The Last Mile of Monetary Policy: Inattention, Reminders, and the Refinancing Channel*

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Abstract

We test whether targeted communication can reduce the attention frictions that inhibit accommodative monetary policy transmission to the household sector. Analyzing a field experiment with 12,000 Irish households, we find that reminder letters increase refinancing 76%, from 8.9% to 15.7% in contrast to small effects of disclosure design improvements. Extending a model of inattentive financial decision-making to allow for experimental treatment effects, we estimate reminders increase refinancing by increasing borrower attentiveness by over 60%. Our results illustrate that direct communication can stimulate borrower consumption even when policy rates are constrained by a lower bound or set in a monetary union.

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1 Introduction

Across many countries, researchers have documented a widespread "failure to refinance," where substantial savings available to mortgage holders through refinancing remain unclaimed.[1] From a macroeconomic perspective, suboptimal refinancing may significantly limit the power of the refinancing channel of monetary policy transmission (Beraja et al., 2019; Di Maggio, Kermani and Palmer, 2020a; Cloyne et al., 2020).[2] The modulation of monetary policy transmission by refinancing frictions is an example of what we term the "last-mile problem" of monetary policy because it inhibits the delivery of accommodative policy to the real economy.[3] From a microeconomic perspective, suboptimal refinancing implies many households are overpaying mortgage interest and foregoing current or future consumption as a result (Abel and Fuster, 2021).

Building on a growing body of work that finds that reminders can focus otherwise-inattentive consumers on a task, in this paper we demonstrate experimentally how reminders can be a monetary or regulatory tool supporting attentive refinancing decision-making. We analyze a field experiment conducted by a large retail bank in Ireland testing whether behavioral design changes to mandatory consumer disclosures prompt borrowers to take-up beneficial internal refinancing opportunities. Reminders significantly increase refinancing in every treatment arm and borrower subgroup, with the best-performing reminder treatment increasing the probability of internal mortgage refinancing by 79%, from 8.9% to 15.7%. By contrast, we see only modest improvements from non-reminder disclosure design enhancements, consistent with small effects found by the other two mortgage refinancing trials of which we are aware (neither of which tested reminders) and similar to overall inattention to disclosure (e.g., Adams, Hunt, Palmer and Zaliauskas, 2021). A conservative back-of-the-envelope calculation suggests that our reminder treatment generates €42 of mortgage borrower consumption for every €1 spent on reminders (€605 per refinancing household), highlighting the value of improving the last mile of monetary policy delivery.

We interpret our treatment effects through the lens of the Andersen, Campbell, Nielsen and Ramadorai (2020) behavioral model of inattentive refinancing. After extending their

[1] See evidence of mortgage borrowers' low take-up of seemingly advantageous refinancing opportunities in the US (Campbell, 2006; Keys et al., 2016; Agarwal et al., 2016; Johnson et al., 2019), Italy (Bajo and Barbi, 2018), Denmark (Andersen et al., 2020), the UK (Financial Conduct Authority, 2019), Ireland (Byrne et al., 2020), and Australia (Australian Competition and Consumer Commission, 2018).

[2] Below, we illustrate this transmission friction by documenting how US outstanding mortgage interest rates react to monetary policy rates slowly and incompletely relative to interest rates on new mortgages.

[3] In supply-chain management and telecommunications the "last-mile problem" refers to the disproportionate difficulty of delivery to a final user in contrast to the relative ease of intermediate transmission. We mention research documenting what we see as other last-mile problems for monetary policy transmission in section 2.1 below.

framework to allow for treatment effects on attention, maximum likelihood estimates of the model imply that reminders have large attention effects, increasing the share of attentive households from 24% to 39%. We use the estimated model to contrast the partial-equilibrium relative effectiveness of cutting interest rates with sending refinancing reminders.[4] Holding baseline inattention fixed, we find that the best performing communication combination increases refinancing by significantly more than would be achieved by a 100 bp decrease in mortgage rates. To demonstrate the representativeness of our setting, we estimate the model without experimental treatment effects on US data and also compare to estimates from Denmark, finding similar results. Heterogeneity exercises below demonstrate that this effect is unlikely to be an artifact of the contemporaneous Covid pandemic.

These findings contribute to a nascent literature exploring the potential effectiveness of direct central bank communication. The simplicity of reminder letters suggests that they could be a cost-effective communication tool to help policy rates reach the household sector directly. Moreover, refinancing reminders also have the potential to be effective even when conventional monetary tools are de facto constrained by a lower bound or a monetary union, both of which limit the flexibility of monetary policymakers to decrease policy rates. Research also finds that conventional monetary policy is less powerful in recessions than expansions, further motivating the development of alternative accommodative tools (e.g., Tenreyro and Thwaites, 2016).

The failure to refinance puzzle continues to attract considerable academic and policy attention for at least three reasons.[5] First, recent work shows the refinancing channel of monetary policy transmission to be quite significant such that frictions impeding refinancing have first-order implications for effective monetary stimulus. See Amromin et al. (2020) for a review and Altavilla et al. (2020) for recent evidence of subdued pass-through of European Central Bank (ECB) policy rates to retail interest rates. Second, there are financial stability implications which potentially arise from low mortgage switching rates. Failing to realize substantial potential savings on mortgage repayments from refinancing leaves borrowers with an elevated debt service ratio and more vulnerable to mortgage distress from income shocks (Gerardi et al., 2013; Giordana and Ziegelmeyer, 2020; European Commission, 2015, 2019). Third, a low propensity of customers to switch mortgage providers could both diminish the incentive for providers to compete on the basis of price and send a discouraging signal to

[4] As we discuss in section 5.3 below, these partial-equilibrium arguments are unlikely to be reversed by the typical magnitude of the general equilibrium effects estimated in the mortgage literature.

[5] As we discuss below, even though there are several rational reasons why a household may not refinance in the face of interest-rate savings, the evidence—including our results—suggests that behavioral factors are important in explaining slow refinancing. Our paper is the first to demonstrate that some of these frictions are partially addressable.

potential entrants who might otherwise bring competition to the market (Aghion and Bolton, 1987; Farrell and Klemperer, 2007).

To illustrate the connection between this work on pass-through to refinancing frictions and the last-mile problem, we construct the time series of average interest rates on new and outstanding mortgages in the US using CRISM data (as in Di Maggio et al., 2020a). Figure 1 plots these interest rates against US policy rates (the effective Federal Funds Rate). All three series are highly correlated in levels. However, the figure shows that while the interest rates on *new* originations follow policy rates reasonably closely, *outstanding* interest rates move slowly and incompletely with policy rates. At the quarterly frequency, while changes in the Federal Funds Rate have a pass-through coefficient to changes in new interest rates of 0.35 with an R^2 of 0.24, FFR changes have a pass-through coefficient to outstanding mortgage rates of 0.03 with an R^2 of 0.05. Figure 2 demonstrates similar dynamics between outstanding and new mortgages in Ireland. The combination of many mortgages without indexed rates and the last-mile friction of inattention leads low policy rates to have limited impact on outstanding mortgage rates in both the US and Ireland. Figure 3 summarizes these arguments by contrasting the magnitude of refinancing effects from the experimental treatments with the implied effect of 100 bp lower mortgage rates holding attention fixed in Ireland, the US, and Denmark. Given these pass-through dynamics, our experimental results highlight the potential of non-monetary interventions by policymakers, including national central banks in a currency union, to stimulate the refinancing channel.

The experiment we study is the first large-scale refinancing experiment targeted at a wider population of outstanding mortgage holders instead of distressed or low-income mortgage borrowers. To our knowledge, only two previous papers undertake field experiments in the domain of mortgage refinancing. Johnson et al. (2019) carry out a series of field experiments (but not reminders) to encourage uptake by low- or negative-equity borrowers of the US Home Affordable Refinance Program. Keys et al. (2016), among other things, test for effects of mailed notices to 193 borrowers from lower-income communities in Chicago. The small effects we find of non-reminder disclosure design nudges replicate the findings of these other papers, helping confirm the external validity of our setting. However, we are the first to provide experimental evidence that inattention is a key refinancing friction, that such frictions can be addressed with statistically and economically meaningful experimental impacts on monetary policy transmission, and to document a role for central-bank communication.[6]

The paper proceeds as follows. In section 2, we contextualize our contribution in the rel-

[6]For example, the statistically insignificant effects of the treatments studied by Johnson et al. (2019) on applying to refinance range from -0.6 to +0.1 percentage points (pp) in contrast to our estimated refinancing effect of +6.8 pp.

evant literatures. Section 3 provides a brief overview of the Irish mortgage market, including a description of the regulation on which the experiment is based. Section 4 presents the experimental design and summarizes the data. Section 5 reports and discusses experimental treatment effects. In section 6, we extend the inattentive refinancing model of Andersen et al. (2020) to allow for disclosure effects, estimate treatment effects on attention, and contrast the effectiveness of targeted communication with rate changes. Section 7 concludes with back-of-the-envelope consumption estimates and qualifications.

2 Context in Literature

To explain the relevance of our contribution to several related literatures, we first situate our findings in the literature on monetary policy transmission to the real sector. The backdrop of generally imperfect and sluggish pass-through of monetary policy to the household sector heightens the importance of last-mile studies such as ours, which document policy interventions that could successfully strengthen monetary policy transmission. Next, we summarize the literature on central bank communication and argue that our results demonstrate that personalized messaging about refinancing opportunities is a promising policy tool. We then overview the literature on demand- and supply-side factors that impact household engagement with interest rate changes. Finally, we consider the literature on the effectiveness of reminders and consumer-facing disclosures, which shows that the design of communications matters for their capacity to prompt consumer engagement.

2.1 The Last-Mile Problem of Monetary Policy

Analogous to the high marginal cost of delivering a good or service to dispersed and possibly remote end-users, the last-mile problem of monetary policy is that frictions inhibit monetary stimulus from reaching its final destination in the real economy. A range of frictions could slow and reduce the pass-through of monetary policy shocks to retail interest rates or prevent final demand by households and firms from responding to interest rate changes even when they do adjust. In this section, we briefly review recent research documenting other frictions that we see as last-mile problems for monetary policy relative to the faster pass-through of policy rates to other yields in financial markets.

For example, the key mechanism behind the investment channel of monetary policy is that changes to benchmark interest rates change a firm's cost of capital and thereby affect its real investment decisions. However, many firms do not update their investment hurdle rates when nominal rates change, thus preventing their investment decisions from responding to

monetary policy (Gormsen and Huber, 2022). Other work shows that many households find it costly to acquire information on the menu of available retail interest rates on car loans and mortgages, reducing their responsiveness to policy rate changes when passthrough is uneven (Argyle et al., 2022; Kim, 2022). Similarly, households with low financial literacy are less responsive to rate changes when they fail to appreciate the connection between rate changes and their budget constraints (Blinder et al., 2022).

The degree to which monetary policy transmits to retail interest rates for households has also been explored in detail over recent decades. The importance of the refinancing channel of monetary policy has focused particular attention on mortgage rates (Calza et al., 2013; Di Maggio et al., 2020a; Amromin et al., 2020; Cloyne et al., 2020). In the US, Gertler and Karadi (2015), Gilchrist et al. (2015), and Wong et al. (2019) estimate contemporaneous pass-through coefficients from policy rates to mortgage rates of 0.17-0.68. However, the literature generally documents sluggish and heterogeneous policy interest rate pass-through across retail financial products and across countries, motivating research into policies that can successfully improve pass-through (Andries and Billon, 2016). Early research for the euro area found a typical pass-through rate of about 30% from policy rate changes to retail interest rates in the first month following a change and nearly 100% within 3-10 months, with business loan rates converging faster than loans to households (De Bondt, 2002). More recently, Altavilla et al. (2020) find sluggish and incomplete interest-rate adjustments to policy rates, with a medium-run pass-through coefficient of 0.65.

We study the last-mile problem of monetary policy within the context of the refinancing channel, with implications for why policy rate changes sluggishly filter through to household consumption and investment decisions. Relative to the literature above, our work provides experimental evidence that monetary policy pass-through to final demand is often limited because of the last-mile problem of household inattention. In so doing, we document the first intervention that significantly improved refinancing responsiveness and thus could increase the pass-through of monetary policy to the household sector.

2.2 Central Bank Communications

Our paper also contributes to a developing literature on central bank communication by demonstrating the effectiveness of a new form of messaging that central banks could use to provide non-monetary stimulus. We briefly review the frontier of this literature and explain its connection to our work.

Central bank communications affect economic outcomes through routine communications such as Federal Open Market Committee (FOMC) minutes and press releases, unconven-

tional measures such as forward guidance (McKay et al., 2016), and informal leaks (Vissing-Jørgensen, 2019).[7] Recent research has explored how both standard and non-standard central bank communications are ultimately processed by the general public. Evaluating the status quo, Haldane and McMahon (2018) find central banks' main communications relatively inaccessible to a wide audience in part due to linguistic complexity; typical central bank publications have reading grade levels equivalent to college-level (Hernández-Murillo et al., 2014). Lamla and Vinogradov (2019) find that FOMC announcements have little impact on consumers' perceptions and expectations of inflation or interest rates, with 65% of consumers in their data unaware of a FOMC announcement during the average announcement week. Coibion et al. (2022) find that while receiving communication about the Federal Reserve's inflation target affects household inflation expectations, the effects fade quickly. Binder (2017) and Blinder et al. (2022) offer progress reports detailing the lack of both policy and research consensus about optimal central bank communications with the general public, despite overall calls for more and better communication (e.g., Ehrmann et al., 2021). Such emerging communications strategies range from social media and music videos to listening events to reach different stakeholders and reduce the cost of acquiring and processing information. Overall, households seem to have a low desire to be informed about monetary policy and are inattentive to news linked to it except when adverse conditions arise.

Relative to this developing literature on central bank communication aimed at the general public, our paper is the first to demonstrate the potential for targeted, direct communication to households to have lasting real effects. In particular, we illustrate how the credit registries available at many central banks can be leveraged to support more attentive financial decision-making by households through customized communication. While the typical style of informational disclosure mandated by banking regulation appears relatively ineffective on its own, we show how simple reminder letters can strengthen the transmission of accommodative monetary policy to final demand for every subgroup of borrowers tested. Moreover, because such a tool could be deployed without control over interest rates or interest-rate expectations, it has the potential to be effective even when monetary policy is constrained by the zero-lower bound or because rates are set in a currency union.

2.3 Barriers to Refinancing

As cited above, a growing literature has documented under-refinancing in multiple countries. In the US, for example, Keys et al. (2016) find that 20% of households for whom refinancing

[7]See Blinder et al. (2008) for a survey of the literature on traditional central bank communication, which has often focused on the roles of communication to reduce financial market volatility and influence expectations.

would be advantageous have not refinanced, with a foregone annual savings of almost $2,000. Both supply- and demand-side factors can inhibit refinancing and thus the final delivery of monetary policy to the household sector—see Amromin et al. (2020) for a review. Supply-side barriers to refinancing can result from underwriting constraints that are binding for households most acutely during a recession (Beraja et al., 2019; Di Maggio et al., 2020a; DeFusco and Mondragon, 2020).

On the demand side, inattention, low financial literacy, present bias, and distrust have each been implicated in sluggish refinancing.[8] Andersen et al. (2020) show that Danish borrowers with lower education, income, housing wealth, and financial wealth are less likely to consider refinancing and less attentive to the financial incentive to do so. Similarly, Bajo and Barbi (2018) find that take-up of an attractive no-cost refinancing program in Italy was positively correlated with loan size, remaining term, age, wealth, experience with financial products, and financial literacy. Johnson et al. (2019) document the role played by borrowers' trust in financial institutions and their present bias, which discourages households from incurring time costs today for lower interest payments realized in the future. Agarwal et al. (2016) find a large role for financial sophistication in explaining suboptimal refinancing. Maturana and Nickerson (2018) find evidence of peer effects in refinancing, with coworkers providing salient and personally relevant information about the benefits and process of refinancing. Consistent with our findings, Keys et al. (2016) find that two-thirds of low-income survey respondents who received refinancing offers did not open the letters or "planned to call the loan officer but did not get around to it or were simply too busy to make the phone call."

Building on the literature studying barriers to refinancing, our contribution is demonstrating that inattention in financial decision-making is partially addressable through direct communication to households. In contrast to the prior literature that generally documents less responsive refinancing without providing effective or actionable prescriptions, we provide experimental evidence that the refinancing channel of monetary policy can be strengthened. The strong effects for all subgroups of a simple reminder letter and the small effects of disclosure design improvements suggest that inattention in the form of absentmindedness or procrastination is a significant impediment to refinancing (Schacter, 1999). Moreover, the contrast even within our paper between strong reminder effects and weak disclosure design effects helps confirm that the unique driver of our results is reminders. Our estimates imply

[8]The pattern of inertia and disengagement in mortgage markets echoes many other product markets, including retirement accounts, deposit accounts, energy, telephone, and internet broadband markets, which also have subdued levels of consumer switching against a market backdrop of meaningful price dispersion (Madrian and Shea, 2001; Yang, 2014; Shcherbakov, 2016; Lunn and Lyons, 2018; Harold et al., 2019; Adams et al., 2021).

that reducing monetary policy's last-mile problem caused by the final demand sector's inattention can result in cost-effective stimulus. We discuss the literature exploring potential general equilibrium implications of increased refinancing responsiveness in section 5.3.

2.4 Consumer Disclosures and Reminders

In many settings, the policy response to potentially suboptimal consumer choice has been to provide additional information, leading to a proliferation in mandatory disclosures and plausibly contributing to inattention (Ben-Shahar and Schneider, 2014; Kell, 2016). Our results contribute to a growing body of evidence that behaviorally-informed disclosures can—but do not always—deliver meaningful impacts on various public policy challenges (e.g., Lee and Hogarth, 2000; Thaler and Sunstein, 2009; Bar-Gill, 2012; Adams et al., 2015; Wang and Burke, 2022). One approach to address inattention to information provision is to use personalized services to improve household engagement or the take-up of publicly provided services (e.g., Bergman et al., 2019; Finkelstein and Notowidigdo, 2019; Guryan et al., 2023; Evans et al., 2023). One advantage of the approach we test is that, while personalized, the reminder treatments we study are low-cost and readily scalable because they do not require personally provided services.

Reminders have been popular in health care, with evidence that reminders can increase vaccination take up and cancer check-up rates (e.g., Mayer et al., 2000; Hirani, 2021; Milkman et al., 2022). In consumer finance, research finds some scope for well-timed reminder letters positively affecting financial behavior (Adams and Hunt, 2013; Adams et al., 2015, 2021; Karlan et al., 2016). These studies demonstrate the significant effect that well-timed reminders can play in prompting financial action, pointing to the role of procrastination and inattention in shaping consequential financial decisions.[9] Nevertheless, looking across settings, the effects of reminders are mixed, with reminders in domains with straightforward choice architecture and salient, personalized benefits more likely to have significant effects. For example, we see the key difference between reminders in a retirement savings context and an internal refinancing context to be the relatively few number of options in the refinancing space.

