SUPPLEMENTAL APPENDIX

Household-Level Responses to the European Energy Crisis

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A Data appendix

This section provides a detailed description of the datasets and variables. The data are proprietary and accessible through Statistics Finland. All identifiers (such as social security numbers, electricity meter numbers and firm identifiers) are pseudonymized. The main outcome variables are measured at the monthly level for the period from March 2022 to June 2023. To examine heterogeneous responses across different types of households, we use a more comprehensive cross-sectional background dataset from 2021, one year prior to the crisis. Figure A.1 illustrates how the final dataset is constructed.

First, we construct households by using preliminary population statistics, then link each individual's background, earnings, benefits, and default data to a specific household using a pseudonymized social security number. Electricity data are linked to households through the social security number of the individual who signed the electricity contract. Once the electricity data are linked, we aggregate other data to the household level. The aggregation process involves the following steps:

- **Summable variables:** Variables such as earnings, pensions, and benefits are summed across all household members to produce household-level totals.
- **Indicator variables:** For binary variables, such as default status, the household is assigned a value of one if any household member meets the condition. For example, if at least one household member has a recorded default, the household-level default variable is set to 1.
- **Categorical variables:** These are determined based on the characteristics common to the household members (e.g. electricity heating). For individual-level characteristics such as age (in Table B.1), we use the household member with highest earning.

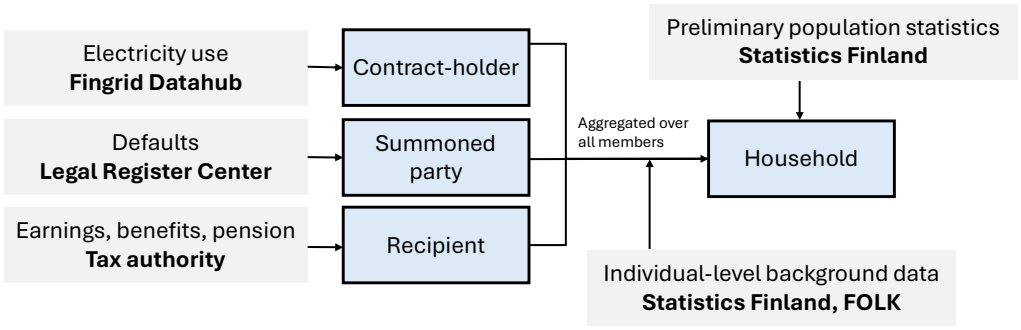


Figure A.1: Illustration of the datasets

In total, 2.5 million households in Finland have an electricity contract (excluding the autonomous region of Åland, which accounts for approximately 0.5% of the total population). Our identification strategy is based on the expiration of fixed-price contracts, and the main sample consists of households with a two-year fixed-price contract at the start of the study period (March 2022). Approximately one-third of households have such contracts, reducing the sample size to 926,000 households. Treated households are those whose contracts expire during the crisis period (August 2022–January 2023), resulting in 133,972 unique treated households. We identify a nearest neighbor match for each treated household, bringing the total number of unique households – including both treated and control households – to 231,491. Households are matched within cohorts, and the same household may serve as a control in multiple treated cohorts. In the main analysis we use household-cohort (ih) fixed effects to treat these controls separately.

Household-level background datasets The analysis is conducted at the household-dwelling level (or 'households' for short). A household-dwelling unit consists of the permanent occupants who consistently use a given electricity meter. If the electricity meter changes, for instance, if a household moves, they are dropped from our sample. However, fewer than 1% of households in our matched dataset move during the period of analysis. Households are constructed at the monthly level using preliminary population statistics, which provide monthly information on all individuals residing in Finland, including their place of residence and pseudonymized building, dwelling, and social security numbers.

We use Statistics Finland's FOLK (income and basic dataset modules) for 2021 (the year preceding the crisis) for the background information. The FOLK module for basic data contains annual individual-level data from the statistics on the structure of the population, employment statistics, and some data from the family statistics, as well as the statistics on the educational structure of the population. The FOLK income module contains annual data on individuals' disposable income, current transfers received, wealth, and outstanding debts. Disposable income in 2021 comprises labor and capital earnings as well as benefits, net of taxes. Households are divided into income quintiles based on household disposable income per consumption unit in 2021. The upper quintile cutoffs for annual disposable incomes in our final sample are €18,300 (1st quintile), €24,100 (2nd quintile), €29,900 (3rd quintile) and €38,100 (4th quintile).

Electricity consumption and contract data The data are obtained from Fingrid Datahub, a centralized information exchange system for Finland's electricity retail market. Datahub is operated by Fingrid Oyj, Finland's transmission system operator (TSO). A more detailed description of the dataset is provided in Ahlvik et al. (2023). The dataset includes monthly electricity consumption and contract details at the electricity-meter level. The analysis uses the following

variables: the pseudonymized electricity meter number, the pseudonymized social security number of the contract holder, contract type (fixed-term or temporary), consumer type (e.g., apartment block, detached house, farm), and whether the dwelling relies on electricity for heating.

Contract-level electricity prices were not available to us through Datahub. Instead, when calculating the residual consumption and savings term, we use monthly average electricity prices from the Finnish Energy Authority, categorized by contract type (variable price, 1-year, and 2-year fixed price) and user type (e.g., apartment type and fuse size). Electricity distribution companies and retailers are required to report existing prices to the Finnish Energy Authority, which then publishes monthly electricity price statistics by contract type (fixed-term, variable price, and other temporary contracts) and consumer type. For fixed-term contracts, we use the electricity price from the month the contract was signed for its entire duration (one or two years). Because the contract-type variable in the Datahub dataset does not distinguish between variable-price and other temporary contracts, we use the average price of these two contract types within each user type. The user types included in this study are: (i) K1: Apartment, no electric sauna stove, main fuse 1x25 A, (ii) K2: Detached house, electric sauna stove, no electric heating, electricity consumption 5,000 kWh/year, (iii) L1: Detached house, room-specific electric heating, main fuse 3x25 A, and (iv) L2: Detached house, partially storage-based electric heating, main fuse 3x25 A. The price we use also includes the average transmission fee by user type.

