the Danish central bank, which in turn is linked to the policy rate of the European Central Bank due to the fixed exchange rate between the Danish kroner and the euro. Figure B.2 shows the yield on short-term mortgage debt, which is the average yield on ARMs, and the Danish monetary-policy rate (the rate on certificates of deposits) on a weekly basis since 2003. The monetary-policy rate is lower than the short-term mortgage yield for good reasons (certificates of deposits have a one-week maturity, whereas ARMs have longer maturities), but they follow each other closely even during times of turmoil. For instance, the monetary-policy rate was hiked during the period leading up to the financial crisis, in particular in the fall of 2008 when the fixed exchange-rate policy was under pressure, but has been lowered thereafter. Mortgage rates have followed these movements closely. The fact that yields on mortgage bonds did not rise relative to the monetary-policy rate is again a testimony to the fact that Danish mortgage bonds are viewed as safe and liquid even during times of turmoil, as mentioned above, and changes in mortgage rates are due to changes in monetary-policy rates. Since 2012, the monetary-policy rate has been negative and short-term mortgage rates have been negative since 2015.[5]

We analyze the $2009 - 2018$ period, which was characterized by significant cuts in the monetary-policy rate and consequently in yields on ARMs. The monetary-policy rate exceeded 5% in late 2008, fell to basically 0% in 2015, and is negative by the end of our sample period in 2018. The yield on ARMs followed a similar path.

[5]Note that a negative mortgage rate does not mean that the individual household does not pay anything on its mortgage, as the borrower pays a fee on top of the mortgage rate to the mortgage bank. The fee compensates the mortgage bank for administration costs, accumulation of equity capital, etc. The size of the fee depends on the loan-to-value (LTV) ratio of the mortgage and the mortgage type (e.g., it is higher for ARMs than for fixed-rate loans), but is independent of borrower characteristics and monetary policy. Since 2012, the fee has been close to 1% for ARMs, depending on the LTV ratio, resulting in a positive total mortgage payment in spite of a negative yield on the mortgage bond.

C Treatment of Inheritance Data

To yield variation in windfall gains, we identify individuals that receive an inheritance due to the death of a close relative. The type and amount of inheritance are derived from the deceased relative's asset portfolio.

In the Danish Inheritance Act passed in 1964, relatives are divided into three subgroups: the spouse, children, and grandchildren of the deceased (Group 1), the legal parents and siblings of the deceased (Group 2), and grandparents and their children (Group 3). The default rule is that Group 1 inherits, but if there are no living Group 1 (or 2) relatives, Group 2 (3) relatives inherit. Within Group 1, the default sharing rule is that the spouse and children divide the estate of the deceased evenly (before 2008, one-third was assigned to the spouse and two-thirds to the children), unless the spouse delays the children's inheritance until their death.

We focus on deaths where Group 1 relatives exist and the deceased had no spouse (due to being widowed, divorced/separated, or never having married). This simplifies the analysis as the default sharing rule for children is an even split.[6] The default sharing rule can be changed with a will, but not to less than 25% of what would have been inherited according to the default rule (50% before 2008). In addition, less than 10% have a will and, thus, we generally assume that the default rule applies.

Group 1 is subject to an estate tax of 15% if the net wealth of the estate exceeds 301,900 DKK. This tax applies to all assets and as unrealized capital gains are furthermore not taxed directly, there is no tax motive to keep or liquidate specific assets (Andersen and Nielsen, 2010). The Probate Court will soon after an individual's death take control of the deceased's assets to meet the liabilities, and will then transfer the remainder to the beneficiaries according to the default rule or a will.