Our work extends the literatures on behavioral interventions, disclosures, and reminders in several ways. First, despite mixed success of reminders in other settings, this paper is the first to test their use in the high-stakes mortgage refinancing context. Second, the uniformly strong reminder effects in our setting—including the contrast to the small effects of disclosure design—add the first experimental evidence indicating a role for limited attention

[9]See also Banerjee et al. (2010), who find that small nudges can have large effects by helping people coordinate their attention to a task at a specific time instead of delaying it indefinitely.

in explaining both the failure to refinance and the typical ineffectiveness of disclosures more broadly. Third, we show that reminder letters can be a cost-effective tool for policymakers such as central banks that had previously not considered such communication in their toolkits.

3 Institutional Setting

To provide context for our experimental setting, this section briefly overviews the Irish mortgage market and its relevant institutional features. By several metrics, the Irish mortgage market is fairly representative of mortgage markets in other advanced economies (Calza et al., 2013). Nearly one in three Irish households has an outstanding mortgage on their main residence (Central Statistics Office, 2020). Ireland's mortgage debt-to-GDP ratio (50%), typical loan-to-value ratio (70%), typical mortgage duration (20 years), and score on the IMF Mortgage Market Index measuring market development (0.39) are all roughly average compared with mortgage markets in the US, Canada, Europe, Japan, Australia, and New Zealand.

There are three primary types of residential mortgages in Ireland: fixed-rate mortgages, variable-rate mortgages, and tracker mortgages, accounting for approximately 55%, 20%, and 25% of 2021 outstanding balances respectively.[10] Fixed-rate mortgages in Ireland are similar to those in the UK and to adjustable-rate mortgages in the US: a fixed interest rate for an initial term (usually 1-5 years) that converts to a variable rate thereafter. There is generally a prepayment penalty of approximately 2% of the outstanding balance if a borrower prepays their mortgage during the fixed-rate period. Variable-rate mortgage interest rates in Ireland adjust at the sole discretion of the lender (as opposed to floating debt in other markets that is usually indexed to an interest-rate benchmark). There is no penalty for prepayment of a variable-rate mortgage and refinancing internally (i.e., with the current lender) is allowed without any closing costs, unless the borrower wishes to pay for an appraisal to justify a lower loan-to-value ratio classification.[11] Tracker mortgages in Ireland usually track the ECB refinancing rate plus a spread of around 100 bp. However, Irish lenders stopped originating new tracker mortgages in 2008, and their share of outstanding mortgages has steadily declined since then (Appendix Figure A1). In March 2020, the Irish government instituted a mortgage forbearance program, through which mortgage borrowers who had experienced financial

[10] Appendix Figure A1 provides a time series of this breakdown, highlighting the growing prominence of fixed rate mortgages in recent years.

[11] The absence of closing costs for the internal refinances on which we focus below is unlikely to explain the difference between our results and other studies given that external refinancing in many countries allows rolling any up-front closing costs into monthly payments to eliminate liquidity constraints.

hardship due to the Covid pandemic could apply for a temporary payment break. We observe and exploit heterogeneity in who received Covid payment forbearance in section 5.2.

Refinancing in Ireland generally maintains the original maturity at origination and does not extend a mortgage's term. Similar to other mortgage markets, subdued refinancing activity in the Irish mortgage market contrasts with widespread opportunities to realize substantial financial savings from refinancing and with policy and commercial advertising efforts to facilitate refinancing. Byrne et al. (2020) estimate that three in every five mortgages could save over €1,000 within a year of refinancing (over €10,000 over their remaining term) but that just 2.9% of mortgages switched provider during the second half of 2019.[12] As we discuss below, this level of potential savings is representative of the savings available to borrowers in the experimental sample. A 2016 survey found that while most surveyed borrowers would consider refinancing for interest-rate savings, over half were uncertain how much money they could save and many borrowers believed that the refinancing process would be too complex or time-consuming (Central Bank of Ireland, 2017b).

To provide mortgage borrowers with enough information to refinance their mortgages when advantageous to do so, Irish banking regulations require mortgage lenders to disclose the availability of such opportunities at least annually (Central Bank of Ireland, 2017a). Provision 6.5(g) of the Central Bank of Ireland's Consumer Protection Code requires regulated lenders to provide variable-rate mortgage holders a letter summarizing other mortgage products that could provide them with savings on their mortgage at that point in time. Because fixed-rate mortgages automatically convert to variable-rate mortgages after their fixation periods end, many borrowers are on variable rates at any given time and qualify for these disclosures. Notably, the regulations do not stipulate how the disclosed information should be presented. As we discuss below, these mandatory annual disclosures form the basis of our field experiment.

4 Experimental Design

We partnered with a large retail bank in the Irish mortgage market to analyze the results of a field experiment testing whether a series of behaviorally enhanced versions of the mandatory financial disclosures described above have an effect on refinancing behavior. The letters were delivered by mail to a total of 12,050 variable-rate mortgage holders between January 28 and

[12] Appendix Figure A2 plots mortgage external refinancing rates over time, showing that there was no significant increase in the level of external refinancing during the pandemic in 2020. During the period of the experiment, external refinancing options included both mortgages with more attractive interest rates and mortgages with less attractive interest rates but with upfront, and highly advertised, cash-back bonuses (King and Singh, 2018).

February 3, 2020, randomly drawn from the population of variable-rate mortgage customers of the partnering bank. Figure 2 illustrates the interest-rate savings available to the average bank customer receiving the disclosure, from an average outstanding interest rate of 4.2% to the shortest fixed-rate mortgage offered on the disclosure of 2.9% (s.d. = 0.3%).

Participants were randomly assigned to seven equally sized groups: six treatment arms and one control group. The modified version of the disclosure letter sent to each treatment arm was rigorously evaluated to ensure it provided at least the baseline information required by the Consumer Protection Code (i.e., no key information was removed, which could have led to a treated mortgage borrower having less information available than they would under the baseline scenario).[13]

Power analysis indicates that with a sample size of approximately 1,700 customers per group, the minimum detectable treatment effect on mortgage refinancing is a 1.6 pp improvement over the baseline rate of refinancing, an increase of 13%. Within each treatment arm, the sample was randomly divided in half, with one half receiving an additional follow-up reminder notification by mail between February 27 and March 6, 2020 (4-6 weeks after the original communication).

We gathered detailed baseline data on each trial participant from prior to the intervention and assess the impact of the intervention using data snapshots provided by the bank four and seven months after the disclosure distribution. The loan-level dataset recorded loan and borrower characteristics, including the interest rate prevailing on the loan, years to maturity, outstanding loan balance, current loan-to-value ratio, pre-trial available savings on the mortgage with respect to the best available alternative product option, borrower age, and indicators for Dublin residents, first-time homebuyers, and borrowers who had received Covid-related payment forbearance. Follow-up administrative data from the bank allow us to identify those loans that refinanced internally, refinanced externally, reached maturity, or otherwise exited the bank. We drop around 300 borrowers in arrears from the final estimation sample (less than 3%) to remove borrowers who may have perceived themselves ineligible for refinancing, although our results are robust to including them (see Appendix Table A3).

4.1 Treatment Arms

The treatment arms' disclosure redesigns addressed informational, procedural, financial literacy, and behavioral obstacles to refinancing and showed promise in the encouragement of consumer engagement in other settings. Below, we explain the disclosure design elements

[13]To avoid the potential for observer effects that could affect the integrity of the experimental design, the bank did not inform treatment-group participants that the version of the mandatory disclosure they received was experimental.

featured by various treatment arms and provide citations for their rationale. Table 1 summarizes how each treatment arm incorporates a particular combination of the disclosure redesign elements described below. Appendix Figures A3 and A4 reproduce example control-group and treatment-group letters, respectively. Additionally, as reviewed above, half of each of the six treatment arms received a follow-up reminder letter 4-6 weeks after the initial treatment disclosure. See Appendix Figure A5 for an example reminder letter.

Simplification: Each treatment communication included a box on the front page of the letter with key points highlighted, including the current interest rate and monthly repayment and the lowest alternative interest rate and associated monthly repayment available to the customer from refinancing internally. The box was designed to ensure that key information could be accessed quickly to target customer inattention and information overload, both of which have been found to affect the ability of consumers to make informed choices (e.g., Adams and Hunt, 2013; Lunn et al., 2016; Goldin et al., 2020).

Personalized Savings: The retail bank's standard communication (Appendix Figure A3) included a table of the interest rate associated with each alternative product option available to the customer, but there was no translation into the associated monthly savings. Each treatment supplemented the interest-rate table with the monthly payment amount associated with each option and the savings (where available) relative to the current monthly repayment. This personalization was designed to target financial illiteracy and present bias by making the immediate savings more salient (Financial Conduct Authority, 2016).

Prominent Subject Line: The subject line in the control letter stated, "You may be able to save money on your mortgage." To increase the likelihood consumers would perceive the letter worth their attention, Treatment groups 3-6 tested bolded the subject-line text (Behavioural Insights Team, 2014).

Framing: Three of the treatment arms varied the presentation of financial savings to be either in a gain frame or a loss frame to counteract loss aversion, the tendency for people to prefer avoiding losses to acquiring equivalent gains (Genakos et al., 2015; Adams et al., 2015). For the one gain-framed treatment arm (#4), the language read "With a different rate, you could save up to €X a year on your mortgage," and in the two loss-framed treatment arms (#5-6), the language used was "You could be missing out on savings of up to €X a year by not choosing a lower mortgage interest rate." The remaining treatment arms' letters adopted a more neutral tone.

Color: Treatment group 2 received letters that used color at key junctures to draw attention to salient information (Behavioural Insights Team, 2014).

Process Clarification: To reduce potential ambiguity aversion over the potentially unknown complexity of the refinancing process, Treatment group 6 included a clarified pro-

cess box, which clearly delineated the steps required for a mortgage holder to take action and move onto a lower cost interest rate option (Behavioural Insights Team, 2014). Testing whether a series of disclosure redesigns had any effect on savings account holders switching to a higher-paying savings product, Adams et al. (2021) found the strongest treatment effects by simplifying the switching process.

4.2 Descriptive Statistics

The trial sample of loans consists of a random subsample of the outstanding variable rate mortgages held by the partnering institution because this is the cohort eligible for receipt of the mandatory disclosure from which we build our experimental treatment arms. Our total sample of 12,050 letter recipients reduces to an estimation sample of 11,200 following the attrition of 850 observations which exited the loan book, reached maturity during the trial period, or were excluded from estimation due to mortgage arrears history (although we verify below that our results are robust to including borrowers in arrears). Of our estimation sample, 1,345 (12%) go on to refinance internally, and 373 (3%) refinance externally with a different provider.

Table 2 reports summary statistics for several mortgage and borrower characteristics in our data, pooling treatment arms together with the same reminder status.[14] Around 20% of borrowers in our sample live in Dublin and 40% are currently living in their first purchased home. The average borrower in our data is 50 years old and has 13 years left on their €83,000 mortgage. At baseline, the average interest rate among trial borrowers is 4.2% with a standard deviation of 20-30 basis points. Calculating the interest savings that borrowers could realize in the first year if they refinanced to the shortest fixed-rate mortgage available (2.9%), the average savings is €1,044 in the control group and similar for the treatment groups.

To formally test for random assignment and the balance of treatment status across observables, Appendix Table A2 shows a regression of an indicator for each treatment arm (in a sample restricted to observations from that treatment arm and the control group) on a vector of covariates. We find a high degree of statistical balance and low R^2 values ranging from 0.001-0.005. Although years to maturity shows marginally statistically significant differences across treatment and control, a coefficient of 0.003 years corresponds to an economically meaningless difference in remaining maturity of approximately one day. In every column, we fail to reject that all of the slope coefficients are jointly equal to zero.

[14] See Appendix Table A1 for descriptive statistics by treatment arm.

External Validity To assess the representativeness of our experimental sample, we compare its summary statistics to the near-universe of outstanding Irish mortgages. Columns 4-5 of Table 2 report summary statistics representing about 90% of outstanding mortgages in Ireland using a database updated every six months by the largest banks in Ireland. Column 4 describes outstanding variable rate mortgages and column 5 describes residential mortgages in Ireland. Overall, the mortgages held by our partnering bank have similar characteristics to the nationwide sample, helping to address external validity concerns. Exceptions include that mortgages in the experimental sample are 7-8 pp less likely to be in Dublin and have €20,000 smaller balances on average.

The average variable-rate mortgage at the partner bank has a 0.6 pp higher interest rate than the average variable-rate mortgage across all providers, consistent with Figure 2. A priori, the high potential interest-rate savings of our sample has theoretically ambiguous effects on the effectiveness of the treatments we study. On the one hand, borrowers with high savings might be the most responsive to refinancing nudges. On the other hand, borrowers who have not yet refinanced in the face of high available savings may be particularly inattentive or constrained for unobservable reasons and therefore the least responsive to the treatments. However, when compared to the sample of all outstanding variable-rate mortgages in column 4 of Table 2, the amount of savings available to the experimental sample looks typical, suggesting that our experimental sample is not particularly unique within Ireland.[15] Moreover, the research cited in section 2.3 also demonstrates that mortgage borrowers worldwide are frequently found to be ignoring in-the-money refinancing options. Finally, the lack of substantial treatment-effect heterogeneity found in section 5.1—especially for the reminder treatment effects—further indicates that our results do not seem to be driven by some particular, unique characteristic of our sample.

5 Results

In this section, we estimate the causal effects of the experimental treatments on the observed rate of internal mortgage refinancing compared to the baseline standard represented by the control group. Our impact analysis is based on administrative bank data captured in June 2020, four months after the distribution of disclosure letters, although our results are robust to using outcomes measured as of September 2020. After measuring treatment effects and testing for heterogeneity in effectiveness across treatment arms, we test for differences in

[15]In the near-population of outstanding mortgages in column 5, the average mortgage has no savings available from refinancing, mostly owing to tracker mortgages pegged to the ECB policy rate that were significantly below market-available refinancing rates at the time. Excluding tracker mortgages, the average outstanding mortgage in Ireland has €834 of first-year interest payment savings available.

treatment effects across observable demographic and financial differences in borrowers. We also examine whether our results are likely to be an artifact of the extra time and motivation to refinance some borrowers may have had during the Covid pandemic. Finally, we conclude this section by discussing general equilibrium considerations in the degree to which our findings would potentially change in a large-scale implementation.

Figure 3 summarizes our core results, which we examine in more detail below. We find that without a follow-up reminder letter, the average disclosure redesign treatment increases refinancing 20% (1.8 pp) from a base of 8.9 pp (the control-group internal refinancing rate) to 10.7 pp. An accompanying follow-up reminder letter 4-6 weeks after the initial disclosure increases the refinancing rate by an additional 3.6 pp for a total communication effect for the average treatment arm of 5.6 pp. The best-performing treatment arm with a reminder letter (V2) increases internal refinancing by a total of 76% (6.8 pp). The average 12-month savings realized by refinancing mortgage borrowers in our data is €1,209.

These results—particularly the large proportional effects of treatments with reminders—contrast with much smaller effects found in two preceding mortgage refinancing experiments. Keys et al. (2016) found no statistical differences in refinancing across three treatment arms that drew attention to the amount of savings that mortgage holders could achieve in different ways.[16] Similarly, although also personalized, none of the experimental interventions tested by Johnson et al. (2019) had a statistically or economically meaningful impact on refinancing. The small effects that, similar to these studies, we also find of disclosure redesigns helps isolate that our uniquely strong reminder effects result from solving an attention problem for a significant fraction of borrowers.

Table 3 reports the magnitude and formally tests for the significance of these treatment effects, with and without controls. We estimate

$$Refinance_i = \beta_0 + \beta_1 Treament_i + \beta_2 Treatment_i \times Reminder_i + X_i'\gamma + \varepsilon_i, \qquad (1)$$

The indicator $Refinance_i$ equals one if borrower i internally refinanced within four months of receiving the legally required refinancing opportunities disclosure. Without controls, β_0 estimates the refinancing rate of the control group. The treatment effects β_1 and β_2 capture the increase in refinancing by borrowers randomly assigned to a redesigned disclosure treatment arm. The coefficient β_2 on the interaction term estimates the differential refinancing by treated borrowers who also received the follow-up reminder letter (no borrowers received the reminder letter without also receiving a redesigned disclosure). In specifications that include them to improve precision, the individual- and loan-level controls X_i are the covariates listed

[16]However, a much smaller sample size (N=193) meant that the authors were underpowered and unable to reject the possibility of economically meaningful results.

in Table 2. All of our estimates use heteroskedasticity-robust standard errors.

Across all columns in Table 3, effects are statistically significant at at least a 95% confidence level and most often at the 99% confidence level. Column 1 reports a treatment effect on refinancing pooling all six treatment arms of 3.6 pp without conditioning on reminder status, thus conflating the effects of the treatment redesigns alone and the reminders. Adding the borrower and loan controls from Table 2 as covariates in column 2 increases this estimate slightly. In column 3, we additionally control for whether each borrower also received a follow-up reminder communication. The main pooled treatment effect decreases to 1.8 pp, indicating that mortgage borrowers who received a redesigned disclosure but *not* a reminder were only 1.8 pp more likely to internally refinance than the control group, of which 8.9% refinanced internally (the constant term when there are no other controls). Adding the pooled reminder effect of 3.6 pp to the pooled treatment effect of 1.8 pp yields a total refinancing effect of the average treatment and reminder of 5.4 pp. Column 4 again adds individual-level controls, with the treatment and reminder effects changing only slightly.

Given the modest overall effectiveness of the treatment redesigns without an accompanying reminder, is any one of the treatments particularly effective? Is a reminder letter more effective when combined with some treatments than others? To test for the relative performance of the various redesigns elements, we next estimate treatment effects separately by treatment arm. We first plot internal refinancing rates by treatment arm for subsamples without and with reminders in Figures 4 and 5, respectively. In both figures, there are economically small differences in refinancing rates across treatment arms. In the no-reminder sample of Figure 4, only some treatments have refinancing rates that are individually significantly different from the control group. Moreover, across treatment arms, the sizes of disclosure redesign effects and reminder effects seem negatively correlated; the strongest treatment effects without reminders are not from the treatment arms with the strongest reminder effects.