Income and benefits registers We obtain monthly earnings, pensions, and other benefits data from the Finnish Tax Administration's Incomes and Benefits Register, which serves as the source for the dependent variables in our analysis. The registers are electronic databases that record monthly reports on paid wages, pensions, and benefits. The datasets exclude sensitive information, such as social assistance payments. Furthermore, the reporting obligation does not cover most capital income or a self-employed person's earnings if they are insured under the Self-Employed Persons' Pensions Act or the Farmers' Pensions Act. Note that these limitations apply only to the monthly data and not to FOLK disposable income (for 2021), which we use as a background variable. The Incomes Register includes various income types, as well as the pseudonymized social security number and employer business identity code. The Benefits Register contains information on paid benefits and pensions. Benefits are reported similarly to income data, except that each benefit must be classified under a specific income type. We aggregate labor earnings and the net benefits at the monthly and household levels.

Court-reported payment defaults Data on payment defaults are obtained from the Finnish Legal Register Centre. The dataset includes individual-level and firm-level payment defaults enforced by district courts in Finland from 2016 to 2023. Each default record contains pseudonymized

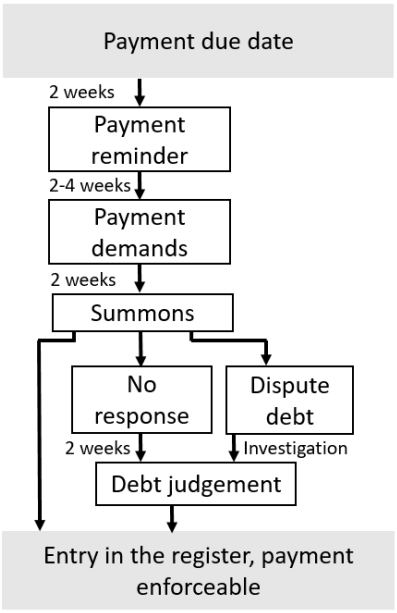


Figure A.2: The default process: from unpaid bills to enforceable payments

Notes: The figure, based on Luotonen et al. (2022), illustrates the progression of a payment default in Finland, beginning with the payment due date and progressing through reminders, demands, and court-issued summons. The process diverges depending on whether the debtor responds or disputes the debt. Time intervals between steps highlight the typical delays in the default resolution timeline.

register and social security numbers, along with details such as the subject matter of the default, the principal, interest, fees, penalties, and collection expenses. We link defaults to households based on the defaulter's social security number and include only individual-level enforced defaults in our analysis. The dataset also records the summons date, court decision date, and default status (annulled or enforced). We define the month of the summons date as the time of default, which typically occurs 1–2 months after a missed payment. We use the summons date instead of the decision date due to the relatively short time window of our data and the potentially long interval between the summons and the court's decision.

Figure A.2 illustrates the process from an unpaid bill to an enforced default. We cannot separately identify utility bill defaults in the dataset, but there is a broader category for services that includes utility bills. However, financial distress may lead households to default on other types of bills – for instance, they may rely on payday loans to cover expenses. Therefore we include defaults for all purposes and calculate each individual's total accumulated defaults (including interest, penalties, and other fees) starting from March 2022. We construct an indicator variable equal to one if an individual – or any individual in a household – has a default in the register after March 2022 and zero otherwise.

B Additional results and robustness analyses

We present the following robustness checks, organized in the order they appear in the text.

Descriptive statistics, balance across groups and matching To address observable differences between the treatment and control groups, we perform one-to-one nearest-neighbor matching, pairing each treated household with one control household based on heating technology, electricity use, default indicators, benefits, earnings, and earnings indicator (capturing labor force participation) for the period March–July 2022 following Goldschmidt and Schmieder (2017); Adams-Prassl et al. (2024). The final matched dataset includes 231,491 households.

Appendix Table B.1 reports mean values and standard deviations of outcomes and background characteristics–averaged over the first five months of the pre-crisis period (March–July 2022)–for treated households, the matched control group, the unmatched control group, and the full population. Standardized mean differences (SMD) are reported relative to the treated group. Between the treated and matched control the outcome variables (first four rows) are very similar across groups, with all SMDs well below 0.1. These differences fall well within conventional thresholds (see Imbens and Rubin, 2015). For background variables, the matched controls (columns 2–3) are closely aligned with the treated group, whereas comparisons with the full control group (columns 4–5) and the full population (columns 6–7) reveal larger differences, particularly in pensions, age of the household head and the share of households with heads over 70. In the full population, electricity consumption, electric heating and tenancy status also differ. This pattern provides evidence that treatment – signing contracts that expire amid the crisis – is not entirely random; a problem that we deal with by matching. Despite these quantitative differences, we believe that the estimated treatment effects (on the treated) generalize reasonably well for the entire population.

The outcomes of matching are shown in Table B.1. We find good balance also for variables that are not used for matching, and the observed variables indicate that our matched control group is valid. Figure B.6 demonstrates matching visually using the months before the crisis (March-July 2022). The continuous variables, electricity use (panel a), earnings (panel b), and benefits (panel c) fall nicely on the 45-degree line except at the highest and lowest values. The binary variables, electricity heating (panel d), earnings indicator (panel e), and default indicator (panel f) are almost perfectly replicated by the matched control group.

Placebo tests, robustness analyses and additional results We provide a series of robustness analyses and additional results, and present the dynamic treatment effects in the figures as described below.

Figure B.7 reports results based on a simplified matching algorithm that matches treated and

control households using average values from the pre-crisis period (March–July 2022) – heating technology, electricity use, default indicators, benefits, earnings, and indicators of labor force participation – instead of monthly observations. The results are shown for the August cohort, and we find patterns consistent with those in the main analysis.

Our analysis compares treated households whose contracts end during the crisis (August 2022-January 2023) to the matched control group, whose contracts end a year after the crisis (from July 2023 on). A potential worry is that, if the control group has an anticipation effect for a long time (more than 6 months) before their contracts end, this would bias our results. To test this possibility, Figure B.8 shows 'placebo' event-study estimates for households whose contracts expired in June 2023, eleven months after the crisis became salient. We find no pre-treatment (anticipation) effects for any outcome: these households knew about contract ending, but the date was distant enough that they did not adjust their behavior. When expiration drew near, prices had already fallen (see Figure 1). This supports the Stable Unit Treatment Value Assumption (SUTVA), that the control group is not affected by the treatment.