In particular, we consider the subset of sudden deaths to exploit the fact that it is unlikely that the beneficiaries anticipate the timing of inheritances stemming from such sudden deaths. We follow Andersen and Nielsen (2010) in defining sudden deaths. They distinguish between natural deaths and non-natural deaths. Natural deaths are due to disease and declining health, while non-natural deaths are caused by accidents and violence. We consider the following natural death causes to constitute sudden deaths (ICD-10 code in parentheses): acute myocardial infarction (I22-I23), cardiac arrest (I46), congestive heart failure (I50), stroke (I60-I69), and sudden death where the cause is unknown (R95-R97). We consider most non-natural death causes to constitute sudden deaths (vehicular accidents: V00-V99; exposure to harmful substances and forces of nature: X00-X59; death related to drugs or chemical substances: X86-X90), but exclude suicide and violent assault as those could potentially be anticipated.

[6] Most widows choose to delay the inheritance to their children, so when the widow dies, the children inherit the entire shared estate of both their parents (Andersen and Nielsen, 2010). Additionally, some individuals might inherit twice due to their unmarried parents dying at different times. We exclude these beneficiaries from our analysis.

D Alternative Approaches to Estimating the Aggregate Consumption Response to Monetary Policy in Denmark

D.1 "Internal-instrument" SVAR

In this section, we summarize the theoretical underpinnings of the "internal-instrument" recursive SVAR framework of Plagborg-Møller and Wolf (2021). The internal-instrument recursive SVAR is similar to the proxy SVAR (Miranda-Agrippino, 2016; Stock and Watson, 2018) in achieving identification of structural shocks by using external proxies, estimated outside the system of endogenous variables, to instrument reduced-form VAR innovations. Suppose y_t is an n-dimensional vector of endogenous observables, and its responses to the structural shocks in e_t are expressed as:

$$y_t = [A(L)]^{-1}u_t = C(L)Be_t, \tag{D.1}$$

where $C(L)B$ are the structural impulse response functions, u_t are the reduced-form innovations, $C(L)$ is the matrix of reduced-form coefficients in the VAR, and B is the impact matrix containing contemporaneous transmission coefficients from the structural shocks to u_t.

While in a proxy SVAR the aim is to identify the column of B which links the reduced-form innovations to the structural shock of interest, Plagborg-Møller and Wolf (2021) suggest to include the proxy variable z_t ordered first within the VAR system in y_t, and then use a classic recursive scheme. The advantage of this procedure is to recover the right *relative* impulse responses even in the presence of non-invertibility issues.[7] This is not true for the classic SVAR-IV, where non-invertibility might cause misidentification of the shock $\tilde{e}_t \neq e_t$.

We pursue the internal-instrument SVAR approach and use the vector $\{z_t, y_t'\}'$ in our system. Let y_t and x_t be the variable of interest and the instrumented variable, respectively. Plagborg-Møller and Wolf (2021) show that the *relative* impulse response function with respect to the first shock $\theta_{y,h}/\theta_{x,0}$ is correctly identified for each horizon h.[8] Successful identification of the contemporaneous transmission coefficients relies on correctly specifying the VAR model and choosing a valid proxy. Although the evidence collected suggests that the VAR specification matters in practice, enriching the VAR information set helps to produce cleaner innovations, thereby mitigating potential distortions. Here, we focus on a monetary-policy shock $e_t^{\bullet}$, which we identify using the proxy variable z_t, as provided by the Trilemma IV of Jordà et al. (2020).

[7] A shock is invertible if it is a linear combination of the present and past values of the VAR variables, i.e., a contemporaneous linear combination of the VAR residuals.

[8] In this case, the matrix B in (D.1) is a simple Cholesky matrix.