Table 4 reports the magnitude of the refinancing treatment effect differences across treatment arms, including reporting a formal test of joint equality across treatment arms. Column 1 pools treatments and reminders and finds small differences across treatment arms. The p-value of 0.99 for a joint F-test of the null hypothesis that all of the treatment arm coefficients are equal to each other fails to reject that all of the treatments had the same effect on refinancing. Adding controls in column 2 increases the estimates slightly compared with column 1, with the joint test again failing to reject equality of the effects across treatment arms. In column 3, we add controls for the interaction of each treatment arm indicator with the reminder indicator for whether each mortgage borrower received a reminder letter; column 4 adds controls. In both columns 3 and 4, only some treatment arms have treatment effects

or reminder effects that are individually statistically different from zero. As was apparent in Figures 4 and 5, the treatment arms with the largest and most statistically significant treatment effects are not the treatment arms with the largest or most statistically significant reminder effects. However, consistent with Figure 5, the *total* treatment and reminder effect is statistically significant for each treatment arm. Testing for equality of the treatment without reminder effects across treatment arms and the equality of the reminder effects across treatment arms in columns 3 and 4, we again fail to find statistically significant evidence of differential effects across disclosure redesign versions.

The results above indicate that the reminder communication had particularly strong effects stimulating internal refinancing. Do redesigned disclosures or the reminder letters affect *external* refinancing, where borrowers refinance and switch providers? We evaluate whether any treatment effect can be observed in terms of this secondary channel in Table A4. In a series of regression specifications that mirror our main regression analysis in Table 3, we find no evidence for treatment or reminder effects, estimating economically small, precise, and statistically insignificant effects on external refinancing. This contrast between internal and external refinancing effects could be one reason for the strength of our estimated treatment effects. Whereas most other studies focus on the drivers of external refinancing and document a reticence among borrowers to switch providers, our results suggest that inertia is much weaker when borrowers have the opportunity to refinance while staying with their current provider.[17] Policymakers seeking to support active refinancing could consider efforts to facilitate internal refinancing, with the caveat that success improving refinancing responsiveness could be partially offset in general equilibrium (see discussion in section 5.3).

5.1 Treatment Effect Heterogeneity

Next, we test whether some types of borrowers responded to the treatments more strongly than others. This exercise tests whether the overall effectiveness of the communication treatments is driven by strong effects for a particular subset of borrowers, is an input into questions of cross-subsidization (Fisher et al., 2022; Zhang, 2022), informs external validity assessments, and guides estimates of welfare effects (Finkelstein and Notowidigdo, 2019).[18] We estimate heterogeneous treatment effects by augmenting (1) with interaction terms be-

[17] We note that despite also studying internal refinancing opportunities with their small-scale field experiment, Keys et al. (2016) found insignificant effects, as discussed above.

[18] Recent work by Gerardi et al. (2023) documents disparities between groups in their responsiveness to refinancing incentives. In this section, we test whether such disparities are compounded by potential differential responsiveness to disclosures and reminders.

tween the treatment variables and a given borrower or loan characteristic x:

$$Refinance_i = \beta_0 + \beta_1 Treament_i + \beta_2 Treatment_i \times Reminder_i + \beta_3 x_i + \beta_4 Treament_i \times x_i + \beta_5 Treatment_i \times Reminder_i \times x_i + \varepsilon_i. \quad (2)$$

To ease interpretation, we discretize the controls in Table 2 into indicator variables. For example, instead of the variable age measured in years, we calculate an indicator for age greater than 50, which is the mean age in our sample. When $x = 1(\text{Age} > 50)$, β_0 and β_3 correspond to the refinancing rate of younger and older control-group borrowers, respectively, and β_1 and β_2 correspond, respectively, to the disclosure redesign and reminder treatment effects for younger borrowers. The interaction terms coefficients β_4 and β_5 measure the differential treatment effects for older borrowers, with the t-test on each of these coefficients testing the hypothesis of no heterogeneity in treatment effects along the age dimension.

Table 5 estimates (2), with each column reporting results of a separate regression with a different characteristic standing in as x, as indicated by each column header.[19] Panel I estimates heterogeneous treatment effects along borrower characteristics (indicators for Dublin residence, age over 50, first-time homebuyers, and borrowers with Covid forbearance), and panel II tests for heterogeneity by loan characteristics (outstanding loan balances above €75,000, baseline interest rates above 4.2%, more than 13 years left in the mortgage, and first-year potential refinancing savings exceeding €1,000). As expected given the literature reviewed in section 2.3, we find evidence that baseline refinancing rates differ across consumer types. Examining estimates of β_3 in the rows labeled Covariate x, control-group borrowers who are younger, with high balances, have longer until loan maturity, or stand to save more after refinancing have higher incentives to refinance and are 3, 9, 5, and 9 percentage points more likely to refinance, respectively.

Turning to treatment effect heterogeneity, overall we find modest but statistically insignificant heterogeneity in the disclosure resdesign's effectiveness and small and insignificant heterogeneity in the reminder's effectiveness.[20] While the estimated main effect $\hat{\beta}_1$ of the disclosure redesigns continues to be small, several borrower types have disclosure redesign treatment effects estimated to be more than 2 percentage points higher. Unfortunately, these tests also appear to be underpowered, and we cannot reject that a Treatment $\times$ x coefficient

[19] To test whether we can reject that all of the treatment-covariate interaction terms are jointly equal to zero, we also estimate a version of (2) controlling for all of the treatment-covariate interaction terms and covariate main effects simultaneously. However, because of the conceptual similarity between some of these variables (e.g., baseline interest rates and potential savings), interpreting the jointly-estimated coefficients is less intuitive than the one-at-a-time version presented in Table 5.

[20] Joint tests that all of the Treatment $\times$ x or Treatment $\times$ Reminder $\times$ x interaction terms are simultaneously equal to zero fail to reject at the 0.05 significance level.

of, for example, 2.5 pp is statistically different from zero. The exception is for years to maturity, where we estimate that the disclosure redesign treatments had a de minimis effect for borrowers with 13 or fewer years until maturity and a statistically significant 4.6 pp treatment effect for borrowers with more than 13 years until maturity. The pattern for reminder effects is significantly more uniform. While the estimated reminder effect $\hat{\beta}_1 + \hat{\beta}_2$ is consistently large and statistically significant across columns, we estimate the reminder treatment effect heterogeneity β_5 to be economically small in magnitude across all characteristics and in every case less than 2 pp.

Because of power and multiple hypothesis testing concerns, we hesitate to draw strong conclusions from the generally marginal evidence in Table 5 for treatment effect heterogeneity. Moreover, data limitations prevent us from testing for heterogeneity by income, race, or financial sophistication. However, the contrast between a) significant heterogeneity in baseline refinancing rates along the dimensions we observe, b) the imprecise and modest disclosure redesign effect heterogeneity, and c) the negligible heterogeneity in reminder effectiveness suggests that reminder communication stimulates refinancing for a majority of borrowers and that such messages do not favor one group over another. Furthermore, the similarity in refinancing effects supports applying a representative mortgage borrower MPC when estimating aggregate mortgage borrower consumption effects, as opposed to a lower (higher) MPC if primarily unconstrained (constrained) borrowers responded to reminders.

5.2 Assessing Potential Covid Impacts

Finally, we address the possibility that our treatment effects are driven by borrowers with an atypical surplus of time or refinancing motivation due to Covid lockdown measures during our estimation window. Such circumstances might plausibly facilitate, for certain households, a greater degree of attention to administrative mail communications than they would ordinarily allocate. The potential for our results to be driven by the unique circumstances induced by the pandemic is particularly important from an external validity standpoint. If our results are driven by something about the specific state of the world at the time of our experiment then that lessens the likelihood that direct communication to households about refinancing activities in other times or in other markets would also have strong effects.

We have three main strategies to test whether our results are driven by pandemic effects. First, our use of a randomized controlled trial helps us not misattribute contemporaneous time-series variation in refinancing to the experimental treatments. If refinancing during Covid were extraordinarily high or low for reasons unrelated to our treatments, such aggregate effects would impact the control group as well as our treatment groups. Because both

groups would be affected, our treatment effects estimates of the differential refinancing by treated borrowers relative to control-group borrowers would not be biased. Two findings we discuss above suggest that this channel is not a large concern. First, we do not observe a significant increase in external refinancing rates in Ireland from 2019-2020 (Appendix Figure A2). Second, control-group borrowers with and without Covid forbearance had similar internal refinancing rates (t-statistic of 0.6 in Table 5). Similarly, some types of borrowers may be more likely to respond to the pandemic by refinancing than other types of borrowers. However, the large sample and balance of both observable and unobservable borrower types across treatment and control ensured by randomization—including the balance of the Covid forbearance flag—prohibits such heterogeneity from affecting our results. In specifications with covariate controls, we control for the Covid forbearance flag and find it to have a positive but relatively small and statistically insignificant coefficient.

Second, we address potential interplay between the pandemic and the treatments by interacting our treatment indicator with the Covid forbearance flag. The concern addressed here is that the use of a randomized control-group does not help if the treatment effects themselves are driven by Covid. For example, imagine hypothetically that the reminder treatment is only effective because a subset of treated borrowers had ample time to respond actively to a reminder follow-up (or because they were distracted with background stress and were thus particularly in need of a reminder). Such a mechanism could lead to our estimating large reminder effects that would be unlikely to replicate in other settings where the Covid mechanism would not be present. Around 9% of borrowers in our data received mortgage payment forbearance by documenting a Covid-induced financial hardship limiting their ability to make their mortgage payments. If the strong reminder effects are only because of Covid, then the most Covid-affected borrowers in our data should show the strongest treatment effects. In Table 5, we indeed find that the Treatment $\times$ Covid Forbearance coefficient is positive, suggesting that the treatment may be more effective for people who have time or particular motivation to seek payment savings. However, as in the case of the Covid Forbearance main effect, the Treatment $\times$ Covid Forbearance interaction term coefficient is modest in magnitude and statistically insignificant, with a t-statistic of 0.5-0.6. We conclude that strength of the reminder effects is not driven by any heightened responsiveness by the most Covid-affected borrowers.

Finally, we test for other forms of differential treatment effects due to Covid that might not line up with whether a borrower received forbearance. Even among workers without sufficient Covid-related financial distress to qualify for forbearance, the pandemic might have been a much busier time or a time with much more slack depending on a borrower's employment situation. To test for Covid-related drivers of our treatment effects that are

not captured by the forbearance flag, we estimate specifications that allow for heterogeneous effects across employment sectors. This approach allows us to examine whether the treatment effects are driven by an interaction between the treatments and pandemic-specific employment situations that would be unlikely to be present in future implementations of the communication treatments we study here.

We group mortgage borrowers into employment sectors that differ in the likelihood they face Covid-related disruptions using data on the employment industry of each borrower. Beginning in June 2020, the Central Bank of Ireland's administrative loan-level data contains a field collected at origination by lenders recording a borrower's employment industry using Eurostat's Statistical Classification of Economic Activities in the European Community. For loans in the June 2020 data, we merge their employment industry to our estimation sample using unique loan identifiers. Of our original 11,200 observations in our estimation sample, we obtain employment sector information on 10,260 loans, a 92% match rate. We then split our estimation sample into employment sectors that are more likely to be working from home (WFH), experiencing business as usual (BAU), and at home but not working (AHNW), using information on the borrower sector of employment at point of loan origination.[21]

Figure 6 summarizes this subgroup analysis by plotting the refinancing rates of each employment sector by treatment status. The left-hand side of the figure reports refinancing rates for borrowers that were treated with a redesigned disclosure letter but not a follow-up reminder letter. The right-hand side reports refinancing rates for borrowers that received both redesigned disclosure letters and follow-up reminder letters. If treated borrowers have similar refinancing rates across employment sectors that have very different levels of exposure to pandemic disruptions, this suggests that the treatment is not particularly effective or ineffective *because* of the pandemic. With the caveat that we are underpowered for this heterogeneity analysis—with wide confidence intervals especially for borrowers in the relatively small business-as-usual sector—refinancing rates are quite similar across employment sectors, inconsistent with our results being driven by the uniqueness of life during the pandemic.[22]

[21]See Appendix Table A5 for this classification of employment industries. The WFH category includes those employed in industries more likely to have flexibility to work from home: information and communication, financial and insurance, professional, scientific and technical, public administration, and other service activities sectors. The BAU category includes those industries likely to have continued requiring in-person work: agriculture, forestry, and fishing, electricity and gas supply, and transport and storage sectors. The AHNW category includes those more likely to be laid off or furloughed, who were employed in manufacturing, construction, wholesale and retail trade, and vehicle repair, and accommodation and food services sectors.

[22]Appendix Table A6 reports corresponding treatment and reminder effects by subgroup relative to the control group. We fail to reject equality of the treatment effects across employment sectors, although the standard errors are large for this subgroup analysis. Moreover, although the business-as-usual reminder effect is relatively large, it is also the most imprecise and has the noisiest constant term such that the total refinancing rate looks more similar across sectors, as seen in Figure 6.

5.3 General Equilibrium Considerations

An important and complementary series of recent papers explores how equilibrium mortgage rates might change if inertial refinancing were reduced at scale.[23] The core idea behind this line of inquiry is that prevailing pricing anticipates status quo refinancing behavior. If refinancing were to become more responsive for a meaningful fraction of borrowers, in equilibrium firms may raise their interest rates to recoup lost interest income from formerly sluggish refinancers. We also note that the resulting competitive forces may alternatively motivate firms to lower their interest rates—either at origination or by offering internal refinancing opportunities—to eliminate the incentive to switch providers. Similarly, the economic efficiency of monetary policy might improve if origination interest rates were higher but more closely passed through policy rate changes. These papers generally consider hypothetical successful and yet-to-be demonstrated policy interventions. One contribution of our paper is providing such a policy that we show—experimentally in the field and in a partial equilibrium attention model—can stimulate refinancing.

To understand the degree to which general equilibrium forces might offset some of the stimulative benefits from a targeted communication policy, we briefly review the literature on the theoretical general equilibrium effects of refinancing interventions. Zhang (2022) uses a structural model of the US mortgage market to show that automatically refinancing mortgage contracts could simultaneously reduce inequality in the market and improve total consumer welfare. Similarly, Fisher et al. (2022) develop a structural model of the UK mortgage market in which the elimination of cross-subsidies from slow to fast refinancers "democratizes" the mortgage market, with a potential for increased mortgage uptake by relatively poorer households. On the other hand, Berger et al. (2022) use an equilibrium pricing model for the US mortgage market to demonstrate that mortgage reforms can have negative distributional consequences when they increase equilibrium mortgage rates and reduce credit access. In particular, if a reform increases equilibrium origination rates because it causes more households to actively refinance, the households who still do not respond are worse off. Complementing these efforts, our results below provide attention effects plausibly generated by actually tested policies.[24] These magnitudes could be incorporated into general

[23] More broadly, Campbell (2006) notes the theoretically ambiguous welfare impact of financial product innovations or interventions designed to eliminate the cross subsidization of financially sophisticated households by naïve ones. Outside of finance, Grubb (2014) and Grubb and Osborne (2015) develop and estimate a model of inattentive consumers in which disclosure regulation that improves consumer attention leads to a pricing response by firms. We discuss potential offsetting effects on bank shareholders in the conclusion.

[24] In a similar spirit, an exercise in Berger et al. (2022) uses observational data on the correlation between borrowing from non-banks and subsequent refinancing sensitivity to imagine a world where only non-banks originate mortgages. Considering how our attention effects might affect equilibrium rates would directly connect general equilibrium model results to a feasibly scalable and already-demonstrated policy.

equilibrium models to consider what total effects could be expected from scaled versions of the treatments we study.

Finally, while these results suggest that research and policy efforts to reduce optimal refinancing barriers should consider equilibrium effects, we also note that the countervailing effects estimated by the papers above are generally modest. For example, the estimated average present value of the cost of the status quo cross-subsidization from slow to quick refinancers relative to no cross-subsidization in Zhang (2022) is equivalent to 26 bp higher origination interest rates. Similarly, Fisher et al. (2022) find that average UK outstanding rates would be 20 bp higher in a counterfactual world with no cross-subsidization. Berger et al. (2022) estimate that a counterfactual in which attention increases by 12 pp (roughly similar to the estimated effects on attention below) would increase origination rates 50 bp, followed by a steeper decline in rates over time as households refinance more frequently.

6 Inattention Estimates

In this section, we interpret our treatment effects through the lens of the Andersen et al. (2020) model of inattentive refinancing. We adapt and build upon this existing work to introduce a model of refinancing behavior adjusted to the Irish context and our experimental setting. First, we assess the degree to which inattention helps a model of refinancing fit the data with realistic and reasonable fixed cost parameters. Second, because attention is unobservable in our setting, we use the model to estimate the degree of inattention and the extent to which the effects of the disclosure treatments and reminder letters are consistent with a mechanism that operates through reducing inattention. Third, this exercise also allows us to contrast estimates of attention treatment effects with changes to refinancing induced by conventional monetary stimulus holding the level of household attentiveness fixed.

6.1 Baseline Refinancing Model

The baseline model builds on the optimal refinancing model of Agarwal et al. (2013), which assumes that households are risk neutral and fully attentive, refinancing their mortgages if the expected net benefits of refinancing are positive. The model captures several reasons why not refinancing a mortgage might be a perfectly rational financial decision for mortgage holders. First, mortgage holders might deem the available savings insufficient to justify actual or psychological switching costs. We allow for and estimate the threshold of savings that is sufficient for attentive borrowers to consider the benefits of refinancing net such costs sufficient. Second, mortgage holders might be ineligible to switch as a consequence of their

loan-to-value positions or their repayment history. We calculate our interest rate savings conservatively assuming that borrowers do not qualify for a lower LTV category. Third, if mortgage borrowers are ex-ante likely to move in the near-term, they might decide not to switch or refinance because they will not be in the home long enough to recoup the fixed costs of switching or refinancing. The Agarwal et al. (2013) optimal refinancing model allows for the risk of exogenous mortgage prepayment, which we estimate from data on about 90% of outstanding Irish mortgages. Fourth, borrowers may expect rates to fall further soon and prefer to take the chance that an even more advantageous refinancing opportunity will soon arise. The optimal refinancing decision incorporates such forward-looking behavior with an expected interest-rate process calibrated to the historical volatility of Irish interest rates.