The anticipation period is based on the *known* contract expiration in the future. Similar effects should not arise if contracts end abruptly. To test this, in Figure B.9, we compare households whose electricity retailer unexpectedly went bankrupt in September 2022, causing abrupt contract terminations, with households that knew in advance that their contracts would expire in September. Note that these households are not part of our main analysis, which only uses known contract expiration dates. Panel (a) of Figure B.9 shows results for households whose contracts ended due to bankruptcy. We find that these households did not reduce their electricity consumption before the termination date, even as the crisis unfolded, as they had no reason to expect that high prices would affect them. In contrast, panel (b) of Figure B.9 shows that households with scheduled September terminations began reducing consumption in advance of the termination date. The comparison supports the interpretation that households are forward-looking and react to known price increases.

In Figure B.10, we show the results for households who choose fixed-price contracts after their initial contract expires, and again report results for the August-cohort. The contract choice is endogenous: it correlates with observable household-level characteristics and past behavior (Vesterberg, 2018), and therefore should not be taken as causal evidence of different contract types. However, we can compare these results to those estimated with the full sample, which also includes households who switch to variable-price contracts, to make three observations. First, the electricity use response is more muted, indicating that households with lower demand responsiveness are more likely to rationally choose fixed-price contracts, consistent with the mechanism in Ito et al. (2023). Second, even for fixed-term contracts, the effect fades toward the summer months, indicating seasonality in electricity demand (as in Rubin and Auffhammer, 2024) that is roughly one third

compared to the winter months. Third, aside from the electricity use shown in panel (a), the results are largely similar across the two groups for the other outcome variables in panels (b)-(d).

Constructing bounds under sample selection To address potential bias from sample selection in our labor supply estimates, we follow the approach of Rathnayake et al. (2024) and construct bounds on the intensive margin estimates. The concern is that if treatment changes the probability of remaining in the sample, the difference-in-differences estimator does not identify a well-defined average treatment effect on the treated. We therefore restrict attention to the always-observed population and implement a trimming procedure following Lee (2009) and Rathnayake et al. (2024). The method exploits the distributional tails of outcome changes to bound the mean for always-observed households, recognizing that the observed sample also contains units who appear only when treated.

We first define the baseline using five pre-crisis months. For each cohort and post-treatment month, we trim outcome changes at rates determined by the estimated mixing proportions, and then re-estimate our specification in Equation (1) on the trimmed sample to obtain upper and lower bounds. We assume negative monotonicity: treatment weakly reduces the probability of remaining observed. In theory, the sign of this selection could be ambiguous, as income and substitution can have opposite effects on selection. Our extensive margin results show a small negative effect that is also visible in Figure 4. As discussed in Section 3.3, these effects seem to be driven by households with very modest initial earnings. Table B.2 reports the estimated bounds (columns 1 and 2) alongside the main estimates using the unrestricted sample (column 3, corresponding to column 3, Panel A of Table 1). We also present untrimmed estimates using only households that are observed in all periods (column 4), and extensive margin estimates (column 5). The results are shown for all households in Panel A. To focus on the selection into and out of employment, rather than retirement, we also separately study a subsample in Panel B, where we only keep households where the highest earner was born after 1957.

In Panel A, the upper bound is 0.0120 and statistically significant, whereas the lower bound is -0.0012 and not different from zero. The main estimate, 0.0062, falls within the calculated bounds. Estimates based on the restricted sample are smaller than the naive estimate. The extensive-margin effect is slightly negative and statistically significant, consistent with modest negative selection into observation. In Panel B, with the working-age population, both the lower and upper bounds remain above zero, indicating positive effects even under the most adverse selection scenario. For this subsample, the extensive-margin effect is negative but close to zero and not statistically significant, consistent with weaker selection than in the full population.

Table B.1: Descriptive Statistics and Balance Across Groups

Variable	Treated	Control	Std. diff	Full Control	Std. diff	Full Sample	Std. diff
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Panel A: Outcome variables							
Electricity use +	584.36	571.77	0.021	579.15	0.008	474.04	0.182
	(627.93)	(583.30)		(631.50)		(585.74)	
Payment defaults +	0.010	0.010	0.002	0.011	0.005	0.010	0.001
	(0.100)	(0.099)		(0.103)		(0.100)	
Labor earnings per worker +	3,774.4	3,761.5	0.003	3,820.6	0.010	3,721.7	0.009
	(5,003.9)	(3,250.5)		(4,470.8)		(6,162.9)	
Share of workers in household	0.416	0.419	0.007	0.452	0.086	0.451	0.085
	(0.415)	(0.416)		(0.414)		(0.423)	
Residual consumption and savings	3,267.8	3,217.4	0.015	3,440.4	0.041	3,212.8	0.012
	(3,538.6)	(3,029.6)		(4,807.8)		(5,549.8)	
Panel B: Demographic and background variables							
Share of indebted households	0.308	0.322	0.029	0.342	0.071	0.297	0.024
	(0.462)	(0.467)		(0.474)		(0.457)	
Household receives earnings +	0.574	0.574	0.001	0.622	0.099	0.605	0.064
	(0.495)	(0.495)		(0.485)		(0.489)	
Household receives pension	0.422	0.422	0.000	0.362	0.124	0.349	0.151
	(0.494)	(0.494)		(0.481)		(0.477)	
Electric heating +	0.607	0.607	0.000	0.575	0.064	0.499	0.218
	(0.488)	(0.488)		(0.494)		(0.500)	
Households with children	0.259	0.252	0.018	0.284	0.055	0.257	0.005
	(0.438)	(0.434)		(0.451)		(0.437)	
Age of household head	59.03	58.56	0.028	55.80	0.186	55.10	0.221
	(16.97)	(17.31)		(17.69)		(18.53)	
Household head over 70	0.317	0.318	0.003	0.266	0.113	0.264	0.116
	(0.465)	(0.466)		(0.442)		(0.441)	
Tenant	0.213	0.214	0.002	0.243	0.072	0.315	0.233
	(0.409)	(0.410)		(0.429)		(0.464)	
Rural household	0.664	0.653	0.023	0.628	0.074	0.578	0.177
	(0.472)	(0.476)		(0.483)		(0.494)	
N	669,995	629,835		1,471,509		12,245,863	

Notes: Means and standard deviations (in parentheses) of outcome variables and background characteristics are reported for treated group (column 1), matched control group (column 2), full control groups consisting of the entire sample of households with a 2-year fixed-price contract in March 2022 (column 4), and the entire population of households that have an electricity contract in Finland (column 6). Columns (3), (5), (7) show the standardized mean differences relative to the treated group (column 1). Outcomes are averaged over the five months before the electricity crisis (March–July 2022). "+" indicates that the variable is used in matching. Panel A shows the outcome variables in the main analysis. Labor earnings, and residual consumption and savings are in euros per month; electricity consumption is in kWh per month. Payment default is the indicator turning one after at least one default. Share of workers is the number of workers divided by household size. Indebted households are those with debt per annual disposable income exceeds quintile median in 2021. Pension, tenants, rural household and electric heating are indicators turning one if anyone in the household fulfills the criterion. The household head is defined as the member with the highest labor earnings. N is the number of household-month observations.