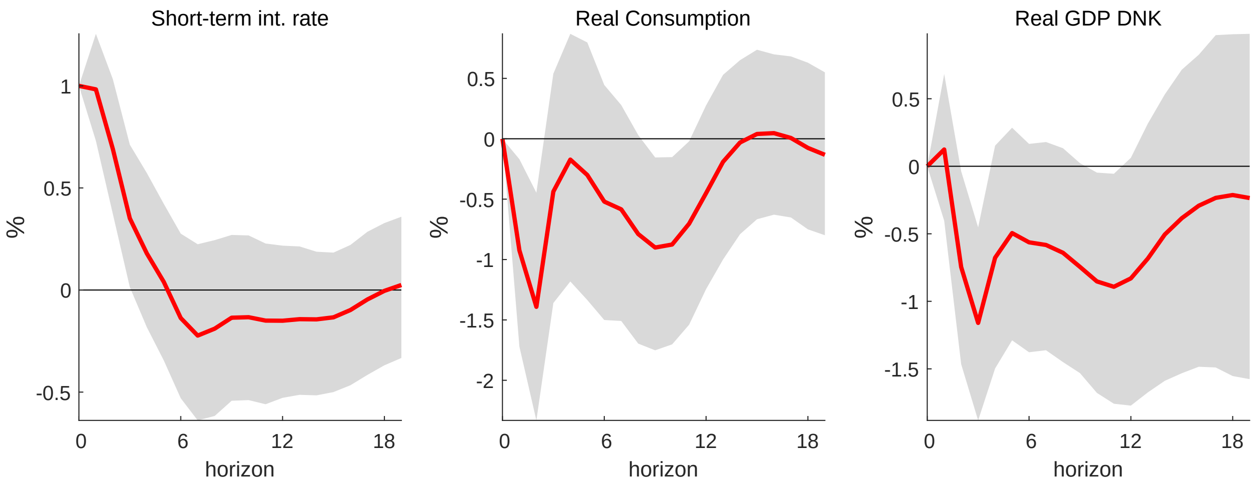

Figure D.1: Response to a Monetary-Policy Shock—Zero Short-Run Restriction This figure plots the impulse responses of the interest rate, real consumption, real GDP, and CPI to a 1 p.p. increase in interest rates. The responses are estimated according to equation (D.1), using a zero short-run restriction (Cholesky). The 90% bootstrapped confidence bands are obtained through 1,000 replications using the "wild bootstrap" of Gonçalves and Kilian (2004).

D.2 Zero Short-Run Restriction

We next relax the assumption of pegged exchange rates, and replicate the analysis using a standard zero short-run restriction, i.e., assuming that monetary policy has no contemporaneous effect on the other variables, ordering the interest rate as last in a Cholesky triangularization. The results are in Figure D.1. Comparing them to our SVAR baseline in Figure 5, consumption and GDP have smaller drops, around 1.5% after a few quarters. One interpretation is that the zero short-run identification might capture only a small portion of the actual effect of a monetary-policy shock, since individuals in Denmark are likely to respond to ECB actions even before the Danish Central Bank reacts by adjusting its rates.

D.3 LP-IV

To trace the effect of a 1 p.p. increase in the monetary-policy rate on real consumption and GDP, we estimate impulse response functions (IRFs) using a panel local projections instrumental variable (Panel LP-IV) approach (Jordà, 2005; Stock and Watson, 2018).

Because Denmark pegged its exchange rate to the euro (and the Deutsche Mark before), its interest rate follows that of the euro area and—before the introduction of the euro—Germany, which is the largest economy in the euro area. Otherwise, as prescribed by the trilemma of international finance (Obstfeld and Taylor, 1997; Obstfeld et al., 2005), there would have been unsustainable capital outflows. Moreover, since changes in the base country's interest rate are mainly determined by the base country's economic conditions, their variation is less related to the economic fundamentals in Denmark.

The Trilemma instrumental variable, z_t, for local policy rate changes in Denmark, Δr_t,

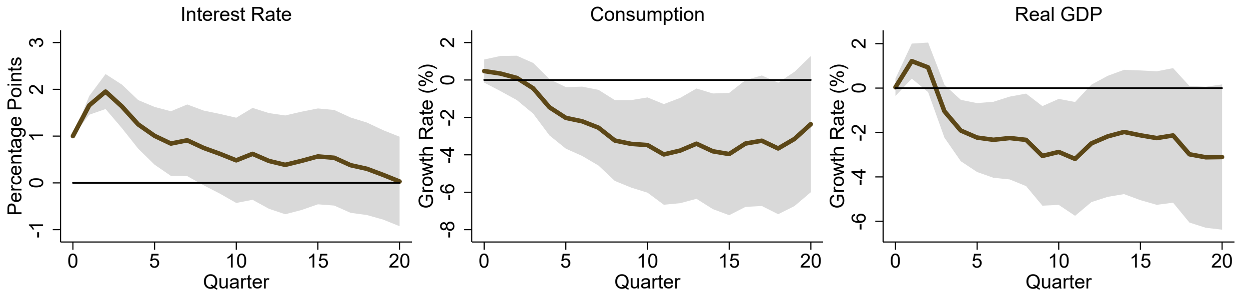

Figure D.2: Response of Real Consumption and GDP to a 1 p.p. Increase in the Interest Rate—Trilemma IV