There are two components in the model to the net benefits of refinancing: the incentive to refinance $I(x_i, \theta)$ that depends on observable mortgage characteristics x_i through parameter vector θ and an idiosyncratic random shock ϵ_i to the net benefits of refinancing. The unobserved component ϵ_i of the decision allows for borrowers to differ in the private benefits or costs they derive from refinancing. The incentive to refinance $I(x_i, \theta)$ is a function capturing a household's incentive to refinance in interest-rate points

$$I(x_i, \theta) = \left(r_i^{old} - r_i^{new}\right) - O_i(x_i, \theta) \tag{3}$$

where r^{old} is the household's current mortgage rate and r^{new} is the best prevailing mortgage rate available to the household. Each household in the model has a minimum decrease in interest rates O_i they require to be willing to refinance, and $I(\cdot, \cdot)$ measures how far above that threshold they are currently. The household's optimal refinancing threshold O_i is calculated using the Agarwal et al. (2013) closed-form solution to optimal refinancing option exercise:

$$O_i = \frac{1}{\psi_i}\left[\phi_i + W(-\exp(-\phi_i))\right] \tag{4}$$

$$\psi_i = \frac{\sqrt{2(\rho + \lambda_i)}}{\sigma} \tag{5}$$

$$\phi_i = 1 + \psi_i(\rho + \lambda_i)\frac{\kappa(m_i)}{m_i(1-\tau)} \tag{6}$$

where $W(\cdot)$ is the Lambert W -function, ρ is the fixed household discount rate, σ is the volatility of r, τ is marginal tax rate (for the tax deductability of mortgage interest), m_i is the outstanding mortgage balance, and $\kappa(m_i)$ is refinancing costs. In practice, we will allow for an additive term $\exp(\gamma)$ in $\kappa(\cdot)$ that will capture any non-monetary cost of refinancing that borrowers face, such as time or hassle costs. The expected rate of decline in real principal

λ_i is the sum of expected inflation π, the exogenous probability of early termination μ, and the amortization rate of the borrower's mortgage.[25]

Table 6 reports our calibration of these parameters, and the appendix discusses alternative formulations of the model to account for differences between US and Irish mortgage products. Appendix Figure A6 plots the distribution of refinancing incentives $I(x_i, \theta)$ defined by (3) using the parameters in Table 6. The median and modal incentive to refinance is around 100 bp, reflecting substantial unclaimed refinancing opportunities in the experimental sample. Indicating that modeled $I(x_i, \theta)$ relates to actual refinancing incentives, the refinancing share of each histogram bin (pooling treatment and control) plotted in Appendix Figure A6 is strongly increasing in the refinancing incentive. Further, the refinancing share is essentially zero for mortgage borrowers with negative refinancing incentives. Still, the absolute level of refinancing is small even for borrowers with substantial refinancing incentives, pointing to frictions such as inattention that limit borrower responsiveness.

In the baseline full attention model, the household refinances if

$$e^{\beta} I(x_i, \theta) + \epsilon_i > 0, \tag{7}$$

where β measures the household's responsiveness to the incentive. For estimation, ϵ is assumed to be distributed logistic, in which case the probability a borrower refinances is

$$\Pr(Refinance_i = 1 | x_i; \beta, \theta) = \Pr\left(e^{\beta} I(x_i, \theta) + \epsilon_i > 0\right) = \Lambda(e^{\beta} I(x_i, \theta)),$$

where $\Lambda(\cdot)$ is the inverse logistic function $\Lambda(x) = e^x/(1+e^x)$. We can then estimate β and θ by maximum likelihood, finding the parameters that maximize the likelihood that we would observe the vector of refinancing decisions in the data.

6.2 Allowing for Inattention

Inattention to a refinancing opportunity can take a number of forms. Inattention may be rational for the stressed consumer with a high current cost of processing all available information relative to low expected returns to doing so. A consumer may be distracted and simply overlook the potential savings in the moment they receive the information. Following an appreciation of the contents of a letter or other communication, inattention may occur as absent-mindedness, described by Schacter (1999) as shallow processing contributing to weak memories of key information and a related to-do action. Related to this third form of

[25] Following Agarwal et al. (2013), we approximate a borrower's time-varying amortization rate using the current amortization rate, measured as the difference between the borrower's annual mortgage payment to current mortgage balance ratio and the current interest rate: $payment_i/m_i - r_i^{old}$.

inattention is procrastination, often defined as postponing, delaying, or putting off a task or a decision in a way that is problematic rather than strategic.[26]

To allow for the possibility that a household is inattentive and thus not paying any attention to their refinancing incentive, we follow Andersen et al. (2020) and estimate a mixture model with each household inattentive with some probability. Inattentive households do not refinance regardless of their incentive to do so. We model households as inattentive if

$$\delta' w_i + \eta_i > 0$$

where η is a random shock to a household's attention and

$$\delta' w_i = \delta_0 + \delta_1 Treatment_i + \delta_2 Reminder_i.$$

This specification integrates experimental variation in treatment assignment into the probability that a household is attentive. The attention intercept δ_0 facilitates estimating the baseline attention level of the control group, and the disclosure redesign treatment and reminder treatment effects δ_1 and δ_2 allow for the attentiveness of each consumer to be impacted by the communication they receive.

Identification Intuitively, the sensitivity β to refinancing incentives is identified by cross-sectional variation in refinancing incentives $I(x_i, \theta)$. In practice, because r_i^{new} in (3) is constant in our experiment, variation in $I(x_i, \theta)$ is driven by cross-sectional differences in initial interest rates r_i^{old}, mortgage balances m_i, and loan maturities (the remaining determinant of monthly payments). Heterogeneity in these variables leads to the distribution of incentives shown in Appendix Figure A6; β is related to the slope of the refinancing share line, indicating how refinancing propensities vary with refinancing incentives. Whereas β helps the model *scale* the refinancing incentive appropriately, the refinancing cost parameters in θ—including the extension below to allow for an unobserved hassle cost of refinancing γ—help the model help *locate* the refinancing incentive. By imposing the condition in (3) of refinancing only when facing a positive incentive to do so, the functional form of $I(\cdot, \theta)$ identifies the unknown terms in θ by essentially shifting the refinancing incentive bins in Appendix Figure A6 to satisfy (3).

Extending the model to allow for treatment effects on attention helps with an identification challenge driven by the unobservability of attention in many empirical settings.

[26] As we mention above, many survey respondents in Keys et al. (2016) cite procrastination as a reason for their inaction. Studies suggest that procrastination chronically affects 15-20% of adults, and that approximately 25% of adults consider procrastination to be a defining personality trait for them (Steel and König, 2006; Nguyen et al., 2013).

Specifically, the mapping of δ_0 to purely attention is not necessarily identified given that δ_0 will capture any reason for a given borrower to not act on positive financial incentives to refinance. In many contexts, what this model estimates as under-refinancing due to inattention could also be driven by other state variables. For example, if all borrowers are fully attentive but some are missing required documentation to be approved by a bank's underwriting department, the model described here will attribute these constraints to inattention, loading such unexplained refinancing failures onto δ_0.

Our approach addresses this empirical challenge in four ways. First, in contrast to prior work estimating inattention, we exploit the random assignment of treatment and control to ensure a balance of borrower unobservables across treatment variables. If some unobserved constraint besides inattention leads some borrowers to not refinance, such a refinancing barrier would be present among both treatment-group and control-group borrowers. This balance allows us to identify δ_1 and δ_2 even if the interpretation of δ_0 is confounded by unobserved heterogeneity. Second, studying internal refinancing opportunities for which borrowers in our sample are eligible allows us to abstract away from settings where many borrowers face underwriting constraints unobservable to the econometrician. Again, by virtue of random assignment, any *mis*perceptions about refinancing eligibility should be balanced across treatment and control. Third, economic intuition supports our interpretation of δ_1 and δ_2 as causal effects on inattention, given that, for example, a reminder letter more plausibly affects attention than overcomes unmeasured refinancing constraints. Finally, we note that Andersen et al. (2020) address potential unobserved heterogeneity by estimating an extended version of their model that allows for random coefficients and unobserved borrower heterogeneity.[27] Their estimates with and without borrower heterogeneity are quite similar, suggesting that unobservable differences across borrowers are not a main driver of inattention estimates in their setting.

Estimation If the inattention shock η is also distributed logistic, then the probability that a given household is inattentive in any given period can be written as

$$\Pr(\delta_0 + \delta_1 Treatment_i + \delta_2 Reminder_i + \eta_i > 0) = \Lambda(\delta' w_i). \tag{8}$$

To refinance, households need to both be attentive (probability $1-\Lambda(\delta' w_i)$) and have positive net benefits of refinancing (probability $\Lambda(e^{\beta} I(x_i, \theta))$). Households that do not refinance are either inattentive or attentive but do not have sufficient incentive to refinance. The likelihood that a household refinances at time t is then $(1-\Lambda(\delta' w_i))\Lambda(e^{\beta} I(x_i, \theta))$. The overall likelihood

[27] Andersen et al. (2020) also demonstrate several predictions of a model where refinancing thresholds γ_{it} vary arbitrarily across borrowers and time that are inconsistent with panel data on refinancing behavior.

$\mathcal{L}(\cdot|\cdot)$ of observing a sample of refinancing given covariates x is the product of the relevant probabilities for the refinancers and the non-refinancers

$$\mathcal{L}(\beta, \delta, \theta|x, w) = \prod_{refi_i=1} (1-\Lambda(\delta' w_i))\Lambda(e^{\beta} I(x_i, \theta)) \prod_{refi_i=0} \Lambda(\delta' w_i) + (1-\Lambda(\delta' w_i))\Lambda(-e^{\beta} I(x_i, \theta)).$$

where the first and second products are taken over all borrowers i that did and did not refinance, respectively.

To estimate the model, we first set certain parameters in θ that govern mortgage contracts and market expectations to fit the Irish context. Using a variety of data sources, we estimate expected Irish inflation as of 2020, household discount rates, nominal interest-rate volatility, mortgage-interest tax deductability, the likelihood of exogenous early mortgage termination, and the fixed cost of refinancing—see Table 6 for details. The maximum likelihood estimates $(\hat{\beta}, \hat{\delta}, \hat{\theta})$ then maximize the log of the likelihood function above. These parameters estimate the importance β of the refinancing incentive, the importance δ of the covariates in shifting attention, and the importance θ of the covariates in determining private refinancing costs. Estimating this model in our setting with exogenous treatment variables allows us to characterize how valuable a given treatment is at focusing consumer attention on refinancing.

Table 7 reports estimates of this model using Maximum Likelihood along with robust standard errors. In column 1, we essentially constrain the model to follow only the Agarwal et al. (2013) model of refinancing without any fixed cost of refinancing or possibility of borrower inattention. In this specification, we estimate a strongly negative β such that the estimated coefficient $\exp(\beta)$ on the incentive to refinance in (7) is essentially 0. Without allowing for fixed costs of refinancing or inattention, it would appear as if the model is a poor fit to actual behavior and that borrowers are completely insensitive to the incentive to refinance.

Starting in column 2, we allow for there to be an unobserved fixed cost of refinancing in the refinancing cost function $\kappa(\cdot)$ in equation (6). Specifically, we let the total cost of refinancing be $\kappa(m_i) = \kappa_0 + \exp(\gamma)$.[28] As before, borrowers refinance when their expected gain from refinancing (including their logit private shock to refinancing costs) exceeds their optimal threshold, which—starting in column 2—also depends on γ. Once we model these unobserved refinancing costs with γ, estimates of β increase significantly. The estimate of β in columns 2 implies that a 10 bp decrease in rates increases refinancing conditional on

[28] In our internal refinancing context, no fee is required paid to the lender as in other settings. We set a small nominal $\kappa_0 = €100$ for estimation purposes to bound the refinancing cost function away from zero.

being attentive by approximately 50 bp.[29]

However, the implied estimate of fixed costs in the specification of column 2, which does not yet allow for attention effects, is implausibly high ($\exp(\hat{\gamma}) \approx$ €514,000). Even allowing for the interpretation of this fixed cost to include the mental, time, and hassle costs of refinancing, the large estimates are perhaps more consistent with mortgage borrower inattention, which the specification in column 2 is constrained to attribute to borrowers behaving as if their costs of refinancing were incredibly high. When we allow for attention effects in column 3, the fixed cost parameter is reduced substantially from 13.2 to 6.4, demonstrating how allowing for a certain fraction of mortgage borrowers to be inattentive to refinancing improves the model's fit of the data. The estimate of γ in column 3 implies a cost of refinancing of approximately €620.

The estimate of the probability of being inattentive is $\Lambda(\hat{\delta}_0) \approx 78\%$ in column 3. Although consistent with a substantial likelihood of being inattentive, this estimate pools the control group and the treatment group. Columns 4 and 5 allow mortgage borrowers who received disclosure letters with design improvements and those that additionally received follow-up reminder letters 4-6 weeks later to have different levels of attention. The estimate of δ_0 in column 4 measures the control group's average probability of inattention to be $\Lambda(1.13) \approx 76\%$. The treatment effects estimates in column 4 imply that the combination of redesigned disclosures and follow-up reminders decreased inattention by 16 pp in total: 6 pp from redesigned disclosure letters and 10 pp from the reminders.

The fixed-cost estimate increases when we allow for treatment effects on inattention, with the estimate of γ in column 4 of Table 7 implying a €6,000 cost of refinancing. This higher cost of refinancing in column 4 than column 3 suggests that the specification in column 3 was misattributing some of the more responsive refinancing of the treatment groups to having a lower cost of refinancing. Once allowing for the treatment groups to have lower inattention in column 4, it is clear that the control group still behaves as if they have a high cost of refinancing, consistent with overall pessimistic beliefs about the time and effort required to refinance a mortgage (Central Bank of Ireland, 2017b). Column 5 adds controls that allow for heterogeneity in refinancing costs along observable dimensions to test whether certain groups have stronger inertia, with refinancing inertia increasing in age and first-time homebuyer status and decreasing in Covid forbearance. The estimates of the treatment effects on attention and the fixed cost estimates are similar to column 4. Overall, the redesigned disclosure treatment and subsequent follow-up reminder decrease the probability

[29] To interpret refinancing magnitudes, we consider effects in the neighborhood of a refinancing incentive of 100 bp, which is roughly the median incentive in Appendix Figure A6. A 10 bp increase in the refinancing incentive then increases attentive refinancing by $\Lambda(1.1e^{\beta}) - \Lambda(e^{\beta})$.

of being inattentive using the column 5 estimates by 20 pp from 76% to 56%.

The estimates are consistent with the reminders having a large effect on refinancing by increasing the probability that a given borrower is attentive. Reconciling the nontrivial effects of the treatments without reminders on inattention in Table 7 with the more modest effects in Figure 4, recall that the total effect of the treatment on refinancing is the increase in the probability of attending to the task of refinancing times the probability of refinancing for a given refinancing incentive conditional on paying attention. Because this second term is low, the total effect of improving attention by a few percentage points through redesigned disclosures is still somewhat muted, consistent with the material implied fixed cost of refinancing γ in columns 2-4.

6.3 Comparison to Interest-Rate Changes

We use our model estimates to measure the relative partial-equilibrium effectiveness of cutting interest rates (which increases the refinancing incentive I by lowering r^{new}) versus sending a reminder as effective as our field experiment reminders that decrease the probability of inattention $\Lambda(\delta' w_i)$. This exercise is particularly policy relevant when monetary policy is de facto constrained by a zero lower bound, complicating efforts to decrease interest rates through conventional monetary policy, or when pass-through from policy rates to mortgage rates is otherwise impaired. Similarly, when rates are set in a monetary union as in the euro zone or in the United States, the optimal policy rate may differ across regions, in which case non-monetary measures available to individual regions to stimulate demand may be valuable.

The estimates suggest that there is significant scope for direct-to-household communication from the central bank or other policymakers in the form of reminder notices to provide monetary stimulus by spurring refinancing. Even when the incentive to refinance is approximately 0, the estimates in columns 4-5 of Table 7 predict that reminders will increase refinancing by 8-9 percentage points, which is within the 95% confidence interval of half of the treatment-reminder combinations in Figure 5. Reminders and lower interest rates are also complementary. When the average incentive to refinance is 100 bp, reminders increase refinancing by about 9% (an additional percentage point), with the modesty of the complementarity driven by the presence of inattention and our small estimates of β.

We can further use the model to contrast the effectiveness of targeted communication with the implied effect of the more conventional approach stimulating refinancing through decreasing mortgage interest rates. Figure 3 contrasts estimates of our experimental treatment effects from section 5 with the implied effect of decreasing mortgage rates by 100 bp in

Ireland, the US, and Denmark. The estimates of column 4 in Table 7 imply that if mortgage interest rates fell by 100 basis points, refinancing would only increase by 1.2 pp, comparable to the small effect of the average redesigned disclosure treatment without reminders.[30] This effect of even a large interest-rate change is small both because baseline inattention is so high (76% in column 4) and because the coefficient $e^{\hat{\beta}}$ on refinancing incentives is small even when accounting for inattention. The latter reason for large equivalent effects could arise from the limited amount of cross-sectional variation in refinancing incentives in our data (Appendix Figure A6). However, even using the Andersen et al. (2020) estimate for Denmark of $\hat{\beta} \approx 0.7$, the effect of a 100 bp interest rate change on refinancing is still 2.4 pp—much smaller than the best performing treatment and reminder combination (6.8 pp in Figure 3).

To validate our refinancing model estimates and test whether they are representative of other contexts, we estimate the specification of column 3 of Table 7 on US data and calculate the implied effects on refinancing in Denmark using estimates from Andersen et al. (2020). While we cannot estimate experimental treatment effects on attention in other contexts, we can use observational data and the functional form of the mixture model to describe the sensitivity of US and Danish borrowers to refinancing incentives. This exercise also illustrates the degree to which Irish borrowers in our 2020 experiment are particularly unique in their refinancing attention or elasticity. Using the CRISM data used in Figure 1, we estimate the model on a similarly sized random sample of US mortgages from May to September of 2019, a period when US interest rates were relatively stable.[31] Using the results that $\hat{\beta} = -.36$ and $\hat{\delta}_0 = 2.15$, we estimate how much decreasing mortgage interest rates by 100 bp in the US would increase mortgage prepayment rates over a four-month period. Given the interest-rate sensitivity among borrowers in the US data and holding their inattention fixed, the model predicts that increasing refinancing incentives from 100 to 200 bp would increase mortgage prepayment by 1.4 pp.

We can also use the estimates of Andersen et al. (2020) to calculate the implied effect on external refinancing in Denmark of lowering mortgage rates by 100 bp. The estimates of β and δ_0 (χ) from Model 4 of Table 2 in Andersen et al. (2020) imply that increasing

[30]The model-implied change in refinancing from increasing the incentive to refinance from 100 to 200 bp while holding inattention fixed at its baseline level is $\left(1 - \Lambda(\hat{\delta}_0)\right)\left(\Lambda(2e^{\hat{\beta}}) - \Lambda(e^{\hat{\beta}})\right)$.

[31]We treat r_i^{new} from equation (3) as 4.15%, the average of the weekly Freddie Mac new mortgage conventional interest-rate series over the four-month window we consider. For comparability with our Irish sample, we restrict the sample to conventional 30-year fixed-rate first mortgages with balances between \$50,000 and \$1,000,000 that were outstanding, current, and not scheduled to mature before September 2019. In the US data, we cannot observe the difference between refinancing and non-distressed prepayment (e.g., from a borrower selling her home).

refinancing incentives from 100 to 200 bp in Denmark would increase refinancing by 1.5 pp.[32] The small size and similarity of these effects (1.2, 1.4, and 1.5 pp in Ireland, the US, and Denmark, respectively) are consistent with reminders being potentially more powerful than monetary stimulus at stimulating refinancing and further supports the representativeness of the Irish setting to study attention effects and mortgage refinancing.