Table B.2: Bounds under sample selection

	Upper bound	Lower bound	Main estimate	Always observed	Extensive margin
	(1)	(2)	(3)	(4)	(5)
Panel A: Full population					
Treatment	0.0120***	-0.0012	0.0062***	0.0034*	-0.0017*
	(0.0022)	(0.0021)	(0.0017)	(0.0014)	(0.00077)
Anticipation	0.0062**	-0.0040	0.0030	0.0010	-0.0020**
	(0.0022)	(0.0021)	(0.0019)	(0.0015)	(0.0007)
N	2,024,240	2,024,229	2,343,273	2,020,096	4,116,737
Panel B: Working age					
Treatment	0.0127***	0.0005	0.0071***	0.0041**	-0.0008
	(0.0022)	(0.0021)	(0.0018)	(0.0014)	(0.0010)
Anticipation	0.0069**	-0.0022	0.0040*	0.0017	-0.0014
	(0.0022)	(0.0021)	(0.0019)	(0.0015)	(0.0008)
N	1,870,442	1,870,431	2,154,730	1,926,069	2,571,500

Notes: This table reports coefficients for the intensive margin of labor earnings. Columns (1)–(2) present upper and lower bounds constructed under monotone sample selection, following Lee (2009) and Rathnayake et al. (2024). The trimming procedure restricts attention to the always-observed population and accounts for potential bias from selective attrition. Columns (3)–(5) report the naïve estimate without trimming, estimates using households always observed, and extensive-margin estimates, respectively. We assume negative monotonicity: treatment weakly reduces the probability of remaining observed. Panel A uses the full population; Panel B restricts to the working-age subsample defined as households where the highest earner was born after 1957. Standard errors are shown in parentheses. * p<.05, ** p<.01, *** p<.001.

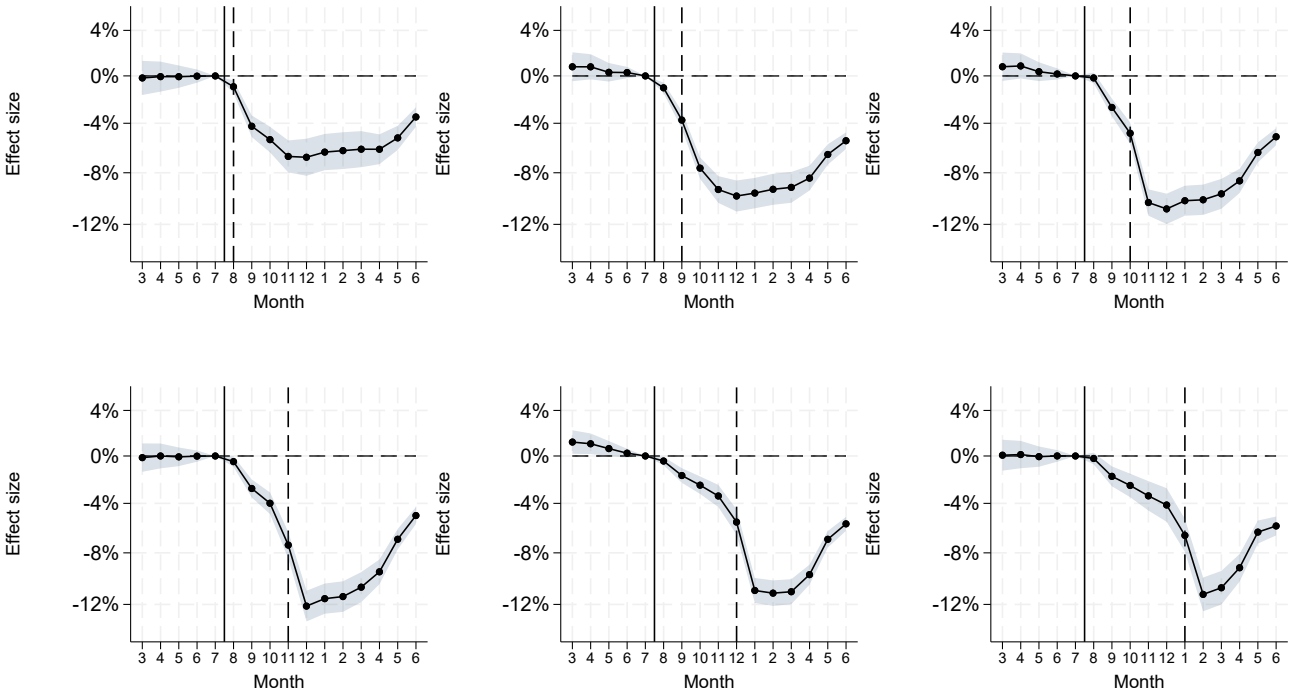


Figure B.1: Electricity use: individual event studies for all treatment cohorts

Notes: The figure reports coefficients per calendar month following equation (2) separately for all treatment cohorts (six panels). Dependent variable is logarithm of electricity use. Vertical dashed line denotes the contract expiration month. Shaded areas show the 95% confidence intervals. Standard errors are clustered at the household level.