This figure plots the response of the interest rate, real consumption, and real GDP, estimated according to (D.3), using the Trilemma IV. Standard errors display a 90% confidence band.

is defined as follows:

$$z_t \equiv (\Delta r_t^{\text{Euro area/Germany}}) \times k_{c,t}, \tag{D.2}$$

where t is the year index, $\Delta r_t^{\text{Euro area/Germany}}$ is the change in the interest rate of the base economy, and k_t is an index from 0 to 1 that reflects the degree of capital mobility (Quinn et al., 2011).

We then estimate impulse response functions as follows:

$$\Delta \mathrm{Y}_{t+h} = \beta_h \Delta r_t + \zeta_h \vec{W}_t + \epsilon_{c,t+h} \forall h \in \{0, 1, ..., 20\}, \tag{D.3}$$

where Δr_t is the change in the Danish interest rate which is instrumented by the Trilemma IV z_t, and $\vec{W}_{c,t}$ is a vector of control variables including four lags of inflation, the interest rate, real consumption, real GDP growth for Denmark, and including contemporaneous real GDP growth and inflation as well as four lags thereof for the euro area. The dependent variable $\Delta \mathrm{Y}_{t+h}$ can be either the growth rate of real consumption or GDP in percent, or the change in the interest rate in percentage points, between quarter $t-1$ and quarter $t+h$, therefore reflecting cumulative changes.

The results are in Figure D.2. The interest rate peaks at 1.95 p.p. in the second quarter. Aggregate consumption drops by up to 3.2%, materialized over the first eight quarters. This is mirrored by the response of real GDP, which drops by up to 2.3% over the first eight quarters.

E Further Dimensions of Heterogeneity

E.1 Transfer Income

Table E.1: Heterogeneity—Transfer Income

		% w/ mortgage	% w/ mortgage reset	Avg. reset (yrs)	Avg. agg. cons.	Cumulative Δ cons. (bn DKK) t	$t+1$	$t+2$
Low transfer	Q1	66%	47%	2.72	238	2.12	2.35	1.63
	Q2	43%	22%	2.73	217	1.02	1.14	0.78
	Q3	20%	8%	2.85	168	0.30	0.29	0.17
	Q4	17%	8%	2.97	140	0.25	0.28	0.17
High transfer	Q5	21%	14%	2.82	81	0.35	0.40	0.28
NA		76%	62%	3.34	108	1.28	1.42	0.95
Total (sum):					952	5.31	5.88	3.98
Macro-equivalent stimulus (heterogeneous MPCs)								
Total:					**3,836**			
Macro-and-distributional-equivalent stimulus								
Total:					**4,788**			