Several qualifications apply to this exercise. First, an increase in refinancing incentives from an average of 100 bp to 200 bp is outside the data for the majority of borrowers in our sample, suggesting that caution should be exercised when extrapolating to larger rate swings (although we also note that estimated refinancing incentives are more dispersed in the US and Danish data). Relatedly, general equilibrium considerations loom when predicting the effects of large interest-rate changes from our partial equilibrium model using cross-sectional parameter estimates. Many features of the economy could change if rates were to fall by a large amount (Ascari and Haber, 2021). Particularly relevant to our setting is the possibility that aggregate attention to refinancing could increase substantially in response to a large rate cut given non-linearities in refinancing incentives (Berger et al., 2021; Eichenbaum et al., 2022). However, despite these caveats, the strong qualitative conclusion from this exercise is that the reminder effects are significantly more powerful than a typical change in policy rates. Importantly, we also note that the comparison above is to a *mortgage* rate decrease of 100 bp, which would generally require aggressive or extraordinary measures to achieve given the limited pass-through from ECB policy rates to mortgage interest rates and the apparent lower bound on nominal policy rates.

7 Conclusion

In this paper, we study an intervention targeted at improving the last-mile delivery of monetary policy to the real economy. We use a field experiment combined with a mixture model of inattentive financial decision-making to demonstrate that targeted communication can help overcome the attention frictions that inhibit the refinancing channel of monetary policy transmission. A follow-up reminder to the redesigned mandated disclosure increased the take-up of in-the-money refinancing opportunities by 76%—substantially more than any prior effort in the literature.

What impact might the refinancing effects we document have on borrower consumption? The average 12-month savings realized by refinancing mortgage borrowers was €1,209. Using the MPC out of interest rate changes among UK mortgage holders estimated by the Bank

[32]Figure 3 lacks confidence intervals for the Denmark effect because the variance-covariance matrix of the Andersen et al. (2020) ML estimates is unavailable.

of England of 0.5 (Anderson et al., 2014), we estimate that refinancing households increased their consumption by €605.[33] Averaged across all households receiving a reminder letter, this suggests that the best redesigned disclosure letter and accompanying reminder increased borrower consumption by an expected €42 per household. Conservatively assuming that the redesigned disclosure and reminder letters cost €1 each to produce and deliver, this implies a borrower consumption multiplier of €42 for every €1 spent on communication to households about the opportunity to refinance. The expected effects on *aggregate* consumption may be less than the effect on borrower consumption after taking into account a potential offsetting loss in income by the bank's domestic shareholders.[34]

Estimates of an extended version of the Andersen et al. (2020) model of inattentive refinancing suggest that the reminder disclosures had large effects precisely because they increased the probability that a given consumer was attentive to the task of refinancing. These results suggest that other policies that have tried to stimulate refinancing have not succeeded because they have not sufficiently stimulated attention to the refinancing task. Using our model estimates, we find that communication reminding mortgage borrowers of refinancing opportunities has significant potential to be an effective monetary policy tool to complement or substitute for lowering rates. We estimate that mortgage interest rates falling by 100 bp in Ireland, the US, or Denmark would have much smaller effects on refinancing than the reminders we study. Moreover, given limited pass-through of policy rates to retail interest rates, a large decrease in mortgage interest rates would likely require unconventional monetary stimulus to achieve.

Several caveats apply to our estimates. Repeated reminders may be more or less effective than the one-shot reminder we studied here. Repeated reminders may lose their salience if households learn to rely on them instead of proactively acquiring their own information on refinancing activities (Ericson, 2017). Mailed reminder letters may be less effective in markets with a higher prevalence of junk mail. However, it's also possible that as consumers become attuned to reminder letters, they would trust them more with potential spillovers through social learning. We also note that reminders are more effective when rates have fallen and may not be as impactful in a rising rate environment. However, broadly speaking, policymakers are generally not keen to stimulate refinancing in such an environment anyway.

The treatment effects we study here are also likely to be more effective when the status-quo disclosure letter is less transparent to begin with. Streamlining, personalizing, simpli-

[33] We caveat that the Bank of England estimate relates to the MPC of borrowers out of higher mortgage interest payments instead of the mortgage repayment savings we study here. However, this MPC choice is conservative relative to the Di Maggio et al. (2017) estimated MPC out of interest savings in the US of 0.75.

[34] Any offsetting consumption effects by bank shareholders is muted when many bank shareholders are foreign or high-wealth (Di Maggio et al., 2020b).

fying, and highlighting are more valuable in the context of confusing, onerous, and overly detailed disclosures. Given the importance of household trust in the disclosing entity, it may be advantageous for the communication to be sent directly by a government agency or central bank than from a for-profit bank, although emphasizing that the letter itself is mandated could help. Finally, as discussed above, some of the benefits of increased refinancing may be eroded by higher initial interest rates in general equilibrium. However, it's also possible that the need for reminders would decrease in equilibrium if more attentive refinancing led banks to decrease the spread between their offered variable rates and policy rates in the first place.

Communication with mortgage holders about refinancing opportunities may interest policymakers at different points in the business cycle. First, central banks engaging in accommodative monetary policy could send direct and customized communication reminding borrowers of refinancing opportunities to improve interest rate pass-through and strengthen the impact of interest rate reductions on the real economy. Second, during contractionary monetary policy phases, central banks may seek to improve household financial stability by encouraging households into fixed rate products as interest rates rise. In addition, several categories of governmental entities could employ the policy lever we evaluate here, including fiscal authorities seeking to stimulate refinancing and consumption, competition authorities aiming to improve the competitiveness of the mortgage market, and consumer protection authorities focused on improving households debt service burdens.

Finally, our results contribute to a growing body of evidence that demonstrates the value of behaviorally informed approaches in delivering effective consumer protection in essential product markets. In particular, the results we document here are the first to demonstrate statistically and economically meaningful improvements in the stubbornly persistent puzzle of low take up of advantageous mortgage refinancing opportunities.

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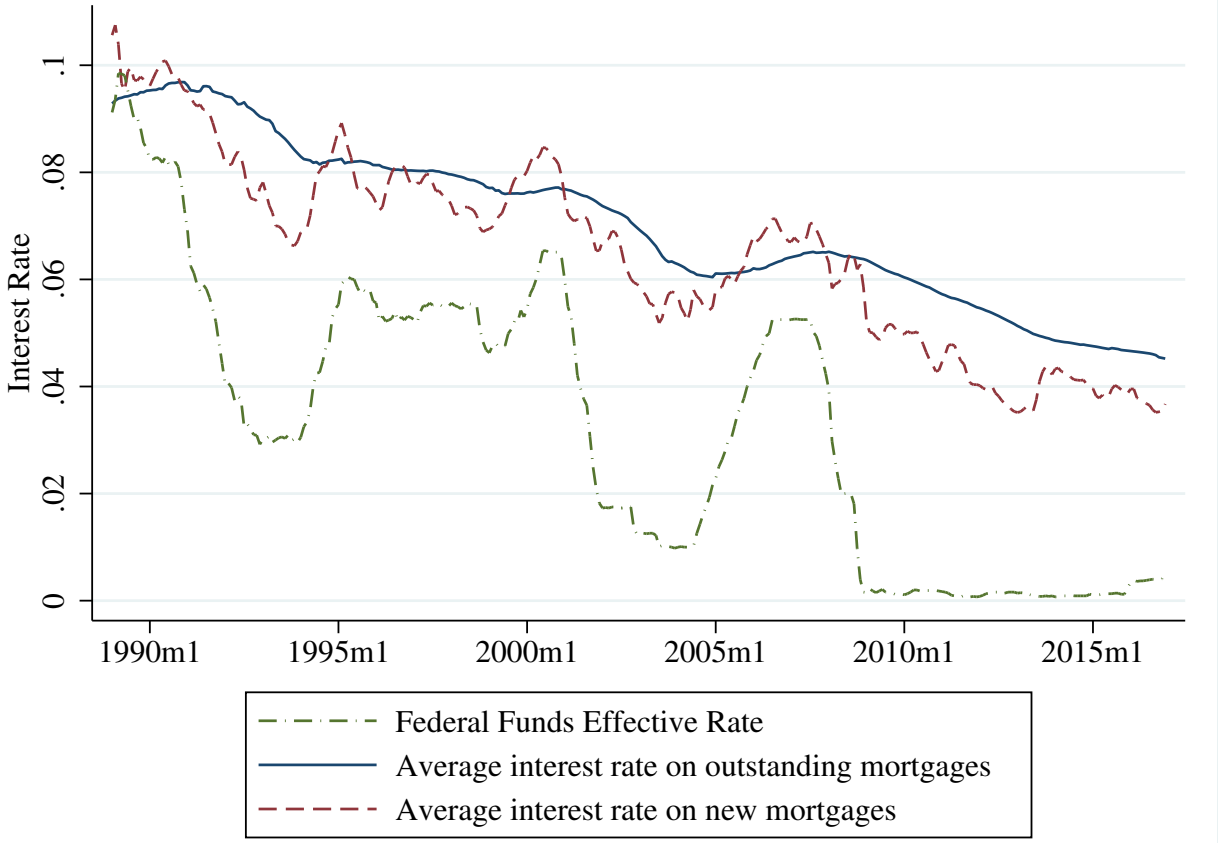
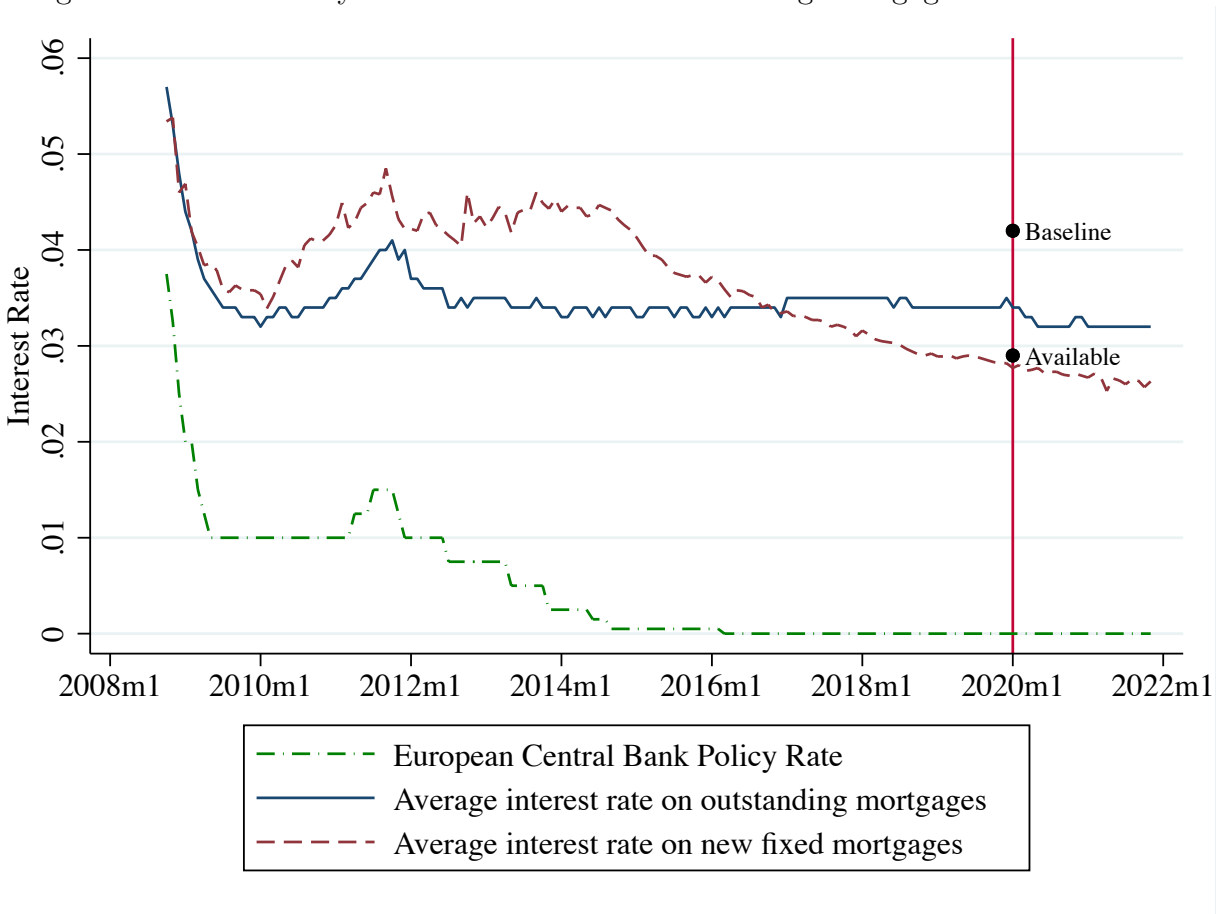
Figure 1: US Policy Rates and New and Outstanding Mortgage Interest Rates



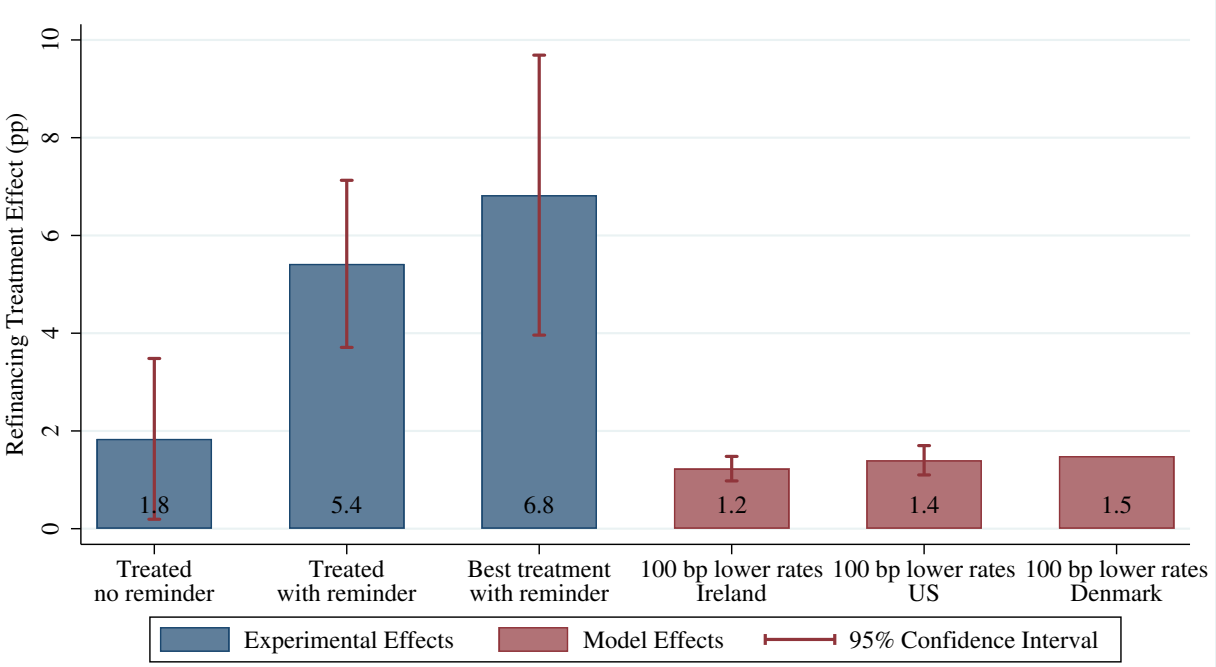
Note: Figure plots average mortgage interest rates by month for outstanding mortgages (solid blue line), newly originated mortgages (dashed red line), and the effective Federal Funds Rate (dashed-dotted green line). Outstanding and new mortgage interest rates are calculated from CRISM (see Di Maggio et al., 2020a for details). Effective Federal Funds Rate is from the Board of Governors of the Federal Reserve System.

Figure 2: Ireland Policy Rates and New and Outstanding Mortgage Interest Rates



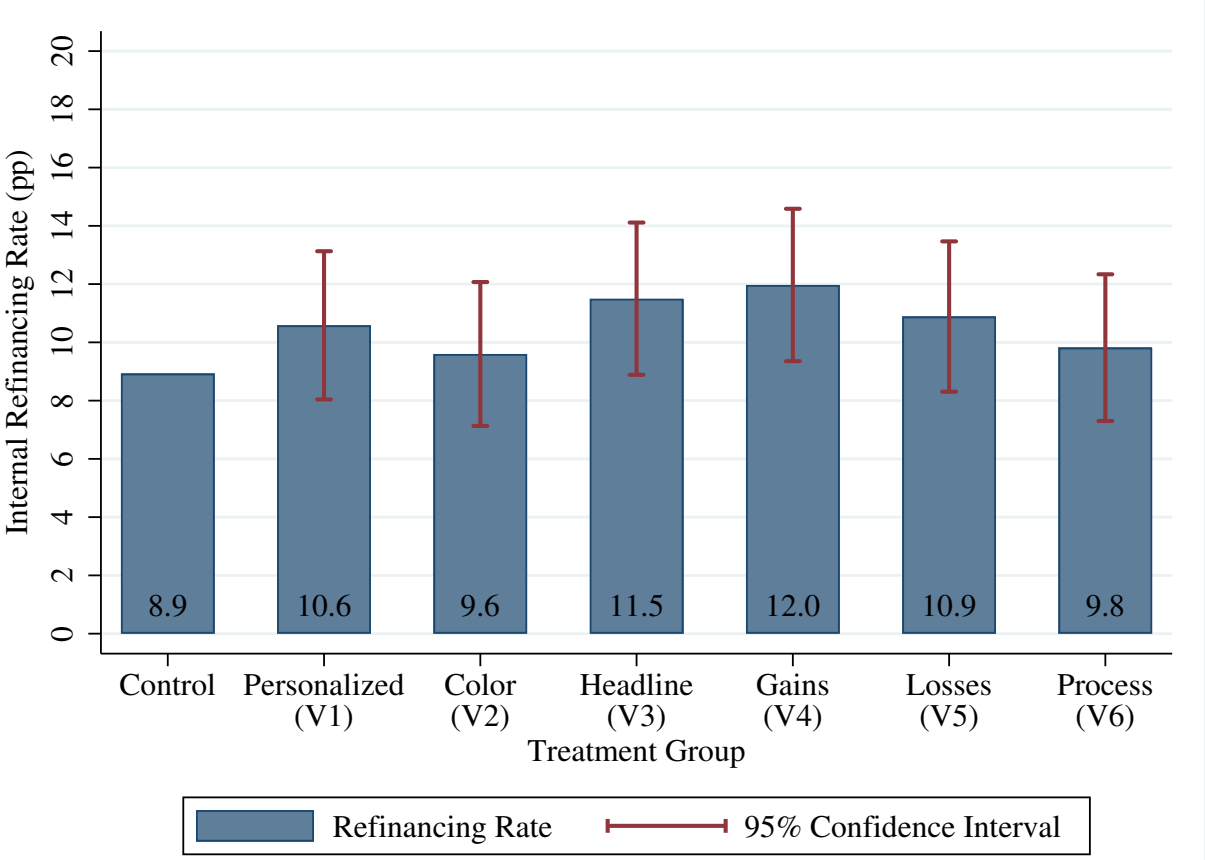
Note: Figure plots average mortgage interest rates by month for outstanding mortgages (solid blue line), newly originated fixed rate mortgages (dashed red line), and the European Central Bank Main Refinancing Operations Rate (dashed-dotted green line). Outstanding and new mortgage interest rates are calculated from Central Bank of Ireland Retail Interest Rate data. The European Central Bank rate is from the ECB Statistical Data Warehouse. The baseline dot represents the average interest rate at the outset for those cohorts within the experiment. The available dot is the interest rate achievable by taking up the refinancing offer.