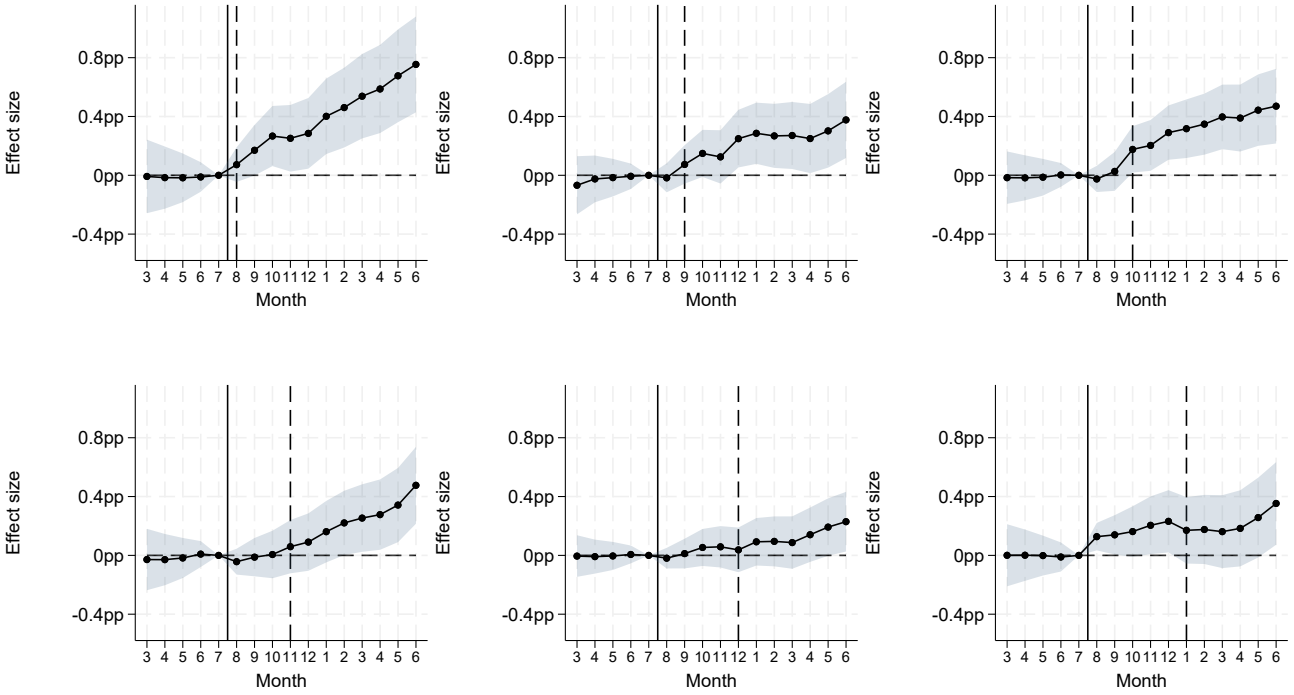


Figure B.2: Payment defaults: individual event studies for all treatment cohorts

Notes: The figure reports coefficients per calendar month following equation (2) separately for all treatment cohorts (six panels). Dependent variable is cumulative indicator for payment defaults. Vertical dashed line denotes the contract expiration month. Shaded areas show the 95% confidence intervals. Standard errors are clustered at the household level.

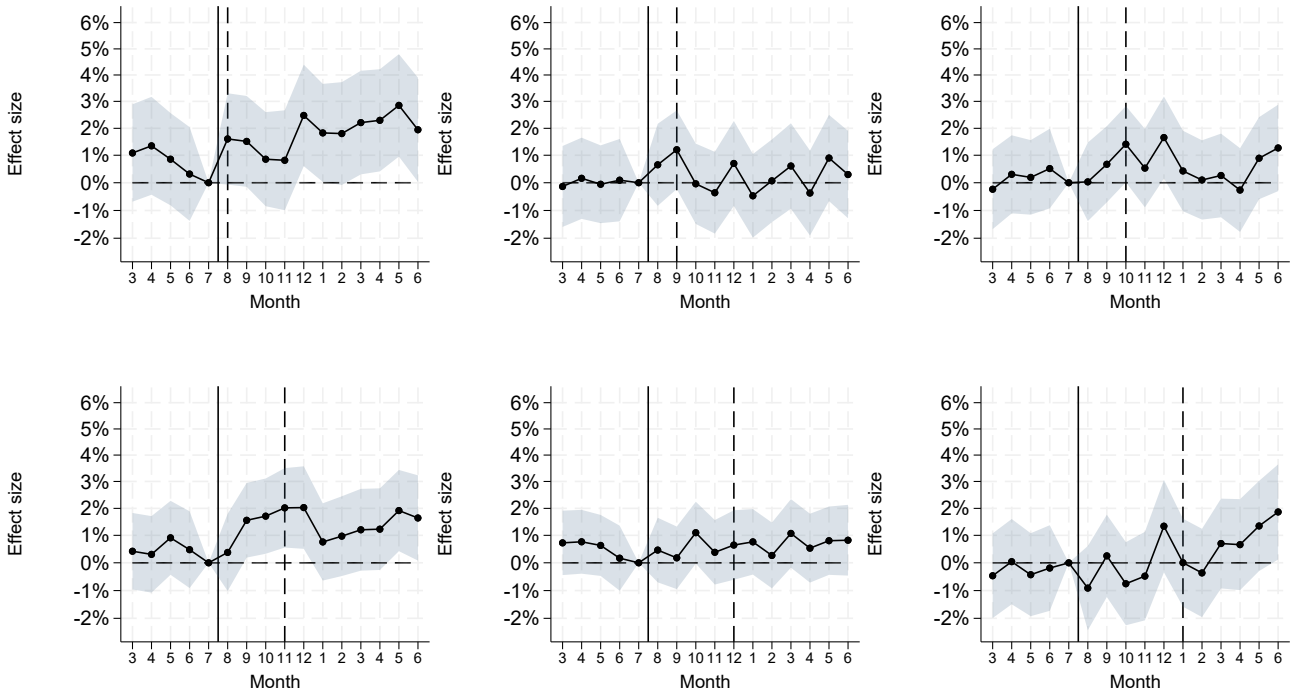


Figure B.3: Labor earnings: individual event studies for all treatment cohorts

Notes: The figure reports coefficients per calendar month following equation (2) separately for all treatment cohorts (six panels). Dependent variable is logarithm of labor earnings. Vertical dashed line denotes the contract expiration month. Shaded areas show the 95% confidence intervals. Standard errors are clustered at the household level.