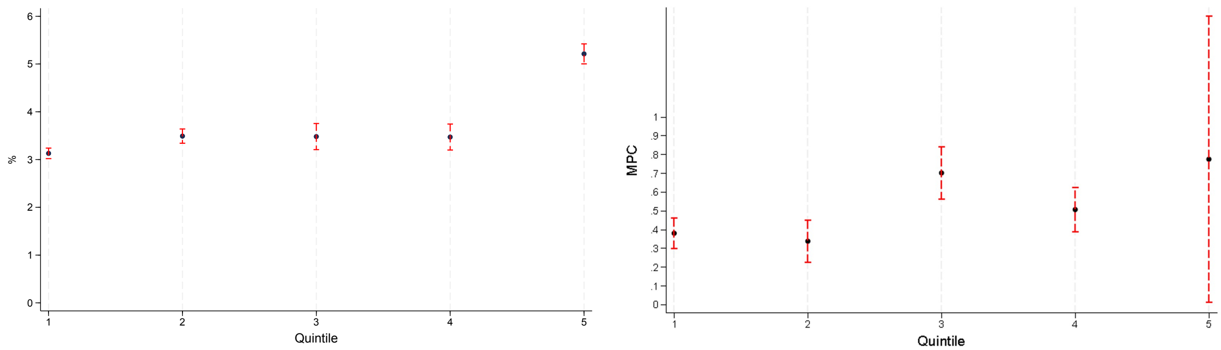

Figure E.1: Monetary-Policy and Fiscal-Policy Heterogeneity—Transfer Income
The left panel plots the estimates from the contemporaneous consumption-response regression in column 1 of Table 2, Panel A, separately for each transfer-to-total-income quintile, alongside 99% confidence intervals. The right panel plots the estimates from the contemporaneous consumption-response regression in column 1 of Table 2, Panel B, separately for each transfer-to-total-income quintile, alongside 99% confidence intervals. Transfer income is defined as the sum of any kind of public transfer income (including unemployment benefits, children's allowance, etc.) and paid out private pension.

E.2 Age

Table E.2: Heterogeneity—Age

		% w/ mortgage	% w/ mortgage reset	Avg. reset (yrs)	Avg. agg. cons.	Cumulative Δ cons. (bn DKK) t	$t+1$	$t+2$
Low age	Q1	6%	1%	2.62	103	0.02	0.03	0.01
	Q2	38%	19%	2.66	190	0.95	1.06	0.67
	Q3	52%	34%	2.69	240	1.82	2.02	1.40
	Q4	41%	25%	2.83	200	0.97	1.05	0.73
High age	Q5	22%	13%	3.09	111	0.35	0.40	0.27
NA		76%	62%	3.34	107	1.27	1.41	0.95
Total (sum):					952	5.39	5.97	4.03
Macro-equivalent stimulus (heterogeneous MPCs)								
Total:					**4,686**			
Macro-and-distributional-equivalent stimulus								
Total:					**5,726**			

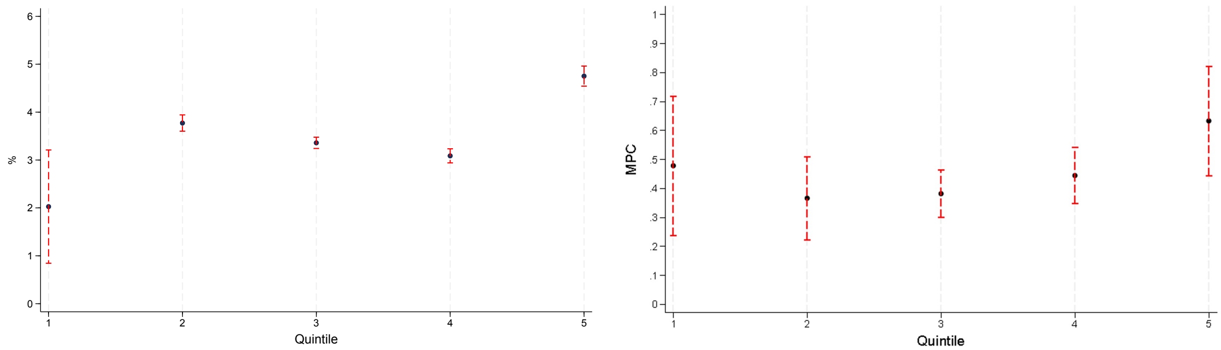

Figure E.2: Monetary-Policy and Fiscal-Policy Heterogeneity—Age
The left panel plots the estimates from the contemporaneous consumption-response regression in column 1 of Table 2, Panel A, separately for each age quintile, alongside 99% confidence intervals. The right panel plots the estimates from the contemporaneous consumption-response regression in column 1 of Table 2, Panel B, separately for each age quintile, alongside 99% confidence intervals.

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