Figure 3: Refinancing Treatment Effects



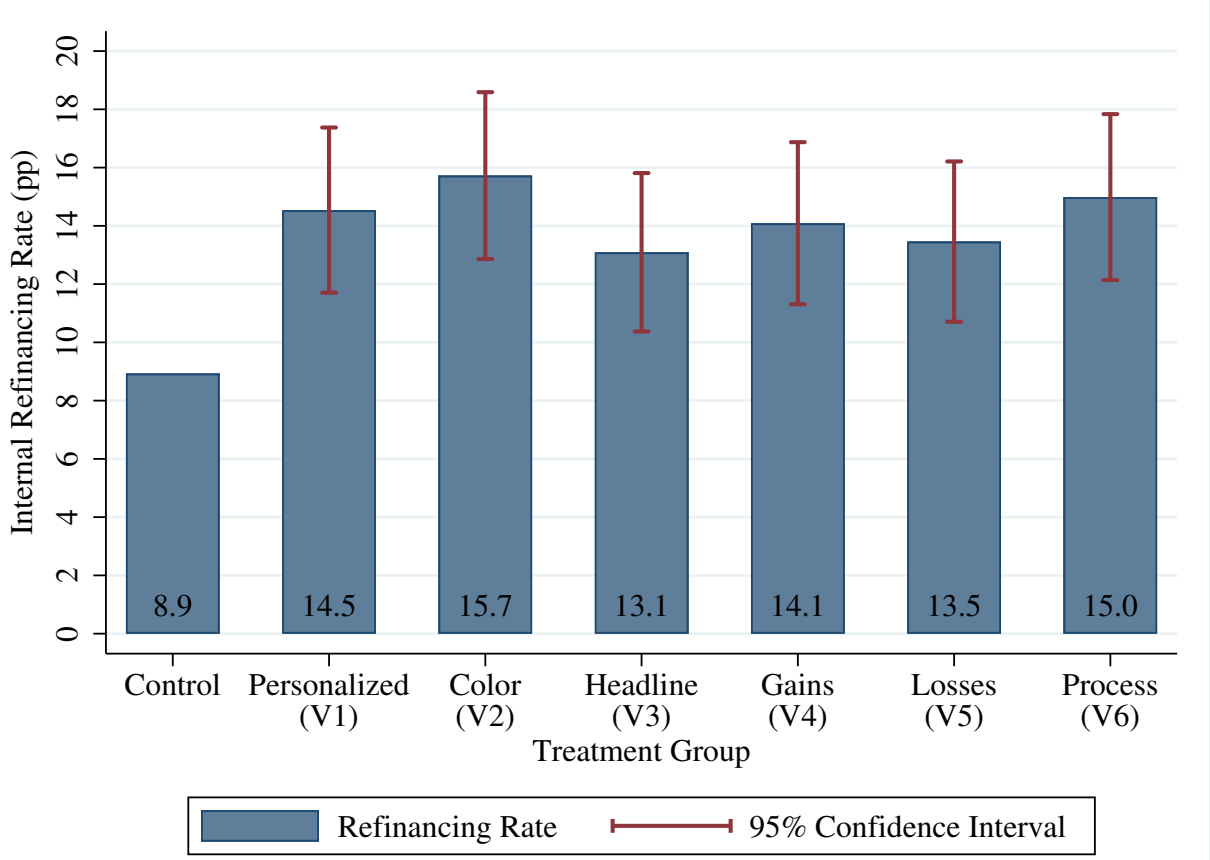
Notes: Figure plots treatment effects on refinancing rates in percentage points for observed and hypothetical events. Blue bars on the left report treatment effects estimated using our experimental data. Red bars on the right report the implied effect of a 100 bp decrease in mortgage rates using estimates of the Andersen et al. (2020) model using data from the indicated country. Treated no reminder is the increase in the internal refinancing rate relative to the control group for borrowers across all treatment arms who did not receive a follow-up reminder letter. Treated with reminder is the increase in the internal refinancing rate relative to the control group for borrowers randomly assigned to any treatment arm who did receive a follow-up reminder letter. Best treatment with reminder is the increase in the internal refinancing rate relative to the control group for borrowers randomly assigned to the best performing treatment arm with a follow-up reminder letter (V2). Effect of 100 bp lower mortgage rates in Ireland is the implied increase in the internal refinancing rate for the control group from an increase in the refinancing incentive from 100 to 200 bp holding attention fixed, calculated using the estimates in column 4 of Table 7 as $(1-\Lambda(\hat{\delta}_0))(\Lambda(2e^{\hat{\beta}})-\Lambda(e^{\hat{\beta}}))$. Effect of 100 bp lower mortgage rates in the US similarly uses $\hat{\beta} = -.36$ and $\hat{\delta}_0 = 2.15$ from estimating the specification of column 3 of Table 7 on US data on mortgage prepayment from May-September 2019. See section 6.3 for details. Effect of 100 bp lower mortgage rates on external refinancing in Denmark uses estimates from Model 4 of Table 2 in Andersen et al. (2020) to estimate the effect of increasing the refinancing incentive from 100 to 200 bp holding attention fixed. Internal refinancing is defined as a borrower switching mortgage products with the partner bank within four months of initial treatment. Error bars denote robust 95% confidence intervals. For the model-implied refinancing effects, confidence intervals are calculated by the Delta method using the robust covariance matrix of the model estimates, which is unavailable for the Denmark point estimates.

Figure 4: Refinancing Rates by Treatment Arm: No Reminder Sample



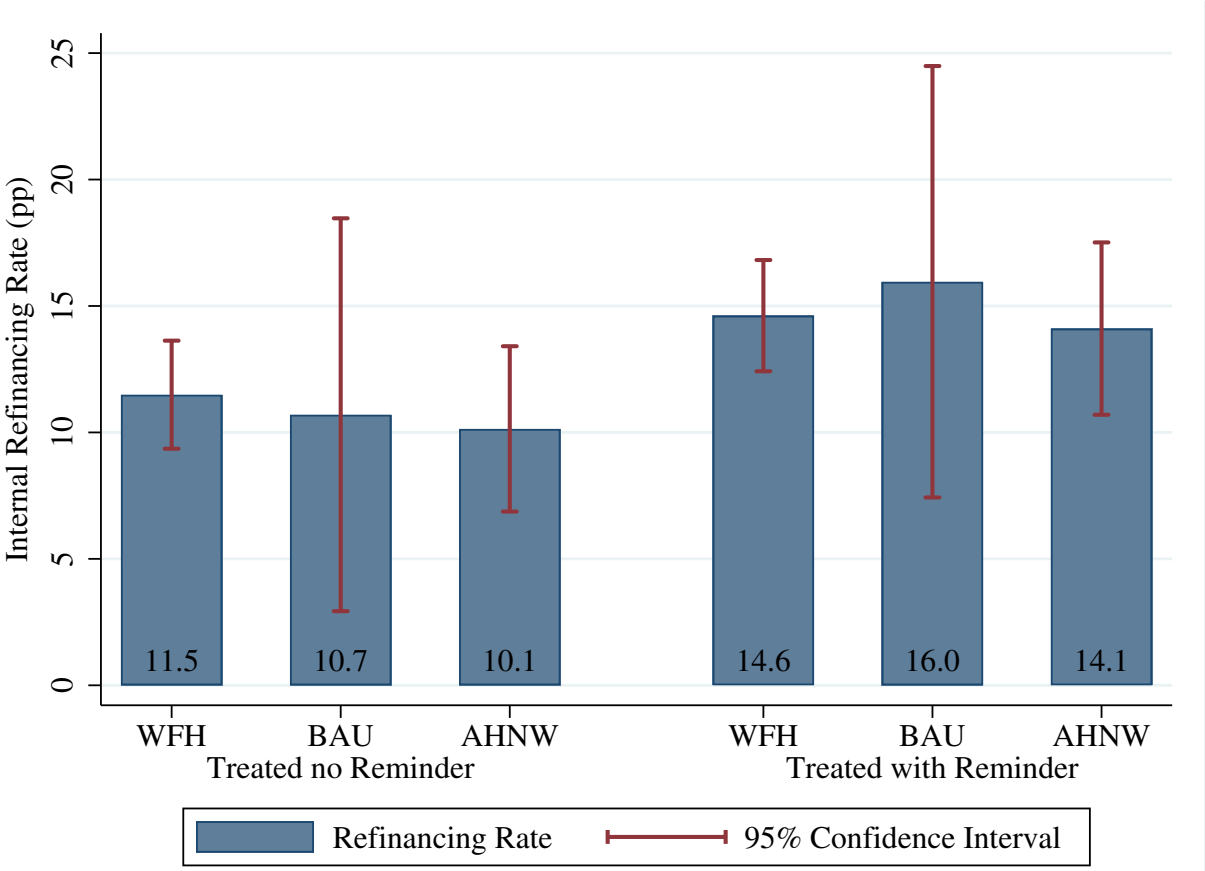
Notes: Figure plots internal refinancing rates by treatment arm for the subset of the sample that did not receive a reminder letter. Error bars denote 95% confidence intervals based on robust standard errors for the difference between the control group and each treatment arm.

Figure 5: Refinancing Rates by Treatment Arm: Reminder Sample



Notes: Figure plots internal refinancing rates by treatment arm for the subset of the sample that did receive a reminder letter along with the control group. Error bars denote 95% confidence intervals based on robust standard errors for the difference between the control group and each treatment arm.

Figure 6: Refinancing Rates by Employment Sector



Note: Figure plots internal refinancing rates by employment sector and treatment category. WFH, BAU, and AHNW denote mortgage borrower employment industries more likely to be working from home, experiencing business as usual, and being at home but not working, respectively, during the estimation window. See Appendix Table A5 for our employment sector classification scheme. The left three bars plot refinancing rates for borrowers that received the redesigned disclosure treatment but not a reminder. The right three bars plot refinancing rates for borrowers that received the redesigned disclosure treatment and a follow-up reminder letter. Error bars denote 95% confidence intervals based on robust standard errors for the within-sector difference between the control group and each treatment arm.

Table 1: Treatment Arms Overview

Disclosure Redesign Element	Treatment Group						
	Control	V1	V2	V3	V4	V5	V6
Simplification		✓	✓	✓	✓	✓	✓
Personalized savings estimate		✓	✓	✓	✓	✓	✓
Neutral frame		✓	✓	✓			
Color			✓				
Prominent subject line				✓	✓	✓	✓
Gain frame					✓		
Loss frame						✓	✓
Clarified process box							✓
Follow-up reminder letter		1/2	1/2	1/2	1/2	1/2	1/2

Notes: Chart overviews the additional design elements incorporated into each treatment arm. See section 4.1 for a description of each element. The control group column indicates that the control group received the existing standard disclosure without any additional design elements. The reminder row indicates that a randomly assigned half of each of the six treatment arms received a follow-up reminder letter 4-6 weeks after the initial treatment.

Table 2: Descriptive Statistics

Group	(1) Control	(2) Treated no reminder	(3) Treated with reminder	(4) Market (variable rate)	(5) Market (all)
Dublin	0.20	0.19	0.20	0.27	0.28
	(0.40)	(0.39)	(0.40)	(0.44)	(0.45)
Borrower age	49.74	50.10	49.99	48.99	48.24
	(9.26)	(9.41)	(9.31)	(9.90)	(9.63)
First Time Buyer	0.41	0.41	0.39	0.39	0.38
	(0.49)	(0.49)	(0.49)	(0.49)	(0.49)
Mortgage balance	83,503	80,617	82,027	102,688	128,238
	(84,125)	(87,748)	(92,103)	(95,037)	(111,522)
Interest rate	0.042	0.042	0.042	0.036	0.026
	(0.003)	(0.002)	(0.002)	(0.006)	(0.01)
Years to maturity	13.87	13.22	13.29	14.63	15.90
	(8.54)	(8.47)	(8.49)	(8.85)	(8.64)
1-Year savings	1,044	1,019	1,028	968	-60.88
	(1,010)	(1,115)	(1,093)	(1,120)	(1,827)
Covid forbearance	0.09	0.08	0.08	0.11	0.13
	(0.28)	(0.27)	(0.28)	(0.32)	(0.34)
Observations	1,613	4,796	4,791	206,083	538,956

Notes: Table reports means and standard deviations in parentheses of mortgage borrower characteristics for the control group in column 1, loans treated with redesigned disclosures but without a reminder in column 2, and loans treated with redesigned disclosures and with a follow-up reminder letter in column 3. In columns 4-5, we report descriptive statistics from the Loan Level Data of the Central Bank of Ireland covering about 90% of outstanding mortgages, regardless of lender. Column 4 reports statistics on all outstanding variable-rate mortgages and column 5 reports on all outstanding residential mortgages. Dublin is an indicator for whether the mortgaged property is located in Dublin. Borrower age of the oldest borrower on the mortgage. First Time Buyer indicates whether the borrower is a first time-buyer. Mortgage balance is amount outstanding on loan at the time of experiment in euros. Interest rate is the interest rate applicable on the loan at the outset of the experiment. 1-year savings is the amount in euros of savings available to the borrower in the first year after refinancing to the best available rate. Covid forbearance indicates whether the borrower was using Covid payment break (introduced in Ireland in March 2020 to alleviate short-term liquidity constraints faced by borrowers experiencing financial difficulties due to the impact of the pandemic). Covid forbearance shares for the market comparisons are measured from loan-level data collected by the Central Bank of Ireland as at June 2021, while all other variables are measured at the outset of the field trial.

Table 3: Internal Refinancing Treatment Effects

	(1)	(2)	(3)	(4)
Disclosure Redesign Treatment	0.036***	0.040***	0.018**	0.022***
	(0.008)	(0.008)	(0.008)	(0.008)
Treatment × Reminder			0.036***	0.035***
			(0.007)	(0.007)
Constant	0.089***	-0.311***	0.089***	-0.307***
	(0.007)	(0.067)	(0.007)	(0.066)
Controls		✓		✓
Observations	11,200	11,200	11,200	11,200
R-squared	0.002	0.042	0.004	0.044

Notes: Table reports treatment effects on an indicator variable equal to one if the borrower internally refinanced, defined as a borrower changing their mortgage product with the partner bank within four months of initial treatment, and zero otherwise. Disclosure Redesign Treatment is an indicator that the borrower was randomly assigned to one of the six treatment arms. Reminder is an indicator for whether that borrower received a follow-up reminder letter 4-6 weeks after the initial treatment as in Appendix Figure A5. Control variables in columns 2 and 4 are listed in Table 2. Robust standard errors in parentheses. *** $p<0.01$, ** $p<0.05$, * $p<0.1$.

Table 4: Internal Refinancing Treatment Effects by Treatment Arm

	(1)	(2)	(3)	(4)
Personalized Treatment (V1)	0.036***	0.040***	0.017	0.021*
	(0.011)	(0.011)	(0.013)	(0.013)
Color Treatment (V2)	0.038***	0.042***	0.007	0.011
	(0.011)	(0.011)	(0.013)	(0.012)
Headline Treatment (V3)	0.034***	0.038***	0.026*	0.029**
	(0.011)	(0.011)	(0.013)	(0.013)
Gains Treatment (V4)	0.041***	0.044***	0.030**	0.032**
	(0.011)	(0.011)	(0.013)	(0.013)
Losses Treatment (V5)	0.032***	0.036***	0.020	0.025*
	(0.011)	(0.011)	(0.013)	(0.013)
Process Treatment (V6)	0.035***	0.038***	0.009	0.012
	(0.011)	(0.011)	(0.013)	(0.013)
Reminder ×				
Personalized Treatment (V1)			0.040**	0.037**
			(0.017)	(0.016)
Color Treatment (V2)			0.061***	0.060***
			(0.016)	(0.016)
Headline Treatment (V3)			0.016	0.018
			(0.016)	(0.016)
Gains Treatment (V4)			0.021	0.024
			(0.017)	(0.016)
Losses Treatment (V5)			0.026	0.023
			(0.016)	(0.016)
Process Treatment (V6)			0.052***	0.050***
			(0.017)	(0.016)
Constant	0.089***	-0.312***	0.089***	-0.309***
	(0.007)	(0.067)	(0.007)	(0.066)
Controls		✓		✓
Treatment effects equality p-value	0.986	0.989	0.609	0.685
Reminder effects equality p-value			0.310	0.362
Observations	11,200	11,200	11,200	11,200
R-squared	0.002	0.042	0.005	0.045

Notes: Table reports treatment effects by treatment arm on internal refinancing, defined as a borrower changing their mortgage product with our partner bank within four months of initial treatment. See Table 1 for summary of treatment arm features. Reminder is an indicator for whether that borrower received a follow-up reminder letter 4-6 weeks after the initial treatment as in Appendix Figure A5. Control variables in columns 2 and 4 are listed in Table 2. Treatment effects equality p-values are from a joint F-test that the six treatment coefficients are equal to each other. Reminder effects equality p-values are from a joint F-test that the six reminder × treatment arm coefficients are equal to each other. Robust standard errors in parentheses. *** $p<0.01$, ** $p<0.05$, * $p<0.1$.