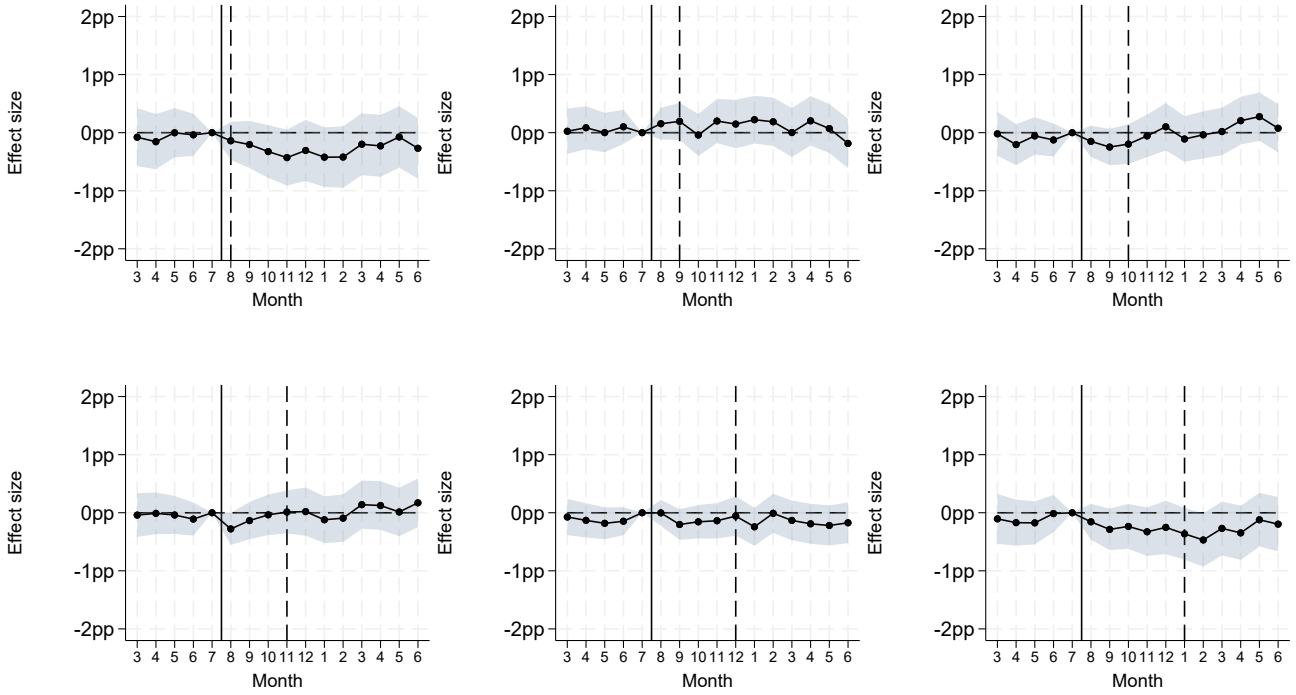


Figure B.4: Worker share per household: individual event studies for all treatment cohorts

Notes: The figure reports coefficients per calendar month following equation (2) separately for all treatment cohorts (six panels). Dependent variable is worker share per total household size. Vertical dashed line denotes the contract expiration month. Shaded areas show the 95% confidence intervals. Standard errors are clustered at the household level.

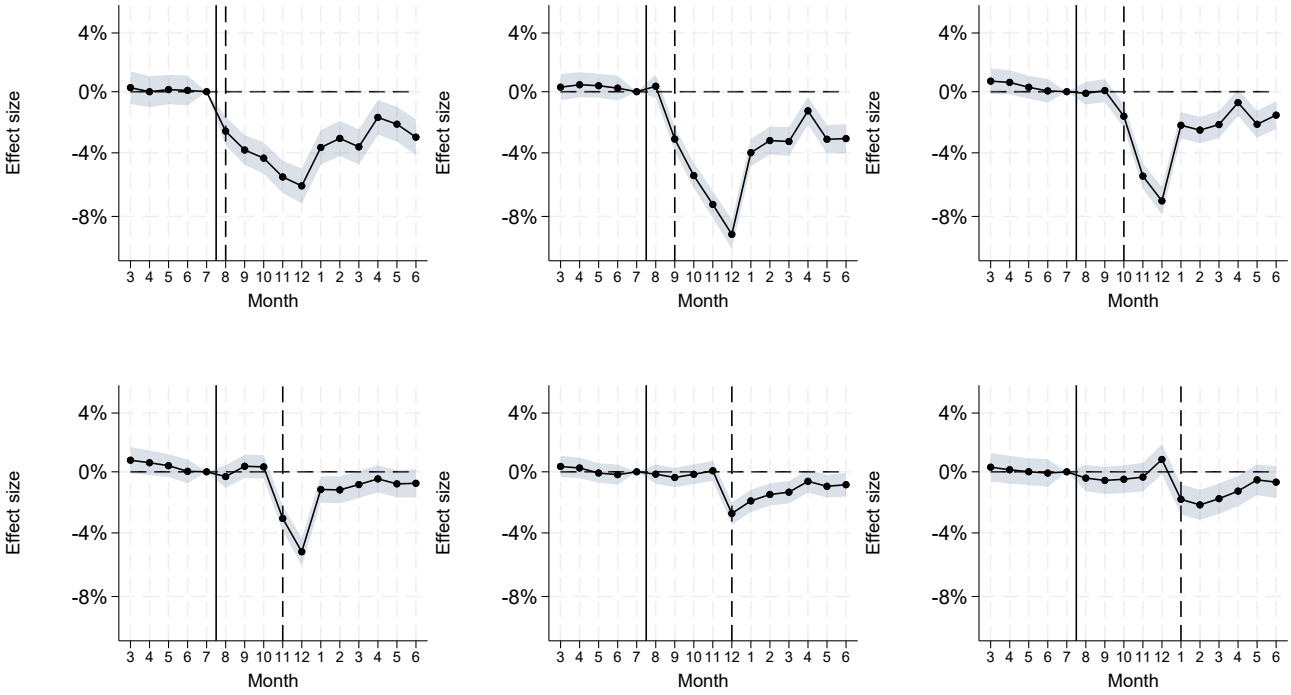


Figure B.5: Residual consumption and savings: individual event studies for all treatment cohorts

Notes: The figure reports coefficients per calendar month following equation (2) separately for all treatment cohorts (six panels). Dependent variable is logarithm of the imputed residual consumption and savings. Vertical dashed line denotes the contract expiration month. Shaded areas show the 95% confidence intervals. Standard errors are clustered at the household level.

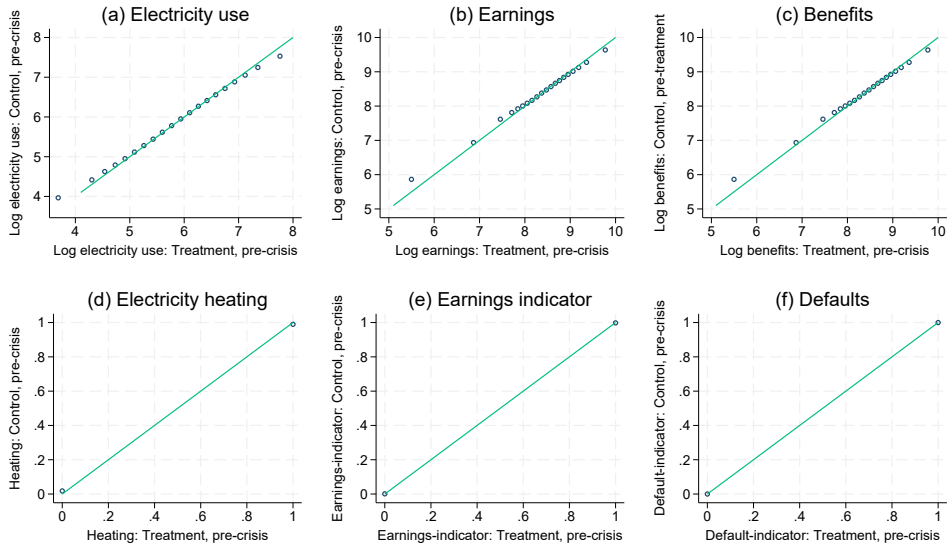


Figure B.6: Binscatter plot of treatment vs. matched control groups

Notes: The figure shows a binscatter plot of treatment group and the matched control group values of electricity use, earnings, benefits in logs and electricity heating, earnings and defaults indicator in March - July 2022. If both sets of households have the same distribution of outcomes, the dots are close to the green 45-degree line. Panels (a)-(c) use logarithmic transformations, panel (d)-(f) use an indicator which is either zero or one.

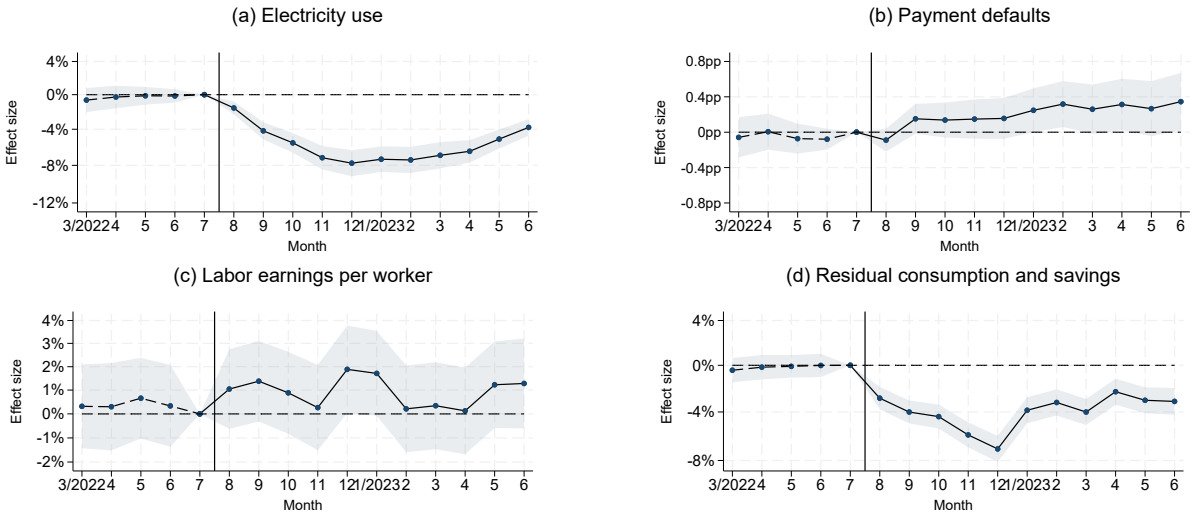


Figure B.7: Timing of the treatment effects for simple matching

Notes: The figure reports coefficients per calendar month for the four main variables of interest, (a) the logarithm of electricity use, (b) an indicator for defaults, (c) the logarithm of labor earnings, and (d) the logarithm of residual consumption using simple matching where matching is based on average of the pre-crisis months for the same variables: heating technology, electricity use, default indicators, benefits, earnings, and earnings indicator. Shaded areas show the 95% confidence intervals. Standard errors are clustered at the household level.

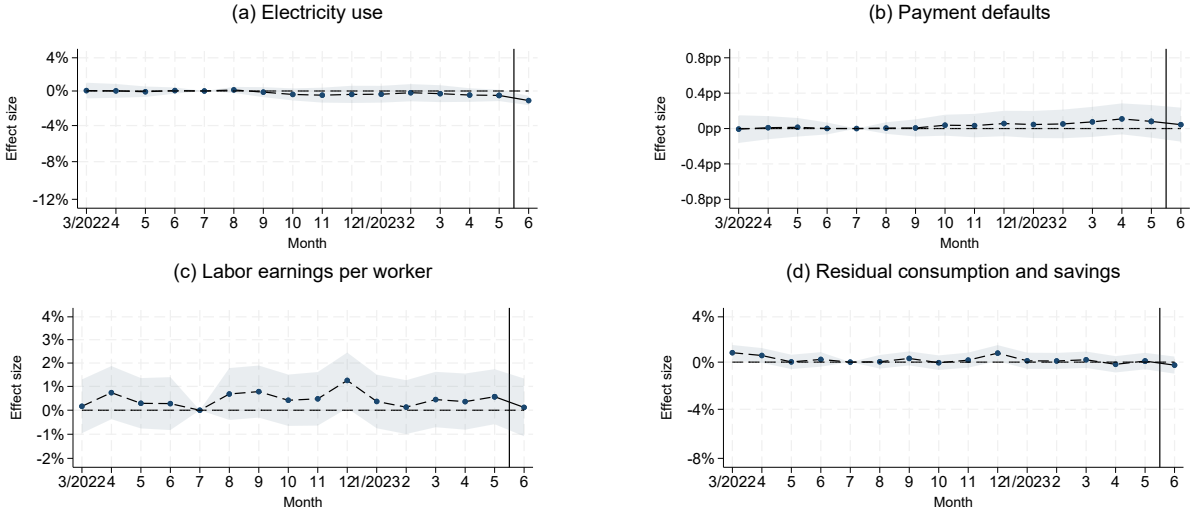


Figure B.8: Placebo: treatment effects for households whose contract ends in June 2023