Table 5: Refinancing Treatment Effect Heterogeneity

	(1)	(2)	(3)	(4)
		I. Individual Characteristics		
Covariate $x \longrightarrow$	Dublin	Age $>$ 50	FTB	Covid
Treatment	0.014	0.028**	0.009	0.016*
	(0.009)	(0.012)	(0.010)	(0.009)
Treatment $\times$ Reminder	0.036***	0.044***	0.030***	0.036***
	(0.007)	(0.010)	(0.008)	(0.007)
Treatment $\times$ x	0.020	-0.020	0.024	0.025
	(0.021)	(0.017)	(0.017)	(0.033)
Treatment $\times$ Reminder $\times$ x	-0.001	-0.019	0.016	-0.0002
	(0.017)	(0.013)	(0.014)	(0.027)
Covariate x	-0.007	-0.031**	0.016	0.017
	(0.017)	(0.014)	(0.015)	(0.027)
Constant	0.091***	0.103***	0.083***	0.088***
	(0.008)	(0.010)	(0.009)	(0.007)
Observations	11,200	11,200	11,200	11,200
R-squared	0.004	0.012	0.009	0.005
		II. Loan Characteristics		
Covariate $x \longrightarrow$	High Balance	$r > 4.2\%$	YTM $>$ 13	High Savings
Treatment	0.013	0.018	-0.003	0.011
	(0.009)	(0.013)	(0.010)	(0.008)
Treatment $\times$ Reminder	0.036***	0.025**	0.038***	0.035***
	(0.007)	(0.010)	(0.008)	(0.007)
Treatment $\times$ x	0.018	-0.0004	0.046***	0.027
	(0.018)	(0.017)	(0.017)	(0.019)
Treatment $\times$ Reminder $\times$ x	-0.003	0.017	-0.004	0.001
	(0.015)	(0.014)	(0.014)	(0.015)
Covariate x	0.089***	0.015	0.053***	0.091***
	(0.015)	(0.014)	(0.014)	(0.016)
Constant	0.052***	0.080***	0.063***	0.052***
	(0.007)	(0.011)	(0.009)	(0.007)
Observations	11,200	11,200	11,200	11,200
R-squared	0.028	0.005	0.024	0.033

Notes: Table estimates treatment-effect heterogeneity by interacting the disclosure redesign treatment variable and the reminder treatment variable with borrower and loan characteristics in panels I and II, respectively. Each column estimates a different regression replacing the covariate x with the binary measure of heterogeneity indicated in that column's header. FTB stands for first-time buyer. Covid stands for Covid mortgage-payment forbearance. High Balance is an indicator for outstanding principal in excess of €75,000. The indicator $r > 4.2\%$ refers to the baseline prevailing interest rate on each borrower's mortage. The indicator YTM $>$ 13 denotes there are at least 13 years remaining until a mortgage matures. High savings denotes borrowers who stand to save more than €1,000 in their first year after refinancing. Robust standard errors in parentheses. *** $p<0.01$, ** $p<0.05$, * $p<0.1$.

Table 6: Parameter Values Used in Optimal Refinancing Model

Parameter	Name	Value	Source
Inflation	π	0.02	Average IE inflation
Real discount rate	ρ	0.05	Agarwal et al. (2013)
Nominal interest rate volatility	σ	0.002	CBI monthly interest rate series
Marginal tax rate for interest deduction	τ	0	Eliminated in Ireland in 2019
Exogenous Pr(termination)	μ	0.11	Microdata from partner bank
Perceived fixed costs of refinancing (€)	κ_0	100	Usual internal-refi cost is zero

Notes: Table reports parameter values used in the Agarwal et al. (2013) model of optimal refinancing discussed in section 6 adapted to the Irish mortgage market context.

Table 7: Inattentive Refinancing Model Maximum Likelihood Estimates

Parameter	(1)	(2)	(3)	(4)	(5)
Incentive Sensitivity (β)	-125.48***	-1.61***	-0.23	-1.58***	-1.65***
	(1.12)	(0.01)	(0.51)	(0.05)	(0.05)
Fixed Cost of Refinancing (γ)		13.15***	6.43***	8.71***	8.71***
		(0.70)	(0.49)	(0.03)	(0.20)
Inattention Constant (δ_0)			1.28***	1.13***	1.02***
			(0.19)	(0.11)	(0.12)
Treatment on Inattention (δ_1)				-0.31**	-0.33**
				(0.12)	(0.13)
Reminder on Inattention (δ_2)				-0.43***	-0.44***
				(0.08)	(0.09)
Fixed Cost Controls					✓
Observations	11,200	11,200	11,200	11,200	11,200
Log likelihood	-7,763	-4,111	-3,977	-3,912	-3,907

Notes: Table reports maximum likelihood estimates of the mixture model of inattentive refinancing described in the text. Incentive Sensitivity is the coefficient on the Agarwal et al. (2013) refinancing incentive described in section 6 using the parameters defined by Table 6, with coefficient $\exp(\beta)$. The fixed cost of refinancing constant γ estimates an average fixed cost term to rationalize observed refinancing variable. The fixed-cost controls allow for differences across groups in the estimated fixed cost of refinancing. The inattention constant δ_0 allows the inattention index in (8) to have a constant term. The inattention treatment effects allow borrowers who treated with redesigned disclosures (δ_1) and disclosure reminders (δ_2) to have different levels of attention. Age is demeaned. Covid indicates whether the borrower was approved for mortgage-payment forbearance with a Covid hardship. Robust standard errors in parentheses. *** $p<0.01$, ** $p<0.05$, * $p<0.1$.

Internet Appendix

A Robustness to Alternative Refinancing Parameters

A concern with our use of the Agarwal et al. (2013) model of optimal refinancing is that their model considers only US-style fixed-rate mortgages. Specifically, Agarwal et al. (2013) study the optimal exercise of the option to refinance a US-style fixed-rate mortgage into another fixed-rate mortgage, resetting the term of the new mortgage back to 30 years. In contrast, Irish fixed-rate mortgages do not have fixed interest rates for their entire duration, instead converting to variable-rate mortgages by default after an initial fixation period of usually 1-5 years. Furthermore, when mortgage borrowers in Ireland refinance, they generally keep their remaining term constant instead of restarting at 30 years or switching to an entirely different duration. In this appendix, we consider alternative formulations of the incentive to refinance that account for these differences in mortgage product design. Before proceeding, we note that despite the shorter fixation periods in Ireland relative to the US, Irish mortgage borrowers still behave similarly in terms of duration, with a typical mortgage lasting for around 12 years despite rolling over to a variable rate.

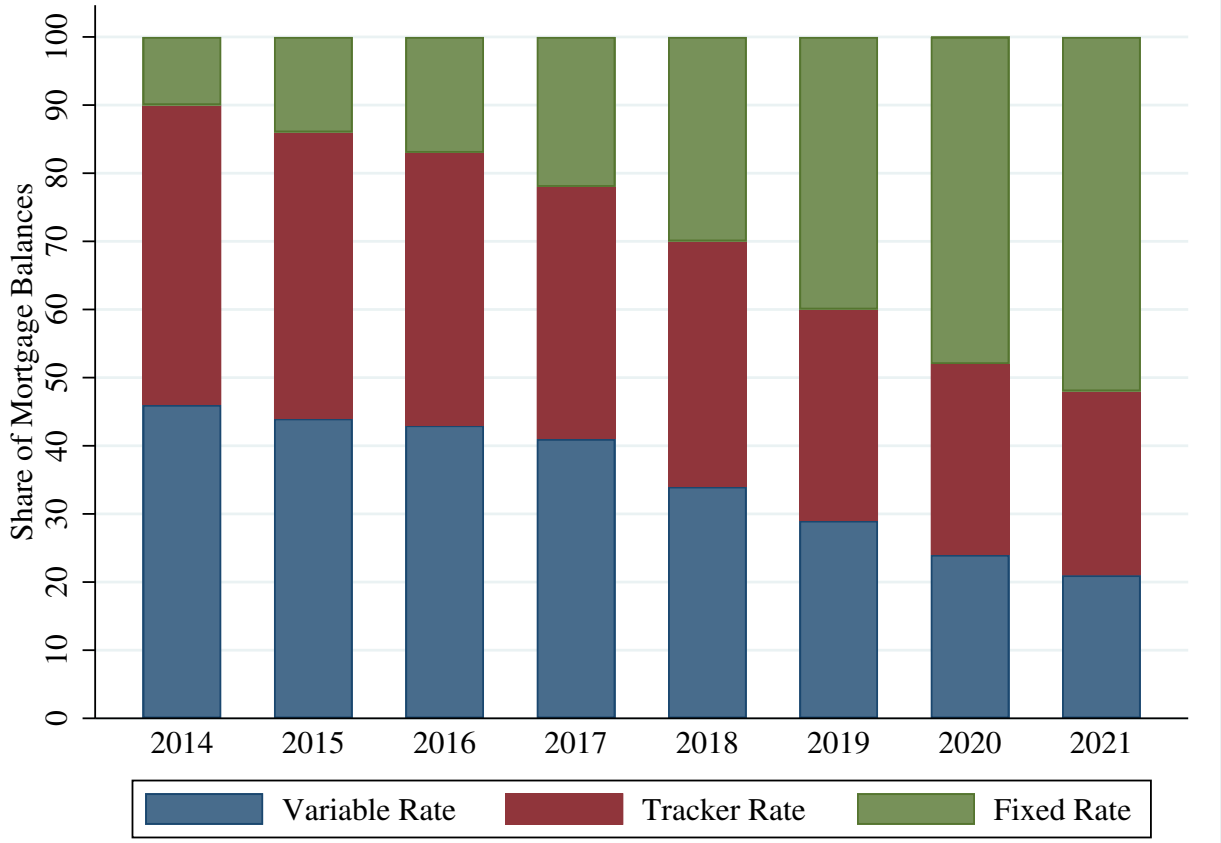
One approach to tweak the Agarwal et al. (2013) model to accommodate differences between mortgage systems is used by Fisher et al. (2022). They set the likelihood of prepayment for exogenous reasons to $\mu = 0.5$, which makes the actual duration of a typical mortgage approximately two years. By making borrowers expect the need to go back to the market for a new market-rate mortgage with such a high probability, this mimics the effect of having a fixation period end with the mortgage rolling over to a variable rate. Strictly speaking, this is not what happens in the data in Ireland—typical borrowers hold their mortgages much longer. However, we also adopt this approach in a robustness check of setting $\mu = 0.5$ to demonstrate that our core estimates are relatively insensitive to the particulars of the optimal mortgage refinancing model parameterization.

In Appendix Table A7, we report estimates from reestimating the maximum likelihood specification of section 6, formulating $I(x_i, \theta)$ with $\mu = 0.5$ and the other parameters the same as in Table 6. On the whole, the estimates are similar across the two tables. The fixed cost estimates are generally bigger in Appendix Table A7, with $\exp(\hat{\gamma}) \approx$ €3,133 in column 3, for example, but also more stable across specifications. The biggest change is a decrease in the baseline estimated rate of inattention $\Lambda(\hat{\delta}_0) \approx 64\%$ in column 3, down from 78% in Table 7. While a majority of borrowers are inattentive to the opportunity to refinance in either parameterization of the refinancing decision, it is intuitive that the model would find fewer households inattentive when borrower horizons are short because exogenous prepayment is high. In this case, which approximates a world where mortgages are not fixed rate for their entire duration, households may optimally fail to refinance because they will likely have to refinance soon anyway, reducing the length of time over which they should expect to have enjoyed the benefits of refinancing. This force serves to alleviate some of the pressure for inattention to explain low refinancing levels, reducing the estimated baseline inattention rate. However, even when allowing for this possible force to be stronger in the model than it seems in the data given slow Irish refinancing, inattention is still high. Moreover, even in Appendix Table A7, the combined treatment effect of receiving a redesigned disclosure and

follow-up reminder letter is still large and of a similar magnitude to the original maximum-likelihood results in Table 7.

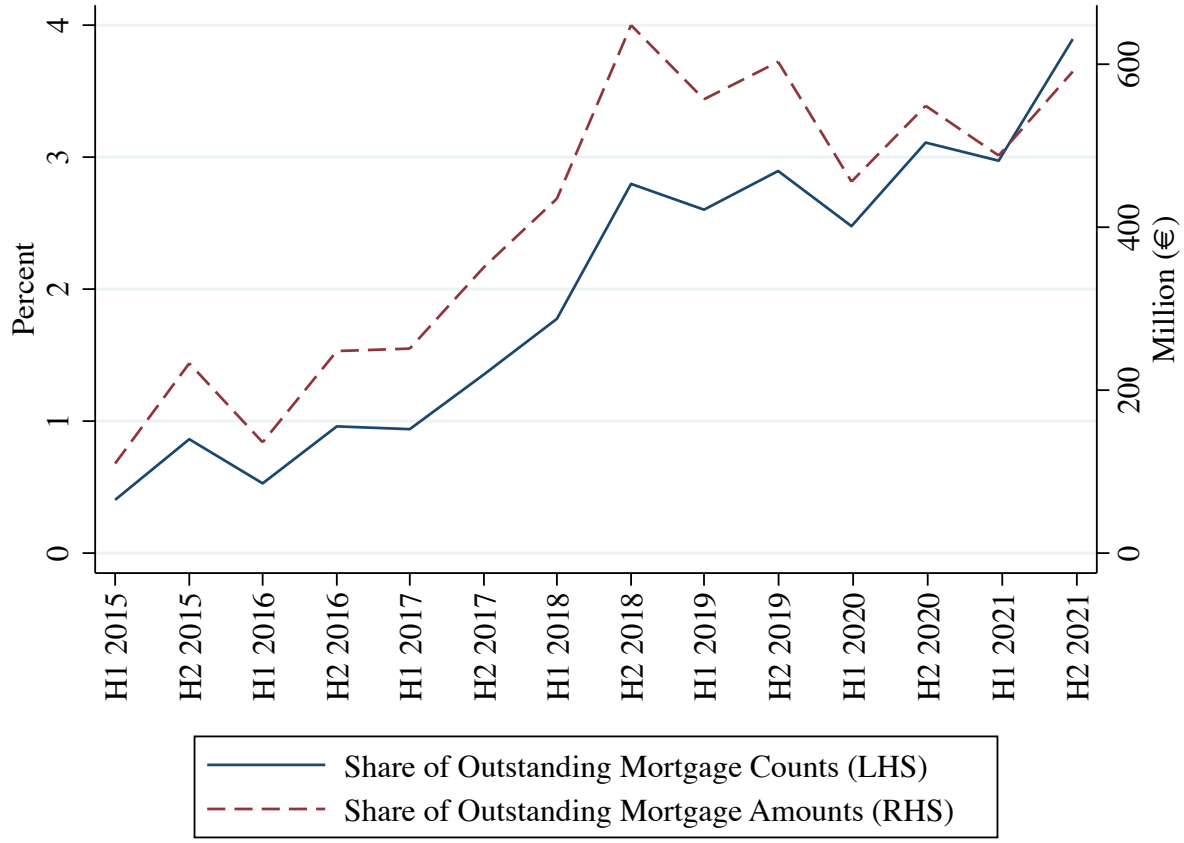
A final approach, also explored by Fisher et al. (2022) that abstracts away from the Agarwal et al. (2013) model is to remove the optimal threshold term $O(x_i, \theta)$ from the specification of the incentive to refinance given in equation (3). This interest-rate gap definition of the refinancing incentive is also popular in the mortgage refinancing literature (for recent uses, see, e.g., Berger et al., 2021; Eichenbaum et al., 2022). Doing so remains agnostic about the precise threshold for optimal options exercise and instead lets the incentive to refinance just be proportional to the interest-rate gap, defined as the difference between a borrower's current interest rate r_i^{old} and their potential rate if refinancing r_i^{new}. Again, we find that our core results are unchanged, further emphasizing that our conclusions are not driven by the particular form or parameterization of the Agarwal et al. (2013) model.

Figure A1: Market Share of Outstanding Mortgages by Product Type



Notes: Figure plots the share of total balances of outstanding residential mortgages in Ireland that are fixed rate, variable rate, or tracker rate. Source is the Central Bank of Ireland Retail Interest Rate Statistics series.

Figure A2: External Mortgage Refinancing Rates in Ireland



Notes: Figure plots the share of mortgages that switched lenders each half year (left axis) and the volume of outstanding balances in millions of euros that switched lenders each half year (right axis). Source is Central Bank of Ireland Loan Level Data.

Figure A3: Example Control-Group Disclosure Letter

Mortgage Account Number: 1234567

You may be able to save money on your mortgage

Dear John,

This letter supplements the information we sent with your annual mortgage loan statement in the leaflet called "Information about your mortgage (You may be able to save money on your mortgage)".

The standard variable interest rate we currently charge you on your mortgage loan is 4.34%. However, we want to make sure you are getting the best deal and we may have a lower interest rate for your mortgage.

What rates are available?

The lowest interest rate currently available to you is a one or two-year fixed rate of 2.9%. We also offer fixed rates for periods of three, five and ten years. The ten-year rate varies depending on your Loan to Value (LTV). We explain Loan to Value at the end of this letter.

Explaining the tables below

These tables show you the interest rates along with the Annual Percentage Rate of Charge (APRC). We explain APRC at the end of this letter.

Fixed interest rates

Fixed interest rate options	Loan to Value Up to 60%	Loan to Value 61-80%	Loan to Value over 80%
1-year	2.9% (3.9% APRC)	2.9% (4.2% APRC)	2.9% (4.4% APRC)
2-year	2.9% (3.8% APRC)	2.9% (4.0% APRC)	2.9% (4.3% APRC)
3-year	3% (3.7% APRC)	3% (3.9% APRC)	3% (4.1% APRC)
5-year	3.2% (3.7% APRC)	3.2% (3.8% APRC)	3.2% (4.0% APRC)
10-year	3.5% (3.7% APRC)	3.5% (3.8% APRC)	3.7% (4.0% APRC)

Notes: Figure shows page one of an example mandatory disclosure letter sent to the control group. Letterhead with customer and bank information is omitted.

Figure A4: Example Treatment-Group Disclosure Letter

Mortgage Account Number: 1234567

You may be able to save money on your mortgage

Dear John,

Your current mortgage interest rate is a standard variable rate of 4.25%. We want to make sure you are getting the best deal and we may have a lower interest rate for your mortgage.

Current monthly repayment at 4.25%:	€717
Potential monthly repayment at 2.9% fixed:	€586
Estimated difference in monthly repayments	-€131
Potential difference over the year:	-€1,572

- We have a range of interest rates that could save you money.
- Our lowest rate is a fixed rate of 2.9%, which could result in an immediate monthly saving to you of about €131. Over the course of a full year, that's approximately €1,572 in savings.
- Below, we outline the full range of interest rate options currently available, along with the next steps to take if you wish to choose one of these alternative options.

Explaining the tables below

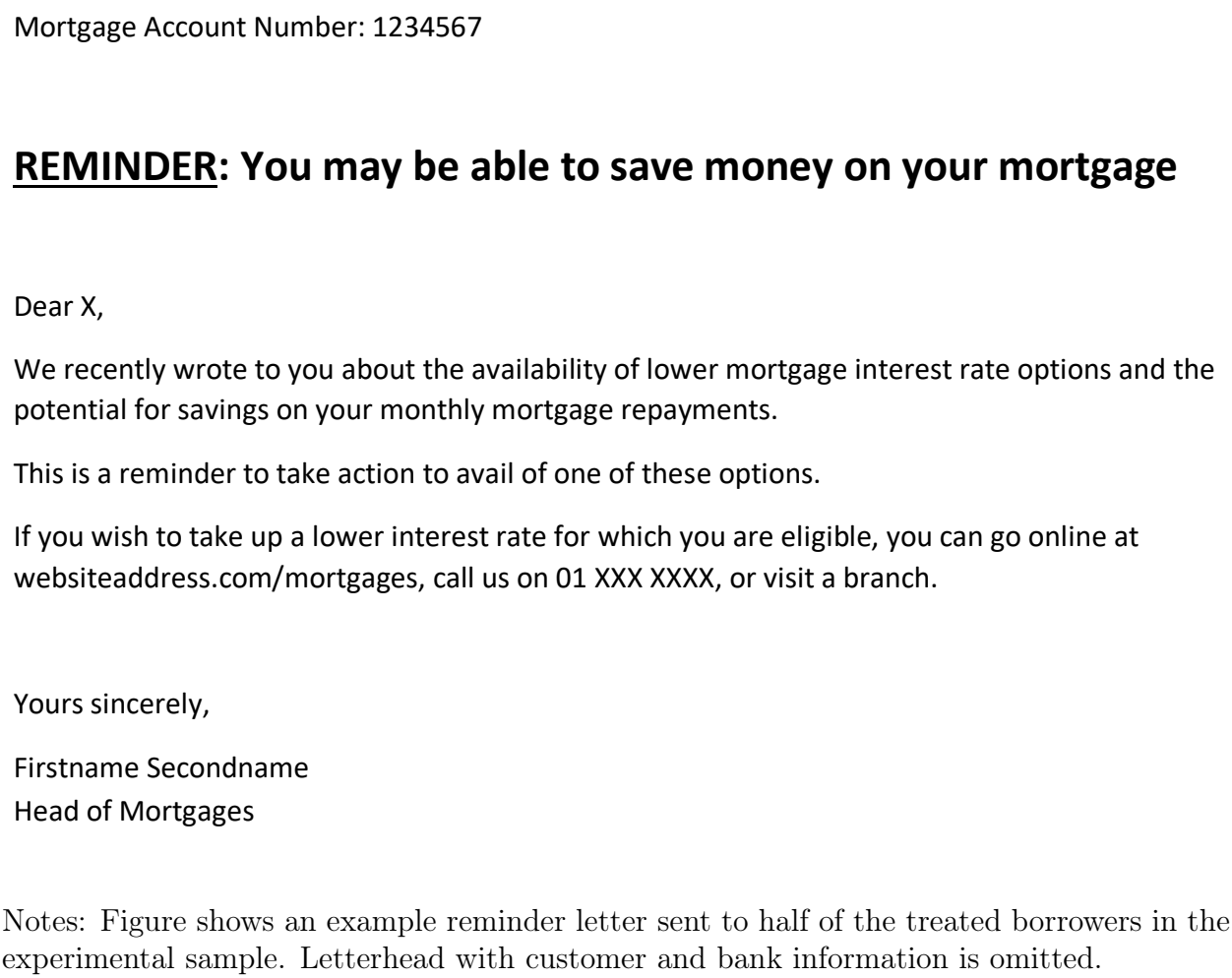
These tables show you the interest rates along with the Annual Percentage Rate of Charge (APRC). We explain APRC at the end of this letter. The rates may vary by Loan to Value (LTV) ratio. We also explain LTV at the end of this letter.

Fixed interest rates

Fixed interest rate options	Loan to Value Up to 60%	Loan to Value 61-80%	Loan to Value over 80%	Difference in monthly repayments	Difference over the year
1-year	2.9% (3.9% APRC)	2.9% (4.2% APRC)	2.9% (4.4% APRC)	-€131	-€1,572
2-year	2.9% (3.8% APRC)	2.9% (4.0% APRC)	2.9% (4.3% APRC)	-€131	-€1,572
3-year	3% (3.7% APRC)	3% (3.9% APRC)	3% (4.1% APRC)	-€123	-€1,476
5-year	3.2% (3.7% APRC)	3.2% (3.8% APRC)	3.2% (4.0% APRC)	-€108	-€1,296
10-year	3.5% (3.7% APRC)	3.5% (3.8% APRC)		-€84	-€1,008
10-year			3.7% (4.0% APRC)	-€67	-€804

Notes: Figure shows page one of an example redesigned mandatory disclosure letter sent to Treatment group 2. Letterhead with customer and bank information is omitted.

Figure A5: Example Reminder Letter



Mortgage Account Number: 1234567

REMINDER: You may be able to save money on your mortgage

Dear X,

We recently wrote to you about the availability of lower mortgage interest rate options and the potential for savings on your monthly mortgage repayments.

This is a reminder to take action to avail of one of these options.

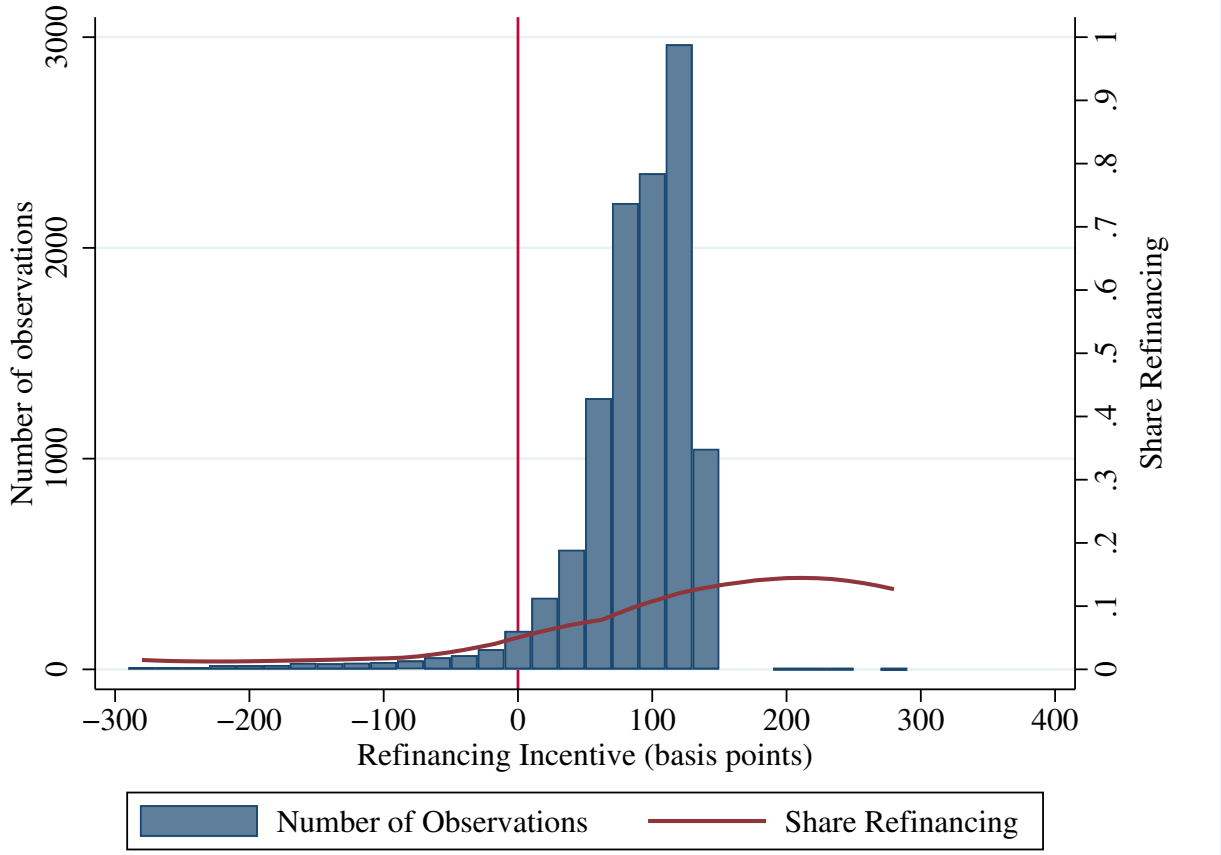
If you wish to take up a lower interest rate for which you are eligible, you can go online at websiteaddress.com/mortgages, call us on 01 XXX XXXX, or visit a branch.

Yours sincerely,

Firstname Secondname
Head of Mortgages

Notes: Figure shows an example reminder letter sent to half of the treated borrowers in the experimental sample. Letterhead with customer and bank information is omitted.

Figure A6: Distribution of Refinancing Incentives



Notes: Figure plots a histogram (left axis) of the refinancing incentives calculated in the experimental data using the model of Agarwal et al. (2013) along with the share of each histogram bin that refinanced within four months of initial treatment (right axis).

Table A1: Descriptive Statistics Across Treatment Cells

Treatment group	Control	V1	V2	V3	V4	V5	V6
Dublin	0.20	0.21	0.20	0.19	0.21	0.19	0.19
	(0.40)	(0.40)	(0.40)	(0.39)	(0.41)	(0.39)	(0.39)
Borrower age	49.74	50.29	49.80	50.08	50.13	50.10	49.87
	(9.26)	(9.37)	(9.22)	(9.26)	(9.61)	(9.30)	(9.40)
First Time Buyer	0.41	0.40	0.40	0.40	0.40	0.38	0.40
	(0.49)	(0.49)	(0.49)	(0.49)	(0.49)	(0.49)	(0.49)
Mortgage balance	83,503	81,425	80,098	81,530	81,020	81,351	82,548
	(84,125)	(89,826)	(80,088)	(90,834)	(91,867)	(98,831)	(87,424)
Interest rate	0.042	0.042	0.042	0.042	0.042	0.042	0.042
	(0.003)	(0.002)	(0.002)	(0.002)	(0.002)	(0.003)	(0.002)
Years to maturity	13.87	13.21	13.21	13.25	13.36	13.16	13.38
	(8.54)	(8.54)	(8.47)	(8.48)	(8.50)	(8.41)	(8.50)
1-Year savings	1,044	1,037	1,007	1,021	1,022	1,018	1,037
	(1,010)	(1,155)	(980)	(1,137)	(1,101)	(1,178)	(1,065)
Covid forbearance	0.09	0.07	0.08	0.07	0.09	0.09	0.08
	(0.28)	(0.25)	(0.28)	(0.25)	(0.28)	(0.29)	(0.28)
Observations	1,613	1,587	1,616	1,602	1,629	1,585	1,568

Notes: Table reports means and standard deviations in parentheses of mortgage borrower characteristics in each treatment and control group. Dublin is an indicator for whether the mortgaged property is located in Dublin. Borrower age of the oldest borrower on the mortgage. First-time buyer indicates whether the borrower is a first time-buyer. Mortgage balance is amount outstanding on loan at the time of experiment in euros. Interest rate is the interest rate applicable on the loan at the outset of the experiment. 1-year savings is the amount in euros of savings available to the borrower in the first year after refinancing to the best available rate. Covid forbearance indicates whether the borrower was using Covid payment break (introduced in Ireland in March 2020 to alleviate short-term liquidity constraints faced by borrowers experiencing financial difficulties due to the impact of the pandemic).

Table A2: Test of Covariate Balance by Treatment Arm

Treatment group	V1	V2	V3	V4	V5	V6
Dublin	0.011	0.002	-0.018	0.022	-0.009	-0.017
	(0.023)	(0.023)	(0.023)	(0.023)	(0.023)	(0.023)
Borrower age	0.001	-0.002	-0.000	0.000	-0.001	-0.001
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
First Time Buyer	0.007	0.000	0.001	0.003	-0.026	-0.001
	(0.020)	(0.020)	(0.020)	(0.020)	(0.020)	(0.020)
Mortgage balance	-0.000**	-0.000	0.000	-0.000	0.000	-0.000
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Interest rate	-6.971	-0.800	2.058	0.263	2.649	-3.091
	(4.784)	(4.894)	(4.825)	(4.916)	(4.510)	(4.922)
Years to maturity	-0.003*	-0.004**	-0.003*	-0.002	-0.003**	-0.003*
	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)
1-year savings	0.063**	0.011	-0.004	0.013	-0.002	0.026
	(0.028)	(0.030)	(0.027)	(0.030)	(0.023)	(0.031)
Covid forbearance	-0.072**	-0.013	-0.071**	-0.002	0.010	-0.017
	(0.033)	(0.031)	(0.033)	(0.031)	(0.031)	(0.032)
Constant	0.790***	0.684***	0.470**	0.510**	0.491**	0.710***
	(0.214)	(0.222)	(0.218)	(0.221)	(0.207)	(0.222)
Equality p-value	0.054	0.453	0.263	0.835	0.408	0.740
Observations	3,200	3,229	3,215	3,242	3,198	3,181
R-squared	0.005	0.002	0.003	0.001	0.003	0.002

Notes: Table reports estimates of a regression of treatment status (an indicator for the treatment heading each column) on a vector of covariates. Each column's sample consists of participants assigned to the control group and the indicated treatment group. Equality p-value is from the F-test for joint equality of all of the slope coefficients in a given column. Robust standard errors in parentheses. *** $p<0.01$, ** $p<0.05$, * $p<0.1$.

Table A3: Internal Refinancing Treatment Effects: Sample Including Borrowers in Arrears

	(1)	(2)	(3)	(4)
Disclosure Redesign Treatment	0.035***	0.038***	0.018**	0.021***
	(0.008)	(0.008)	(0.008)	(0.008)
Treatment × Reminder			0.035***	0.035***
			(0.007)	(0.006)
Constant	0.087***	-0.321***	0.087***	-0.317***
	(0.007)	(0.065)	(0.007)	(0.065)
Controls		✓		✓
Observations	11,532	11,532	11,532	11,532
R-squared	0.001	0.043	0.004	0.045

Notes: Table reports treatment effects on an indicator variable equal to one if the borrower internally refinanced, defined as a borrower changing their mortgage product with the partner bank within four months of initial treatment, and zero otherwise. Unlike Table 3, sample includes borrowers in arrears (defined as 90+ days past due). Disclosure Redesign Treatment is an indicator that the borrower was randomly assigned to one of the six treatment arms. Reminder is an indicator for whether that borrower received a follow-up reminder letter 4-6 weeks after the initial treatment as in Appendix Figure A5. Control variables in columns 2 and 4 are listed in Table 2 with the addition of an indicator for whether the borrower was in arrears. Robust standard errors in parentheses. *** $p<0.01$, ** $p<0.05$, * $p<0.1$.

Table A4: External Refinancing Treatment Effects

	(1)	(2)	(3)	(4)
Disclosure Redesign Treatment	-0.005	-0.005	-0.003	-0.003
	(0.005)	(0.005)	(0.005)	(0.005)
Treatment × Reminder			-0.003	-0.003
			(0.004)	(0.004)
Constant	0.037***	0.177***	0.037***	0.177***
	(0.005)	(0.047)	(0.005)	(0.047)
Controls		✓		✓
Observations	11,200	11,200	11,200	11,200
R-squared	0.000	0.007	0.000	0.007

Notes: Table reports treatment effects on external refinancing, defined as a borrower prepaying their mortgage with our partner bank and taking out a mortgage with another provider. Control variables in columns 2 and 4 are listed in Table 2. Robust standard errors in parentheses. *** $p<0.01$, ** $p<0.05$, * $p<0.1$.

Table A5: Covid Classification of Employment Sectors

Sector Covid Status Category	Employment Sector
1. Working from home (WFH)	J: Information and communication K: Financial and insurance M: Professional, scientific, technical O: Public administration S: Other service activities
2. Business as usual (BAU)	A: Agriculture, forestry, fishing D: Electricity, gas supply H: Transport and storage
3. At home not working (AHNW)	C: Manufacturing F: Construction G: Wholesale and retail trade, vehicle repair I: Accommodation and food services

Notes: Table reports the classification of employment sectors into groups more likely to be working from home (WFH), experiencing business as usual (BAU), and at home not working (AHNW). Prefix letters represent Eurostat Statistical Classification of Economic Activities in the European Community, Rev. 2 (2008) available at Eurostat.

Table A6: Internal Refinancing Treatment Effects by Employment Sector

Employment sector	WFH	BAU	AHNW
Disclosure Redesign Treatment	0.015	0.028	0.026
	(0.011)	(0.040)	(0.017)
Treatment × Reminder	0.046***	0.080*	0.066***
	(0.011)	(0.043)	(0.017)
Constant	0.100***	0.079*	0.075***
	(0.0009)	(0.034)	(0.014)
Observations	7,218	494	2,548
R-squared	0.003	0.008	0.006

Notes: Table reports internal refinancing treatment effects within the employment sector subgroups more likely to be working from home (WFH), experiencing business as usual (BAU), and being at home but not working (AHNW) during the estimation window. See Appendix Table A5 for our employment sector classification scheme. Dependent variable is an indicator variable equal to one if the borrower internally refinanced, defined as a borrower changing their mortgage product with the partner bank within four months of initial treatment, and zero otherwise. Disclosure Redesign Treatment is an indicator that the borrower was randomly assigned to one of the six treatment arms. Reminder is an indicator for whether that borrower received a follow-up reminder letter 4-6 weeks after the initial treatment as in Appendix Figure A5. Robust standard errors in parentheses. *** $p<0.01$, ** $p<0.05$, * $p<0.1$.

Table A7: Mixture ML Estimates: Robustness to Alternative Prepayment Assumptions

Parameter	(1)	(2)	(3)	(4)	(5)
Incentive Sensitivity (β)	-125.48***	-1.44***	-1.63***	-1.63***	-1.72***
	(1.56)	(0.02)	(0.05)	(0.05)	(0.05)
Fixed Cost of Refinancing (γ)		9.66***	8.05***	8.04***	9.01***
		(0.03)	(0.04)	(0.04)	(0.15)
Inattention Constant (δ_0)			0.57***	1.05***	0.78***
			(0.05)	(0.11)	(0.13)
Treatment on Inattention (δ_1)				-0.32**	-0.35**
				(0.13)	(0.14)
Reminder on Inattention (δ_2)				-0.45***	-0.51***
				(0.09)	(0.10)
Fixed Cost Controls					✓
Observations	11,200	11,200	11,200	11,200	11,200

Notes: Table reports maximum likelihood estimates of the mixture model of inattentive refinancing described in the text, but where we adjust the model parameters to take account of the typically short fixation periods which predominate in Irish (and UK) mortgage markets, as distinct from the long-term fixation periods to which the Agarwal et al. (2013) model is originally attuned. Incentive Sensitivity is the coefficient on the Agarwal et al. (2013) refinancing incentive described in section 6 using the parameters defined by Table 6, with coefficient $\exp(\beta)$. The fixed cost of refinancing constant γ estimates an average fixed cost term to rationalize observed refinancing variable. The fixed-cost controls allow for differences across groups in the estimated fixed cost of refinancing. The inattention constant δ_0 allows the inattention index in (8) to have a constant term. The inattention treatment effects allow borrowers who treated with redesigned disclosures (δ_1) and disclosure reminders (δ_2) to have different levels of attention. Age is demeaned. Covid indicates whether the borrower was approved for mortgage-payment forbearance with a Covid hardship. Robust standard errors in parentheses. *** $p<0.01$, ** $p<0.05$, * $p<0.1$.