Notes: The figure reports coefficients per calendar month for the four main variables of interest, (a) the logarithm of electricity use, (b) an indicator for defaults, (c) the logarithm of labor earnings, and (d) the logarithm of residual consumption for the households whose contract ends in June 2023. Shaded areas show the 95% confidence intervals. Standard errors are clustered at the household level.

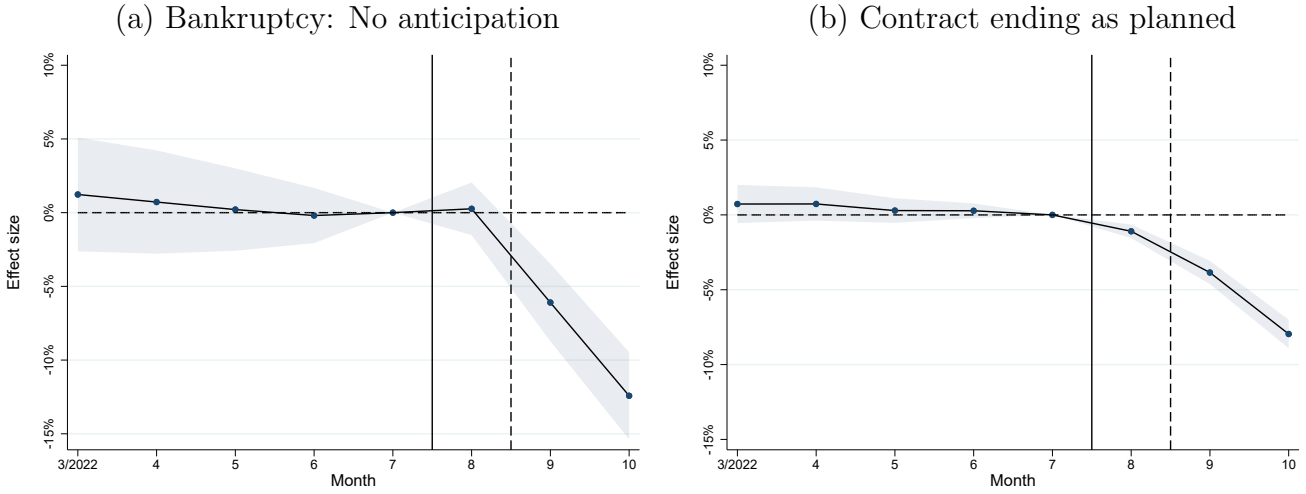


Figure B.9: Robustness for anticipation: Impact of unexpected and expected contract termination on electricity consumption

Notes: The left panel (a) shows households with abrupt contract terminations due to the bankruptcy of the electricity retailer in September 2022. The right panel (b) shows households that knew the termination date in advance, corresponding to our main event study Figure 2. Shaded areas show 95% confidence intervals and standard errors are clustered at the household level.

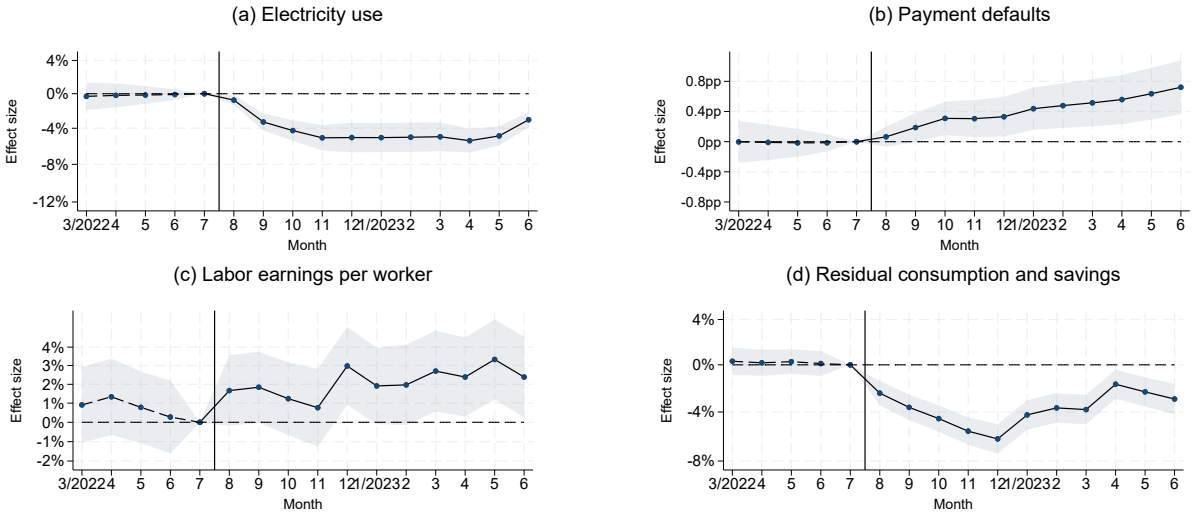


Figure B.10: Main results for households that select a fixed-price contract after initial contract expiration

Notes: This Figure shows the effects for four main outcome variables following equation (2) for only households who choose a fixed-price contract after their initial contract expires. The figure reports coefficients per calendar month for the four main variables of interest, (a) the logarithm of electricity use, (b) an indicator for defaults, (c) the logarithm of labor earnings, and (d) the logarithm of residual consumption. We show results only for households whose contracts end in August. Shaded areas show 95% confidence intervals and standard errors are clustered at the household level.

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